

Agency Costs and Strategic Speculation in the U.S. Stock Market

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This study investigates the notion that agency-driven information asymmetry may affect a firm's stock liquidity. I postulate that less uncertainty about managerial agency problems may enhance liquidity provision by lowering dealers' perceived adverse selection risk from trading with better-informed speculators. Consistent with my conjecture, I find that the staggered adoption of antitakeover provisions across U.S. states in the 1980s and 1990s — a plausibly exogenous shock unambiguously reducing the threat of (and speculators' information advantage about) value-enhancing intervention — robustly *improves* the stock liquidity of affected firms relative to peer firms, especially at prior *high* fundamental or agency uncertainty and with *poor* governance. (*JEL* D22, G14, G34)

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The separation of ownership and control is one of the main features of the modern corporation. Since [Jensen and Meckling \(1976\)](#), a vast literature has investigated the relevance of the ensuing conflicts between principals (e.g., outside investors) and agents (e.g., managers) for a firm's financing and investment policies. This study introduces and provides novel causal evidence for the notion that managerial agency problems may also affect a firm's stock liquidity. Understanding the frictions affecting the quality of price formation in capital markets is among the

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most important endeavors in financial economics.¹ I make an original contribution to this understanding by showing that corporate governance may have significant, previously ignored externalities on those frictions and liquidity provision. In doing so, I bridge two areas of research, corporate finance and market microstructure, that have seldom interacted.²

I propose that a firm's agency problems may affect the provision of liquidity for its stocks by contributing to the perceived adverse selection risk from strategic, better informed speculation. Intuitively, agency problems are important determinants of firm value (Tirole 2001, 2006); hence, uncertainty about their severity magnifies speculators' informational advantage relative to such market participants as equity dealers or market-makers — e.g., leading to greater price impact of order flow (Kyle 1985) or wider bid-ask spreads (Glosten and Milgrom 1985; Easley and O'Hara 1987) — especially when frictions to governance or price formation are ex ante more severe. In the Internet Appendix, I formalize this argument in a novel stylized model of strategic trading based on Kyle (1985) in which the liquidation value of a firm's risky asset depends on private and potentially suboptimal managerial actions such that its equilibrium market depth is, ceteris paribus, decreasing in agency uncertainty exploited by informed speculation.

I test those implications in the U.S. stock market, where agency and adverse selection problems have been separately found to be important by much governance and microstructure research (e.g., see Hasbrouck 2007; Atanasov and Black 2016, and references therein). Performing such a test is, however, challenging. Market liquidity is by its nature elusive, multifaceted (e.g., featuring tightness, immediacy, breadth, depth, and resiliency), and difficult to quantify, and especially so are its determinants (which include not only information asymmetry but also inventory considerations, transaction costs, and order-processing fees, among others). Accordingly, I compute not only many well-established firm-level proxies for stock illiquidity in the literature — some closer to the concept of market depth in Kyle (1985) and my model (e.g., the price impact of a trade of Amihud 2002), some with broad interpretation and sample coverage (e.g., the effective cost of trading of Hasbrouck 2009), and some more scarcely available but explicitly extracting its portion due to adverse selection risk (e.g., as in George, Kaul, and Nimalendran 1991, for the half-spread of Roll 1984) — but also several composite indexes capturing their (adverse selection) commonality (such as one including all

¹ Comprehensive surveys of this research include O'Hara (1995), Madhavan (2000), Hasbrouck (2007), Vives (2008), and Foucault, Pagano, and Röell (2013).

² See, for example, Chen, Goldstein, and Jiang (2007), Bharath, Pasquariello, and Wu (2009), Bond, Edmans, and Goldstein (2012), and the references therein. I will discuss related studies in greater detail in Section 1.

three proxies above), while minimizing idiosyncratic shocks and measurement noise (e.g., [Bharath, Pasquariello, and Wu 2009](#)).

It is equally difficult to assess the severity of managerial agency problems within a firm, as the effectiveness of various observable forms of firm or country-level corporate governance is controversial and the ensuing agency costs are often unobservable (see, e.g., [Shleifer and Vishny 1997](#)). The literature proposes numerous proxies for firms' (external and internal) governance — e.g., voting rights, restrictions to shareholder rights and investor activism, institutional ownership, board structure, managerial power, and executive compensation ([Gompers, Ishii, and Metrick 2003](#); [Bhagat, Bolton, and Romano 2008](#); [Bebchuk, Cohen, and Ferrell 2009](#); [Gillan, Hartzell, and Starks 2011](#)). Yet, any estimated relationship between those proxies and firm-level stock illiquidity (see, e.g., [Bacidore and Sofianos 2002](#); [Brockman and Chung 2003](#); [Chung, Elder, and Kim 2010](#); [Lang, Lins, and Maffett 2012](#)) may be biased by their potential endogeneity. Omitted variable bias could arise if firms differ on observable and unobservable characteristics (e.g., related to their riskiness) influencing both agency costs ([Tirole 2006](#)) and liquidity provision ([Vives 2008](#); [Foucault, Pagano, and Röell 2013](#)). Simultaneity bias could arise if both corporate governance and liquidity are jointly determined (e.g., as liquidity may facilitate or respond to block formation, activism, or block disposition; see [Kyle and Vila 1991](#); [Maug 1998](#); [Edmans 2009](#); [Collin-Dufresne and Fos 2015](#); [Back et al. 2018](#)).

I address these concerns by investigating the impact of the staggered adoption of *business combination* (BC) laws in U.S. states in the 1980s and 1990s on firm-level stock illiquidity. Numerous studies (surveyed in [Atanasov and Black 2016](#)) interpret the passage of BC laws in a state as a plausibly exogenous event unambiguously weakening the governance of firms there incorporated (i.e., treated firms) — by mitigating the threat of hostile takeover (and replacement) that may otherwise limit their managers' ability to exert value-destroying effort (e.g., [Jensen and Meckling 1976](#); [Jensen 1988](#)). I then conjecture that the adoption of BC provisions may have improved the stock liquidity of treated firms by reducing any prior uncertainty about those future value-relevant activities by activists, raiders, or management (e.g., [Pound 1987](#); [Karpoff and Malatesta 1989](#); [Szewczyk and Tsetsekos 1992](#); [Bertrand and Mullainathan 2003](#); [Karpoff and Wittry 2018](#)), hence lowering speculators' agency-driven information advantage.

To determine the importance of these effects, my difference-in-differences (DiD) identification strategy compares changes in the illiquidity of treated firms around the adoption of BC laws to changes in the illiquidity of otherwise similar control firms (e.g., operating in the same state as the treated ones) but incorporated in different states. I use “full model” DiD regressions which include firm and high-dimensional fixed

effects (as in [Bertrand and Mullainathan 2003](#); [Gormley and Matsa 2014, 2016](#)) to account for a variety of unobserved differences (across firms, time, states, and industries) that may bias my inference by coinciding with the passage of BC laws or the treatment and control groups, observable firm characteristics (size and age; as in [Giroud and Mueller 2010](#)), as well as controls for the potentially confounding institutional and legal context of their enactment (endogenous lobbying, preexisting takeover protections, changing legal regimes; as in [Karpoff and Wittry 2018](#)).

I find that the liquidity of firms' stocks *improves* following the state adoption of antitakeover provisions. This result is statistically and economically significant, as well as robust to a variety of alternative liquidity, sample, and regression specifications. For instance, [Amihud's \(2002\)](#) estimated percentage price impact of a stock trade for firms incorporated in a state adopting BC laws declines by an average of 1.38% per \$10,000 of daily trading volume (or \$138, i.e., by above 27% of its samplewide mean) after their enactment relative to firms located (i.e., headquartered) in the same state and operating in the same industry but incorporated in different states where BC laws have not (or not yet) been passed. The estimated improvement in liquidity is consistent across different proxies and aggregations, samples, and regressions, does not dissipate over time, and cannot be explained by differences in ex ante characteristics of treated and control firms (including past illiquidity), pre-event differential trends in illiquidity, policy anticipation and transience, unobserved local economic or political shocks, motivating firms, Delaware incorporation, or firms being treated in their state of location.

This result may be only indirectly suggestive of the conjectured joint effect of agency costs and strategic speculation on stock liquidity, since both firm-level agency-driven information asymmetry and the impact of BC laws on it are unobservable, while prior findings in the aforementioned literature on BC provisions may question the validity of the exclusion restriction for my inference (or any subsequent such analysis). For instance, [Karpoff and Wittry \(2018, table A-1\)](#) list more than 80 recent studies finding the enactment of restrictions on takeover activity to have differentially affected treated firms' financing, investment, risk-taking, accounting, liquidity management, and compensation policies, as well as their information environment, ownership, governance, and trading, among others. Hence, treatment of any of these potential stock liquidity covariates ([O'Hara 1995](#); [Bharath, Pasquariello, and Wu 2009](#); [Foucault, Pagano, and Röell 2013](#)) suggests multiple potential alternative interpretations of my findings. Explicitly controlling for these simultaneous effects — while commonplace in the literature and prima facie appealing — is plagued by bias due to endogenous controls (e.g., [Angrist and Pischke 2009](#); [Gormley and Matsa 2016](#)), sample selection, missing

observations, and unbalanced panel lengths (e.g., [Flannery and Hankins 2013](#)), while [Karpoff and Wittry \(2018\)](#) show that many of them also may be sensitive to (and weakened by) institutional and legal controls.

Accordingly, I assess the robustness of my inference to these concerns in two ways. First, notwithstanding the above caveats, I allow for the inclusion of up to 10 groupings of proxies for those firm-level policies and characteristics (as adequately available to me) in my full regression model. I also repeat my baseline analysis within each of the same ensuing subsamples, to account for their uneven and possibly endogenous composition. Remarkably, I find that the differential response of my measures of firm-level illiquidity to the staggered adoption of antitakeover laws remains nearly always negative, large, and statistically significant (despite its heterogeneous estimation samples) — except only when it is not so in the (few and sparse) corresponding baseline cases to begin with.

Second, I provide more direct support for my specific transmission channel by testing for its conjectured heterogeneity in that differential response. In particular, I expect (and my model postulates) such a response to be more pronounced for firms *ex ante* displaying higher adverse selection risk, more severe agency problems, and greater uncertainty about their severity — since in those circumstances, speculators' agency-driven information advantage about firm value is likely stronger (and so is its effect on stock liquidity provision). These firm-level characteristics are also not directly observable. Thus, I use multiple proxies from the literature for each of them. Matching DiD estimates of the heterogeneous response of firms' stock illiquidity to BC laws based on *ex ante* (i.e., prior-year) realizations of these proxies are consistent with my predictions. For instance, I find that following the adoption of a BC law, [Amihud's \(2002\)](#) estimated price impact per \$10,000 stock trade for treated firms with above-median prior stock return volatility or analyst earnings-per-share (EPS) forecast dispersion (i.e., *high* prior fundamental uncertainty), below-median prior board independence, institutional ownership, or incentive-based executive compensation (i.e., *high* prior agency problems), and above-median prior return on assets (ROA) or insider ownership (i.e., *high* prior agency uncertainty) improves (relative to similar control firms in the same state and industry) by an average of \$88 (or 64% of the baseline case; and as high as \$304 [202%]) more than when comparing similarly treated and control firms with previous below-median such characteristics.

These findings indicate that the passage of BC provisions may have not only impaired corporate governance for the affected firms but also enhanced their stock market liquidity by reducing speculators' value-relevant, agency-driven information advantage. More generally, my analysis suggests that managerial agency problems may play a nontrivial role for the process of price formation in financial markets. I believe this to be

an important, original insight into the economics of capital market quality.

1. Literature Review and Hypothesis Development

I am interested in the causal effects of firm-level agency costs on equity market liquidity. Thus, my study aims to connect two different but equally vast strands of corporate finance and market microstructure focused on the economics of a firm's agency problems and the process of price formation for its stocks, respectively.

A comprehensive survey of these literatures would be both challenging and beyond the scope of this paper. [Jensen and Meckling \(1976\)](#) and [Tirole \(2001, 2006\)](#) discuss the main conflicts between firm insiders and outsiders and the many proposed incentive contracts or securities to mitigate the conflicts between them (and better align their interests). [O'Hara \(1995\)](#), [Hasbrouck \(2007\)](#), and [Foucault, Pagano, and Röell \(2013\)](#) outline how the multiple facets of the liquidity of a firm's stock may depend on such frictions as inventory considerations, transaction costs, order-processing fees, and adverse selection risk, among others.

Prior work has suggested linkages between the two. For instance, several studies find that better investor protection (measured by differences in various firm-level corporate governance indices or in the legal and regulatory environments of firms' listing markets) may improve stock market liquidity and price informativeness by fostering transparency and information production and curbing insider trading (e.g., [Bacidore and Sofianos 2002](#); [Brockman and Chung 2003](#); [Ferreira and Laux 2007](#); [Fernandes and Ferreira 2008](#); [Chung, Elder, and Kim 2010](#); [Lang, Lins, and Maffett 2012](#)). However, this inference may be plagued by the endogeneity of agency problems and stock illiquidity. Numerous other studies consider the reverse-causation arguments that a firm's stock market liquidity may either weaken its corporate governance, by facilitating the "exit" of blockholders who may otherwise monitor the firm (see, e.g., [Bhide 1993](#); [Admati, Pfleiderer, and Zechner 1994](#); [Bolton and Von Thadden 1998](#); [Back et al. 2018](#)), or strengthen it, by facilitating either the emergence of those blockholders or more information production otherwise (see, e.g., [Kyle and Vila 1991](#); [Holmström and Tirole 1993](#); [Maug 1998](#); [Edmans 2009](#); [Fang et al. 2009](#); [Bharath, Jayaraman, and Nagar 2013](#); [Edmans, Fang, and Zur 2013](#)).

[Holmström and Tirole \(1993\)](#) postulate that lower insider ownership (assumed to be inversely related to noise trading) may increase both the liquidity of a firm's shares and the marginal value of its fundamental information, thus inducing more monitoring of management by informed speculation and allowing the firm to design more efficient

managerial contracts. Further related studies investigate the relation between firms' stock market liquidity and such corporate outcomes as their investment and leverage decisions (e.g., [Chen, Goldstein, and Jiang 2007](#); [Bharath, Pasquariello, and Wu 2009](#)) or consider the feedback effects between financial markets and product markets when the former reveal information to firm managers about the latter either in the absence of managerial agency problems (e.g., [Subrahmanyam and Titman 2001](#); [Goldstein and Guembel 2008](#); [Goldstein, Ozdenoren, and Yuan 2013](#); [Edmans, Goldstein, and Jiang 2015](#)) or in the presence of suboptimal managerial behavior and blockholders exerting governance by exit (e.g., [Admati and Pfleiderer 2009](#); [Edmans 2009](#)) or voice ([Dumitrescu 2015](#)). [Dumitrescu \(2015\)](#) argues that when a firm manager is also the only informed trader in its stocks, stringent external disclosure requirements may worsen stock liquidity by inducing the manager to manipulate earnings reports and increase project uncertainty when internal governance is poor.

Relative to this body of work, I propose and offer novel causal evidence in support of the notion that privately optimal but socially suboptimal managerial behavior may affect stock liquidity in the presence of adverse selection risk from better informed speculation. I describe the conceptual underpinnings of this mechanism and its main predictions next.

Managers (or insiders) may either have private information about the firm (adverse selection) or exert effort unobservable to firm outsiders (moral hazard); in the presence of either form of information asymmetry, managers may undertake “second-best” (“socially suboptimal”) actions, that is, ones that, while beneficial to them (“privately optimal”), are detrimental to outsiders and overall firm value (in a “first-best” sense; e.g., [Jensen and Meckling 1976](#); [Tirole 2001, 2006](#)). In turn, information asymmetry about (fundamental and agency-driven) firm value may reduce the liquidity of its traded assets (or securities) by exposing uninformed market participants (e.g., dealers or market-makers) to expected losses from liquidity provision to better-informed speculators (e.g., by increasing the price impact of order flow [as in [Kyle 1985](#)] or the bid ask spread [as in [Glosten and Milgrom 1985](#)]). Thus, uncertainty about the extent of firm-level agency problems makes its stocks less liquid, by increasing speculators' informational advantage about firm value and liquidity providers' perceived adverse selection risk. In Section A of the [Internet Appendix](#), I formalize this argument within a stylized model of speculation based on [Kyle \(1985\)](#) — one of the most common representations of price formation in financial markets in the literature — in which equilibrium illiquidity (finite market depth or nonzero price impact of order flow) is induced by adverse selection risk from strategic trading based on private fundamental information. The model's main, original

insight is that when the liquidation value of the traded risky asset depends on private and potentially suboptimal managerial effort, equilibrium illiquidity also can be positively ascribed to adverse selection risk from managerial agency problems.

Empirically establishing this link between agency-driven information asymmetry and stock liquidity is challenging. First, suboptimal managerial effort (or investment) may arise from multiple, often unobservable sources of agency conflicts (e.g., [Jensen and Meckling 1976](#); [Gompers, Ishii, and Metrick 2003](#); [Bebchuk, Cohen, and Ferrell 2009](#)). Second, measuring the liquidity of a firm's stock, namely, the ability to trade it promptly, cheaply, and with small price impact, is both difficult and controversial, as its intrinsically elusive and multifaceted nature prevents a precise yet general characterization (e.g., [Amihud 2002](#); [Hasbrouck 2007](#); [Bharath, Pasquariello, and Wu 2009](#)).³ Third, as noted earlier, while the literature has proposed several proxies for either notion, the causal interpretation of any statistical (cross-sectional or within-firm) relation among them is clouded by their potential endogeneity (e.g., [Bertrand and Mullainathan 2003](#); [Gormley and Matsa 2016](#)).

I tackle these challenges in two steps, further discussed in Sections 2.1 and 2.2, as follows. First, I consider both the popular measure of price impact of a trade developed by [Amihud \(2002\)](#) — that is, the one feasibly closest in spirit to the aforementioned notion of illiquidity in [Kyle \(1985\)](#) and my model — and multiple alternative (broad or information-based) measures of stock market liquidity and multiple ways to aggregate them — to reduce the dimensionality of the analysis, increase sample size, and more accurately capture the intensity of their adverse selection component (as in [Bharath, Pasquariello, and Wu 2009](#)). Second, I examine the differential response of those illiquidity measures to the staggered adoption of antitakeover laws (also known as business combination [BC] laws) across U.S. states (listed in Table IA-1 of the [Internet Appendix](#)) during the 1980s and 1990s. These events are customarily deemed to have not only exogenously weakened the governance of treated firms (since [Bertrand and Mullainathan 2003](#); see also [Atanasov and Black 2016](#) and the references therein) but also (and importantly for my argument) resolved prior uncertainty about future value-enhancing activities by prospective activists or raiders, as well as about future value-destroying activities by firm management, hence potentially diminishing prior, agency-driven information asymmetry about firm value. For example, the media extensively covered BC laws, and both raiders and target companies litigated the laws in court ([Bertrand and Mullainathan 2003](#); [Karpoff and Wittry 2018](#)), while the stock prices of firms affected by

³ For instance, [Amihud \(2002, p. 35\)](#) notes that “[i]t is doubtful that there is one single measure [of liquidity] that captures all its aspects.”

their adoption promptly and significantly declined when the laws were announced (e.g., Pound 1987; Karpoff and Malatesta 1989; Szcwcyk and Tsetsekos 1992). Accordingly, *ceteris paribus*, their introduction may have improved the stock liquidity of treated firms, also consistent with my model.

Intuitively, agency problems cause firm value to fall below its optimum (especially when those problems are more severe). In those circumstances, privately informed speculators have a greater information advantage about (socially suboptimal) firm value (especially when *ex ante* uncertainty about the future extent of distorted managerial choices is high). For instance, in the presence of a realistic threat of value-improving activist intervention or takeover bids — as for treated firms prior to the introduction of BC laws — speculators may possess more value-relevant private information (e.g., about whether disciplinary actions or managerial turnover may occur; Jensen and Ruback 1983; Tirole 2006) and trade on it, such that a firm's stock liquidity may be lower although firm value may be higher on average. Consistently, extant studies provide evidence of wider bid-ask spreads and greater aggregate trading volume for the stocks of target firms on days prior to (successful and unsuccessful) corporate acquisition announcements (see, e.g., Conrad and Niden 1992 and the references therein).

However, when those threats from hostile activists and/or raiders are mitigated — as for treated firms following the introduction of BC laws — firm value may be lower but so may be speculators' agency-driven information advantage about it, such that stock liquidity provision is improved (especially in face of greater *ex ante* adverse selection risk). For example, the adoption of antitakeover provisions may have also either discouraged informed trading activity by potential blockholders or motivated the exit of existing ones (e.g., Bacidore and Sofianos 2002; Brockman and Chung 2003; Bhagat, Bolton, and Romano 2008; Back et al. 2018). Accordingly, my model shows that the postulated linkage between equilibrium illiquidity and agency-driven adverse selection risk is uniquely sensitive to those measurable features of a firm's agency problems and information environment, as conjectured above. I assess the empirical relevance of these considerations by examining the heterogeneous differential response of stock illiquidity to BC laws conditional on *ex ante* firm-level proxies for their intensity; a more detailed discussion is in Section 2.4.4.

Below, I will summarize the testable hypotheses developed in this section.

Hypothesis 1 *The introduction of BC laws improves the (adverse selection portion) of the stock liquidity of treated firms.*

Hypothesis 2 *Ceteris paribus, BC-induced improvement in stock liquidity is more pronounced for treated firms displaying ex ante greater managerial agency problems, greater uncertainty about those problems among market participants, and greater perceived adverse selection risk to their stocks' liquidity provision.*

2. Empirical Analysis

In Section 1, I argue that a firm's managerial agency problems may affect the liquidity of its securities when traded in financial markets plagued by information asymmetry problems. In this section, I assess the empirical relevance of this notion within the U.S. stock market, while tackling the numerous aforementioned challenges posed by such an investigation. I find that agency-driven adverse selection influences firm-level stock market liquidity as predicated by Hypotheses 1 and 2.

2.1 Measuring stock market liquidity

A vast market microstructure literature indicates that the liquidity of a firm's stock depends on such frictions as inventory considerations, transaction costs, order-processing fees, and adverse selection risk, among others (e.g., O'Hara 1995; Huang and Stoll 1997; Hasbrouck 2007; Foucault, Pagano, and Röell 2013). This literature has proposed many broad measures of firm-level stock market liquidity in general, and market depth in particular. Most of these measures — while often only weakly correlated with each other (Chordia, Roll, and Subrahmanyam 2000; Korajczyk and Sadka 2008; Bharath, Pasquariello, and Wu 2009; Hasbrouck 2009) — can be easily computed from available data, at relatively low frequencies, and over long sample periods, for virtually all stocks traded in major U.S. exchanges. However, the discussion in Section 1 highlights a potential linkage between a firm's managerial agency costs and the depth of its traded securities in the presence of strategic, better-informed speculation — that is, the portion of dealers' liquidity provision that is affected exclusively by their perceived adverse selection risk. Measuring such a portion is a more difficult task — one generally requiring higher-quality, higher-frequency data that is typically available only for fewer stocks over shorter, more recent periods of time.

In light of these issues, in this paper I concentrate on two complementary measures of firm-level (i) annual (t) illiquidity, $ILLIQ_{i,t}$, an individual one capturing the price impact of a trade (consistent with the conceptual framework of Section 1) and a composite one made of multiple illiquidity proxies (to extract their informational commonality, of interest in that framework).

Specifically, I begin by estimating up to 10 such proxies. The first set of proxies provides me with the longest simultaneous coverage of as many stocks as possible in the universe of U.S. firms. It includes four broad liquidity measures based on observed trading costs (1); the serial covariance properties of stock returns (2 and 3); or the estimation of structural models of stock price formation (4): (1) the closing quoted proportional bid-ask spread (e.g., George, Kaul, and Nimalendran 1991; Chung and Zhang 2014), $PBA_{i,t}$; (2) the implicit (or twice the effective) percentage bid-ask spread (or cost of immediacy) of Roll (1984), $ROLL_{i,t}$; (3) (the negative of) the reversal coefficient of Pastor and Stambaugh (2003), $PS_{i,t}$; and (4) the percentage effective cost of trading (or Roll's [1984] half-spread) of Hasbrouck (2009), $EC_{i,t}$. The second set of proxies is made of two measures capturing the interaction between stock returns and trading volume (5 and 6), in the spirit of Kyle (1985): (5) the percentage price impact of trading volume of Amihud (2002), $AMIHU_{i,t}$; and (6) (the negative of) the liquidity ratio (or market depth measure) of Cooper, Groth, and Avera (1985) and Amihud, Mendelson, and Lauterbach (1997), $AMIVEST_{i,t}$. The third set of proxies provides me with a more direct assessment of the extent to which better-informed trading affects stock price formation. It includes four measures of more involved construction (and model dependence) and with often more limited availability: the adverse selection portions of (7) the closing quoted proportional and (8) Roll's (1984) implicit percentage bid-ask spread (as in George, Kaul, and Nimalendran 1991), $ASYPBA_{i,t}$, and $ASYROLL_{i,t}$; (9) the return-volume coefficient of Llorente et al. (2002), $C2_{i,t}$; and (10) the (basic and extended) probability of informed trading of Easley et al. (1996), Venter and de Jongh (2006), Brown and Hillegeist (2007), and Easley, Hvidkjaer, and O'Hara (2010), $PIN_{i,t}$. Table IA-2 of the Internet Appendix contains detailed definitions and intuition (see also Bharath, Pasquariello, and Wu 2009; Hasbrouck 2009); their summary statistics are reported in Table 1 and discussed in Section 2.3.

By construction, the higher is each proxy the worse is a firm's stock market liquidity, that is, the greater is the illiquidity of its stocks. Yet, also by construction, each proxy has a different scale, and is only an imprecise estimate of a specific facet of that illiquidity, one that may be plagued by noise and idiosyncratic shocks. Accordingly, I concentrate my attention on two complementary such measures of annual firm-level illiquidity $ILLIQ_{i,t}$, an individual one and a composite one. The first one, $AMIHU_{i,t}$, is among the most commonly used in the literature, is consistent with the notion of Kyle's (1985) price impact from informed speculation in the aggregate order flow (as the price change per unit of trading volume), and can be easily computed without sacrificing sample coverage.

Several recent studies also propose aggregating multiple proxies to produce a more precise assessment of firm-level or marketwide commonality in liquidity (Chordia, Roll, and Subrahmanyam 2000; Amihud 2002; Pastor and Stambaugh 2003; Acharya and Pedersen 2005; Korajczyk and Sadka 2008; Bharath, Pasquariello, and Wu 2009). Aggregation across all three sets of proxies, including those broadest in scope, may further isolate the portion of this commonality due to firm-level adverse selection risk (Bharath, Pasquariello, and Wu 2009), as well as mitigate a potential downward bias in its more direct measures due to the tendency of such possibly better-informed speculators as activist investors to trade when markets are broadly more liquid, as argued by Collin-Dufresne and Fos (2015), while also increasing sample size. To that end, I construct a composite firm-year illiquidity measure $ILLIQ_{3,i,t}$ as the equal-weighted average of three standardized proxies — $EC_{i,t}$, $AMIHU_{i,t}$, and $ASYROLL_{i,t}$ — that is, one for each grouping above, when available. Both $EC_{i,t}$ and $ASYROLL_{i,t}$ are intuitive, generally available, and conceptually related to the notion of implicit (or effective) cost of trading (or immediacy) in Roll (1984); however, Hasbrouck (2009) finds its Bayesian-Gibbs estimates $EC_{i,t}$ (based on implied trade directions) to be less biased than the conventional ones ($ROLL_{i,t}$, based on first-order return autocovariances), while George, Kaul, and Nimalendran (1991) show that $ASYROLL_{i,t}$ is an unbiased, efficient, and easily obtained estimator of its adverse selection component. This approach is transparent and parsimonious, yet comprehensive; I discuss in detail the robustness of my inference to numerous alternative specifications — including aggregation of all ($ILLIQ_{10,i,t}$, or other subsets) of those 10 illiquidity proxies or via principal component analysis (PCA) — in Section 2.4.2.

2.2 BC laws and stock market liquidity

Firm management routinely resists a hostile takeover, as it often leads to its replacement and so threatens its ability to continue to pursue actions that may not be in the firm's best interest. Accordingly, the corporate finance literature considers the severity of hostile takeover threats an important form of corporate governance — hence an important determinant of managerial agency problems (Jensen and Meckling 1976; Jensen 1988; Tirole 2006).

Between 1985 and 1997, 33 U.S. states (listed in Table IA-1 of the Internet Appendix) adopted BC laws preventing a variety of corporate transactions between a target firm and a raider (e.g., mergers, sale of assets, or business relationships) and so ultimately restricting hostile takeovers of firms incorporated (i.e., legally organized) in those states. Bertrand and Mullainathan (2003) and numerous subsequent studies

(surveyed in [Atanasov and Black 2016](#)) interpret these events as a well-suited source of exogenous variation in managerial agency problems for the affected firms, since BC statutes (1) effectively weakened the corporate governance of those firms; (2) were unlikely to stem from organized lobbying efforts by those firms (see also [Romano 1987](#)); and (3) were enacted in a staggered fashion across states and over time, allowing for multiple treatment events.

Importantly, as noted earlier, evidence suggests that the adoption of BC statutes may represent a plausible shock reducing agency-driven information asymmetry from speculative trading (for example, by lessening speculators' pretreatment information advantage about value-improving activism or managerial effort), hence potentially enhancing firm-level stock market liquidity, as conjectured in Section 1. [Bertrand and Mullainathan \(2003\)](#) and [Karpoff and Wittry \(2018\)](#) note that these laws were extensively covered by both specialized and popular press, as well as extensively litigated by both raiders and target companies. Several studies find significantly negative effects of BC laws on the stock prices of affected firms, especially on the first press announcement date (e.g., [Pound 1987](#); [Karpoff and Malatesta 1989](#); [Szewczyk and Tsetsekos 1992](#), among others). This evidence suggests that the enactment of BC provisions in a state may have left less ambiguity — that is, less inside information — among stock market participants about whether affected firm management might face disciplinary forces or heightened monitoring in the future. *Ceteris paribus*, Hypothesis 1 then predicts that such an exogenous decrease in a treated firm's agency-driven uncertainty may mitigate its perceived adverse selection risk and improve its stock liquidity provision.

I assess the empirical relevance of this argument by using the difference-in-differences (DiD) methodology of [Bertrand and Mullainathan \(2003\)](#) and [Gormley and Matsa \(2014, 2016\)](#). It compares changes in stock illiquidity among (once) treated firms incorporated in states where BC laws had been passed to changes in stock illiquidity among otherwise similar control (or untreated) firms (e.g., located in the same states) but incorporated in different states where BC laws have not (or not yet) been passed (i.e., with replacement). The main identification assumption behind this approach is that stock illiquidity of both sets of firms follows parallel trends over time, namely that, if not for being incorporated in states passing a BC law, their stock illiquidity would have experienced similar changes.

I consider three DiD specifications for this setting. In the first one, based on [Bertrand and Mullainathan \(2003\)](#), I estimate the following baseline regression:

$$ILLIQ_{i,j,l,s,t} = \alpha_t + \alpha_i + \delta BC_{s,t} + \varepsilon_{i,j,l,s,t}, \quad (1)$$

where $ILLIQ_{i,j,l,s,t}$ is my measure of stock illiquidity of firm i , in industry j , located in state l , incorporated in state s , on year t ; α_t are year fixed effects controlling for aggregate liquidity fluctuations over time; α_i are firm fixed effects controlling for time-invariant differences in stock illiquidity between treated and control firms; and $BC_{s,t}$ is a dummy variable equal to one if a BC law has been passed in state s by year t . Thus, estimates of the coefficient δ capture the differential response to the passage of BC laws of the stock illiquidity of firms incorporated in different states, only some of which have passed those laws.

These estimates may be biased if failing to control for other observable factors thought to affect stock illiquidity of treated and control firms, as well as if failing to control for unobserved heterogeneity between treated and control firms, for example, local shocks (e.g., local business cycles) affecting the stock illiquidity of firms located and incorporated in the same state ($l = s$) at the same time when state-level antitakeover provisions were adopted; local shocks influencing the adoption itself of those provisions; or any potential differential trends in stock illiquidity between the industries of treated and control firms over time. To account for these possibilities, Equation (1) is commonly augmented to include a variety of time-varying controls (including state-year and industry-year averages of the dependent variable, as in Bertrand and Mullainathan 2003; see also Section 2.4.2) related to both firm-illiquidity and firm-level corporate policies and outcomes. However, in two recent studies, Gormley and Matsa (2014, 2016) argue that this approach is biased and inconsistent — because time-varying controls may themselves be affected by the passage of BC laws (e.g., Angrist and Pischke 2009) or plagued by measurement error (Hausman 2001; Gormley and Matsa 2014) — and can distort inference (e.g., by even yielding estimates of δ of the opposite sign of the true coefficient). To address these issues, Gormley and Matsa (2014, 2016) propose the use of high-dimensional (state-year and industry-year) fixed effects in Equation (1); I estimate the following general regression:

$$ILLIQ_{i,j,l,s,t} = \alpha_i + \alpha_{l,t} + \alpha_{j,t} + \delta BC_{s,t} + \beta' X_{i,j,l,s,t} + \varepsilon_{i,j,l,s,t}, \quad (2)$$

where $\alpha_{l,t}$ are state of location-by-year fixed effects controlling for unobserved, time-varying differences in stock illiquidity across states; $\alpha_{j,t}$ are (four-digit SIC) industry-by-year fixed effects controlling for unobserved, time-varying differences in stock illiquidity across industries; and $X_{i,j,l,s,t}$ is a vector of such time-varying firm characteristics as size and age (MKT CAP, MKT CAP squared, AGE, AGE squared; Giroud and Mueller 2010). Equation (2) relaxes the parallel trends assumption behind Equation (1) as estimates of δ are identified from within-state-year

and within-industry-year variation — insofar as (like in my sample, whose construction I discuss next) a sufficiently large fraction of firms i (about two-thirds) is located and incorporated in different states ($l \neq s$). Thus, δ from Equation (2) captures the differential response to the passage of BC laws in year t of the stock illiquidity of firms in the same industry j , located in the same state l , but incorporated in different states s on that year. This approach accounts for many types of unobservable heterogeneity by allowing for both unobserved, time-varying state-level factors differentially affecting stock illiquidity and differential trends in stock illiquidity across industries over time.⁴

In a recent study, Karpoff and Wittry (2018) convincingly argue that Equation (2) is a “short regression” (in the spirit of Angrist and Pischke 2009) for it fails to effectively account for omitted variables, political economy, and selection issues that may invalidate any causal inference from its estimation. In particular, Karpoff and Wittry (2018) advocate for amending Equation (2) to control for such confounding effects as the active and public endogenous lobbying activity of more than two dozen influential firms (p. 662) in favor of BC laws, as well as for both preexisting state-level takeover defenses (first- and second-generation laws; pp. 660-661) and subsequent court decisions on their legality (*MITE*, *CTS*, *Amanda*, or *Moran*; section VI.A, p. 682) that may have mitigated the impact of BC laws on the severity of firm-level agency costs. I do so by estimating the following “full model” regression in Karpoff and Wittry (2018, p. 681):

$$ILLIQ_{i,j,l,s,t} = \alpha_i + \alpha_{l,t} + \alpha_{j,t} + \delta BC_{s,t} + \beta' X_{i,j,l,s,t} + \gamma' CE_{i,j,l,s,t} + \varepsilon_{i,j,l,s,t}, \quad (3)$$

where $CE_{i,j,l,s,t}$ is a vector made of all the aforementioned confounding effects (p. 683): dummies for first-generation laws, poison pill laws, control share acquisition laws, directors’ duties laws, fair price laws, control share acquisition laws, firms covered by BC laws after *Amanda*, and BC-motivating firms covered by BC laws. I consider alternative specifications in Section 2.4.2.

2.3 Data

I study all firms in the COMPUSTAT database between 1976 and 2006 for which my broadest measure of stock market illiquidity ($ILLIQ_{10,i,t}$) can be computed and information about state of incorporation (for treatment status) can be identified. My sample is constructed following standard practices in the relevant literatures (e.g., Bertrand and Mullainathan

⁴ In their seminal study of the effect of BC laws on managerial behavior, Bertrand and Mullainathan (2003, p. 1057) also advocate the use of high-dimensional fixed effects but argue that “computational difficulties make [their estimation] infeasible.” I estimate those fixed effects using a Stata code developed by Gormley and Matsa (2014) and available on Matsa’s website at <http://www.kellogg.northwestern.edu/faculty/matsa/htm/fe.htm>.

2003; Bharath, Pasquariello, and Wu 2009; Hasbrouck 2009; Gormley and Matsa 2016). I concentrate on a sample period allowing for no fewer than 10 years of data before and after the adoption of a BC law. I exclude regulated utilities (SIC codes 4900-4999), as well as firms incorporated or located outside of the U.S. or in U.S. territories, firms with only one observation, and firms with negative or missing assets or sales. I use the legacy version of COMPUSTAT to fill missing firm-level corporate domicile information in its most recent version.⁵ I estimate (or obtain) the 10 illiquidity proxies described in Section 2.1 from standard approaches and data sources in the literature (see Table IA-2 of the [Internet Appendix](#); e.g., CRSP and TAQ). I winsorize each of these proxies and all other firm-level variables used in the analysis at the 1% and 99% levels to mitigate the effect of potentially spurious outliers in my data set. The final sample (i.e., where both the event dummy $BC_{s,t}$ and at least one of those proxies are available) includes 134,393 firm-year observations. Robustness checks to alternative sample construction are in Section 2.4.2.

Summary statistics for my measure of illiquidity $ILLIQ_{i,t}$ and each of its components are in [Table 1](#), together with their pairwise Pearson correlation matrix. Consistent with the aforementioned literature, most liquidity proxies are only weakly correlated with each other. The means for the four of them more closely related to the notion of better-informed trading ($ASYPBA_{i,t}$, $ASYROLL_{i,t}$, $C2_{i,t}$, and $PIN_{i,t}$; in column 1) are all positive, large (e.g., about 37% of the implicit bid-ask spread $ROLL_{i,t}$), and statistically significant — suggesting that adverse selection risk is an important determinant of firm-level stock market liquidity over my sample period. The mean percentage price impact of a stock trade $AMIHU_{i,t}$ is about 5% per \$10,000 of daily trading volume (i.e., a \$500 cost), in line with prior estimates (e.g., [Hasbrouck 2009](#)). The ternary composite index $ILLIQ_{3,i,t}$ loads positively on most of them (in column 4), but especially so on broad (and often available) estimates of effective trading costs (e.g., both its constituent $EC_{i,t}$ [averaging 1.31%, i.e., a half-spread of about 33 cents for a \$25 stock] and $PBA_{i,t}$) and market depth (its constituent $AMIHU_{i,t}$), as well as on more precise

⁵ While common in the aforementioned literature, this practice may lead to incorrect treatment assignment (and possible endogeneity) for firms that changed their state of incorporation or location (e.g., in response to the adoption of BC laws) over my sample period, since COMPUSTAT updates this information to current status ([Cohen 2011](#)). However, some studies suggest that any ensuing measurement error and endogeneity bias are likely to be small. For instance, when augmenting a sample that is similar to ours with additional historical incorporation and location information (unavailable to us) and then removing firms that reincorporated either away from or into a state with a BC law over 1976-2006, [Gormley and Matsa \(2016\)](#) find that (1) only a small fraction of firms reincorporate (see also [Dodd and Leftwich 1980](#); [Romano 1993](#); [Daines 2000](#)); (2) only about 6% of firm-year observations are affected; (3) treatment changes for only 2% of firm-year observations; and (4) the augmented database does not significantly affect their estimates of the effect of BC laws on corporate risk-taking. See also the discussion in [Bertrand and Mullainathan \(2003\)](#).

Table 1
Summary statistics

	(1)	(2)	(3)	(4)										
	Summary statistics			Pairwise Pearson correlation matrix										
	N	Mean	SD	PBA	ROLL	PS	EC	AMIHUD	AMIVEST	ASYPBA	ASYROLL	C2	PIN	ILLIQ_3
PBA	65,102	4.56	4.79	1										
ROLL	134,307	1.25	3.45	0.77	1									
PS	126,299	-27.15	204.64	-0.23	-0.15	1								
EC	115,130	1.31	1.48	0.95	0.85	-0.31	1							
AMIHUD	124,352	5.02	15.94	0.78	0.50	-0.33	0.74	1						
AMIVEST	121,125	-798.75	3,181.22	0.17	0.10	-0.03	0.17	0.08	1					
ASYPBA	61,650	92.08	15.82	-0.05	-0.05	0.00	-0.06	-0.03	0.00	1				
ASYROLL	125,371	37.47	27.26	-0.16	-0.09	0.06	-0.17	-0.11	-0.17	0.05	1			
C2	118,889	1.57	8.79	-0.09	0.07	-0.08	0.08	-0.02	0.04	-0.01	-0.06	1		
PIN	84,558	23.18	10.73	0.38	0.33	-0.12	0.36	0.27	0.33	0.07	-0.27	0.09	1	
ILLIQ_3	133,097	-0.01	0.70	0.75	0.55	-0.24	0.80	0.82	0.04	-0.02	0.43	0.00	0.15	1

This table reports summary statistics (in columns 1-3) for each of the firm-level illiquidity measures described in Section 2.1 (PBA, ROLL, EC, PS, AMIHUD, AMIVEST, ASYPBA, ASYROLL, C2, and PIN) and for the illiquidity measure ILLIQ_3 (based on AMIHUD, EC, and ASYROLL), as well as the corresponding pairwise Pearson correlation matrix (in column 4). Specifically, PBA is the closing quoted proportional bid-ask spread (e.g., [George, Kaul, and Nimalendran 1991](#); [Chung and Zhang 2014](#)); ROLL is the implicit (or twice the effective) percentage bid-ask spread of [Roll \(1984\)](#); EC is the percentage effective cost of trading of [Hasbrouck \(2009\)](#); AMIHUD is the percentage price impact measure of [Amihud \(2002\)](#); AMIVEST is (the negative of) the liquidity ratio of [Cooper, Groth, and Avera \(1985\)](#) and [Amihud, Mendelson, and Lauterbach \(1997\)](#); PS is (the negative of) the reversal coefficient of [Pastor and Stambaugh \(2003\)](#); ASYPBA and ASYROLL are the adverse selection portions of the closing quoted and [Roll's \(1984\)](#) effective bid-ask spread (as in [George, Kaul, and Nimalendran 1991](#)), respectively; C2 is the return-volume coefficient of [Llorente et al. \(2002\)](#); and PIN is the (basic and extended) probability of informed trading of [Easley et al. \(1996\)](#), [Venter and de Jongh \(2006\)](#), [Brown and Hillegeist \(2007\)](#), and [Easley, Hvidkjaer, and O'Hara \(2010\)](#). Table IA-2 of the [Internet Appendix](#) contains more detailed definitions. Firm-level illiquidity ILLIQ_3 is then computed as the equal-weighted average of the available standardized illiquidity proxies AMIHUD, EC, and ASYROLL. N is the number of available firm-year observations in my sample for each variable over 1976-2006.

Table 2
BC laws: Ex ante firm-level characteristics

	(1)		(2)		(3)
	Treated firms		Control firms		<i>p</i> -value of difference
	Mean (SD)	N	Mean (SD)	N	
MKTCAP	439.3 (1,619.4)	3,760	510.5 (1,895.2)	26,422	.195
AGE	12.83 (12.35)	3,767	12.53 (12.30)	26,523	.929
RETVOL	60.50 (31.71)	3,765	54.93 (33.44)	26,507	.223
ROA	-1.91 (22.23)	3,766	-2.68 (23.19)	26,529	.198
CASHFLOW	4.89 (23.12)	3,150	5.23 (22.75)	21,837	.702
DEBT	25.38 (21.80)	3,751	24.39 (21.70)	26,407	.011
AMIHUD	5.25 (15.11)	3,691	5.74 (16.33)	25,909	.215
ILLIQ_3	0.09 (0.65)	3,739	0.01 (0.70)	26,347	.088
DISPERSION	4.42 (13.68)	1,539	3.99 (13.57)	11,032	.372
INACCURACY	9.60 (34.34)	1,661	8.69 (32.98)	11,709	.353
IDIOVOL	45.85 (23.43)	3,006	43.39 (25.44)	20,984	.401
GINDEX	8.99 (2.95)	854	8.76 (2.85)	4,267	.097
INDBOARD	77.30 (22.76)	847	78.16 (21.82)	6,850	.197
EXINC	26.34 (16.18)	30	31.24 (21.50)	1,521	.064
INSTOWN	16.69 (20.80)	3,766	17.74 (22.01)	26,506	.409
INSOWN	9.88 (15.95)	2,242	10.32 (16.36)	12,917	.420

This table reports means and standard deviations (in parentheses) of various firm-level characteristics (defined in Table IA-2 of the [Internet Appendix](#)) in the year before a BC law is adopted for (once) treated firms (i.e., both available and incorporated in states adopting a BC law in the following year; column 1) and (manyfold) control firms (i.e., both available and incorporated in states not adopting a BC law in the following year; column 2, including matches in multiple events), as well the *p*-value from a *t*-test of the difference between their means based on robust standard errors adjusted for clustering at the state-of-incorporation level (column 3). N is the number of available treated and control firm-event year observations in the final sample of Section 2.3 for each variable over 1976-2006.

estimates (when available) of the intensity of informed trading (e.g., both its constituent $ASYROLL_{i,t}$ and $PIN_{i,t}$).⁶

Table 2 compares various average characteristics (also defined in Table IA-2 of the [Internet Appendix](#)) of available treated firms in the year before a BC law is adopted in their state of incorporation to those of

⁶ Neither the mean of $ILLIQ_{3,t}$ is zero nor its standard deviation is one in [Table 1](#) because its standardized constituents are neither always all contemporaneously available for all firms nor perfectly correlated among them.

equally available control firms incorporated in a state where a BC law has not (or not yet) been adopted in that year (including matches in multiple events). Treated and control firms do not exhibit any meaningful prior difference in size (MKT CAP), financial ratios (e.g., return on assets [ROA], cash flow on assets [CASHFLOW]), riskiness (stock return volatility [RETVOL]), illiquidity, information quality (e.g., analyst forecast dispersion [DISPERSION]), governance (e.g., board independence [INDBOARD]), (incentive-based) executive compensation (EXINC), and (institutional and insider) ownership (INSTOWN and INSOWN): nearly all p -values from t -tests of their differences in means (using robust [heteroscedasticity-consistent] standard errors clustered at the state-of-incorporation level; in column 3) are large, and all such differences are economically small. Overall, my sample's main features are similar to those of related studies in the literature.

2.4 Results

The discussion in Sections 1 and 2.2 motivates the study of the effect of the staggered adoption of BC laws in U.S. states — a plausibly exogenous negative shock to agency-driven information asymmetry among strategic speculators about the treated firms — on firm-level stock illiquidity.

2.4.1 Baseline analysis. I start by estimating the three DiD specifications of Equations (1) to (3) described in Section 2.2 for the two proxies for stock illiquidity $ILLIQ_{i,t}$ proposed in Section 2.1, $AMIHU_{i,t}$ and $ILLIQ_{3,i,t}$. In both sets of regressions, the coefficient δ for the treatment dummy $BC_{s,t}$ captures the differential response of $ILLIQ_{i,t}$ of treated and untreated firms incorporated in different states to the enactment of BC provisions in treated firms' state of incorporation. Equation (1) controls for unobserved heterogeneity (e.g., from differences in year of observation, location, industry, or other characteristics across treated and untreated firms) only with year and firm fixed effects; in Equation (2), that response is identified from within-state and within-industry variation (i.e., by comparing treated and untreated firms in the same state of location and [four-digit SIC] industry) after controlling for firm size and age; the full model of Equation (3) further accounts for firms' institutional and legal context around BC adoptions.⁷

I report estimates of δ in Table 3, together with robust standard errors adjusted for clustering at the state-of-incorporation level (as in Gormley

⁷ In all regressions, I also drop iteratively all singleton groups (i.e., groups with only one observation) resulting from multiple fixed effects (as those groups can bias the statistical significance of coefficient estimates; e.g., Correia 2015), using a Stata code available on Correia's website at <http://scorreia.com/software/reghdfe/>; keeping those singletons leads to virtually identical inference.

Table 3
Illiquidity and BC laws

	(1)	(2)	(3)	(4)	(5)	(6)
ILLIQ =	AMIHUD			ILLIQ_3		
BC	-1.686*** (0.520)	-0.846* (0.486)	-1.381*** (0.441)	-0.0701** (0.0281)	-0.0373* (0.0199)	-0.0569*** (0.0205)
Year FE	YES	NO	NO	NO	NO	
Firm FE	YES	YES	YES	YES	YES	YES
State-year FE	NO	YES	YES	NO	YES	YES
Industry-year FE	NO	YES	YES	NO	YES	YES
Baseline controls	NO	YES	YES	NO	YES	YES
Confounding effects	NO	NO	YES	NO	NO	YES
N	124,221	113,297	113,297	133,035	121,309	121,309
R ²	.45	.56	.56	.35	.48	.48

This table reports estimates for the coefficient (δ) of firm-year panel regressions (fixed effects [FE], Equation (1), columns 1 and 4; high-dimensional FE, Equation (2), columns 2 and 5; full model, Equation (3), columns 3 and 6) of stock illiquidity ILLIQ (AMIHUD or ILLIQ_3 of Section 2.1) on a dummy variable (BC) equal to one if a firm is incorporated in a state that has adopted a BC law and equal to zero otherwise. State-year and industry-year FE (as in Gormley and Matsa 2016) are at the state of location and four-digit SIC level, respectively. Baseline controls include a firm's MKTCAP, MKTCAP squared, AGE, and AGE squared (as in Giroud and Mueller 2010). Confounding effects include dummies for BC-motivating firms and other types of takeover states and court decisions (Karpoff and Wittry 2018; see also the discussion in Section 2.2). N is the number of available, single-ton-free firm-year observations in my sample over 1976-2006; R² is the coefficient of determination; robust standard errors (in parentheses) are adjusted for clustering at the state-of-incorporation level. * $p < .1$; ** $p < .05$; *** $p < .01$.

and Matsa 2016) to control for the potential covariation of stock illiquidity among firms incorporated in the same state. I find that firm-level stock market liquidity *improves* after the adoption of BC laws, consistent with Hypothesis 1. My estimates of this effect are both statistically and economically significant. For instance, estimation of the full model of Equation (3) for $AMIHUD_{i,t}$ (in column 3) indicates that the percentage price impact of a \$10,000 stock trade in firms incorporated in states passing BC provisions drops on average by 1.38% (or \$138, with a t -statistic of 3.1), that is, by more than 27% of its sample mean (in column 2 of Table 1), after their enactment relative to firms located in the same states and operating in the same industries but incorporated in states where those provision have not (or not yet) been passed. As noted earlier, ignoring unobserved heterogeneity may lead to overestimate the liquidity effect of BC laws (as in column 1). However, failing to control for potentially confounding effects (as in the short model of Equation (2)) may either underestimate or imprecisely estimate that effect (as in column 2).

As important, BC-treated firms' composite illiquidity measure $ILLIQ_{3,i,t}$, which adjoins $EC_{i,t}$ and $ASYROLL_{i,t}$ to $AMIHUD_{i,t}$ to isolate their information-based commonality, also nontrivially declines even after accounting for all potential sources of misspecification in the literature (in column 6), for example, by 8.1% of its sample standard deviation

(in column 3 of Table 1; with a t -statistic of 2.8). This additional evidence is preliminarily consistent with the notion, predicated in Section 1, that the observed improvement in treated firms' stock market liquidity following the adoption of antitakeover provisions may stem from such adoption reducing the agency-driven information advantage by strategic speculators in those firms and the accompanying adverse selection risk to their stocks' liquidity provision.

2.4.2 Robustness. I now assess the robustness of the baseline evidence reported in Table 3 to numerous alternative illiquidity, sample, and regression specifications. I consider alternative interpretations of this evidence and its sensitivity to several additional controls in Section 2.4.3 next.

I begin by investigating alternative construction of my composite measure of (adverse selection-driven) firm-level illiquidity $ILLIQ_{3,it}$ in Table 4. First, further estimation of the full model of Equation (3) indicates that all of its components, that is, not only $AMIHU_{i,t}$ (in Table 3) but also both $ASYROLL_{i,t}$ and $EC_{i,t}$ (in columns 1 and 2, respectively), decline in correspondence with the state-level enactment of antitakeover provisions. More generally, the (unreported) separate analysis of each of the 10 illiquidity proxies defined in Section 2.1, while of more difficult interpretation given their heterogeneous samples (see Table 1), yields noisier yet both similar and largely consistent inference across all three proxy groupings described in Section 2.1 (i.e., broad, depth, and information-based).

My inference is similarly unaffected by replacing $EC_{i,t}$ with either $PS_{i,t}$ (equally available yet notably noisier; $ILLIQ_3P_{i,t}$, in column 3) or with all the remaining (but unequally available) proxies for market depth and adverse selection risk ($AMIVEST_{i,t}$, $ASYPBA_{i,t}$, $C2_{i,t}$, $PIN_{i,t}$; $ILLIQ_{6,it}$, in column 4) — to mitigate or eliminate the reliance of $ILLIQ_{3,it}$ on broad estimates of Roll's (1984) effective half-spread; by averaging across either all 10 standardized illiquidity proxies ($ILLIQ_{10,it}$, whose estimated δ in column 5 amounts to 8.4% of its sample standard deviation) or an expanded set also including the (log) inverse turnover ratio ($INVTURN_{i,t}$) and Lesmond, Ogden, and Trzcinka's (1999) proportion of zero returns ($PROPZERO_{i,t}$), two broadly available proxies of conceptually more ambiguous interpretation ($ILLIQ_{12,it}$, in column 6); as well as by restricting the analysis to the subsample where all three components of $ILLIQ_{3,it}$ are jointly available ($ILLIQ_3IN_{i,t}$; in column 7).

As discussed in Section 2.1, I compute all of these composite measures as equal-weighted averages of their standardized available components. Principal component analysis (PCA) is also used to aggregate (and extract the commonality in) multiple variables of interest (e.g., Baker and

Table 4
Illiquidity and BC laws: Alternative illiquidity measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ILLIQ =	ASYROLL	EC	ILLIQ_3P	ILLIQ_6	ILLIQ_10	ILLIQ_12	ILLIQ_3IN	ILLIQ_PC1	ILLIQ_PCE	ILLIQ_PCV
BC	-2.021*** (0.652)	-0.0832** (0.033)	-0.0711*** (0.0182)	-0.0246* (0.014)	-0.0408*** (0.0144)	-0.0212* (0.011)	-0.0812*** (0.0132)	-0.0789** (0.0384)	-0.0698*** (0.0238)	-0.0723** (0.0271)
N	112,785	104,537	121,714	121,453	122,522	122,522	97,553	121,309	121,309	121,309
R ²	.38	.74	.34	.48	.57	.67	.58	.59	.44	.51

This table reports estimates for the coefficient (δ) of firm-year panel regressions (full model, Equation (3)) of alternative measures of stock illiquidity ILLIQ (described in Sections 2.1 and 2.4.2) on a dummy variable (BC) equal to one if a firm is incorporated in a state that has adopted a BC law and equal to zero otherwise. All specifications include firm, state-year, and industry-year fixed effects [FE], as well as baseline and confounding effects controls. State-year and industry-year FE (as in Gormley and Matsa 2016) are at the state of location and four-digit SIC level, respectively. Baseline controls include a firm's MKTCAP, MKTCAP squared, AGE, and AGE squared (as in Giroud and Mueller 2010). Confounding effects include dummies for BC-motivating firms and other types of takeover states and court decisions (Karpoff and Wittry 2018; see also the discussion in Section 2.2). N is the number of available, singleton-free firm-year observations in my sample over 1976-2006; R² is the coefficient of determination; robust standard errors (in parentheses) are adjusted for clustering at the state-of-incorporation level.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Wurgler 2006; Korajczyk and Sadka 2008; Bharath, Pasquariello, and Wu 2009). Using PCA for this purpose in my setting is less than ideal, however, since (1) it requires all firm-year observations, thus potentially introducing a look-ahead bias in my analysis; and (2) as noted earlier, my illiquidity proxies do not provide uniform coverage across firms and over time, while their samplewide pairwise correlations (in column 4 of Table 1) are relatively low. Accordingly, when replacing each missing standardized illiquidity proxy-firm-year observation in $ILLIQ_{3,i,t}$ with the equal weighted average of the other contemporaneously available proxies (e.g., Connor and Korajczyk 1987), I find that (1) only the first two principal components have eigenvalues above the conventional threshold of one (1.19 and 1.01); (2) the first principal component (PC1) loads (evenly) on only two of $ILLIQ_{3,i,t}$'s components ($AMIHU_{i,t}$, $EC_{i,t}$) and accounts for 58% of their variance, while the next one (PC2, loading almost exclusively on $ASY_ROLL_{i,t}$) accounts for an additional 33%; (3) the correlation of either PC1 or an equal-weighted (or variance explained-weighted) average of PC1 and PC2 with $ILLIQ_{3,i,t}$ is high (between 0.87 and 0.99); and (4) replacing $ILLIQ_{3,i,t}$ with PC1 or either average in the analysis that follows leads to the same inference ($ILLIQ_{PC1,i,t}$, $ILLIQ_{PCE,i,t}$, $ILLIQ_{PCV,i,t}$; in columns 8-10).

Next, I evaluate the sensitivity of the evidence in Table 3 to either employing a standard specification in the spirit of Bertrand and Mullainathan (2003) and Giroud and Mueller (2010) or altering the underlying sample construction described in Section 2.3. As to the former, I replace the high-dimensional fixed effects $\alpha_{i,t}$ and $\alpha_{j,t}$ in Equation (3) with their corresponding state-year ($\overline{ILLIQ}_{l,t}$) and (four-digit SIC) industry-year ($\overline{ILLIQ}_{j,t}$) averages of $ILLIQ_{i,j,l,s,t}$ as well as year fixed effects α_t , as follows:

$$ILLIQ_{i,j,l,s,t} = \alpha_i + \alpha_t + \delta BC_{s,t} + \beta' X_{i,j,l,s,t} + \gamma' CE_{i,j,l,s,t} + \rho \overline{ILLIQ}_{l,t} + \eta \overline{ILLIQ}_{j,t} + \varepsilon_{i,j,l,s,t}. \quad (4)$$

I report estimates of δ from Equation (4) for $AMIHU_{i,t}$ and $ILLIQ_{3,i,t}$ in Table 5, either without (BMGM1; columns 1 and 12, respectively) or with those average effects (BMGM2; columns 2 and 13). The estimated effect of the adoption of antitakeover provisions on illiquidity is lower than in Table 3 (consistent with Gormley and Matsa's [2014, 2016] aforementioned criticism of this approach as being potentially biased; see also Section 2.2), but remains both strongly statistically significant (at the 1% level) and economically large, for example, amounting to an average drop in the percentage price impact of a stock trade $AMIHU_{i,t}$ in column 2 (the composite measure $ILLIQ_{3,i,t}$ in column 12) by 0.89% (0.0353) or 18% of its sample mean (5% of its sample standard deviation).

Table 5
Illiquidity and BC laws: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ILLIQ =	AMIHU										
ALT =	BMGM1	BMGM2	NO WIN	SIC3	NO FIN	1976-95	NO 97BC	NO DE	LOC-INC	NO LOB	PRE-BC
BC	-1.536*** (0.360)	-0.894*** (0.289)	-4.383*** (1.611)	-1.469*** (0.455)	-1.426*** (0.363)	-1.294*** (0.473)	-1.358*** (0.453)	-1.376 (1.074)		-1.385*** (0.437)	-2.024** (0.918)
BC loc = inc									-1.957*** (0.654)		
BC loc $\neq$ inc									-1.206*** (0.445)		
Pre-BC											-0.643 (0.808)
Year FE	YES	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
State-year FE	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry-year FE	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES
State-year AE	NO	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
Industry-year AE	NO	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
N	122,433	122,433	113,297	120,090	90,631	58,766	112,057	49,217	113,297	112,977	113,297
R ²	.45	.56	.34	.52	.55	.59	.56	.60	.56	.56	.56

	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
ILLIQ =	ILLIQ_3										
ALT =	BMGM1	BMGM2	NO WIN	SIC3	NO FIN	1976-95	NO 97BC	NO DE	LOC-INC	NO LOB	PRE-BC
BC	−0.0646*** (0.0159)	−0.0353*** (0.0104)	−0.0547*** (0.0146)	−0.0566*** (0.0197)	−0.0555*** (0.0171)	−0.0506** (0.0221)	−0.0568*** (0.0205)	−0.0328 (0.0566)		−0.0571*** (0.0203)	−0.101*** (0.0312)
BC loc = inc									−0.0750** (0.0291)		
BC loc ≠ inc									−0.0515** (0.0197)		
Pre-BC											−0.0449* (0.0264)
Year FE	YES	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
State-year FE	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry-year FE	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES
State-year AE	NO	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
Industry-year AE	NO	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
N	130,896	130,896	121,309	128,562	97,198	66,580	120,070	53,796	121,309	120,989	121,309
R ²	.35	.49	.38	.42	.48	.50	.48	.52	.48	.48	.48

This table reports estimates for the coefficient (δ) of firm-year panel regressions (fixed effects [FE], columns 1 and 12; average effects [AE], columns 2 and 13; full model, Equation (3), columns 3-11 and 14-22) of stock illiquidity ILLIQ (AMIHU or ILLIQ_3 of Section 2.1) on a dummy variable (BC) equal to one if a firm is incorporated in a state that has adopted a BC law and equal to zero otherwise for alternative specifications or samples (ALT, described in Section 2.4.2) to the baseline analysis of Section 2.3. All specifications include firm FE, as well as baseline and confounding effects controls. State-year and industry-year AE (as in [Bertrand and Mullainathan 2003](#)) and FE (as in [Gormley and Matsa 2016](#)) are at the state of location and four-digit SIC level, respectively, unless indicated otherwise. Baseline controls include a firm's MKTCAP, MKTCAP squared, AGE, and AGE squared (as in [Giroud and Mueller 2010](#)). Confounding effects include dummies for BC-motivating firms and other types of takeover states and court decisions ([Karpoff and Wittry 2018](#); see also the discussion in Section 2.2). N is the number of available, singleton-free firm-year observations in my sample over 1976-2006; R² is the coefficient of determination; robust standard errors (in parentheses) are adjusted for clustering at the state-of-incorporation level.

* $p < .1$; ** $p < .05$; *** $p < .01$.

As to the latter, I repeat the estimation of Equation (3) for different versions of the baseline data set used in Table 3: without winsorizing (NO WIN; columns 3 and 14); using three-digit SIC industry fixed effects (SIC 3; columns 4 and 15); excluding financial firms (SIC codes 6000-6999, as in Bharath, Pasquariello, and Wu 2009; about 25,000 firm-year observations; NO FIN; columns 5 and 16); ending the sample in 1995 (i.e., with fewer BC events and shorter post-event periods, as in Bertrand and Mullainathan 2003; 1976-95; columns 6 and 17); excluding the latest and most delayed BC adoptions (1997, in Iowa and Texas, 6 years after the previous one in Table IA-1 of the Internet Appendix; about 1,300 firm-year observations; NO 97BC; columns 7 and 18) to account for any attenuation bias in potentially anticipated (or perceived as temporary) treatment responses to binary policy assignments (see, e.g., Angrist and Pischke 2009; Hennessy and Strebulaev 2015; Atanasov and Black 2016). Table 5 indicates that none of these alternatives materially affects my inference.

Many U.S. firms are incorporated in a single state, Delaware, where a BC law was passed in 1988 (see Table IA-1 of the Internet Appendix; Daines 2001). Delaware is also the only state in Table 1 without any of the other potentially confounding antitakeover laws discussed in Karpoff and Wittry (2018 pp. 660-661; see also Section 2.2). Excluding the more than half of observations made of firms incorporated in Delaware from the analysis (NO DE; columns 8 and 19) yields similar yet much noisier estimates of δ in Equation (3) for it removes above 55% of treated firms from the sample and reduces within-state-year and within-industry-year variation and identification power (see also Gormley and Matsa 2016). As noted earlier, such variation is also important to alleviate the concern that the estimated impact of BC laws on stock illiquidity may be driven by unobservable local economic shocks affecting both stock price formation and the passage of antitakeover provisions; however, it leaves open the possibility of policy endogeneity from local politicians being more responsive to shocks affecting firms located in their state of incorporation than to shocks affecting locally incorporated firms operating elsewhere (Gormley and Matsa 2016; Karpoff and Wittry 2018). Nonetheless, I find that the estimated differential responses δ to the adoption of BC laws of the stock illiquidity of firms incorporated and located either in the *same* ($s = l$) or in a *different* state ($s \neq l$, as in Gormley and Matsa 2016; LOC-INC; columns 9 and 20) are both qualitatively and statistically similar (by [unreported] F -test, with p -values of 0.2336 and 0.2979) as well as consistent with the overall estimates for δ in Table 3, as so are those same estimates after removing the aforementioned BC-lobbying firms listed by Karpoff and Wittry (2018 p. 662) from the sample (328 firm-year observations; NO LOB; columns 10 and 21).

Lastly, I verify that my inference is not driven by preexisting differential trends in firm-level stock illiquidity before BC provisions are passed. I do so by amending Equation (3) to either allow for the coefficient δ to change by event year n from 5 years before to 10 years after a BC event (e.g., Gormley and Matsa 2011, 2016) or include only the equivalent pre-BC dummy (i.e., equal to one on year t if state s has passed a BC law up to 5 years later). I plot annual point estimates δ_n in Figure IA-4 of the Internet Appendix (solid lines and markers), together with 90% confidence intervals (adjusted for clustering at the state-of-incorporation level; dashed lines); pretreatment and treatment dummy coefficients δ_- and δ are in columns 11 and 22 of Table 5. Point treatment estimates ($n \geq 0$, while jointly strongly statistically significant by F -test, with p -values of 0.0002 and 0.0015) are noisier than the average differential responses in Table 3 since the number of available firm-event year observations in each such event year is considerably smaller than over the event dummy $BC_{s,t}$, providing low power especially for within-state-year and within-industry-year identification in Equation (3). Nonetheless, and consistent with Table 3, both plots in Figures IA-4a ($AMIHUD_{i,t}$) and IA-4b ($ILLIQ_{3,i,t}$) show that the stock liquidity of treated firms does not discernibly change before the adoption of BC laws ($n < 0$) but nontrivially and prolongedly improves relative to control firms afterwards (suggesting a sustained response of firm-level illiquidity to slow resolution of agency-driven information asymmetry). Accordingly, estimates of their corresponding pretreatment illiquidity averages in Table 5 (PRE-BC) are small, statistically insignificant, or both, estimated treatment (BC) is large and negative, and their equality is strongly rejected for both $AMIHUD_{i,t}$ and $ILLIQ_{3,i,t}$ (by F -test, with p -values of 0.0025 and 0.0076).

2.4.3 Alternative explanations. According to Section 1 (and Hypothesis 1), the negative estimates of δ in Tables 3 to 5 and Figure IA-4 of the Internet Appendix preliminarily suggest that the widely publicized passage of antitakeover provisions in U.S. states in the 1980s and 1990s may have reduced strategic speculation's agency-driven information advantage about treated firms' value (for example, about value-improving activist intervention or takeover bids), hence mitigating dealers' and market-makers' perceived adverse selection risk and ultimately facilitating their liquidity provision for the stocks of treated firms relative to untreated ones.

As noted earlier, alternative interpretations based on prior findings in the literature on state-level BC provisions are nonetheless possible, potentially questioning the exclusion restriction in my identification strategy. In particular, the adoption of BC laws has been shown to affect many firm policies and characteristics. In a recent survey, Karpoff and

Wittry (2018, table A-1, pp. 702-706) list more than 80 such studies in the literature reporting evidence of a potential linkage between the introduction of antitakeover provisions and multiple firm-level variables broadly related to firms' financing, investment, governance, risk-taking, accounting transparency, information environment, ownership, trading, liquidity management, and executive compensation, among others. Therefore, my evidence could stem from the effect of treatment on any of these potential stock liquidity covariates, for instance, via its separate impact on treated firms' information asymmetry, agency problems, or liquidity provision, rather than via the specific channel postulated in Section 1. As just one example, Gormley and Matsa (2016) find that the stock return volatility of firms treated by the adoption of BC laws declines relative to untreated firms in the same state and industry, consistent with the notion that management insulated by antitakeover provisions may "play it safe" by taking value-destroying actions that reduce overall firm-level risk. Relatedly, Bertrand and Mullainathan (2003) also find that managers of firms treated by BC laws may prefer to "enjoy the quiet life" (for themselves and their firms) by exerting less effort. Extant microstructure research (e.g., Amihud and Mendelson 1980; Ho and Stoll 1981; Grossman and Miller 1988) then suggests that liquidity provision for those stocks may improve — as in Table 3 — because lower firm-level (fundamental and price) risk facilitates dealers' and market-makers' inventory management — despite my attempt to capture (albeit imperfectly) the portion of firm-level stock illiquidity driven primarily by adverse selection risk via the composite measure $ILLIQ_{3,i,t}$.

Many of the aforementioned studies address this issue by explicitly controlling for at least some of those simultaneous effects. This approach, while *prima facie* appealing, raises several concerns. First, the use of endogenous controls may confound the ensuing inference (e.g., Angrist and Pischke 2009), including when investigating the staggered introduction of antitakeover provisions (Gormley and Matsa 2016). As discussed in Section 2.2, I estimate high-dimensional fixed effects and limit those controls to firm size and age in Equations (2) and (3) to mitigate these biases. Second, Karpoff and Wittry (2018) argue and provide evidence that some of the empirical relationships between BC laws and various corporate outcomes in the literature may not be robust to further accounting for a variety of confounding effects clouding their causal interpretation. As also noted earlier, I explicitly implement all of the controls recommended by Karpoff and Wittry (2018) in the full model of Equation (3); most of those studies do not. Last, augmenting my sample with those additional control variables magnifies such common and potentially problematic features of corporate finance data sets as sample selection, missing observations, and unbalanced panel lengths,

among others, which the use of fixed effects instead counteracts (e.g., [Flannery and Hankins 2013](#)).

In light of these concerns, I assess the robustness of my inference to alternative interpretations in two ways. First, I allow for the inclusion of as many of the potentially BC-sensitive firm-level variables listed in [Karpoff and Wittry \(2018, table A-1\)](#) as adequately available to me over the sample period 1976-2006 (or portions thereof) in [Equation \(3\)](#). In the next section, I then evaluate the specific transmission channel postulated in Section 1 and Hypothesis 1 more directly by analyzing the potential heterogeneity in the response of firm-level illiquidity to the passage of BC provisions on the basis of my unique, additional predictions in Hypothesis 2.

I begin by considering 10 groupings of those variables (whose precise definitions are in Table IA-2 of the [Internet Appendix](#)) on the basis of their customary interpretation and within-sample contemporaneous coverage: (1) corporate outcomes often associated not only with a firm's stock illiquidity (or its adverse selection component; e.g., [Hasbrouck 2007, 2009](#); [Bharath, Pasquariello, and Wu 2009](#)) but also with its corporate governance (e.g., [Bhagat, Bolton, and Romano 2008](#)) and overall financial health and riskiness (e.g., [Gormley and Matsa 2016](#); CORP: stock price [PRICE], RETVOL, ROA, CASHFLOW, debt on assets [DEBT], the z-score of [Altman 1968](#) [ZSCORE]); (2) (accounting) information environment (e.g., [Sloan 1996](#); [Bhattacharya, Desai, and Venkataraman 2013](#); INFO: idiosyncratic stock return volatility [IDIOVOL], abnormal accruals [ABNACCR]); (3) forecast quality (e.g., [Armstrong, Balakrishnan, and Cohen 2012](#); [Bradshaw et al. 2012](#); [Gallo 2013](#); ANALYST: analyst earnings per share [EPS] forecast dispersion [DISPERSION], consensus inaccuracy [INACCURACY], uncertainty [UNCERTAINTY], coverage [COVERAGE]); (4) insider trading (e.g., [Chung, Elder, and Kim 2010](#); INSTRADE); (5) institutional ownership (e.g., [Chung, Elder, and Kim 2010](#); INSTOWN); (6) insider ownership (e.g., [Panousi and Papanikolaou 2012](#); INSOWN); (7) board composition (e.g., [Chidambaram, Kedia, and Prabhala 2012](#); BOARD: size [BOARDSIZE], independence [INDBOARD]); (8) the g-index of [Gompers, Ishii, and Metrick \(2003\)](#) (GINDEX); (9) compensation (e.g., [Frydman and Jenter 2010](#); [Gabaix, Laundier, and Sauvagnat 2014](#); COMP: total CEO [CEOCOMP] and executive compensation [EXCOMP], incentive-based CEO [CEOINC] and executive compensation [EXINC]); (10) all of the above (ALL IN).

Next, I estimate the full model of [Equation \(3\)](#) not only when augmented by each of these groupings separately in [Table 6](#) for $AMIHU_{i,t}$ (END = YES; columns 1-10) and $ILLIQ_{3i,t}$ (columns 11-20) but also in their absence but within the *same* subsample (END = NO). The latter allows me to assess more precisely the relative impact of the inclusion of

Table 6
Illiquidity and BC laws: Endogenous controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ILLIQ =	AMIHU									
END =	CORP	INFO	ANALYST	INSTRATE	INSTOWN	INSOWN	BOARD	GINDEX	COMP	ALL IN
	END = YES									
BC	−0.643** (0.316)	−1.428*** (0.374)	−0.142 (0.187)	−1.322*** (0.443)	−1.296*** (0.433)	−1.223*** (0.411)	−0.0237 (0.493)	−0.266 (0.533)	0.0009 (0.0255)	−0.0078 (0.0136)
N	86,605	80,217	49,459	109,528	108,543	62,828	48,718	25,747	18,387	6,275
R ²	.68	.66	.63	.56	.56	.61	.56	.65	.65	.71
	END = NO									
BC	−1.253*** (0.447)	−1.716*** (0.392)	−0.124 (0.196)	−1.325*** (0.443)	−1.321*** (0.436)	−1.224*** (0.411)	−0.0094 (0.479)	−0.271 (0.536)	0.0605** (0.0263)	−0.0087 (0.0191)
N	86,605	80,217	49,459	109,528	108,543	62,828	48,718	25,747	18,387	6,275
R ²	.57	.60	.62	.56	.56	.61	.56	.65	.63	.70

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
ILLIQ =	ILLIQ_3									
END =	CORP	INFO	ANALYST	INSTRADE	INSTOWN	INSOWN	BOARD	GINDEX	COMP	ALL IN
END = YES										
BC	−0.0284* (0.0152)	−0.0738*** (0.0167)	−0.0392* (0.0198)	−0.0561*** (0.0198)	−0.0551*** (0.0194)	−0.0768*** (0.0266)	−0.0915*** (0.0333)	−0.0903*** (0.0277)	−0.0674*** (0.0236)	−0.0079 (0.0856)
N	92,253	80,270	49,626	114,095	113,111	63,263	49,410	25,786	18,416	6,275
R ²	.60	.62	.49	.49	.49	.55	.46	.47	.45	.58
END = NO										
BC	−0.0547*** (0.0187)	−0.0839*** (0.0166)	−0.0384* (0.0206)	−0.0561*** (0.0198)	−0.0559*** (0.0196)	−0.0770*** (0.0264)	−0.0891*** (0.0329)	−0.0910*** (0.0278)	−0.0618*** (0.0226)	−0.0325 (0.0857)
N	92,253	80,270	49,626	114,095	113,111	63,263	49,410	25,786	18,416	6,275
R ²	.49	.58	.48	.49	.49	.55	.46	.47	.45	.54

This table reports estimates for the coefficient (δ) of firm-year panel regressions (full model, Equation (3)) of stock illiquidity ILLIQ (AMIHUD or ILLIQ_3 of Section 2.1) on a dummy variable (BC) equal to one if a firm is incorporated in a state that has adopted a BC law and equal to zero otherwise when additional, potentially endogenous controls (END, described in Section 2.4.3) related to the firm's policies (CORP; columns 1 and 11), information environment (INFO, ANALYST, and INSTRADE; columns 2-4 and 12-14), institutional and insider ownership (INSTOWN and INSOWN; columns 5-6 and 15-16), corporate governance (BOARD, GINDEX, and COMP; columns 7-9 and 17-19), or all of them jointly (ALL IN, columns 10 and 20) are either included (END = YES) or excluded but the same availability restriction is imposed (END = NO). All specifications include firm, state-year, and industry-year fixed effects (FE), as well as baseline and confounding effects controls. State-year and industry-year FE (as in Gormley and Matsa 2016) are at the state of location and four-digit SIC level, respectively. Baseline controls include a firm's MKTCAP, MKTCAP squared, AGE, and AGE squared (as in Giroud and Mueller 2010). Confounding effects include dummies for BC-motivating firms and other types of takeover states and court decisions (Karpoff and Wittry 2018; see also the discussion in Section 2.1). CORP includes PRICE, RETVOL, ROA, CASHFLOW, DEBT, and ZSCORE; INFO includes IDIOVOL and ABNACCR; ANALYST includes DISPERSION, INACCURACY, UNCERTAINTY, and COVERAGE; BOARD includes BOARDSIZE and INDBOARD; and COMP includes CEOCOMP, EXCOMP, CEOINC, and EXINC. N is the number of available, singleton-free firm-year observations in my sample over 1976-2006; R² is the coefficient of determination; robust standard errors (in parentheses) are adjusted for clustering at the state-of-incorporation level.

* $p < .1$; ** $p < .05$; *** $p < .01$.

control variables on my inference, while accounting for their uneven (and possibly endogenous) sample composition. Remarkably, the estimated differential response of those illiquidity measures to the staggered adoption of antitakeover laws remains nearly always negative and almost as often both large and statistically significant after adding controls ($END = YES$), despite the heterogeneous subsamples within which it is measured, especially for the (more widely available) ternary composite index $ILLIQ_{3,it}$. Where it is not, it is always *regardless* of conditioning on additional controls (i.e., also when $END = NO$, e.g., as for $AMIHUD_{i,t}$ by ANALYST, in column 3).

Unsurprisingly, absolute estimates of δ in Table 6 are generally smaller after the inclusion of subsets of potentially endogenous controls in Equation (3) (e.g., $|\delta|$ is nearly halved by CORP in columns 1 and 11), although not in all cases (e.g., $|\delta|$ is increased by BOARD in columns 7 and 17) and often not materially (e.g., $|\delta|$ is virtually unaffected by INSTOWN and INSOWN in columns 5-6 and 15-16). However, those estimates are both statistically insignificant (albeit still negative) only in the smallest subsample where *all* contemporaneously available controls are included (columns 10 and 20), that is, the one made of roughly one-twentieth of the observations in the corresponding baseline cases of Table 3.

Overall, this additional analysis suggests that my baseline inference in Section 2.4.1 is robust to the explicit (yet admittedly problematic) inclusion of numerous additional firm-level covariates based on the literature on the effect of BC laws on corporate outcomes.

2.4.4 Heterogeneous response. Notwithstanding my efforts in Sections 2.4.1 to 2.4.3, the evidence of a relative improvement in the stock liquidity of firms treated by the introduction of antitakeover provisions in Tables 3 to 6 is only indirectly suggestive of the specific agency information-based channel of transmission proposed in Section 1 (and Hypothesis 1), since agency-driven adverse selection risk is unobservable. To evaluate this notion more directly, I assess some of its unique, additional predictions for firm-level illiquidity by analyzing the potential heterogeneity in its response to the passage of BC provisions. According to Hypothesis 2, such a response should be more pronounced (and the absolute magnitude of estimates of δ be larger) for firms where, *ex ante*, agency problems are more severe, uncertainty about their severity is greater, and adverse selection risk is perceived to be higher, that is, where the impact of BC laws on speculators' information advantage about firm value and uninformed market participants' liquidity provision may plausibly be more material.

This analysis raises additional challenges. To begin with, those firm-level characteristics are themselves not directly observable. Thus, I use multiple proxies from the literature for each of them. Firm-level adverse selection risk is commonly positively associated with firm-level fundamental uncertainty (e.g., Kyle 1985; see also O'Hara 1995; Foucault, Pagano, and Röell 2013), and the latter's cross-sectional variation is commonly measured with firm-level stock return (RETVOL) and idiosyncratic return volatility (IDIOVOL; Gormley and Matsa 2016; Herskovic et al. 2016), as well as with such proxies for the quality of speculative private information that are based on professional analyst EPS forecasts as their dispersion (DISPERSION) and consensus inaccuracy (INACCURACY; e.g., O'Brien 1988; Bradshaw et al. 2012; Gallo 2013; Pasquariello and Vega 2015). Ceteris paribus, Hypothesis 2 predicts that estimated $|\delta|$ in Equation (3) should be greater for firms displaying HIGH (e.g., above-median) pretreatment such proxies.

As noted earlier, measuring the severity of and uncertainty about firm-level agency problems prior to the passage of BC laws is a more difficult (and unavoidably subjective) task. To the former, I rely on four popular proxies in the literature for the weakness of a firm's (external and internal) governance: HIGH g-index of Gompers, Ishii, and Metrick (2003; GINDEX), and LOW (e.g., below-median) board independence (INDBOARD; e.g., Chidambaran, Kedia, and Prabhala 2012), institutional ownership (INSTOWN; e.g., Hartzell and Starks 2003; Chung and Zhang 2011), and incentive-based executive compensation (EXINC; e.g., Frydman and Jenter 2010). To the latter, I propose three indirect measures of firm-level uncertainty about the future extent of distorted managerial choices: HIGH firm-level performance (ROA; e.g., Gompers, Ishii, and Metrick 2003), size (MKT CAP; e.g., Love 2011 and references therein), and insider ownership (INSOWN; e.g., Jensen and Meckling 1976; Panousi and Papanikolaou 2012). In those circumstances, firms are less likely to be poorly managed, hence the uncertainty surrounding *future* managerial agency (e.g., after the adoption of antitakeover provisions) among stock market participants is presumably greater than for firms whose agency problems are already well-known as severe. According to Hypothesis 2, estimated $|\delta|$ in Equation (3) should be greater for firms displaying any of these pretreatment features.

Conditioning my analysis on ex ante such firm-level characteristics (e.g., measured prior to the passage of BC laws) is important to overcome endogeneity concerns, since (as I discussed in Section 2.4.3) all of them also may be affected by those laws. To that purpose, one may estimate Equation (3) separately for firms with above or below-median

characteristic in the year *before* a BC law event.⁸ However, Gormley and Matsa (2011, 2016) argue that, because these events are staggered over time, this approach would compare the average response of treated and control firms sorted on those characteristics at different points in time. To address this problem, Gormley and Matsa (2011, 2016) propose an alternative, matching difference-in-differences (MDiD) methodology that (1) in each year when BC laws are passed (e.g., 1991), compares newly treated firms to untreated firms; (2) estimates the impact of that event on illiquidity (δ) exclusively within this specific BC law cohort (g) of firm-years (e.g., $g = 1991$), separately for those cohort- g firms with above and below-median previous-year characteristic, exclusively over a window of up to 15 years before and after the events g occurred (e.g., between 1977 and 2006), while neither requiring a firm to be available for the full estimation window (i.e., up to 30 years) nor preventing a firm from entering multiple cohorts; and (3) reports the average of all DiD coefficients δ across BC law cohorts (eight of them; see Table IA-1 of the Internet Appendix).

This methodology allows me to assess the heterogeneous responses of firm-level illiquidity to BC laws separately for each cohort, that is, by comparing the response of newly treated and untreated firms not only exposed to the same cohort of events (e.g., $g = 1991$) but also sorted on firm-level characteristics in the same prior year (e.g., above and below-median RETVOL in 1990). I implement it by estimating the following amended version of the full model of Equation (3):

$$ILLIQ_{g,i,j,l,s,t} = \alpha_{g,i} + \alpha_{g,l,t} + \alpha_{g,j,t} + \delta BC_{s,t} + \beta' X_{g,i,j,l,s,t} + \gamma' CE_{g,i,j,l,s,t} + \varepsilon_{g,i,j,l,s,t}, \quad (5)$$

where $\alpha_{g,i}$ are firm-by-cohort fixed effects controlling for cohort-level, time-invariant differences in stock illiquidity between treated and control firms; $\alpha_{g,l,t}$ are state of location-by-year-by-cohort fixed effects controlling for cohort-level, unobserved, time-varying differences in stock illiquidity across states; and $\alpha_{g,j,t}$ are industry-by-year-by-cohort fixed effects controlling for cohort-level, unobserved, time-varying differences in stock illiquidity across industries.

I report pairs of estimates of δ from Equation (5) for each of the 11 past-year, below (CHAR = LOW) and above-median sorts (CHAR = HIGH) listed above in Table 7 for $AMIHU_{i,t}$ (MATCH = YES; columns 1-11) and $ILLIQ_{3,i,t}$ (columns 12-22). For each firm-level characteristic, a pair of high-dimensional fixed effects estimates δ in Equation (5) is identified from within-state-year-cohort and within-industry-year-cohort variation; hence, it captures the average heterogeneous, differential

⁸ Also relevant to those comparisons, Table 2 shows that such past-year realizations of these firm-level characteristics are, on average, similar for treated and control firms (see also Section 2.3).

Table 7
Illiquidity and BC laws: Comparative statics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ILLIQ =	AMIHU										
CHAR =	RETVOL	IDIOVOL	DISPERSION	INACCURACY	GINDEX	INDBOARD	INSTOWN	EXINC	ROA	MKTCAP	INSOWN
MATCH = YES, CHAR = LOW											
BC	-0.742 (0.617)	-1.034** (0.480)	-0.940* (0.480)	0.0002 (0.278)	-0.316 (1.164)	-2.806 (1.669)	-1.812** (0.695)	-0.170 (0.3220)	-1.415** (0.583)	-1.184* (0.682)	-0.728 (0.5110)
N	57,769	50,324	29,823	32,895	11,739	19,054	47,359	9,021	60,517	46,594	44,924
R ²	.60	.59	.59	.61	.64	.60	.59	.65	.59	.58	.60
MATCH = YES, CHAR = HIGH											
BC	-1.712*** (0.629)	-2.070*** (0.691)	-1.220* (0.681)	-1.082 (0.717)	-0.394 (0.303)	0.235 (0.692)	-0.847** (0.407)	-0.0786 (1.3920)	-1.519** (0.617)	-1.393*** (0.460)	-1.463*** (0.431)
N	53,228	52,385	31,433	34,957	13,846	19,829	50,561	9,443	60,352	52,500	43,444
R ²	.58	.58	.58	.57	.76	.64	.57	.75	.58	.55	.59
MATCH = NO, CHAR = LOW											
BC	-0.738 (0.581)	-1.100** (0.458)	-0.888* (0.452)	0.0278 (0.297)	-0.270 (1.121)	-2.464 (1.555)	-2.040*** (0.668)	0.0945 (0.2940)	-1.388** (0.556)	-1.349** (0.614)	-0.770 (0.501)
N	69,512	61,177	36,359	39,734	14,575	26,609	57,610	11,417	71,727	55,232	53,252
R ²	.57	.56	.56	.59	.63	.57	.57	.64	.57	.56	.58
MATCH = NO, CHAR = HIGH											
BC	-1.769*** (0.568)	-2.033*** (0.644)	-1.437** (0.651)	-1.215* (0.682)	-0.456 (0.298)	0.326 (0.693)	-0.818** (0.366)	0.225 (1.1790)	-1.410** (0.607)	-1.388*** (0.426)	-1.464*** (0.418)
N	63,171	62,564	36,068	40,426	16,459	27,540	59,838	11,870	72,367	63,844	51,881
R ²	.56	.56	.56	.55	.73	.59	.55	.73	.56	.52	.57

	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
ILLIQ =	ILLIQ_3										
CHAR =	RETVOL	IDIOVOL	DISPERSION	INACCURACY	GINDEX	INDBOARD	INSTOWN	EXINC	ROA	MKTCAP	INSOWN
MATCH = YES, CHAR = LOW											
BC	−0.0459* (0.0249)	−0.0658*** (0.0229)	−0.0637*** (0.0234)	−0.0344* (0.0176)	−0.0225 (0.0504)	−0.122** (0.0497)	−0.0381 (0.0316)	−0.0191 (0.0372)	−0.0466* (0.0271)	−0.0216 (0.0297)	−0.0503** (0.0239)
N	62,918	53,889	31,774	35,073	12,333	19,977	51,188	9,501	65,469	50,743	48,206
R ²	.50	.50	.51	.52	.59	.56	.54	.59	.53	.54	.54
MATCH = YES, CHAR = HIGH											
BC	−0.0605** (0.0269)	−0.0709** (0.0269)	−0.0548 (0.0374)	−0.0491 (0.0335)	−0.0487** (0.0187)	−0.0157 (0.0331)	−0.0457** (0.0223)	−0.0450 (0.0521)	−0.0719*** (0.0248)	−0.0616*** (0.0206)	−0.0436** (0.0200)
N	57,356	55,787	33,658	37,393	14,703	20,733	54,456	9,855	65,121	55,656	46,768
R ²	.53	.55	.55	.54	.56	.55	.52	.59	.52	.49	.54
MATCH = NO, CHAR = LOW											
BC	−0.0531** (0.0219)	−0.0721*** (0.0208)	−0.0655*** (0.0232)	−0.0374* (0.0193)	−0.0127 (0.0491)	−0.109** (0.0457)	−0.0469 (0.0301)	−0.0204 (0.0366)	−0.0469* (0.0255)	−0.0216 (0.0297)	−0.0457* (0.0232)
N	75,365	65,290	38,639	42,278	15,259	27,667	61,984	11,981	77,427	50,743	57,049
R ²	.46	.45	.46	.46	.55	.50	.51	.55	.49	.54	.49
MATCH = NO, CHAR = HIGH											
BC	−0.0662** (0.0256)	−0.0710*** (0.0240)	−0.0588 (0.0370)	−0.0524 (0.0332)	−0.0439** (0.0193)	−0.0233 (0.0333)	−0.0405* (0.0237)	−0.0403 (0.0427)	−0.0695*** (0.0227)	−0.0616*** (0.0206)	−0.0553*** (0.0195)
N	67,922	66,486	38,664	43,263	17,445	28,588	64,368	12,344	77,831	55,656	55,707
R ²	.50	.50	.50	.50	.51	.49	.47	.55	.47	.49	.50

This table reports estimates for the coefficient (δ) of firm-year panel regressions ((high-dimensional fixed effects [FE] with matching [Equation (5)] or without matching [Equation (3)]) of stock illiquidity ILLIQ (AMIHUD or ILLIQ_3 of Section 2.1) on a dummy variable (BC) equal to one if a firm is incorporated in a state that has adopted a BC law and equal to zero otherwise, separately for firms with below-median (LOW) and above-median (HIGH) firm characteristic (CHAR, described in Section 2.4.4) related to adverse selection (RETVOL, IDIOVOL, DISPERSION, and INACCURACY; columns 1-4 and 12-15), agency problems (GINDEX, INDBOARD, INSTOWN, and EXINC; columns 5-8 and 16-19), or agency uncertainty (ROA, MKTCAP, and INSOWN; columns 9-11 and 20-22) in the year before a BC event. All specifications include firm FE, cohort-state-year and cohort-industry-year FE (MATCH = YES) or state-year and industry-year FE (MATCH = NO) as well as baseline controls and confounding effects dummies. State-year and industry-year FE (as in Gormley and Matsa 2016) are at the state of location and four-digit SIC level, respectively. A cohort is made of firm-year observations within up to 15 years before and after each BC adoption year. N is the number of available, singleton-free firm-year observations in my sample over 1976-2006; R² is the coefficient of determination; robust standard errors (in parentheses) are adjusted for clustering at the state-of-incorporation level.

* $p < .1$; ** $p < .05$; *** $p < .01$.

response to the passage of BC laws within each cohort g of the stock illiquidity of firms in the same industry j , located in the same state l , but incorporated in different states s on the year when those BC laws were passed, after further controlling for firm size and age ($X_{g,i,j,l,s,t}$) and Karpoff and Wittry's (2018) confounding effects ($CE_{g,i,j,l,s,t}$). Table 7 also reports pairs of coefficients δ from the separate estimation of Equation (3) for each sort, that is, when ignoring BC law cohort-level effects (MATCH = NO). As in Tables 3 to 6, all robust standard errors are adjusted for clustering at the state-of-incorporation level (also to account for matching with replacement across cohorts).

The evidence in Table 7 provides additional support for my hypotheses. First, the improvement in firm-level liquidity following the adoption of antitakeover provisions ($\delta < 0$) is both more pronounced and more often statistically significant within subsamples of firms displaying greater (HIGH) pretreatment fundamental uncertainty (in columns 1-4 and 12-15).⁹ For instance, the matching full model of Equation (5) implies that after a BC law is adopted, the price impact of a stock trade ($AMIHU_{i,t}$) for treated firms with prior above-median stock return volatility (MATCH = YES; column 1) declines (relative to similarly sorted control firms) on average by 131% more (i.e., by about \$97 more per \$10,000 trade) than among similarly treated and untreated firms with prior below-median stock return volatility. This difference amounts to 70% of the corresponding average baseline liquidity improvement among all treated firms relative to all control firms (δ of Equation (3), in column 3 of Table 3), and is similarly large when ignoring cohort effects (MATCH = NO). The estimated negative response of firm-level stock illiquidity to treatment by BC laws is also generally (albeit less uniformly) larger and more often statistically significant for firms plagued by greater prior agency problems or displaying greater prior agency uncertainty. For example, Table 7 shows that absolute response for the ternary composite index $ILLIQ_{3,i,t}$ to be between 44% and 187% more pronounced than in the baseline analysis of Table 3 (column 6, amounting to up to 15% of its standard deviation in Table 1) for treated (versus control) firms with HIGH GINDEX (column 16), LOW INDBOARD (column 17), HIGH ROA (column 20), or HIGH MKTCAP (column 21) than for their opposite median-sorted counterparts; a similarly conditioned differential response for $AMIHU_{i,t}$ amounts to an average of \$60 per \$10,000 trade (or 43% of its baseline) across firms with LOW INSTOWN (column 7) and EXINC (column 8)

⁹ However, the caveat noted in Section 2.4.3 about using (or, in this case, conditioning on) variables with limited and heterogeneous availability (see, e.g., GINDEX, INDBOARD, or the sparsest EXINC in Table 2) applies here as well. Accordingly, the resultant differences between estimated HIGH and LOW δ coefficients in Table 7 are only seldomly statistically significant by their unreported p -values across those smaller subsamples.

and HIGH INSOWN (column 11) relative to their opposite cohort-matched sorts.

In short, the above analysis indicates that firms incorporated in states passing antitakeover provisions in the 1980s and 1990s experienced a considerable improvement in the liquidity of their traded stocks relative to both similar control firms unaffected by those law changes as well as similarly treated firms whose prior fundamental uncertainty, agency problems, or agency uncertainty were lower. These findings are consistent with the notion that the adoption of those provisions may have mitigated agency-driven adverse selection risk for the stocks of the affected firms, ultimately ameliorating their stock price formation, as postulated in Section 1.

3. Conclusions

This study aims to contribute to the understanding of the frictions affecting the quality of firms' capital markets. Despite a substantial body of evidence on the impact of corporate governance on firms' behavior, much extant literature has either ignored the role of agency conflicts within the firm for security price formation or argued that some features of the firm's security trading may themselves affect its external and internal corporate governance. I propose and test the notion that firm-level agency costs may have nontrivial effects on firm-level stock liquidity.

To characterize this notion, I postulate that privately optimal, but socially suboptimal managerial behavior within a firm may affect its stock liquidity provision (e.g., market depth) in the presence of adverse selection risk from strategic speculation endowed with value-relevant information about these agency problems, especially when frictions to governance or price formation are ex ante more severe. In the [Internet Appendix](#), I formalize these ideas in a stylized model of trading based on [Kyle \(1985\)](#). An empirical analysis of this channel presents many difficulties. Measuring the (adverse selection portion of) liquidity of a firm's stock and the ex ante severity of firm-level agency problems — both intrinsically elusive notions — is challenging and controversial. As importantly, the endogeneity of corporate governance and price formation precludes the causal interpretation of any estimated correlation between measures of firm-level agency costs and stock market illiquidity.

I tackle these issues by first (1) computing numerous firm-level illiquidity proxies in the literature — some capturing the notion of price impact in [Kyle \(1985\)](#); e.g., [Amihud 2002](#)); some broad in scope and widely available (e.g., the effective cost of trading of [Hasbrouck 2009](#)); and some less often available but designed to depend on information asymmetry among stock market participants (e.g., the decomposition by [George](#),

Kaul, and Nimalendran 1991 of the half-spread of Roll 1984), as well as various composite measures of their (adverse selection) commonality; and then (2) considering the impact on these measures of the staggered adoption of antitakeover (business combination [BC]) provisions in U.S. states during the 1980s and 1990s, a plausibly exogenous *negative* shock to the agency-driven information advantage of speculators in treated firms.

Consistent with my predictions, I find that (1) the stock market liquidity of firms incorporated in states enacting BC laws *improves* after their adoption relative to otherwise similar firms (e.g., located in the same state, operating in the same industry, and after controlling for firm size, age, and confounding effects) but incorporated in states where BC laws have not (or not yet) been passed; and (2) the improvement in liquidity is robust to further accounting for many additional firm covariates previously related to the adoption of antitakeover provisions in the literature, as well as most pronounced among treated firms with prior characteristics (such as high fundamental volatility, agency costs, or agency-based uncertainty) that (I conjecture) may be associated with *high* prior agency-driven information asymmetry.

My investigation indicates that firms' agency problems may play an important role for the price formation of their securities. I hope that this insight may stimulate future work on the externalities of various forms of suboptimal corporate behavior for financial market quality.

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