JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 61, No. 6, Sep. 2026, pp. 2688–2736
© The Author(s), 2025. Published by Cambridge University Press on behalf of the Michael G. Foster School of Business, University of Washington. This is an Open Access article, distributed under the terms of the Creative Commons Attribution-NonCommercial-ShareAlike licence (http://creativecommons.org/licenses/by-nc-sa/4.0), which permits non-commercial re-use, distribution, and reproduction in any medium, provided the same Creative Commons licence is used to distribute the re-used or adapted article and the original article is properly cited. The written permission of Cambridge University Press or the rights holder(s) must be obtained prior to any commercial use.
doi:10.1017/S0022109025102482

Does Litigation Risk Deter Insider Trading? Evidence from Universal Demand Laws

Binay K. Adhikari
UTSA: The University of Texas at San Antonio
binay.adhikari@utsa.edu

Anup Agrawal
University of Alabama Culverhouse College of Business
aagrawal@ua.edu (corresponding author)

Bina Sharma
Southern Utah University
binasharma@suu.edu

Abstract

We exploit U.S. states' staggered adoption of Universal Demand (UD) laws to study how the risk of shareholder lawsuits affects insider trading. UD laws, which make it harder for shareholders to bring derivative lawsuits against directors and officers, lead to more profitable insider trades, especially sales. This effect is stronger among smaller firms and firms with lower institutional monitoring. After UD laws, the timing of insiders' trades also appears more opportunistic and riskier, for example, sales increase before negative earnings surprises. Overall, our study offers clean evidence that the threat of shareholder litigation deters opportunistic insider trading.

I. Introduction

Many scholars and policymakers believe that opportunistic trading by corporate insiders erodes outside investors' confidence in the fairness and integrity of financial markets, and if left unchecked, this practice may even lead to market failures.[1] Therefore, there is ongoing interest in understanding the impact of regulatory mechanisms aimed at deterring insider trading. However, empirical

Special thanks are due to an anonymous referee, David Cicero, and Jarrad Harford (the editor) for detailed comments and helpful suggestions. We thank Ian Appel, Yonathan Arbel, Jennifer Arlen, Mehran Azimi, Diego Escobari, Chris Herpfer, Jon Karpoff, Rachel Li, Andre Mollick, Katie Moon, Sugata Ray, Ken Rosen, Shane Underwood, Daniel Weagley, and seminar and conference participants at Emory University, Georgia Tech, Stevens Institute of Technology, Suffolk University, University of Alabama (Economics and Finance, and Law School), University of Miami, University of Oklahoma, University of Texas at RGV, University of Texas at San Antonio, and 2023 AFA and 2020 MFA meetings for useful comments. We are grateful to Shuping Chen for sharing the financial statement disclosure quality data, and Lukasz Pomorski for sharing the insider trade classification data.

[1]Bhattacharya (2014) provides a comprehensive review of the literature that argues for and against this view.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

evidence on the efficacy of insider trading regulations has been mixed. Some studies find that these regulations have effectively reduced the frequency and profitability of opportunistic trades (e.g., Agrawal and Jaffe (1995), Garfinkel (1997), and Xu (2008)), while others cast doubt on their efficacy (see, e.g., Jaffe (1974), Seyhun (1992), and Banerjee and Eckard (2001)).

The mixed findings on the impact of insider trading regulations can be partly attributed to the lack of strong identification techniques that can isolate the causal effect of litigation risk on insider trading. Identification is particularly challenging because most modern insider trading laws in the United States are adopted at the federal level[2] and affect all firms simultaneously. Moreover, regulatory reforms are rarely random and often follow heightened concerns about illegal insider trading activities. The lack of adequate cross-sectional variation and potential endogeneity of regulatory changes limits the ability of existing studies using federal regulations to establish causality. For example, a decrease in insider trading following the passage of a stricter law may simply reflect mean-reversion after a period of rampant insider trading that prompted the law. Studies focusing on enforcement intensity and court decisions (e.g., Cheng, Huang, and Li (2016), Del Guercio, Odders-White, and Ready (2017)) face similar limitations. Accordingly, Bhattacharya (2014) concludes his extensive review of the insider trading literature with the verdict that "We need methodologies (such as natural experiments) to evaluate the efficacy of current and future insider trading rules."

This article attempts to fill this important gap in the literature. We exploit the staggered adoption of Universal Demand (UD) laws in 23 U.S. states and the District of Columbia over 23 years to examine the effect of shareholder litigation risk on opportunistic insider trading. Our research is motivated by recent studies that find that UD laws significantly reduce shareholders' ability to bring derivative lawsuits (DLs) against directors and officers (D&O) for breach of their fiduciary duty to the company (see, e.g., Davis (2008), Houston, Lin, and Xie (2018), and Appel (2019)).

How do UD laws affect insider trading? DLs, which typically allege that D&O breached their fiduciary duty, often also include allegations of insider trading (see Erickson (2010)), especially insider selling. Prior studies find that D&O misconduct is much more likely to result in shareholder lawsuits when it is accompanied by evidence of insider trading by D&O (see, e.g., Johnson, Nelson, and Pritchard (2007)). This is partly because insider trading by D&O strengthens the evidence that the primary alleged wrongdoing was deliberate and financially motivated. Therefore, the threat of DLs should deter insiders from trading opportunistically.[3] Houston et al. (2018) and Appel (2019) estimate that the adoption of a UD law decreases the annual probability of DLs against a firm by as much as about one-half, and decreases in DLs are not substituted by increases in direct shareholder lawsuits. Therefore, we posit that by making it harder to bring DLs, UD laws enable insiders who are subject to derivative lawsuits (i.e., D&O) to trade more opportunistically.

[2]For example, SEC rule 10b-5, Section 16b of the Securities Exchange Act of 1934, Insider Trading Sanctions Act of 1984 (ITSA), and Insider Trading and Securities Fraud Enforcement Act of 1988 (ITSFEA).

[3]We discuss the merit of DLs in Section II.A.

States' adoption of UD laws serves as excellent quasi-natural experiments to study the effect of regulation on insider trading for two reasons. First, the staggered adoption of UD laws by different states over many years provides rich time-series and cross-sectional variation in the ex ante probability of shareholders bringing DLs. Second, UD laws satisfy both the relevance and exclusion restrictions for a natural experiment in our setting. The relevance condition is met because, as discussed in Section II.A. UD laws dramatically reduce the empirical probability of DLs by imposing a demand requirement that significantly hinders the possibility of a successful lawsuit, regardless of the board's response to the demand.[4] The exclusion restriction is satisfied because most states appear to have adopted these laws for reasons largely unrelated to concerns about insider trading. This makes the passage of UD laws plausibly exogenous to preexisting levels and profitability of insider trading.

Our empirical methodology builds on recent studies such as Bertrand and Mullainathan (2003), Gormley and Matsa (2016), and Appel (2019) that employ multiple exogenous shocks for identification to make causal inferences. We create treatment and control groups using indicator variables based on the timing of adoption of UD laws by firms' states of incorporation. We then employ difference-in-differences (DiD) regression specifications to estimate the effect of the ex ante threat of shareholder litigation on the volume and profitability of insider trades.

Using our full sample of trades by D&O, we analyze the effect of UD laws on these trades' profitability as measured by buy-and-hold abnormal return (BHAR).[5] Our DiD regressions show that compared to control firms, the D&O[6] of treatment firms avoid average losses of about 2.6%, 2.8%, and 3.4% in BHAR over 1, 3, and 6 months, respectively, following a sale. These DiD estimates are also economically meaningful. These incremental abnormal returns after UD law adoptions are larger in magnitude than the mean abnormal returns (i.e., loss avoidance) of 0.5%, 1.3%, and 1.9% over the corresponding periods for all insider sales in our sample. They translate into insiders avoiding incremental abnormal dollar losses after UD law adoption of about $27,000 and $52,500 in constant 2013 dollars over 1- and 3-month periods compared to the corresponding values of $2,500 and $7,700 for all insiders. Although the absolute amounts of loss avoidance may still seem modest for highly paid executives, this is perhaps not surprising given that UD laws are not specifically targeted at insider trading. Note that this evidence by itself cannot distinguish between two plausible explanations of our findings: i) insiders exploit negative private information more aggressively after UD laws or ii) insiders are less hesitant to engage in legitimate trades when the threat of shareholder lawsuits decreases. Our later tests try to separate between the two stories.

[4]See Appel (2019) for more elaborate evidence for this argument.

[5]Our conclusions remain generally unchanged when we use alternative ways to measure returns, such as raw return (CRET), or the characteristic-adjusted abnormal return of Daniel, Grinblatt, Titman, and Wermers ((1997), henceforth, DGTW), instead of BHAR. We report these tests in Table A1.

[6]DLs, which are affected by UD laws, do not usually apply to 10% blockholders, who are also required to report their trades to the SEC. We treat these blockholders as a placebo group and analyze their trades separately in Section IV.G. Unlike trades by D&O, blockholders' trades do not become more profitable after UD laws.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

In our full sample, the evidence on the effect of UD laws on the profitability of insider purchases in terms of BHAR is insignificant. However, insiders' abnormal dollar profits on purchases, which account for both trade size and subsequent returns, increase significantly after UD laws over 1- and 3-month holding periods.

We conduct a number of additional tests to understand how UD laws affect the timing and opportunism in insider trades. We find that UD laws predict increases in the dollar value of shares sold within a given period, but not the number of shares. This finding suggests that after UD laws, insiders increase the dollar value of their individual sales transactions, timing them to avoid subsequent underperformance relative to risk-adjusted benchmarks. Furthermore, UD laws lead to a higher ratio of opportunistic sales to routine sales, as defined by Cohen, Malloy, and Pomorski (2012). We also analyze trades before quarterly earnings announcements (pre-QEA trades), which are particularly opportunistic (see, e.g., Ali and Hirshleifer (2017)). We find that after UD law adoption, pre-QEA sales become more profitable over 1- or 3-month horizons. Moreover, insiders significantly increase pre-QEA sales only before negative earnings surprises after the adoption of UD laws. Overall, these results suggest that a decrease in shareholder litigation risk deters more opportunistic behavior by insiders.

Our cross-sectional tests suggest that insiders' ability to benefit from UD laws depends on firm characteristics that facilitate opportunistic trading. Accordingly, UD laws lead to more profitable insider sales in i) firms with lower monitoring by institutional blockholders and ii) smaller firms, which have fewer alternative governance mechanisms (e.g., company policies against insider trading) in place.

Finally, we conduct a rich set of robustness checks of our main results. First, to mitigate potential biases from an unbalanced panel and time-varying treatment effects, we replicate our baseline model using a cohort-level stacked DiD approach. Second, we show that our conclusions are robust to alternative ways of estimating the profitability of trades. Third, we confirm that our results are robust to Heath, Ringgenberg, Samadi, and Werner's (2023) critique about reusing natural experiments. Specifically, as we discussed previously, UD laws appear to satisfy both the relevance and exclusion restrictions for a natural experiment in our setting. We also confirm the statistical significance of our tests that account for multiple hypothesis testing based on the number of previously published studies using the experiment. Fourth, our findings are not affected by the recent financial crisis. Fifth, our results also remain intact when we control for potential confounding effects of many other state laws important for corporate governance and ex ante litigation risks. Finally, our results remain similar when we drop all firms located in states under the jurisdiction of the Ninth Circuit Court to disentangle the effect of a 1999 court decision that restricted shareholders' ability to bring securities class action lawsuits.

We find strong and consistent evidence throughout our analysis that insider sales become more profitable (relative to benchmarks) after UD laws, while there is no consistent result for purchases. This asymmetry can be explained by the nature of derivative lawsuits. The premise of a DL is a breach by D&O of their fiduciary duty (of care, loyalty, and obedience),[7] which often entails negative information or *harm* to the company. By decreasing the threat of shareholder lawsuits, UD laws enable

[7]See, e.g., Wizbowski (2024).

D&O to misbehave in ways that can harm the firm, for example, by earnings manipulation, self-dealing, or neglect. Knowledge of this negative information or harm also provides insiders an opportunity to sell stock based on their private information. Accordingly, many DLs include evidence of insider sales as a secondary complaint, as we discuss in Section II.B. Of course, opportunistic insider trading by D&O, by itself a breach of their fiduciary duty of loyalty to the company and its shareholders, is an issue in some shareholder lawsuits, which explains why we also find evidence of an increase in the profitability of insider purchases in certain cases. The asymmetric impact of UD laws on insider selling versus buying highlights the role of litigation risk in deterring insiders from exploiting negative private information more than positive information.

This study makes several contributions to the literature. First, it contributes to the literature discussed in the first two paragraphs of this section on the efficacy of regulations in deterring opportunistic insider trades. Our novel contribution is our identification strategy, which uses plausibly exogenous shocks to ex ante litigation risk due to states' staggered adoption of UD laws. As discussed earlier, our approach is more suitable for establishing causality than those of many previous related studies which rely on federal laws.

Second, our evidence challenges a view, especially prevalent in the legal literature, that shareholder litigation is often frivolous and imposes a deadweight loss on the firm (see, e.g., Romano (1991), West (2001)). Our evidence suggests that shareholder litigation threat plays a role in deterring some forms of opportunistic insider trading and provides evidence that private enforcement mechanisms can complement public enforcement of insider trading rules.

Third, our article is related to recent papers that examine various economic effects of the adoption of UD laws. Houston et al. (2018) find that UD laws, which make it harder for shareholders to bring DLs, result in a higher cost of capital for firms due to a decrease in information quality, greater risk-taking, and a higher level of insider trading. Boone, Fich, and Griffin (2023) find that UD laws lead to more opaque financial reporting and a worse information environment. Our article complements these studies by focusing on how UD laws affect insiders' incentives for opportunistic trading and provides an in-depth analysis of the resulting changes in the profitability of insider sales and purchases.

Our article is also related to a concurrent working paper by Jung, Nam, and Shu (2021), who analyze the volume of opportunistic insider trading after UD laws, while we analyze both its profitability and volume, both for all insider trades and pre-QEA trades. Our evidence indicates riskier trades and more opportunistic timing as the drivers of higher profitability after UD laws. Jung et al. argue that while direct lawsuits are likely to be effective mainly against sales, DLs can also be effective against opportunistic insider purchases because they are based on a breach of fiduciary duty and do not need to show economic injury to shareholders. But they find (in their Tables 4 and 5) that the volume of opportunistic sales increases much more than purchases after UD laws, which reduce the threat of DLs. Our article complements theirs by showing that the profitability of insider trades increases after UD laws, particularly for sales. Moreover, our research is the first to dig into the question of whether derivative lawsuits complain about insider sales or purchases, an issue on which there appears to be no empirical evidence. Our hand-collected

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

preliminary evidence suggests that allegations of opportunistic purchases rarely show up in derivative lawsuits (see Section II.B), which can explain why our empirical evidence of an increase in the profitability of opportunistic sales after UD laws is much stronger than the evidence on opportunistic purchases.

Fourth, our study contributes to an important but often overlooked issue of public versus private enforcement of opportunistic insider trading. Most prior studies focus on public enforcement of illegal insider trading, that is, prosecution by regulators such as the SEC and the Department of Justice (DOJ), based, for example, on SEC Rule 10(b)-5, ITSA, and ITSFEA. Agrawal and Nasser (2012) conjecture that private enforcement (e.g., by shareholder lawsuits) can sometimes be more effective than public enforcement in deterring opportunistic insider trading.[8] This is plausible because, for insiders of most firms, the risk of being sued by shareholders is higher than the risk of being sued by regulators. Regulators such as the SEC have limited staff and resources, so they are outgunned in monitoring and enforcing insider trading laws against numerous potential insiders in a large number of public companies. Therefore, regulators tend to focus their monitoring and enforcement efforts on a few high-profile cases that are likely "slam-dunks" and likely to generate substantial media coverage (see, e.g., Dechow, Sloan, and Sweeney (1996), Agrawal and Chadha (2005), and Agrawal and Cooper (2015)). While that may be an effective strategy for a resource-constrained regulator, it is unlikely to deter all, or even most, insider trading. Because DLs are filed by shareholders against corporate insiders, our evidence here that roadblocks against shareholder lawsuits increase insider trading profits speaks to the efficacy of private enforcement in deterring opportunistic insider trading.

Finally, our article also contributes to the literature on corporate governance. Specifically, our study complements several recent studies that exploit exogenous shocks to establish causal effects of litigation risk and new laws on corporate governance and firm value (see, e.g., Bertrand and Mullainathan (2003), Gormley and Matsa (2011), (2016), and Appel (2019)).

II. Background, Literature Review, and Hypothesis Development

A. DLs, UD Laws, and Insider Trading

A DL is filed by a shareholder against D&O on behalf of the company for breach of their fiduciary duties of loyalty (e.g., fraud, mismanagement, earnings manipulation, accounting irregularities, self-dealing, or dishonesty) or care (e.g., negligence to timely disclose pertinent, especially negative, information to investors). DLs can be a potent check on the behavior of D&O, who face a high risk of incurring out-of-pocket expenses because state laws typically do not allow firms to

[8]Indeed, Agrawal and Jaffe (1995) find that the threat of DLs by shareholders appears to be quite effective in deterring insider trading in takeover targets in the pre-1961 era, when the SEC did not enforce its main tool against insider trading, namely Rule 10b-5, in public companies. This effect appears to persist in the modern regulatory era (see Agrawal and Nasser (2012)), which is significant given that takeover bids offer a tempting opportunity to target insiders and are a focus of the SEC's enforcement efforts against insider trading (see, e.g., Meulbroek (1992)).

reimburse D&O for losses in cases involving misconduct (e.g., criminal or fraudulent activities) or illegal profits, nor are these losses covered by D&O insurance (see, e.g., Willis (2005), Lin, Officer, and Zou (2011), Embroker (2019), and Jung et al. (2021)). Moreover, lawsuits and the underlying misconduct can cause insiders to lose their positions in the company and damage their reputation in the labor market (see, e.g., Agrawal, Jaffe, and Karpoff (1999), Ferris, Jandik, Lawless, and Makhija (2007), Karpoff, Lee, and Martin (2008), and Brochet and Srinivasan (2014)).

To have the standing to bring a DL, the plaintiff usually needs to have been a shareholder at the time of the wrongdoing by the insider. But in some cases, these lawsuits can be brought by attorneys themselves, who simply have to buy one share to become a shareholder before bringing a lawsuit, as in section 16b cases (short-swing rule; see, e.g., Agrawal and Jaffe (1995)).

To initiate a DL, most states require an eligible shareholder to file a demand on the board (known as the "demand requirement") to sue the alleged wrongdoers. Shareholders can initiate derivative suits themselves only if the board refuses the demand or does not act on it. However, because DLs typically name all or most of the board members as defendants, the board is obviously not eager to act on the demand! So, boards typically ignore the demand or appoint a board committee, which sits on it for months before declaring that it looked into the matter and found no wrongdoing. Therefore, many jurisdictions allow an exception to the demand rule, known as a futility exception. The standards for determining futility vary across jurisdictions (see, e.g., Swanson (1992)). For example, Delaware has a two-prong test requiring shareholders to allege "particularized facts" that create a reasonable doubt that: i) the directors are disinterested and independent and ii) the challenged transaction was a product of a valid exercise of business judgment (Kinney (1994)). While that sets a high hurdle for bringing a DL, in the wake of corporate scandals such as Enron and Worldcom and the adoption of the Sarbanes-Oxley Act in 2002, Delaware made it easier for shareholders to bring DLs (see, e.g., Jones (2004), Qi and Pederson (2019)).

The critics of demand futility argue that the demand requirement allows management to address shareholder allegations and provides a chance to either take corrective action or reject the proposed action. Besides, the demand requirement helps to resolve a dispute without costly litigation (see, e.g., Swanson (1992)). The American Law Institute (ALI) and the American Bar Association (ABA) advocated the need for ending the futility exception. ABA (2008) proposed a demand requirement in all derivative actions (Universal Demand) in its Model Business Corporation Act (MBCA). In response to MBCA, 23 states plus DC have adopted UD laws from 1989 to 2011.

The demand requirement, imposed on all DLs against companies incorporated in a state by its adoption of a UD law, significantly reduces shareholders' incentive to bring a DL by substantially reducing the chances of a lawsuit succeeding. Boards obviously do not want to be sued! So, they either simply reject the demand or appoint a special committee that can (pretend to) investigate the matter endlessly.[9]

[9]Under MBCA, shareholders must wait for 90 days after making a written demand to file a DL. Exceptions include corporations that refuse the demand within 90 days and corporations that will suffer irreparable injury if the suit is not brought earlier (see, e.g., Kinney (1994)).

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

And courts almost always side with the board's decision. This happens because if the board refuses the demand, courts can typically only review if the board failed to exercise a valid business judgment (Pinto and Branson (2013), and Appel (2019)). If the court ascertains that a majority of the board or the special committee is independent (which is true in most cases), they will dismiss the suit (Kinney (1994)). Empirically, Houston et al. (2018) and Appel (2019) find that the probability of DLs indeed decreases substantially after a state adopts a UD law. Thus, time variation in the adoption of UD laws by different states leads to time-series and cross-sectional variations in shareholders' ability to bring DLs against insiders for breach of their fiduciary duties (Davis (2008), Appel (2019)).

DLs typically allege that D&O breached their fiduciary duty. In addition, they often include allegations of insider trading (in about 60% of the cases; see Erickson (2010)).[10] Evidence of insider trading, which is publicly available due to the Section 16a reporting requirement, serves to establish a motive for D&O to engage in the alleged wrongdoing involving the company and increases the odds of the shareholder lawsuit succeeding (see, e.g., Johnson et al. (2007), Choi, Nelson, and Pritchard (2009)). The most common type of settlement in a DL is governance reform, rather than monetary compensation (see, e.g., Ferris et al. (2007), Erickson (2010)), although there have been several large dollar settlements.

States claim to have adopted UD laws primarily to discourage frivolous lawsuits and to allow boards to take corrective actions instead of immediately facing lawsuits. Importantly, their decision to adopt UD laws appears largely unrelated to concerns about insider trading. This feature makes the adoption of UD laws an ideal quasi-natural setting to test the effect of shareholders' litigation risk on insider trading because UD laws are mostly free from concerns about reverse causality concerning insider trading. Thus, our approach contrasts with those of most previous studies, which rely on federal laws or court decisions specifically designed to address elevated concerns about opportunistic insider trading.

B. Are DLs More Relevant for Opportunistic Insider Sales or Purchases?

As discussed in Section I, a DL is filed by a shareholder against D&O on behalf of the company for breach of their fiduciary duties to the company. These breaches often result in *harm* to the company. The primary claims of harm are often accompanied by claims that insiders sold stock to avoid losses from the harm. The additional evidence of insider trading by D&O increases the merit of a lawsuit and therefore increases the probability of its being filed (see, e.g., Johnson et al. (2007)).

[10]Donelson, Kettell, McInnis, and Toynbee ((2022), Table 2) find that 57 out of 1,775 DLs in their sample have insider trading as the *primary* allegation. They do not tabulate allegations of insider trading that accompany other primary allegations (such as disclosure, accounting, and transactions). As we discuss above, most DLs do not involve insider trading as the primary allegation. Instead, allegations of insider trading are included as part of broader allegations of breach of fiduciary duty and to establish a motive for the breach. In fact, the appeal of UD laws as an instrument for litigation risk partly comes from the fact that DLs, which UD laws make harder to bring, are rarely aimed primarily at insider trading. Accordingly, the adoption of UD laws is rarely due to concerns about insider trading. For identification, this is a desirable feature that suggests that UD laws satisfy the exclusion restriction needed for testing the effect of DLs on insider trading behavior.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

An example is a derivative action brought by Citigroup Inc.'s shareholders against the company's current and former D&O in 2009.[11] This lawsuit alleges that the D&O committed five types of wrongdoing in connection with Citigroup's mortgage-related losses: i) breached fiduciary duties of care and loyalty by allowing Citigroup to knowingly make risky mortgage-related investments, ii) failed to inform shareholders of Citigroup's subprime exposure, iii) wasted corporate assets by repurchasing stock, iv) committed securities fraud by making or authorizing misleading statements that omitted the extent of Citigroup's investment in subprime mortgages, and v) some defendants committed insider trading by selling Citigroup stock while in the possession of material, nonpublic information.

Clearly, in this case, the complaint about insider trading is about sales. But do DLs that include complaints of insider trading typically complain about insider sales or purchases? We cannot find any empirical evidence on this issue. To shed some light on this question, we take two tacks. First, we did a search on Google Scholar Case Law in August 2024, using the following search terms: "insider trading" + "derivative action" OR "derivatively" OR "derivative litigation" but *without specifying purchases or sales.* The search covered all state courts for our sample period of 1996–2013. We then carefully read the case files of the top 20 search results listed by relevance and summarize our findings in Table A5. The complaint in 11 (1) of these cases includes allegations of insider sales (purchases).[12] This limited evidence suggests that DL cases that include complaints about insider trading are more likely to complain about insider sales, rather than insider purchases, even though opportunistic purchases also involve a breach of fiduciary duty of loyalty or obedience and can be a subject of DLs. Second, we find that many of the high-profile DLs reported by the media that involve allegations of insider trading are also predominantly about sales.[13]

Jung et al. ((2021), pp. 3–4) point out that direct shareholder lawsuits, which require proof of economic injury to the plaintiffs, are difficult to pursue against informed insider purchases, which usually occur before good corporate news events. But DLs can still be brought here because such trades involve a breach of D&Os′ fiduciary duty. On the other hand, both direct lawsuits and DLs can be brought against insider sales. Therefore, DLs can potentially affect insider sales and

[11]"In re Citigroup Inc. Shareholder Derivative Litigation," retrieved March 21, 2022, from https://caselaw.findlaw.com/court/de-court-of-chancery/1335116.html (see Part D.1, New York Action).

[12]Three of the cases were either duplicate entries or filings. In the rest of the cases, there is either no mention of insider trading or it is mentioned in the context of options backdating.

[13]Here are some prominent recent examples of DLs that involve allegations of insider trading. Oracle CEO Larry Ellison settled for $100 million a lawsuit alleging that he sold about $900 million of stock before the company announced poor earnings results (*New York Times*, September 12, 2005); Chipotle D&O sold stock during the firm's problems with food illness outbreaks (Marketwatch, June 2016); Fitbit D&O sold $115 million of stock in its IPO while being aware of widespread issues with the company's heart-rate monitoring technology (Yahoo! Finance, December 14, 2018); Yahoo! settled a case for $23 million over allegations that D&O sold stock before revealing the company's massive data breaches (D&O Diary, January 21, 2019); Wells Fargo settled a case for $320 million that alleged that D&O sold stock during the company's phony customer accounts scandal (D&O Diary, March 3, 2019); and Facebook D&O sold $1.5 billion of stock while the company faced a looming crisis over privacy concerns and the Cambridge Analytica scandal (Dodd-Frank.com, May 1, 2019).

purchases differently. But whether DLs are more effective against insider sales or purchases is unclear a priori.

C. Are Direct or Derivative Shareholder Lawsuits More Effective Against Insider Trading?

Shareholders often prefer to bring direct lawsuits because any monetary compensation goes directly to them, instead of the company as in DLs. But direct lawsuits require evidence of direct harm to the plaintiffs, which is hard to show in cases involving insider purchases, whereas DLs only need to allege breach of fiduciary duty. So, except for the demand requirement, there is a lower hurdle to pursue a DL. Moreover, plaintiffs often file both types of lawsuits to increase their chances of a favorable verdict or settlement. Appel (2019) shows that DLs are generally as prevalent as direct shareholder lawsuits (often filed as class actions) against public companies.

D. Literature Review and Hypothesis Development

A large literature in law and finance finds that legal protection of shareholder rights is an important mechanism for reducing agency problems between managers and shareholders (see Shleifer and Vishny (1997) for a review). But a sizeable legal literature argues that most shareholder litigation is frivolous and mainly benefits lawyers and insurance companies. A common theme in this literature is that DLs are brought not so much to protect shareholder interests, but rather by plaintiffs' attorneys hoping to extract settlement fees. D&O are usually reimbursed for any financial liability either by the company or by D&O insurance. Therefore, D&O do not bear much financial risk for their misbehavior. Consequently, litigation threat does not really deter managerial misbehavior (see, e.g., Romano (1991), Weiss and Beckerman (1995), Coffee (2006), Baker and Griffith (2008)).

But many empirical studies find that DLs have important effects, especially on corporate governance. Ferris et al. (2007) find that boards improve on several dimensions following DLs. More recently, several studies find that a reduction in the threat of shareholder litigation leads to less institutional blockholding and weaker internal governance provisions (see, e.g., Crane and Koch (2016), Appel (2019), and Huang, Roychowdhury, and Sletten (2020)). Weaker corporate governance, in turn, leads to an increase in corporate misconduct such as hoarding of negative news, earnings management (see, e.g., Houston, Lin, Liu, and Wei (2019), Huang et al. (2020)), a deterioration in firms' information environment (e.g., Boone et al. (2023)), and ultimately an increase in firms' cost of capital (see Houston et al. (2018)).

In the context of insider trading, many studies find that corporate insiders face real litigation risk and take costly actions to circumvent it. For example, Cheng and Lo (2006) find that insiders strategically time changes in firm policies to maximize their profits from insider trading. Lee, Lemmon, Li, and Sequeira (2014) suggest that insiders in firms that put voluntary restrictions on insider trading continuously take advantage of private information while being more cautious with exploiting negative private information. Dai, Kang, and Lee (2016) also suggest that insiders deliberately use their information advantage to avoid litigation risk.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

Since UD laws reduce the risk of derivative lawsuits faced by D&O, we hypothesize that these laws change how insiders trade their firms' stock. Specifically, UD laws may reduce constraints on informed trading based on private information, or alternatively, reduce concerns about litigation for trades that might appear opportunistic ex post, regardless of the motivation.

As discussed in Section II.B, derivative lawsuits may affect insider sales and purchases differently, as sales are more commonly alleged in DLs than purchases. However, the theoretical prediction is rather ambiguous. While direct shareholder lawsuits are difficult to pursue against informed purchases (which typically precede good news), derivative lawsuits can target both sales and purchases since both can involve breaches of fiduciary duty. Therefore, whether UD laws have stronger effects on sales or purchases is an empirical question, and we examine them separately throughout our analysis.

III. Data, Variables, and Summary Statistics

A. Sample and Data

Our main explanatory variable (*After UD Law*) is an indicator variable which equals 1 if a firm's state of incorporation has a UD law in a given year; it equals 0 otherwise. Table A4 presents the timeline of states' adoption of UD Laws. Following the prior literature, we define *After UD Law* based on firms' historical states of incorporation.[14]

Our data on insider trades come from Thomson Reuters Insider Filing data (TIF). Our sample begins in January 1996, when TIF starts reporting these data in earnest and ends in December 2013, 2 years after the last UD law was adopted.[15] These data include all open market trades reported by corporate insiders (directors, officers, and beneficial owners of 10% or more of the company's stock) through SEC Forms 3, 4, and 5. We drop beneficial owners' transactions because they do not have any fiduciary duty to fellow shareholders, so they are not subject to UD laws. We analyze directors' and officers' open-market purchases and sales of common stock (CRSP share codes 10 or 11) of firms listed on NYSE, Amex, or Nasdaq and exclude financial and utility firms. Our sample includes transactions without missing data and verified for accuracy by Thomson Reuters and indicated by cleanse codes of R, H, L, C, or Y.[16]

[14]We use historical state of incorporation to define UD law, following Cohen (2012), Gormley and Matsa (2016), and Appel (2019). We obtain each firm's historical state of incorporation from SEC filings. We use the oldest state of incorporation for backfilling the incorporation state before 1994, when online filing became mandatory. We drop observations where the state of incorporation is not correctly reported. Prior studies exclude firms that reincorporated during the sample period. However, Appel (2019) finds little effect from this adjustment. Besides, the current state of incorporation differs only for less than 5% of the sample.

[15]Our sample includes the period of the recent financial crisis. Our conclusions remain unchanged if we limit our sample to the end of 2007 or include an interaction of the crisis period (years 2008 and 2009) with the *UD Law* variable.

[16]Following Aldredge and Cicero (2015), these cleansing codes indicate the following: R = Data verified through cleansing process; H = Cleansed with a very high level of confidence; L = Cleansed; C = Corresponding record added; Y = Informational.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

Thomson Reuters Insider Filing (TIF) database does not use separate codes for open-market and private transactions. Following prior studies (e.g., Lakonishok and Lee (2001), Marin and Olivier (2008)), we use two data screens to isolate open-market transactions, albeit imperfectly. Accordingly, we exclude trades where the transaction price falls outside the trading range for the day on CRSP and where more than 20% of the outstanding shares are traded. We also exclude trades involving less than 100 shares and trades in penny stocks, that is, where the share price is less than $2 at the beginning of the calendar year. Following the prior literature on insider trading,[17] we separately add up the purchases and sales by all insiders on a given day. We analyze insiders' sales and purchases separately because UD laws can have differential effects on insiders' incentives to sell and buy, as discussed in Section II.B.[18]

We obtain accounting and stock price data from the Compustat and CRSP databases. The main dependent variables include buy-and-hold abnormal return (BHAR), total number and dollar value of shares traded, and total dollar abnormal profits for each trade day. We calculate the BHAR for 6 months (126 trading days; bhar6m), 3 months (63 trading days; bhar3m), and 1 month (21 trading days; bhar1m) beginning on the day of the insider trade.[19] We use Carhart's (1997) four-factor model to calculate the BHARs. When estimating the model, we exclude returns for 50 days before the insider trading day to avoid a bias from any pretrade price run-ups or run-downs. We calculate posttrade expected returns and BHARs after excluding the estimated alpha to avoid a bias from any systematic price trends during our estimation window of $(-250, -50)$ days.[20] The BHAR incorporates compounding effects and is well-suited for testing longer term abnormal returns (see Barber and Lyon (1997)) while accounting for asset pricing factors. Following prior studies (e.g., Huddart, Ke, and Shi (2007), Kallunki, Nilsson, and Hellström (2009), and Kallunki et al. (2018), we therefore use the BHAR for different holding periods computed from daily returns as our main measure of profitability. As robustness checks, discussed in Section V.B, we also use two alternate measures of insider trading profitability that do not require estimating asset pricing parameters or alpha: i) cumulative total returns (CRETs) and ii) DGTW characteristics-adjusted abnormal returns. Our main conclusions do not change when we use these alternate measures.

[17]See, e.g., Huddart and Ke (2007), Kallunki, Kallunki, Nilsson, and Puhakka (2018), and Wu (2018).

[18]The results are similar when we drop the firm-trade days that have both a purchase and a sale (4006 observations, about 1% of our sample).

[19]We examine returns over a 6-month window because the short-swing rule (Section 16b) prohibits insiders from profiting on round-trip trades within shorter periods. However, as we discuss in footnote 29, the rule may not impose a binding constraint on sales by D&O, who often hold excess stock received as part of their compensation via stock and option grants (and option exercises). So they may not have a pressing need to buy back shares they sell. Therefore, we also analyze returns over shorter periods, following numerous prior studies on insider trading (e.g., Aldredge and Cicero (2015), and Ali and Hirshleifer (2017), Bettis, Coles, and Lemmon (2000), and Cohen et al. (2012)). This issue is moot for purchases since insiders do not need to sell the stock in order to profit from an increase in the stock price, unless they want to realize the gain.

[20]We exclude alpha because insiders tend to sell more after good performance and buy more after poor performance, making alphas generally positive for insider sales and negative for insider purchases during the estimation period. Including alpha in the abnormal return calculation can lead to significant decreases (increases) in abnormal returns for insider sales (purchases), potentially biasing our results. However, our main conclusions remain unchanged if we include alpha.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

We use daily, instead of monthly aggregation, to ensure that we capture relevant stock returns precisely after each trade date, without missing any shorter horizon returns where litigation risk is the highest.[21] Using monthly aggregation requires collapsing all the trades within a month into a single indicator (i.e., net purchases or net sales), which ignores the frequency and timing of individual trades.[22] Our approach follows a long line of papers on insider trading that use daily aggregation, many of which also analyze shorter term returns (see, e.g., Bettis et al. (2000), Huddart et al. (2007), Khan and Lu (2013), Gao, Lisic, and Zhang (2014), Ali and Hirshleifer (2017), Kallunki et al. (2018), and Wu (2018)).[23] Nevertheless, we also try a monthly aggregation approach as a robustness check, as discussed in the last paragraph of Section V.B.

For additional analyses, we obtain institutional ownership data from Thomson Reuters' institutional holdings (13F filings) database. We define higher ownership based on the percentage ownership of the largest 10 institutional investors in a firm. We obtain data on quarterly earnings announcement (QEA) dates and measures of standardized unexpected earnings (SUE) from the Compustat Fundamentals Quarterly and IBES databases. SUE equals (Actual EPS for a quarter – Median of analyst forecasts of EPS) / Stock price. Finally, we obtain data on the disclosure quotient (DQ), which measures the richness of financial disclosure in 10-K filings, from the authors of Chen, Miao, and Shevlin (2015).

B. Summary Statistics

Table 1 reports summary statistics of the main variables of interest separately for our insider purchases and sales samples. We winsorize all continuous variables at the 1st and 99th percentiles to minimize the influence of outliers. Our full sample includes 333,201 firm-sale days and 100,941 firm-purchase days, showing that days with insider sales are about three times as frequent as those with insider purchases, on average. Average BHAR is negative for sales and positive for purchases, consistent with prior findings that, on average, stock prices experience abnormal declines after insiders sell and abnormal increases after they buy stock. Over the 1 month (i.e., 21 trading days), 3 months (63 days), and 6 months (126 days) following the insider trade day, insider sales have average BHAR of −0.52%, −1.28%, and − 1.90%, respectively, while the corresponding returns for insider purchases are 3.24%, 5.16%, and 7.95%.[24] The average total number of

[21]For example, if an insider trades on March 1, we calculate returns starting March 2. However, monthly aggregation would pair that trade with April returns, ignoring the returns over March 2–31.

[22]To address potential issues with overlapping periods in our daily aggregation approach, we cluster standard errors by trade month, which accounts for potential correlations among trades within the same month. This adjustment does not change our results.

[23]For example, Cziraki and Gider (2021) say, "Our main approach is to multiply the inflation-adjusted dollar value of the trade, expressed in 2013 U.S. dollars, by the abnormal return in the 20 days after the trade. Many prior studies find that insiders earn high abnormal returns at horizons of 20 days or shorter (e.g., Seyhun (1986), Jeng, Metrick, and Zeckhauser (2003), Fidrmuc, Goergen, and Renneboog (2006), Ravina and Sapienza (2010), and Cohen et al. (2012))."

[24]Average raw cumulative returns following insider sales are positive at all horizons (CRET1m = 0.86%, CRET3m = 1.15%, CRET6m = 2.48%), but much smaller in magnitude than those following purchases. This pattern reflects that most insider sales are liquidity-motivated rather than informed

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 1

Summary Statistics

Table 1 reports summary statistics of the main variables used in this study. The sample consists of open-market trades of D&O in common stock of firms listed on NYSE, Amex, or Nasdaq, excluding financial and utility firms, during 1996–2013. Observations are at the firm-trade day level. All variables are winsorized at their 1st and 99th percentiles. Abnormal returns are in percentages and [a] indicates whether mean and median values are significantly different from zero at the 5% level or better. The Appendix provides variable definitions and data sources.

	Mean	SD	25%	Median	75%	No. of Obs.
Panel A. Insider Sales						
BHAR6m	−1.90[a]	33.99	−21.69	−3.79[a]	13.85	332,549
BHAR3m	−1.28[a]	23.77	−14.28	−2.02[a]	10.03	332,549
BHAR1m	−0.52[a]	13.12	−7.21	−0.72[a]	5.67	332,549
BHAR Profit6m	−16,442[a]	503,153	−46,759	−2,525[a]	25,333	332,549
BHAR Profit3m	−7,720[a]	340,122	−30,446	−1,300[a]	19,168	332,549
BHAR Profit1m	−2,506[a]	181,353	−15,103	−424[a]	11,239	332,549
CRET6m	2.48[a]	0.06	−19.77	0.60[a]	20.54	333,201
CRET3m	1.15[a]	0.05	−13.61	0.37[a]	14.10	333,201
CRET1m	0.86[a]	0.03	−6.85	0.63[a]	8.04	333,201
DGTWadjCRET6m	−2.52[a]	31.19	−21.04	−4.14[a]	12.99	296,244
DGTWadjCRET3m	−1.38[a]	21.86	−13.88	−2.08[a]	9.77	296,244
DGTWadjCRET1m	−0.34[a]	12.48	−7.05	−0.61[a]	5.91	296,244
Inshare	9.06	1.51	8.01	9.10	10.09	333,201
Indolvol	12.13	1.83	10.87	12.16	13.41	333,201
Size	6.58	1.84	5.36	6.49	7.73	327,060
BEME	−1.16	0.81	−1.63	−1.10	−0.62	321,801
Leverage	0.22	0.50	0.00	0.05	0.22	327,060
Ret (*t*-1)	0.05	0.16	−0.04	0.03	0.11	333,201
Ret (*t*-12, *t*-1)	0.20	0.50	−0.05	0.19	0.46	318,462
ROA	0.00	0.29	0.00	0.05	0.10	332,752
PPENT	0.22	0.20	0.07	0.15	0.30	332,661
Cash	0.17	0.17	0.04	0.11	0.24	330,857
Spread	0.20	0.35	0.02	0.06	0.23	328,987
R&D	0.12	0.22	0.02	0.07	0.15	248,461
Largest 10 institutional ownership	0.32	0.32	0.20	0.35	0.44	332,386
SUE (pre-QEA trades)	0.00	0.29	0.00	0.00	0.00	34,402
Panel B. Insider Purchases						
BHAR6m	7.95[a]	53.18	−23.17	−0.60[a]	25.33	100,366
BHAR3m	5.16[a]	33.48	−14.56	0.96[a]	18.69	100,366
BHAR1m	3.24[a]	19.18	−7.48	1.16[a]	11.18	100,366
BHAR Profit6m	5,294[a]	77,233	−5,134	−50[a]	6,172	100,366
BHAR Profit3m	4,744[a]	53,116	−2,949	82[a]	4,877	100,366
BHAR Profit1m	4,262[a]	31,136	−1,439	113[a]	3,109	100,366
CRET6m	15.36[a]	0.19	−19.05	5.43[a]	34.46	100,941
CRET3m	9.41[a]	0.12	−12.5	4.28[a]	24.04	100,941
CRET1m	4.70[a]	0.07	−7.21	2.13[a]	13.33	100,941
DGTWadjCRET6m	3.29[a]	49.80	−25.60	−3.68[a]	20.99	87,534
DGTWadjCRET3m	3.6[a]	32.12	−15.34	0.03[a]	16.86	87,534
DGTWadjCRET1m	2.87[a]	18.39	−7.61	0.91[a]	10.70	87,534
Inshare	8.10	1.58	6.91	8.01	9.21	100,941
Indolvol	9.96	1.71	8.82	9.90	11.05	100,941
Size	5.05	1.90	3.66	4.86	6.28	98,781
BEME	−0.76	0.93	−1.29	−0.70	−0.15	94,838
Leverage	0.58	1.32	0.01	0.16	0.54	98,781
Ret (*t*-1)	−0.04	0.19	−0.15	−0.04	0.05	100,935
Ret (*t*-12, *t*-1)	−0.20	0.63	−0.54	−0.13	0.20	96,155
ROA	−0.10	0.56	−0.10	0.02	0.06	100,448
PPENT	0.25	0.22	0.08	0.18	0.35	100,554
Cash	0.14	0.18	0.02	0.07	0.19	100,107
Spread	0.74	0.95	0.11	0.39	0.98	97,547
R&D	0.13	0.50	0.01	0.05	0.15	66,762
Largest 10 institutional ownership	0.23	0.20	0.03	0.21	0.38	100,531
SUE (pre-QEA trades)	0.00	0.14	0.00	0.00	0.00	7,916

where subsequent price increases simply capture normal expected stock returns, but the presence of some informed selling pulls these averages down. While we focus on abnormal returns relative to risk-adjusted benchmarks (BHARs), we also present robustness tests with raw returns.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

shares traded per day and their dollar values are substantially higher for insider sales than for insider purchases. This pattern is consistent with previous findings that opportunistic insider sales tend to be riskier than purchases. Therefore, a smaller proportion of all sales are likely opportunistic.

The distribution of insiders' abnormal dollar profits is highly skewed. Estimated dollar profits are expressed in constant 2013 dollars, the last year of our sample, to adjust for inflation. The mean estimated dollar abnormal loss avoidance for assumed holding periods of 1 month, 3 months, and 6 months is about \$2500, \$7700, and \$16,400, respectively, while their median values are much smaller. Mean and median abnormal dollar profits from purchases are generally much smaller. It is important to note that these average profitability measures are across all, not just opportunistic, insider trades, most of which are for liquidity purposes. Hence, small average profitability numbers are not surprising. They are also comparable to the magnitudes found in prior studies on insider trading (see, e.g., Cziraki and Gider (2021)). It is more informative to compare these against the additional profit estimates from our DiD models for trades influenced by litigation risk.

Sellers' firms are larger and more profitable, and their stock is more liquid (i.e., has a lower bid–ask spread) than buyers' firms. Sellers' firms also have lower leverage, slightly lower asset tangibility (PPENT), and higher cash holdings than buyers' firms.

IV. Empirical Methodology and Main Results

Our empirical methodology follows recent studies that deal with endogeneity issues by exploiting natural experiments, especially multiple exogenous shocks which vary by time and location (e.g., Bertrand and Mullainathan (2003), Gormley and Matsa (2011), Karpoff and Wittry (2018), and Appel (2019)). Specifically, we use the following difference-in-differences (DiD) regression model to examine the effect of UD laws on opportunistic insider trading:

$$y_{ijkst} = \beta . After\,UD\,Law_{st} + \omega \boldsymbol{X}_{i(t-1)} + \theta_{im} + \gamma_{jt} + \delta_{kt} + u_{ijkst}. \tag{1}$$

The dependent variable (y) measures insider trading outcomes, such as abnormal returns or dollar profits earned by insiders in different holding periods. *ijkst* indicates firm *i*, in industry *j*, state of headquarters *k*, state of incorporation *s*, and time *t*. $After\,UD\,Law_{st}$ is an indicator variable for a firm that is incorporated in a state that has a UD Law in a given year. Notice that *After UD Law* = *UD State* × *After*, where *UD State* = 1 for a firm incorporated in a state that has a UD law, and 0 otherwise; and *After* = 1 for firm-years starting with the year of UD law adoption in the firm's state of incorporation, and 0 otherwise. The regression does not include the main effect variables *UD State*, which is time-invariant, and *After* because the former (latter) is subsumed by firm (time) fixed effects. Thus, β is the DiD parameter measuring the treatment effect of *UD Law* on our outcome variables of interest.

Following previous studies (e.g., Gormley and Matsa (2016)), we control for time-invariant heterogeneity across firms and time-varying heterogeneity within and across industries and states by including firm by trade month (θ_{im}), industry-time (γ_{jt}), and headquarters (HQ) state-time (δ_{kt}) fixed effects. The industry is

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

defined by 3-digit SIC codes. HQ state-time fixed effects are important controls to subsume varying economic and regulatory conditions across states over time, for example, state-level business cycles that affect local companies' stock returns significantly (see, e.g., Korniotis and Kumar (2013)). Similarly, industry-time fixed effects control for time-varying industry effects such as industry momentum in stock returns (see, e.g., Moskowitz and Grinblatt (1999)). Time is the year and month of trade. To account for firm-level insider trading policies such as blackout periods in certain months (see, e.g., Bettis et al. (2000)), and potential seasonality in firm-level stock returns, we include firm-by-trade-month fixed effects.[25]

We also include a set of continuous control variables, lagged by one-period $\left(\boldsymbol{X}_{i(t-1)}\right)$ that may affect our dependent variables.[26] Appel (2019) finds that UD laws decrease the quality of corporate governance, lead to lower profitability, and, in some cases, lead to declines in firm values. Our main variables of interest are abnormal stock returns following insider trades. So one concern is that the negative abnormal returns we observe after UD laws are unrelated to insider trades but are a general effect of these laws in depressing stock prices across the board. So we include control variables important for asset pricing (such as size, book-to-market, past returns) and firm-specific variables that Appel (2019) finds to be affected by UD laws (such as profitability). We also control for bid-ask spread (*Spread*) to account for the possibility that a decrease in litigation threat can lead to changes in a firm's information environment, thus providing profitable trading opportunities to insiders.[27]

A. UD Law Adoptions and Profitability of Insider Trading: Baseline Results

Table 2 summarizes the results from DiD regressions of insider trading profitability measured by buy-and-hold abnormal returns (BHAR) for holding periods of 1, 3, and 6 months. In Panel A, *After UD Law* obtains negative and statistically significant (at 5% or better levels) coefficients in explaining buy-and-hold abnormal returns after an insider sale for all three holding periods. These results suggest that after the passage of UD laws and after controlling for other things, insiders of treatment firms avoid about −2.6%, −2.8%, and −3.4% in abnormal losses (i.e., BHAR) over 1, 3, and 6 months after their open market sale dates, compared to pre-UD law days. These returns are economically large, compared to their unconditional averages (−0.52%, −1.28%, and −1.90%), and support our main hypothesis that insider sales become more profitable after the passage of UD laws, which make it difficult for shareholders to sue insiders for the breach of their fiduciary duties, including opportunistic insider trading. Panel B shows a similar set of results related to the profitability of insider purchases. While the coefficient

[25]A firm can have multiple trade-days in a month. In regressions of the monthly number or dollar value of shares traded, we use firm fixed effects and state and industry fixed effects interacted with year, instead.

[26]The literature is divided on whether or not to include endogenous control variables in these regressions. When using natural experiments such as changes in state laws, Gormley and Matsa (2016) recommend not including time-varying endogenous control variables to avoid any biases caused by the possibility of control variables being affected by the main event. On the other hand, the exclusion of relevant control variables creates an omitted variables problem. As shown in Table A3, several firm characteristics differ before and after UD Laws. Therefore, we include control variables in our tests.

[27]We tackle issues related to information and disclosure more substantially in Section V.D.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 2

Universal Demand Laws and Profitability of Insider Trades

Table 2 reports our baseline regression results. The sample starts in 1996 and ends in 2013. The dependent variables are buy-and-hold abnormal returns (BHAR) over 6 months (i.e., 126 trading days; BHAR6m), 3 months (i.e., 63 trading days; BHAR3m), and 1 month (i.e., 21 trading days; BHAR1m) after the date of an insider trade. BHAR is calculated using Carhart's four-factor model without including the estimated alpha in calculating expected returns. All variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. Control variables are defined in the Appendix. Robust standard errors, clustered within states of incorporation, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

	BHAR6m	BHAR3m	BHAR1m
	1	2	3
Panel A. Insider Sales			
After UD law	−0.034***	−0.028***	−0.026***
	(0.012)	(0.007)	(0.003)
Size	−0.143***	−0.075***	−0.025***
	(0.005)	(0.003)	(0.001)
BEME	−0.026***	−0.016***	−0.007***
	(0.003)	(0.001)	(0.001)
Leverage	−0.029***	−0.011***	−0.006**
	(0.006)	(0.003)	(0.003)
Ret (*t*-1)	−0.028***	−0.007	0.008*
	(0.007)	(0.008)	(0.004)
Ret (*t*-12, *t*-1)	−0.077***	−0.034***	−0.009***
	(0.005)	(0.003)	(0.001)
ROA	0.010	−0.005	0.003
	(0.010)	(0.005)	(0.002)
PPENT	0.172***	0.067***	0.029**
	(0.034)	(0.014)	(0.012)
Cash	0.047***	0.034***	0.010**
	(0.015)	(0.009)	(0.005)
Spread	0.014**	0.009**	0.007***
	(0.006)	(0.005)	(0.002)
Constant	0.886***	0.463***	0.150***
	(0.033)	(0.019)	(0.007)
Firm × month FE	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	291,677	291,677	291,677
Adj. R^2	0.629	0.591	0.468
	BHAR6m	BHAR3m	BHAR1m
	1	2	3
Panel B. Insider Purchases			
After UD law	0.081	0.012	0.027
	(0.059)	(0.040)	(0.029)
Size	−0.170***	−0.088***	−0.015***
	(0.016)	(0.007)	(0.003)
BEME	−0.009	−0.018**	0.003
	(0.012)	(0.009)	(0.003)
Leverage	0.015	0.010	0.002
	(0.017)	(0.007)	(0.004)
Ret (*t*-1)	−0.059	−0.000	0.015
	(0.050)	(0.028)	(0.011)
Ret (*t*-12, *t*-1)	−0.083***	−0.046***	−0.019***
	(0.008)	(0.007)	(0.003)
ROA	−0.017	0.014	−0.012
	(0.023)	(0.013)	(0.008)
PPENT	0.091	0.009	−0.070**
	(0.068)	(0.046)	(0.031)
Cash	−0.005	−0.023	0.011
	(0.055)	(0.028)	(0.019)
Spread	0.004	−0.008	−0.008***
	(0.011)	(0.011)	(0.003)

(continued on next page)

TABLE 2 (continued)
Universal Demand Laws and Profitability of Insider Trades

	BHAR6m	BHAR3m	BHAR1m
	1	2	3
Panel B. Insider Purchases (continued)			
Constant	0.863***	0.471***	0.127***
	(0.073)	(0.033)	(0.023)
Firm × month FE	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	70,432	70,432	70,432
Adj. R^2	0.824	0.767	0.611

estimates on *After UD Law* are positive in predicting BHAR, they are statistically insignificant.

We next check how robust our conclusions are to the specification of estimated returns. While we exclude alpha in calculating our BHAR, one concern is that other asset pricing parameters estimated right before insider trades may be biased for various reasons. To reduce this concern, we exclude the returns for 50 trading days immediately before an insider trade day from the parameter estimation window. To further alleviate this concern, Table A1 shows our baseline results using cumulative total raw returns (CRETs) and DGTW-adjusted returns as dependent variables, neither of which requires estimation of asset pricing parameters.[28] These results largely mirror our main results in Table 2. In Panel A1, the coefficient of After UD Law for insider sales is negative and statistically significant for all three holding periods for both return measures, suggesting that insiders timed their sales to avoid abnormal losses.[29] For purchases, the corresponding coefficient is statistically insignificant at the 5% level in Panel B1 in all six regressions.

Overall, we find that *After UD Law* predicts negative BHAR following insider sales in both economically and statistically significant ways. For purchases, even though the point estimates are often sizable, they are statistically weak or insignificant, and occasionally in the wrong direction. These results are consistent with the notion that UD laws encourage more litigation-prone insider trades, especially sales based on private information.[30] Moreover, the differential results for purchases and

[28]We calculate DGTW-adjusted returns by subtracting the compounded daily DGTW benchmark return from the compounded daily stock return over a given holding period (i.e., 1, 3, or 6 months) after an insider trading day. We use DGTW's characteristic quintile cutoffs, which are available through the year 2012, and extend them to 2013 ourselves.

[29]Can insiders profit from stock price declines within 6 months after a stock sale, given that the short-swing rule effectively removes their incentive to buy back the stock within 6 months of the sale (because if they do, they have to return the profit from the round-trip trade to the company)? Given that stock and option grants are such important parts of D&O compensation packages, many D&Os are unlikely to have a pressing need to buy back the stock within 6 months after they sell it. Moreover, since 1991, even the immediate sale of stock acquired through the exercise of options (granted at least 6 months earlier) is exempt from the short-swing rule (see, e.g., Henry (2003)). Given these realities, the short-swing rule may not pose much hindrance to insiders in profiting from price declines over less than 6 months after stock sales.

[30]In Table 2, the coefficient estimates are noisier for the full sample of purchases (Panel B) than for sales (Panel A). This is likely due to i) smaller sample sizes for the former and ii) the inclusion of many insider purchases that are not relevant for DLs in the full sample of purchases, as discussed in the introduction.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

sales provide further assurance that the negative coefficient on *After UD Law* in predicting BHAR following sales is driven by more informed selling by insiders rather than a general drop in stock prices caused by the adoption of UD laws. A general drop in stock prices would predict no difference in the effect of *After UD Law* on future returns between the sales and purchase samples.[31]

B. Profitability During the Years Surrounding UD Law Adoptions

To validate that the passage of UD laws is followed by increased profitability of insider sales, we next examine pretrends. We create indicator variables for each of the 3 years before (*BeforeUD3*) and 3 years after (*AfterUD3*) UD law passage in a firm's incorporation state, with the adoption year labeled *AfterUD1*. We then re-estimate Table 2 Panel A regressions, replacing *AfterUDLaw* with these six indicators.

Figure 1 presents the coefficient plots for insider sales over the 6-year window. The solid black squares represent point estimates, and the lines above and below represent 95% confidence bands. Point estimates with bands that surround the horizontal axis (0) are statistically insignificant at the 95% level. The figures show that the average coefficients are more negative post-UD than pre-UD in predicting BHARs for all three holding periods, consistent with Table 2. Graph A shows that the difference in 1-month BHAR for sales between UD and non-UD firms has no pretrend. BHARs are insignificant or positive before the passage, with significant negative effects showing up starting the year of passage. Graphs B and C exhibit a similar pattern for 3-month and 6-month BHARs, which are all negative after year of passage, while most pre-UD years are either insignificant or positive. However, for these holding periods, especially for 6 months, the BHAR turns negative right before the year of passage, likely indicating some anticipation effect, but not a sustained pretrend. Overall, we do not find any concerning violation of parallel trends, affirming the causal impact of UD laws.

C. How Do Insiders Exploit Opportunism?

The next sets of tests are aimed at understanding the source of increased profitability of insider trading after UD laws.

1. Trading Volume and Timing

Given a set of profitable trading opportunities, insiders exploit their private information to increase their profits by i) increasing the size of trades before opportunistic but low-risk events, ii) timing the trades more opportunistically and in a riskier manner, or iii) doing both.

To isolate these channels, we examine the effect of UD Laws on the total number of shares bought or sold by insiders in a given month and the total dollar value of such trades (trade price × shares). Table 3 reports the results. The dependent variables in Panel A (for sales) and Panel B (for purchases) are *lnshare* (natural

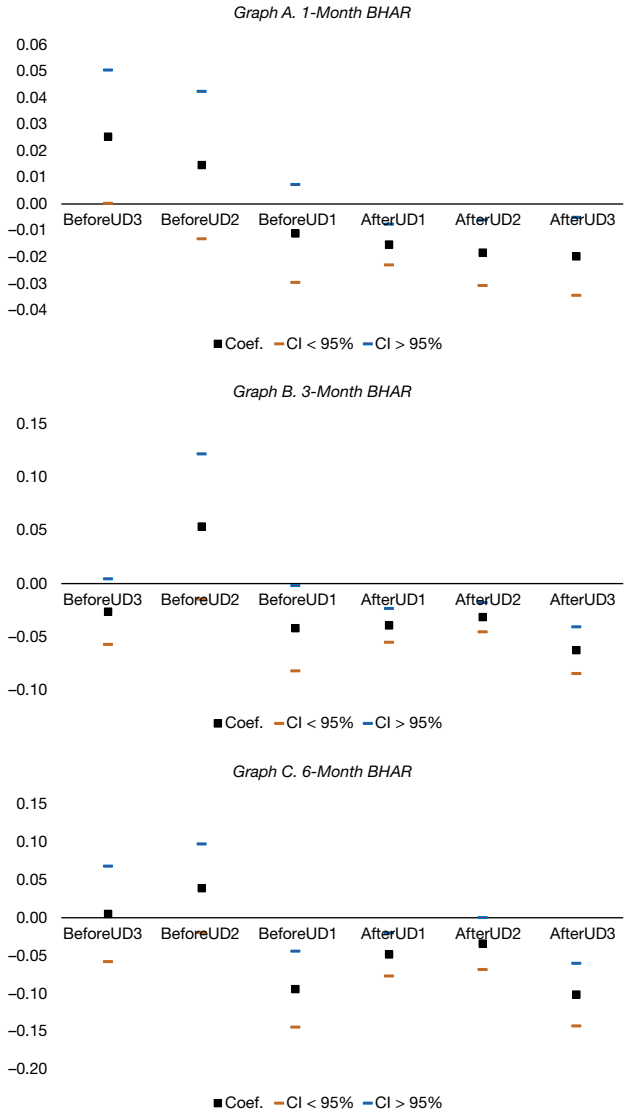
[31]Moreover, Obaydin, Zurbruegg, Hossain, Adhikari, and Elnahas (2021) find that firm-level stock price crashes (i.e., extreme negative returns) are *less* prevalent after UD laws. This finding also supports our conclusion that more profitable insider sales after UD laws are not driven by changes in general stock price dynamics.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

FIGURE 1

Dynamics of Buy-and-Hold Abnormal Returns (BHAR) from Insider Sales

Figure 1 shows estimated average excess buy-and-hold abnormal returns (BHAR) without alpha for 1-month (Graph A), 3-month (Graph B), and 6-month (Graph C) holding periods following the day of insider sales during the years surrounding the passage of UD laws. The solid black squares indicate the point estimates, and the lines above and below indicate 95% confidence intervals from regressions of BHAR similar to Table 2, where the *After UD Law* variable is replaced by indicators for the years before and after the passage of UD laws in a firm's state of incorporation. After UD1 denotes the year of adoption of a UD law.



logarithm of total shares traded) and *lndolvol* (natural logarithm of the total dollar value of trades) of all insider transactions in a firm in each month that has at least one insider trade. Column 1 of Panel A shows that the average number of shares sold by insiders in a given month (*lnshare)* does not change significantly after the adoption

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 3

Trading Volume and Opportunism

Table 3 reports regression results based on the volume and opportunism of insider trades. The dependent variables in each panel are *Inshare* (natural logarithm of total shares traded), *Indolvol* (natural logarithm of total dollar value of trade), and the proportion of opportunistic trades (= Opp sales/All sales or Opp. Buys/All buys). The sample period is from 1996 to 2013. Opportunistic trades are defined as in Cohen et al. (2012). These variables are measured for all insider transactions in a firm within a month with nonzero transactions. Panel A reports the results for sales and Panel B for purchases. Total dollar value is the sum of the dollar values of all trades in the month (trade price × shares). All continuous variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. Control variables are as in Table 2; they are not shown for brevity. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

Panel A. Insider Sales

	Inshare	Indolvol	Opp Sales/All Sales
	1	2	3
After UD law	0.065	0.191**	0.098***
	(0.069)	(0.089)	(0.013)
Control variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
HQ state × year FE	Yes	Yes	Yes
Ind × year FE	Yes	Yes	Yes
No. of obs.	104,589	104,589	33,253
Adj. R^2	0.288	0.467	0.481

Panel B. Insider Purchases

	Inshare	Indolvol	Opp Buys/All Buys
	1	2	3
After UD law	−0.035	0.032	−0.182**
	(0.106)	(0.115)	(0.081)
Control variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
HQ state × year FE	Yes	Yes	Yes
Ind × year FE	Yes	Yes	Yes
No. of obs.	44,390	44,390	7,502
Adj. R^2	0.479	0.488	0.700

of a UD Law. However, the DiD estimate in column 2 shows that the total dollar values of insider sales (*lndolvol*) in these months increase significantly by about 21% (= $e^{0.191} - 1$) in treated firms compared to control firms. Thus, after the adoption of UD laws, insiders do not increase the number of shares sold. They appear to time their sales more opportunistically by using their private information that stock prices are inflated and a price decline is likely.[32] We do not find a similar pattern for purchases. In columns 1 and 2 of Panel B, insiders in treated firms change neither the number of shares nor the dollar value purchased in a significant way compared to control firms.

We next identify opportunistic and routine trades following Cohen et al.'s (2012) methodology. Column 3 in each panel shows estimates of the regressions of the proportion of the number of opportunistic insider (i.e., D&O) trades to all such trades. In our partitionable sales sample using Cohen et al.'s (2012) approach, the

[32]Another possible reason for this result is that the average pre-trade stock returns of treated firms were significantly higher after the adoption of UD laws, increasing the dollar value of the trade. However, this possibility is not supported by our data. As shown in Panels A1 and A2 of Table A3, we find that returns over neither 1 month (Ret_{t-1}) nor 12 months ($Ret_{t-12, t-1}$) before insider selling are higher post-UD in our sample. Moreover, we control for these returns in our regressions to account for such differences.

proportion of opportunistic sales in a month increases by 0.098 in treatment firms after UD laws in Panel A. This increase is both statistically and economically significant.[33] On the other hand, in Panel B, in the partitionable purchase sample, the ratio of opportunistic purchases decreases.[34] Overall, these results suggest that while there is not a significant increase in the level of selling by insiders in treatment firms after the adoption of UD laws, their sales become more opportunistic, and therefore, more profitable.

2. Volume and Profitability of Pre-QEA Trades

Our analysis so far suggests that after the adoption of UD laws, insiders avoid losses by timing their sales more opportunistically. Insiders become more willing to push the boundaries of the law in their trading, presumably because of the reduced risk of shareholder lawsuits. To further investigate this possibility, we examine an especially high-risk form of opportunistic insider trading, namely, insider trades before quarterly earnings announcements (pre-QEA). This analysis is motivated by Ali and Hirshleifer (2017), who find that despite tight regulations, voluntary corporate policies on insider trading and high risks, some insiders continue to trade pre-QEA.[35] They define the pre-QEA period as the 21 trading day period ending three trading days before a QEA date and find that pre-QEA trades tend to be among the most profitable insider trades.

Ali and Hirshleifer (2017) suggest that the high profitability of such trades serves as a robust way to identify the most opportunistic insider trades, as an

[33]Since we identify opportunistic trades by treating all trades made by an insider as opportunistic after the insider's trades satisfy the conditions, the proportion of opportunistic trades can increase over time for a given company if the composition of insiders remains constant. But because insiders move in and out of firms, it is not clear whether we would expect a monotonous increase in the number of opportunistic trades. Moreover, we compare trades between treated and control firms before and after UD, so any secular changes in the number of opportunistic trades should be differenced out by our DiD specification.

[34]Two points are worth noting about these findings on opportunistic and routine purchases. First, these results do not necessarily contradict our occasional findings of more profitable insider purchases after UD laws in the rest of our analyses. Only a small fraction of all insider trades make it to our partitionable sample that follows Cohen et al. (2012), which likely leaves out many profitable trades, especially from smaller firms, which are more likely to be excluded from this analysis (see Table I in Cohen et al. (2012)). Moreover, it is possible that purchases classified as "routine" by Cohen et al.'s algorithm become more profitable after UD laws, especially because their algorithm identifies "traders," not "trades." Second, our purchase result differs from Jung et al. (2021) likely because i) following Cohen et al. (2012), we use monthly aggregation and include the partitionable sample only, whereas Jung et al. (2012) use annual aggregation and do not indicate whether they use their entire sample or only the partitionable sample, and ii) we scale by total traded shares, while Jung et al. (2012) scale by total shares outstanding.

[35]Bettis et al. (2000) and Lee et al. (2014) find that many companies voluntarily adopt internal policies on insider trading and do not allow their insiders to trade before earnings announcements without prior waivers from the firm. Given that firms are not required to disclose the details of such policies or even their existence, let alone what waivers were requested or granted to insiders, and whether and how strictly the policies are enforced, it is hard to know for sure which firms have such policies in place and how strictly they enforce them. Prior studies identify firms that have such policies based either on surveys, predictions based on the pattern of insider trading between successive QEAs, or voluntary disclosures of ethics rules on company websites. Both Bettis et al. (2000) and Lee et al. (2014) find that some insiders trade pre-QEA, though the frequency of trading is lower in firms that have (or are likely to have) such policies.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

alternative to Cohen et al.'s (2012) approach. This is because, regardless of whether a firm has an explicit policy prohibiting pre-QEA insider trading and whether it enforces it strictly, such trading looks opportunistic to investors. While investors cannot observe whether a firm has such a policy, how lenient it is in granting exceptions to it, and whether and how strictly it enforces it, they can observe reported insider transactions, including pre-QEA trades. While pre-QEA trades are somewhat rare, they are still numerous. For example, in Panel A of Table 4, there are 38,774 (= 9862 + 28,912) days with pre-QEA insider sales in our sample, about 11.6% of all 333,201 days with insider sales in Table 1. However, our interest is not in analyzing the frequency of pre-QEA trades, but rather in determining whether they are opportunistic and profitable, a task we turn to next.

We examine two issues here: i) whether the volume of pre-QEA trades depends more on the direction of earnings surprises after UD laws and ii) whether pre-QEA insider trades become more profitable after UD Laws. Table 4 shows the results. The unit of analysis here is a trade-day for the subsample of pre-QEA trades. To examine the first issue, we aggregate the number of shares and dollar value of trades on each pre-QEA trading day and estimate separate regressions based on whether the subsequent SUE is negative or nonnegative. In columns 1 and 2 of Panel A, both the number and dollar value of pre-QEA sales preceding negative SUEs increase significantly after UD law adoption. In contrast, in columns 3 and 4, UD laws have no effect on sales volume when SUE is positive or zero, that is, when profitable selling opportunities are low. This asymmetric volume response provides further evidence that insiders opportunistically time their pre-QEA sales ahead of negative earnings news once the threat of shareholder litigation recedes. The lack of volume change when SUE is nonnegative indicates that insiders specifically ramp up sales in anticipation of negative earnings surprises, rather than increasing their overall pre-QEA sales. The strategic increase in the volume of sales before bad news aligns with insiders taking greater advantage of UD laws to profit from their inside information. Importantly, the fact that these accounting-based SUE results mirror our return-based profitability findings helps further alleviate any remaining concerns about the measurement of our abnormal returns.

Finally, we examine profitability in the subsample of pre-QEA trades, as we do in our baseline models. Panel B shows the results for the profitability of insider sales. We find that pre-QEA sales become significantly more profitable after UD law adoption over the 1- and 3-month periods following the trade. The coefficient of After UD Law is much larger for these periods than in the full sample in Table 2. However, these trades do not become more profitable after UD law adoption over the 6-month period, consistent with our hypothesis that UD laws impact shorter term, hence previously more litigation prone, profits more. This point is particularly important for pre-QEA trades, where the profit is likely to be realized at or soon after the QEA (see Ali and Hirshleifer (2017)). Given the definition of the pre-QEA period, 1-month profits after the trade date are likely the most pertinent here. In constant 2013 dollars (= BHAR × $Trade in constant dollars), the increase in abnormal loss avoided by insiders of treated firms from pre-QEA trades after UD law adoption exceeds that for control firms by about $44,000 and $25,000 for 1- and 3-month holding periods, respectively. Both of them are statistically significant. The additional dollar profit for the 1-month holding period is much larger in this

TABLE 4

Profitability of Insider Trades before Quarterly Earnings Announcements

Table 4 reports regression results for pre-QEA insider trades. The sample starts in 1996 and ends in 2013. The pre-QEA period is defined as the 21-trading day period ending three trading days before a quarterly earnings announcement (QEA) date. In Panels A and C, the dependent variables are the natural logarithm of the dollar value (lndolvol) or of the number of shares traded (lnshare) by D&O on a given trading day during the pre-QEA period as indicated. In Panel A, columns 1 and 2 (3 and 4) are for the subsample where SUE < 0 (SUE ≥ 0); in Panel C, they are for the subsample where SUE ≤ 0 (SUE > 0). In Panels B and D, the dependent variables are BHAR for 6, 3, or 1 month (BHAR6m, BHAR3m, or BHAR1m) after an insider trade day during the pre-QEA period. Abnormal dollar profit for a given holding period is computed as the product of the abnormal return (BHAR) over the period and the dollar value of an insider trade and expressed in constant 2013 dollars. All variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. Control variables are as in Table 2; they are not shown for brevity. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

Panel A. Opportunism in Insider Sales

	SUE < 0		SUE ≥ 0	
	lndolvol	lnshare	lndolvol	lnshare
	1	2	3	4
After UD law	1.399***	0.709**	0.178	0.020
	(0.278)	(0.342)	(0.114)	(0.121)
Control variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × year FE	Yes	Yes	Yes	Yes
Ind × year FE	Yes	Yes	Yes	Yes
No. of obs.	9,862	9,862	28,912	28,912
Adj. R^2	0.620	0.454	0.545	0.370

Panel B. Profitability of Pre-QEA Insider Sales

	BHAR6m	BHAR3m	BHAR1m	$Profit6m	$Profit3m	$Profit1m
	1	2	3	4	5	6
After UD law	0.007	−0.040**	−0.047***	16,332	−25,457**	−44,459***
	(0.034)	(0.020)	(0.008)	(19,295)	(10,822)	(7,315)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	38,008	38,008	38,008	38,008	38,008	38,008
Adj. R^2	0.797	0.771	0.640	0.410	0.359	0.325

Panel C. Opportunism in Insider Purchases

	SUE ≤ 0		SUE>0	
	lndolvol	lnshare	lndolvol	lnshare
	1	2	3	4
After UD law	−0.470	−0.331	2.780*	2.303
	(0.949)	(1.143)	(1.639)	(1.659)
Control variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × year FE	Yes	Yes	Yes	Yes
Ind × year FE	Yes	Yes	Yes	Yes
No. of obs.	4,658	4,658	5,399	5,399
Adj. R^2	0.633	0.547	0.599	0.516

Panel D. Profitability of Pre-QEA Insider Purchases

	BHAR6m	BHAR3m	BHAR1m	$Profit6m	$Profit3m	$Profit1m
	1	2	3	4	5	6
After UD law	−0.374	−0.167	0.136	−36,584	3,563	−14,759
	(0.702)	(0.291)	(0.120)	(48,433)	(30,516)	(10,846)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	9,416	9,416	9,416	9,416	9,416	9,416
Adj. R^2	0.870	0.829	0.688	0.601	0.563	0.500

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

subsample than in the full sample (Table 6). For insider purchases, a similar set of tests shown in Panels C and D yields mostly statistically insignificant and inconsistent results.

Taken together, elevated opportunistic trading volumes and increased profitability around QEAs paint a consistent picture: UD laws enable insiders to more aggressively exploit their information advantage through pre-QEA trading, mainly sales, where multiple metrics point to increased opportunistic behavior during lucrative, but otherwise legally perilous, windows.

D. Cross-Sectional Tests of Risks and Opportunities

We now dig deeper into the role of opportunism as the underlying channel for the increased profitability of insider sales that we observe after the adoption of UD laws. We focus on the role of governance, firm size, and trading opportunities as discussed in the following.

1. Institutional Blockholdings and Firm Size

Davis (2008) argues that DLs are less important for highly visible large corporations, which are subject to many other governance mechanisms that can substitute for the effects of DLs. One such mechanism is company rules on insider trading, which are more common in large firms (see, e.g., Bettis et al. (2000)). This argument implies that the effect of UD laws on insider trading profitability should be greater for firms with fewer alternative governance mechanisms in place and for smaller firms. We first consider institutional blockholdings as an external governance mechanism.[36] We calculate the total percentage equity ownership of a firm's 10 largest 13F institutional owners of each sample firm. We then split the sample annually by the median of such blockholder ownership. Second, we partition the sample into large and small firms annually based on median market capitalization. Table 5 reports the results. To save space, we only tabulate the coefficient estimate and test statistics of the After UD Law variable.

Panel A1 of Table 5 partitions the insider sales sample by blockholder ownership. We find that after UD law adoption, insider sales in treatment firms with below median blockholder ownership exhibit much larger increases in profitability across all horizons compared to firms with higher blockholder ownership. All the differences are highly statistically significant. Table 5, Panel A2 shows that after UD laws, insider sales turn more profitable (as measured by BHAR) both for smaller and larger firms over two horizons. This effect is significantly higher among smaller firms over all three horizons.

Panels B1 and B2 show results for insider purchases with similar sample partitions. Although purchase results are significant in some subsamples, they do not show a consistent pattern expected from differences in size and governance. Overall, the cross-sectional tests support our predictions and suggest that governance substitutes like firm visibility and monitoring by blockholders mitigate the incentives for opportunistic sales created by UD laws.

[36]See, e.g., Ferreira and Matos (2008), Brav, Jiang and Kim (2015), and Appel, Gormley and Keim (2016).

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 5

Cross-Sectional Tests for Insider Trading Opportunities

Table 5 reports the regression results in Table 2 partitioned by measures of profitable insider trading opportunities. The sample period is from 1996 to 2013. The dependent variables are buy-and-hold abnormal returns (BHAR) over 6, 3 or 1 month after the date of an insider trade. BHARs are calculated using the Carhart four-factor model without alpha. All variables are winsorized at the 1st and 99th percentiles. After UD Law is an indicator variable equal to 1 if a firm is incorporated in a state that currently has a universal demand law, and 0 otherwise. The sample of insider sales is partitioned by the largest 10 institutional ownership in Panel A1, firm size in Panel A2, and R&D intensity in Panel A3. Panels B1, B2, and B3 partition the sample of insider purchases similarly. High or large (low or small) values indicate above (below) median values for the partitioning variable in that year. Control variables are as in Table 2; they are not shown for brevity. Sample sizes differ across the panels depending on the availability of data on the partitioning variable. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

Panel A1. Profitability of Insider Sales Partitioned by the Largest 10 Institutional Ownership

	BHAR6m		BHAR3m		BHAR1m	
	High	Low	High	Low	High	Low
	1	2	3	4	5	6
After UD law	0.074***	−0.140***	0.045***	−0.085***	0.001	−0.034***
	(0.008)	(0.020)	(0.007)	(0.016)	(0.006)	(0.004)
Test: high – low = 0 (t-stat)	9.31***		6.45***		3.99***	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,505	139,236	144,505	139,236	144,505	139,236
Adj. R^2	0.724	0.775	0.687	0.727	0.534	0.579

Panel B1. Profitability of Insider Purchases Partitioned by the Largest 10 Institutional Ownership

	BHAR6m		BHAR3m		BHAR1m	
	High	Low	High	Low	High	Low
	1	2	3	4	5	6
After UD law	0.503***	−0.320	0.072	0.119	−0.042	0.313**
	(0.085)	(0.243)	(0.190)	(0.096)	(0.045)	(0.141)
Test: high–low = 0 (t-stat)	3.11***		−0.19		−1.94*	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	32,272	33,686	32,272	33,686	32,272	33,686
Adj. R^2	0.913	0.910	0.853	0.848	0.661	0.675

Panel A2. Profitability of Insider Sales Partitioned by Firm Size

	BHAR6m		BHAR3m		BHAR1m	
	Large	Small	Large	Small	Large	Small
	1	2	3	4	5	6
After UD law	−0.003	−0.098***	−0.017**	−0.042***	−0.021***	−0.031***
	(0.010)	(0.021)	(0.008)	(0.010)	(0.004)	(0.005)
Test: small–large = 0 (t-stat)	3.72***		1.68*		1.96**	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,906	140,859	144,906	140,859	144,906	140,859
Adj. R^2	0.681	0.781	0.652	0.735	0.501	0.587

Panel B2. Profitability of Insider Purchases Partitioned by Firm Size

	BHAR6m		BHAR3m		BHAR1m	
	Large	Small	Large	Small	Large	Small
	1	2	3	4	5	6
After UD Law	−0.045	−0.027	−0.051	−0.073	0.106***	0.013
	(0.096)	(0.139)	(0.069)	(0.094)	(0.034)	(0.097)
Test: small–large = 0 (t-stat)	−0.10		0.17		0.90	

(continued on next page)

TABLE 5 (continued)
Cross-Sectional Tests for Insider Trading Opportunities

Panel B2. Profitability of Insider Purchases Partitioned by Firm Size (continued)

	BHAR6m		BHAR3m		BHAR1m	
	Large	Small	Large	Small	Large	Small
	1	2	3	4	5	6
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	31,249	35,197	31,249	35,197	31,249	35,197
Adj. R^2	0.908	0.911	0.853	0.849	0.666	0.671

Panel A3. Profitability of Insider Sales Partitioned by R&D Intensity

	BHAR6m		BHAR3m		BHAR1m	
	High	Low	High	Low	High	Low
	1	2	3	4	5	6
After UD law	−0.052***	0.045	−0.033***	−0.049***	−0.032***	−0.035**
	(0.009)	(0.031)	(0.006)	(0.018)	(0.002)	(0.015)
Test: high–low = 0 (t-stat)	−2.76***		0.84		0.19	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,893	143,515	144,893	143,515	144,893	143,515
Adj. R^2	0.605	0.827	0.573	0.789	0.453	0.628

Panel B3. Profitability of Insider Purchases Partitioned by R&D Intensity

	BHAR6m		BHAR3m		BHAR1m	
	High	Low	High	Low	High	Low
	1	2	3	4	5	6
After UD law	0.086	−0.783*	0.015	−0.360**	0.003	0.039
	(0.066)	(0.391)	(0.054)	(0.178)	(0.034)	(0.035)
Test: high–low = 0 (t-stat)	2.21***		1.97**		−0.76	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	34,668	33,119	34,668	33,119	34,668	33,119
Adj. R^2	0.824	0.923	0.773	0.853	0.620	0.662

2. Trading Opportunities

Insiders' opportunities to trade on private information should be greater among firms with higher information asymmetry. To test this conjecture, we follow Aboody and Lev (2000), who find that outside investors face greater information asymmetry with insiders in more R&D intensive firms, which provides greater opportunity for profitable insider trading. We define high (low) R&D based on above- (below-) yearly median of a firm's R&D intensity.

Table 5, Panel A3 shows that the profitability of insider sales in terms of BHAR after UD law increases for both high and low R&D intensive firms. The coefficients of *After UD Law* are statistically different and economically higher between high and low R&D firms over the 6-month holding period.

In Panel B3, we also see some moderating effect of information asymmetry on the profitability of insider purchases in the expected directions: higher positive returns in higher R&D firms after UD Laws for BHAR6m and BHAR3m. However, similar to the full sample results, these results are noisier than those in the sales sample.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 6

Insiders' Abnormal Dollar Profits

Table 6 reports regression results for insiders' abnormal dollar profits. The sample starts in 1996 and ends in 2013. The dependent variables are buy-and-hold abnormal dollar profits over 6 months (i.e., 126 trading days; $Profit6m), 3 months (i.e., 63 trading days; $Profit3m), and 1 month (i.e., 21 trading days; $Profit1m) after an insider trade day (day 0). Abnormal dollar profit for a given holding period is computed as the product of the abnormal return (BHAR) over the period and the dollar value of an insider trade and expressed in constant 2013 dollars. BHARs are calculated using Carhart's four-factor model with no alpha. All variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. Control variables are as in Table 2; they are not shown for brevity. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

	$Profit6m	$Profit3m	$Profit1m
	1	2	3
Panel A. Insider Sales			
After UD law	−14,388	−52,508***	−27,091***
	(15,714)	(12,734)	(3,499)
Control variables	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	291,677	291,677	291,677
Adj. R^2	0.325	0.309	0.248
Panel B. Insider Purchases			
After UD law	6,316	11,933**	8,591**
	(9,224)	(5,236)	(3,306)
Control variables	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	70,432	70,432	70,432
Adj. R^2	0.539	0.508	0.415

E. Insiders' Abnormal Dollar Profits

We next estimate the impact of UD laws on the dollar value of insiders' profits as an alternate way to assess economic significance. These measures incorporate both trade size and posttrade stock returns to estimate the potential dollar profits for each trade day. We calculate buy-and-hold abnormal dollar profits for each trade day over the next 1-, 3-, and 6-month horizons (denoted profit1m, profit3m, and profit6m and computed over 21, 63, and 126 trading days, respectively) by multiplying the dollar value of a trade by the BHAR over the holding period. They are expressed in constant inflation-adjusted 2013 dollars.

Table 6 reports the results. Columns 1–3 of Panel A show that after UD laws, each insider sale avoids abnormal losses of approximately $27,000, $52,500, and $14,400 per trade over the subsequent 1, 3, and 6 months, respectively. The 6-month coefficient is smaller and statistically insignificant, in contrast to the significant BHAR6m result in Table 2. This difference arises because BHAR regressions equally weigh each trade, while dollar profits weigh them by the dollar value of the trade. The findings suggest that UD laws encourage some additional opportunistic longer-horizon trades with significant 6-month BHARs, but the dollar volumes of such trades increase less. UD laws predominantly affect sales volumes that are profitable over the shorter 1- to 3-month horizons, which are presumably riskier than those that are profitable over the 6-month period.

Panel B of Table 6 shows that insiders' purchases after UD laws yield abnormal dollar profits of around $8,600, $11,900, and $6,300 over 1-, 3-, and 6-month

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

holding periods. The first two are statistically significant. While smaller in magnitude than the sales results, these gains significantly exceed the unconditional mean abnormal dollar profits in Table 1, Panel B. However, earlier in Table 2, Panel B, we find positive but statistically insignificant effects of UD laws on BHARs for purchases. Again, this divergence arises because BHARs equally weigh each trade, while dollar profits weigh them by trade value. The dollar profit results suggest that insiders time some larger purchases opportunistically after UD laws, profiting more on those trades. But equal-weighted BHARs suggest that such opportunistic purchases do not increase significantly in frequency for the full sample post-UD laws. These differing purchase results align with our earlier evidence that UD laws lead to more opportunistic timing of insider purchases in certain subsamples. These results suggest that UD laws enable some opportunism for purchases, but with weaker effects compared to the pervasive opportunism they induce on insider sales.

F. Size of Insider Trading Profits and the Role of DLs in Curbing Insider Trading

As we conclude our key results and before moving to robustness checks, we address an important question about the interpretation of our findings, particularly regarding the economic magnitude of the effects and what they imply about the role of derivative lawsuits in constraining insider trading. In our full sample (Table 6, Panel A), we find that after UD laws, an average insider sale in treated firms experiences an increase in abnormal dollar profit (i.e., a reduction in underperformance relative to the benchmark) of approximately $27,000 and $52,500 over 1- and 3-month holding periods, respectively, compared to control firms. These amounts appear modest relative to the compensation and wealth of highly paid executives and raise the question whether they are large enough to lead D&O to engage in trades that are legally dubious despite UD laws.

The answer depends on one's priors about the marginal role that derivative lawsuits play in constraining insider trading behavior. If one views derivative lawsuits as a primary deterrent to opportunistic trading, these modest dollar amounts might suggest limited practical importance. However, if one views derivative lawsuits as a supplementary enforcement mechanism, which seems more appropriate given that they rarely target insider trading as a primary allegation and federal enforcement remains in place, then even these modest effects demonstrate that private enforcement extends deterrence beyond what public enforcement alone achieves.

Here are some other contexts for interpreting our magnitudes. First, the effects we find are economically meaningful relative to what the prior literature finds for typical insider trades. Cziraki and Gider (2021), who focus on analyzing dollar profits from insider trading, report abnormal profits of approximately $4000 and $10,000 over 1- and 3-month windows for average insider trades.[37] Bhattacharya and Marshall (2012) note that even some high-profile SEC prosecutions involve only modest gains. For example, Martha Stewart's ImClone trade avoided only $45,673 in loss, representing just 1.7% of her annual pay. Relative to the

[37]See their Table 1, Panel B, and Supplementary Table A.2. Similar to us, they report in constant 2013 dollars but use the market-adjusted model for computing abnormal returns.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

unconditional mean abnormal dollar profits of $2500 and $7700 in our sample (Table 1), the incremental effects we document are economically meaningful.

Second, our sample necessarily includes many nonopportunistic liquidity trades, which dilutes estimates of the effects on purely opportunistic transactions. When we focus on trades that appear more opportunistic, such as sales before quarterly earnings announcements, we find larger effects. One-month abnormal profits from pre-QEA sales increase by approximately $44,000 on average after UD law adoption,[38] suggesting more substantial effects for the subset of trades where litigation risk was previously the highest.

Third, the nature of derivative lawsuits themselves provides important context. DLs are not primarily aimed at insider trading; rather, they target a broader range of alleged harms to the company, with insider trading typically serving as a secondary allegation or supporting evidence.[39] As discussed in the introduction and Section II.A, this feature makes UD laws particularly valuable for identification purposes, since their adoption is rarely motivated by concerns about insider trading, satisfying the exclusion restriction. However, this also implies that UD laws should not be expected to produce large effects on insider trading behavior. While UD laws likely reduce constraints on some insider trading, the most egregious illegal trading remains subject to federal enforcement. Additionally, derivative lawsuits typically seek governance reforms rather than large monetary penalties, limiting their expected deterrent effect.

Regardless of how one weighs these considerations in assessing economic magnitude, our study makes an important contribution by providing clean causal evidence that private enforcement through shareholder litigation has a deterrent effect on insider trading and complements public enforcement.

G. Blockholders as a Placebo Group

Our main analysis excludes trades by 10% blockholders, who are also required to report their trades to the SEC. But unlike D&O, blockholders do not owe any fiduciary duty to other shareholders and therefore are not normally subject to DLs. We next consider whether this distinction allows us to use blockholders as a potential placebo group because UD laws should not affect their trading patterns. But this prediction is not obvious. First, many firms lack a 10% blockholder, and where they exist, many of them are likely passive institutional investors, who do not trade (especially sell) as frequently. This issue is apparent from our untabulated finding that the number of firm-trade days in our sample where blockholders sell (buy) is about 10% (one-third) of those for D&O. Second, even though blockholders are not affected by the decreased risk of DLs, they can mimic trades by D&O, which become more informative after the adoption of UD laws.

Nevertheless, we estimate regressions similar to our baseline (Table 2) regressions separately for the samples of blockholders' sales and purchases. The results, untabulated for brevity, show little resemblance with our main results on D&O

[38]As we discuss in Section IV.C.2, 1-month profits are the most pertinent for pre-QEA insider trades.

[39]Our sample of lawsuits in Table A5 is tilted toward derivative lawsuits with insider trading as a *primary* allegation. This is because, as discussed in Section II.B, we searched for derivative lawsuits with "insider trading" as keywords to identify whether DLs target sales or purchases.

transactions. For non-D&O transactions, *After UD Law* does not obtain signs and significance in the predicted direction in any specification. These results support our litigation risk hypothesis and increase the hurdle for other interpretations of our main results, such as an overall change in stock returns or information asymmetry. UD laws mainly affect opportunistic sales by D&O, who are subject to DLs.

V. Alternative Explanations and Robustness Checks

A. Stacked DiD Regressions

We conduct a rich set of tests to check the robustness of our main results. One concern with our Table 2 results is that, as Table A4 shows, UD laws were adopted over a long period of time, potentially creating a bias if treatment effects vary over time. To alleviate this concern, we re-estimate our baseline model using a cohort-level stacked DiD approach following previous studies (e.g., Cengiz, Dube, Lindner, and Zipperer (2019), Deshpande and Li (2019), and Li, Monroe, and Coulton (2023)). Accordingly, we define firms headquartered in states that do not have UD laws until the end of our sample period as the control group. For each treated state, we construct a balanced panel with 3 years of pretreatment data (i.e., years −3, −2, and −1) and 3 years of posttreatment data (i.e., years +1, +2, and + 3, where +1 is the adoption year). We merge the corresponding years for the clean (i.e., never-treated) control group and stack the data into one data set. We then re-estimate our baseline regression of insider trading profitability on the posttreatment (*After UD Law*) indicator in this stacked sample. We include fixed effects for Firm × Month × Cohort, State × Time × Cohort, and Industry × Time × Cohort, following Li et al. (2023), who analyze a similar setting. In Panel A of Table 7, After UD Law obtains negative and significant coefficients in explaining returns from insider sales. These results are consistent with and stronger than our baseline results in Panel A of Table 2. The results for the purchase sample in Panel B of Table 7 are statistically insignificant at the 5% level, similar to our baseline results in Panel B of Table 2. These results offer additional assurance that our baseline results are not significantly affected by an unbalanced panel or time-varying treatment effects.[40]

To further address the issue of pre- versus posttreatment observations in our staggered DiD setting, we reanalyze our main results in Table 2 by excluding "always adopter" states from our sample, that is, states that adopted UD laws before 1997. This ensures the availability of both pre- and posttreatment observations for all treated states. In this restricted sample, the After UD Law variable yields

[40]Due to data limitations, we are unable to implement the stacked DiD approach for all our tests. Given that insider trading data starts in 1996, and a robust stacked DiD requires 2 or 3 years of data, both pre- and post- treatment, the only states or districts that we can include as treated are HI, IA, MA, RI, SD, and DC. States adopting UD laws prior to 1999 are excluded due to the requirement of pre-treatment IT data. So, the treated sample in this analysis reduces considerably: 5395 observations for sales and 1962 for purchases. Observations from 27 never-treated states serve as clean controls, appearing multiple times across subexperiments. Despite the dominance of the control sample in this analysis, the results are similar to our baseline findings in Table 2, where the sample includes a much larger 39,546 treated observations for sales and 13,388 for purchases. To address concerns about repeated observations, we follow the literature (e.g., Cengiz et al.) and use cohort (subexperiment)-level clustering.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE 7

Robustness Test with Stacked DiD: Baseline Results

Table 7 reports results based on stacked DiD regressions. The sample starts in 1996 and ends in 2013. First, we divide the full sample into clean control firms (i.e., all firms incorporated in the U.S. states that never adopted a Universal Demand law by 2013) and treated firms (i.e., all firms incorporated in the U.S. states that adopted a UD law at some point during our sample period). Treated firms are then divided into 14 cohorts based on the timeline of adoption of UD Laws by different states from 1996 to 2013. Then all treated and clean control firms are kept only for years −3, −2, −1, +1, +2, and + 3, where year +1 is the event year (i.e., the year a state adopts a UD law) and stacked together. The dependent variables are buy-and-hold abnormal returns with no alpha (BHARs) over 6 months (i.e., 126 trading days), 3 months (i.e., 63 trading days), and 1 month (i.e., 21 trading days) after the date of an insider trade. BHARs are calculated using Carhart's four-factor model without alpha. All variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. Control variables are as in Table 2; they are not shown for brevity. Robust standard errors, clustered within states of incorporation, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

	BHAR6m	BHAR3m	BHAR1m
	1	2	3
Panel A. Insider Sales			
After UD law	−0.040***	−0.043***	−0.032***
	(0.008)	(0.008)	(0.003)
Control variables	Yes	Yes	Yes
Firm × month × cohort FE	Yes	Yes	Yes
HQ state × time × cohort FE	Yes	Yes	Yes
Industry × time × cohort FE	Yes	Yes	Yes
No. of obs.	629,095	629,095	629,095
Adj. R^2	0.757	0.716	0.565
Panel B. Insider Purchases			
After UD law	0.051	−0.149*	−0.118
	(0.288)	(0.084)	(0.092)
Control variables	Yes	Yes	Yes
Firm × month × cohort FE	Yes	Yes	Yes
HQ state × time × cohort FE	Yes	Yes	Yes
Industry × time × cohort FE	Yes	Yes	Yes
No. of obs.	153,794	153,794	153,794
Adj. R^2	0.898	0.837	0.656

negative coefficients of −0.032, −0.036, and −0.37, which are close to our baseline estimates in Panel A of Table 2 and statistically significant at the 1% level.

We also conducted an experiment to mimic a balanced panel, albeit imperfectly, in the staggered adoption setting by focusing on a narrower time window around the passage of UD laws. Specifically, we define PostUD3 as the period 1–3 years after the law's adoption and PreUD3 as the period 1–3 years before adoption for treated states. Using these indicators, we re-estimate our baseline regressions in Table 2 to compare how treated states performed after adoption versus before adoption, relative to the control states. For insider sales, the coefficient on PostUD3 is more negative than PreUD3 across all return horizons, indicating that, during this time window, insider sales in treated states experienced more negative abnormal returns after the passage of UD laws compared to before, relative to control states. These differences are statistically significant at the 1% level for the 1-month and 3-month return horizons.

B. Alternate Return Measures

Next, we consider whether our main results hold when we use alternative methods to calculate abnormal returns on insider trades. We consider two other approaches: raw returns and DGTW's characteristic-adjusted abnormal returns, neither of which requires estimation of parameters of asset pricing models.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

We show these results in Panels A1 and B1 of Table A1. Consistent with our main results, After UD Law obtains negative coefficients in predicting abnormal returns from insider sales. The sales results are statistically significant for all time horizons.

We also conduct cross-sectional tests of risks and opportunities using raw returns and DGTW characteristic-adjusted returns as alternate measures of abnormal returns. These results are presented in the remaining panels of Table A1. In Panel A2, the profitability of insider sales after UD law adoption increases more in treatment firms with lower institutional blockholding compared to those with higher institutional blockholding, using both raw returns and DGTW-adjusted returns. Panel B2 reveals a similar pattern for insider purchases, with significantly higher positive returns over 1- and 3-month holding periods among firms with lower (vs. higher) institutional blockholding after UD laws. In Panel A3, the increase in the profitability of insider sales after adoption of UD law is larger for smaller firms; the difference is consistently significant for DGTW returns. Panels A4 and B4 examine the effect of UD laws on insider trading profitability for firms with different R&D intensity. The increase in the profitability of both insider sales and purchases after adoption of UD law is generally larger in more R&D-intensive firms, although the differences are mostly statistically insignificant.

In Panels A5 and B5, we present the results of estimated $Profits in constant 2013 dollars using raw returns and DGTW characteristic-adjusted abnormal returns as alternative measures. Although noisier than our main specifications, the results generally support our main conclusions. UD Laws lead to higher avoidance of dollar losses following sales and generate higher dollar gains following purchases.

In untabulated tests, we also examine results using monthly aggregation, using a regression specification that roughly follows Cohen et al (2012). The dependent variable is the buy and hold return over the 1-, 3-, or 6-month holding periods starting after the month of the trade. The main explanatory variable is After UD Law, and control variables are market equity, book-to-market ratio, 1-month and 12-month lagged returns, and fixed effects for month and firm. We find that the coefficient of After UD Law is −0.6%, −2.0%, and −4.3% in regressions of returns over holding periods of 1, 3, and 6 months after net sales months, and the latter two are statistically significant at 10% level or better. For net purchase months, the coefficient of After UD Law is statistically insignificant in all regressions for all three holding periods. These results generally mirror our baseline results, though they are noisier for the reasons we discussed in Section III.A.

C. Reusing Natural Experiments

Next, Heath et al. (2023) point out that when different researchers use the same natural experiment as a treatment to test different hypotheses, the likelihood of false positives increases. This is the problem of multiple hypothesis testing. Following their recommendation, we justify the relevance and exclusion restrictions for using UD laws as a natural experiment in our setting in the introduction. The relevance condition is satisfied because UD laws dramatically reduce the probability of DLs by as much as one-half during our sample period (see, e.g., Houston et al. (2018), Appel (2019)). The exclusion restriction is satisfied because UD law adoptions by states are usually not in response to elevated concerns about insider trading. UD

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

laws had been used as a natural experiment in about 30 papers at the time that we first shared this article on SSRN (see Heath et al. (2023)). Based on their simulations, the adjusted critical value of the *t*-statistic given 30 prior results for this staggered shock is about 3.1 (see their Table 3, column 1 and Figure 6, Graph A). Most of our results far surpass this modified threshold for statistical significance, which corrects for multiple hypothesis testing.

D. Donelson et al.'s Critiques

We next consider the various critiques of Donelson et al. (2022), who show that Houston et al. (2018) and Appel's (2019) evidence of a decline in DLs after UD laws loses statistical significance, even though the magnitude remains similar, when the sample is extended past 2010. This finding likely has little effect on our conclusions for several reasons. First, our inferences are based on changes in litigation *risk*, not on realized lawsuits per se. Realized changes in the number of DLs underestimate the change in litigation risk if managers adjust their behavior in response to the change in risk. Second, as Donelson et al. (2022) acknowledge, the loss of statistical significance in their study appears solely driven by an unusually large positive coefficient on Louisiana, which adopted its UD law in 2015. Our sample period ends in 2013, so firms incorporated in Louisiana are not part of our treated sample. Moreover, we find that only about 0.40% of U.S. public companies are incorporated in Louisiana, suggesting that this result is likely driven by outliers. Within our sample, less than 0.2% of trades pertain to Louisiana-incorporated firms.

Third, Donelson et al.'s (2022) study is more about UD laws' effect on litigation risk related to accounting or disclosure. We check whether the profitability of insider trading activities after UD is driven by the laws' effects on accounting practices or disclosure. To do so, we use Chenet al.'s (2015) disclosure quotient (DQ) variable, also used by Boone et al. (2023), which is based on the number of nonmissing Compustat line items in financial statements.[41] Higher DQ indicates better disclosure quality. We examine whether changes in the profitability of insider trading are related to DQ. We re-estimate our regressions in Table 2 after adding DQ as an explanatory variable to test the possibility that the increased profitability of insider trades after UD laws is mainly driven by DQ. If so, DQ should subsume the effect of UD laws. In untabulated results, even after controlling for DQ, After UD Law continues to have a significantly negative coefficient in regressions of BHAR following insider sales for all three holding periods, with magnitudes very similar to those in Table 2. In other words, the inclusion of DQ as an explanatory variable does not change our main results.

Finally, Donelson et al. (2022) also document that only 57 out of 2,775 (i.e., about 2%) DLs in their sample have insider trading as the *primary* allegation, raising the question of whether insider trading is a significant issue in DLs. As we point out in the introduction, insider trading is rarely the primary complaint in most DLs. Instead, allegations of insider trading are usually a part of a broader set of complaints about breaches of fiduciary duty by D&O and provide evidence that

[41]We thank Shuping Chen for sharing these data.

bolsters the merit of the lawsuit. Accordingly, Erickson (2010) finds that insider trading is one of the allegations in 60% of DLs.

E. Financial Crisis

Next, our main sample includes the period of the recent financial crisis, raising a concern that our results may be influenced by large negative shocks to stock prices during the crisis. To examine this issue, we re-estimate our baseline regressions in Table 2 after adding the interaction of *After UD Law* and a variable indicating the peak years of the recent financial crisis, 2008 and 2009, as an explanatory variable. The coefficient of this interaction term is statistically insignificant, and the coefficient of After UD Law remains significantly negative and of similar magnitude as in Table 2 for insider sales. The results are also similar when we re-estimate our main regressions after limiting the sample to the end of 2007. We do not tabulate these results for brevity.

F. Impact of Other Regulations

Karpoff and Wittry (2018) argue that a firm's preexisting legal environment can confound the effects of a new law. So, we control for potential confounding effects of other state laws that may have coincided with or affected the states' adoption of UD laws. Following Karpoff and Wittry, we control for control share acquisition law (CS), business combination law (BC), fair price law (FP), directors' duties law (DD), and poison pill law (PP) that were adopted by various states at different times during our sample period. Table A2 shows the results with *bhar6m* as the dependent variable. In Panel A1, the effect of UD Law on the profitability of insider sales remains essentially unchanged after controlling each of these laws separately (in columns 1–5) or jointly (in column 6). The corresponding effect on insider purchases on Panel B1 is statistically insignificant, as in Table 2. The results are quite similar to those in regressions with *bhar3m* and *bhar1m* as dependent variables. We do not tabulate them for brevity.

Finally, prior studies (e.g., Crane and Koch (2016)) find that the 9th Circuit Court ruling of 1999 affected the ownership structure, litigation filing, and corporate governance of firms located in the 9th Circuit. We check the robustness of our results by excluding firms incorporated in states under the jurisdiction of the 9th Circuit Court, namely, Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon, and Washington. The results on BHAR, shown in columns 1–3 of Panels A2 and B2 in Table A2, are similar to our baseline results in Table 2.

VI. Conclusion

Despite a large literature, the question of whether regulations effectively deter opportunistic insider trading remains unresolved. We consider the possibility that mixed prior evidence stems from identification challenges rather than effectiveness of regulations. In this study, we exploit U.S. states' staggered adoption of Universal Demand laws as a natural experiment to examine how shareholder litigation risk affects insider trading behavior. UD laws significantly reduce shareholders' ability

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

to bring derivative lawsuits against corporate insiders, providing clean quasi-experimental variation in litigation risk faced by corporate insiders.

We find that the reduction in the risk of shareholder-initiated litigation following UD laws is associated with more profitable insider trades, mainly sales. This effect is stronger among smaller firms and firms with lower institutional monitoring. By itself, this evidence does not separate whether UD laws reduce deterrence of opportunistic trades based on material nonpublic information or whether they reduce concerns about nuisance litigation for legitimate trades that might appear suspicious ex post. However, our later tests show that following a decrease in litigation risk, insiders engage in trades that appear riskier and more litigation-prone. We find i) an increase in the dollar value of individual insider sales transactions, plausibly timed to avoid subsequent negative abnormal performance, ii) an increase in the ratio of opportunistic sales (see Cohen et al. (2012)), iii) an increase in the profitability of pre-QEA insider sales, which are particularly opportunistic (see Ali and Hirshleifer (2017)), and iv) an increase in pre-QEA sales before negative, but not positive, earnings surprises. Our findings contribute to the ongoing debate, especially in the legal literature, on whether shareholder lawsuits mainly benefit attorneys and impose a deadweight cost on firms by showing that litigation rights also benefit investors.

Funding Statement

Agrawal acknowledges research support from the William A. Powell, Jr. Chair in Finance and Banking.

Appendix

Variable Definitions

Definitions and data sources for the variables are listed below:

After UD Law: An indicator variable that equals 1 if a firm's state of incorporation has adopted Universal Demand (UD) laws in a given year. We use firms' historical states of incorporation obtained from SEC online filings from 1994 to 2013.

BHAR (bhar1m, bhar3m, and bhar6m): A buy-and-hold abnormal return (BHAR) between any two periods T1 and T2 is the compounded daily stock return minus the compounded daily expected return from a risk model for the same period.

$$BHAR_{ji} = \prod_{t-T_1}^{T_2}\left(1+R_{jt}\right) - \prod_{t-T_1}^{T_2}\left(1+R_{RiskModel}\right)$$

We use Carhart's four-factor model as the risk model and estimate parameters over the window from −250 to −50 days. In calculating $R_{Riskmodel}$, we do not include the estimated alpha from the four-factor model. We calculate bhar1m, bhar3m, and bhar6m using 21, 63, and 126 trading days after the insider trading day (day 0), respectively.

DGTW adjusted returns (dgtw1m, dgtw3m, and dgtw6m): Holding period returns adjusted for Daniel et al. (DGTW; 1997) characteristics-based benchmarks by

using daily returns. For example, dgtw1m = A stock's compounded daily returns for 1 month (cret1m) – compound daily returns for 1 month on market value-weighed DGTW benchmark portfolio. The annual stock assignments until 2012 are obtained from Russ Wermers' website: http://terpconnect.umd.edu/~wermers/ftpsite/Dgtw/coverpage.htm, updated to 2013 by the authors.

Total shares traded (lnshares), and total dollar value of trade (lndolvol): Calculated from Thomson Reuters insider trading data. Total shares traded is the total number of shares traded (bought or sold) by all directors and officers in the open market in a firm on a given trading day. Total dollar value is the sum of the dollar values of all trades by D&O on the day (=transaction price × shares). We take the natural logarithm of both variables.

Abnormal dollar profit ($Profit1m, $Profit3m, $Profit6m): Abnormal dollar profit for 1-, 3-, and 6-month holding periods, computed as the product of the abnormal return (BHAR) over the period and the dollar value of an insider trade, and converted to constant 2013 dollars.

Size, large cap, small cap: Natural logarithm of market capitalization, calculated from Compustat as ln(csho*PRCC_C), lagged by 1 year. Large (small) cap indicates larger (smaller) than the sample median market capitalization.

BEME: Natural logarithm of the book to market ratio, calculated from Compustat as ln [ceq/(csho*PRCC_C)], lagged by 1 year.

Leverage: Total debt to market equity, calculated from Compustat as (dltt+dlc)/(csho*PRCC_C)], lagged by 1 year.

Ret ($t - 1$): Past month's total return, calculated from CRSP.

Ret ($t - 12, t - 1$): Cumulative total return for past 12 months, calculated from CRSP.

Spread: [(Ask-Bid)/(Ask+Bid)/2], calculated from CRSP.

ROA: Net income to total assets ratio, calculated from Compustat using (ni/at).

PPENT: The ratio of net property, plant, and equipment to total assets, calculated from Compustat using (ppent/at).

Cash: The ratio of cash to total assets, calculated from Compustat using (ch/at).

SUE: Standardized unexpected earnings = (Actual EPS - Median of analyst forecasts of EPS) / Stock price, calculated from Compustat Fundamentals Quarterly and IBES.

High and low R&D: High (low) R&D indicates higher (lower) than median ratio of research and development expenses to total assets for a given year, calculated from Compustat using (xrd/at).

More (fewer) institutional blockholders: More (fewer) institutional blockholders indicates a firm with above- (below-) median number of institutional investors that own at least 5% of the firm's outstanding equity during the year. Source: Thomson Reuters institutional holdings (13F) file.

Largest 10 institutional ownership: Percentage ownership of a firm's 10 largest institutional investors. Source: Thomson Reuters institutional holdings (13F) file.

Disclosure Quotient (DQ): A disclosure quality score of a firm's financial statements, computed by Chen et al. (2015). DQ is based on a count of nonmissing data items in firms' annual reports as reported by Compustat. It measures the granularity of a firm's financial reports.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A1

UD Law and Insiders' Profitability Using Alternative Measures of Returns

Table A1 reports our baseline regression results with and without firm-level controls. The sample period is 1996–2013. The dependent variables are cumulative total returns (CRET) and DGTW-adjusted abnormal returns (DGTW) over 6 months (i.e., 126 trading days; cret6m, dgtw6m), 3 months (i.e., 63 trading days; cret3m, dgtw3m), and 1 month (i.e., 21 trading days; cret1m, dgtw1m) after an insider trading day. DGTW returns are characteristics-adjusted abnormal returns calculated as in Daniel et al. (1997) but based on compounded daily returns. All variables are winsorized at their 1st and 99th percentiles. After UD Law is an indicator variable that equals 1 if a firm is incorporated in a state that has adopted a UD Law, and 0 otherwise. The abnormal dollar profits in Panels A5 and B5 are in constant 2013 dollars. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively, in two-tailed tests.

Panel A1. Insider Sales

	CRET6m	CRET3m	CRET1m	DGTW6m	DGTW3m	DGTW1m
Variables	1	2	3	4	5	6
After UD Law	−0.033**	−0.025***	−0.023***	−0.028**	−0.021***	−0.020***
	(0.014)	(0.005)	(0.003)	(0.011)	(0.006)	(0.002)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	291,806	291,806	291,806	279,183	279,183	279,183
Adj. R^2	0.706	0.667	0.536	0.643	0.600	0.475

Panel B1. Insider Purchases

	CRET6m	CRET3m	CRET1m	DGTW6m	DGTW3m	DGTW1m
Variables	1	2	3	4	5	6
After UD Law	0.001	0.060*	0.018	−0.055	0.019	−0.020
	(0.049)	(0.034)	(0.031)	(0.044)	(0.039)	(0.027)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	70,547	70,547	70,547	66,537	66,537	66,537
Adj. R^2	0.847	0.799	0.639	0.824	0.774	0.609

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A1 (continued)

UD Law and Insiders' Profitability Using Alternative Measures of Returns

Panel A2. Raw and DGTW-Adjusted Returns from Insider Sales Partitioned by Institutional Ownership

	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD Law	0.086***	−0.166***	0.049***	−0.093***	0.011*	−0.037***	0.095***	−0.130***	0.058***	−0.069***	0.013*	−0.023***
	(0.007)	(0.018)	(0.008)	(0.019)	(0.006)	(0.007)	(0.009)	(0.014)	(0.014)	(0.017)	(0.007)	(0.005)
Test: high–low = 0 (t-stat)	13.64***		5.95***		4.46***		17.58***		4.52***		3.23***	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,534	139,333	144,534	139,333	144,534	139,333	139,400	132,041	139,400	132,041	139,400	132,041
Adj. R^2	0.785	0.812	0.740	0.771	0.589	0.620	0.734	0.781	0.687	0.737	0.537	0.582

Panel B2. Raw and DGTW-Adjusted Returns from Insider Purchases Partitioned by Institutional Ownership

	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown	High Instown	Low Instown
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD Law	0.208*	−0.355	−0.022	0.053	−0.114*	0.239*	0.044	−0.765**	−0.017	−0.123	−0.117***	0.121*
	(0.105)	(0.260)	(0.201)	(0.156)	(0.065)	(0.125)	(0.155)	(0.325)	(0.153)	(0.119)	(0.040)	(0.064)
Test: high–low = 0 (t-stat)	2.17**		−0.25		−2.27**		2.10**		0.63		−2.70***	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	32,311	33,756	32,311	33,756	32,311	33,756	31,022	31,283	31,022	31,283	31,022	31,283
Adj. R^2	0.916	0.919	0.873	0.860	0.680	0.696	0.908	0.909	0.856	0.843	0.649	0.672

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A1 (continued)

UD Law and Insiders' Profitability Using Alternative Measures of Returns

Panel A3. Raw and DGTW-Adjusted Returns from Insider Sales Partitioned by Firm Size

	Big	Small	Big	Small	Big	Small	Big	Small	Big	Small	Big	Small
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD Law	0.005	−0.122***	−0.007	−0.053***	−0.011***	−0.035***	0.029**	−0.130***	0.013	−0.067***	−0.005	−0.042***
	(0.010)	(0.022)	(0.007)	(0.011)	(0.004)	(0.005)	(0.011)	(0.027)	(0.009)	(0.017)	(0.003)	(0.005)
Test: high–low = 0 (t-stat)	4.80***		3.12***		3.80***		4.65***		3.74***		6.70***	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,929	140,965	144,929	140,965	144,929	140,965	140,390	133,022	140,390	133,022	140,390	133,022
Adj. R^2	0.754	0.815	0.715	0.774	0.575	0.621	0.681	0.788	0.646	0.740	0.502	0.588

Panel B3. Raw and DGTW-Adjusted Returns from Insider Purchases Partitioned by Firm Size

	Big	Small	Big	Small	Big	Small	Big	Small	Big	Small	Big	Small
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD Law	−0.120	−0.250*	−0.070	−0.103	0.049	−0.039	0.086	−0.102	0.054	−0.090	0.202***	−0.044
	(0.093)	(0.141)	(0.088)	(0.100)	(0.036)	(0.085)	(0.205)	(0.127)	(0.126)	(0.055)	(0.047)	(0.069)
Test: high–low = 0 (t-stat)	0.66		0.22		0.98		0.92		1.11		3.46***	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	31,256	35,302	31,256	35,302	31,256	35,302	29,314	33,441	29,314	33,441	29,314	33,441
Adj. R^2	0.915	0.919	0.872	0.863	0.688	0.692	0.904	0.909	0.854	0.847	0.653	0.670

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A1 (continued)

UD Law and Insiders' Profitability Using Alternative Measures of Returns

Panel A4. Raw and DGTW-Adjusted Returns from Insider Sales Partitioned by R&D Intensity

	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD Law	−0.053***	0.026	−0.024***	−0.066***	−0.027***	−0.046**	−0.048***	0.024	−0.023***	−0.067**	−0.023***	−0.039*
	(0.010)	(0.035)	(0.007)	(0.023)	(0.003)	(0.020)	(0.011)	(0.046)	(0.007)	(0.026)	(0.003)	(0.021)
Test: high–low = 0 (t-stat)	−1.93*		1.61		0.97		−1.40		1.70		0.80	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	144,981	143,559	144,981	143,559	144,981	143,559	137,378	138,602	137,378	138,602	137,378	138,602
Adj. R^2	0.694	0.853	0.657	0.814	0.529	0.661	0.620	0.830	0.581	0.787	0.457	0.630

Panel B4. Raw and DGTW-Adjusted Returns from Insider Purchases Partitioned by R&D Intensity

	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D	High R&D	Low R&D
	CRET6m	CRET6m	CRET3m	CRET3m	CRET1m	CRET1m	DGTW6m	DGTW6m	DGTW3m	DGTW3m	DGTW1m	DGTW1m
	1	2	3	4	5	6	7	8	9	10	11	12
After UD law	−0.035	−0.750***	0.104***	−0.488***	−0.003	−0.055	−0.153***	−0.678*	0.029	−0.427***	−0.055	0.028
	(0.060)	(0.274)	(0.034)	(0.116)	(0.036)	(0.035)	(0.049)	(0.377)	(0.035)	(0.144)	(0.037)	(0.057)
Test: high–low = 0 (t-stat)	2.62***		5.65***		0.91		1.37		3.12***		−1.11	
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	34,722	33,178	34,722	33,178	34,722	33,178	32,396	31,640	32,396	31,640	32,396	31,640
Adj. R^2	0.851	0.926	0.804	0.868	0.652	0.681	0.831	0.915	0.783	0.849	0.621	0.654

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A1 (continued)

UD Law and Insiders' Profitability Using Alternative Measures of Returns

Panel A5. Abnormal Dollar Profits from Insider Sales

	$CRET6m	$ CRET3m	$CRET1m	$DGTW6m	$DGTW3m	$DGTW1m
Variables	1	2	3	4	5	6
After UD law	−12,247	−33,534**	−23,256***	−6,339	−14,998	−8,700*
	(19,590)	(16,221)	(5,942)	(12,011)	(11,492)	(4,403)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	291,806	291,806	291,806	279,183	279,183	279,183
Adj. R^2	0.360	0.338	0.282	0.327	0.312	0.248

Panel B5. Abnormal Dollar Profits from Insider Purchases

	$CRET6m	$ CRET3m	$CRET1m	$DGTW6m	$DGTW3m	$DGTW1m
Variables	1	2	3	4	5	6
After UD law	12,174	17,660***	9,167**	3,301	12,700**	6,173*
	(7,750)	(6,327)	(3,721)	(7,194)	(5,948)	(3,246)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	70,547	70,547	70,547	66,537	66,537	66,537
Adj. R^2	0.531	0.506	0.437	0.535	0.503	0.413

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A2

Controlling for the Effects of Other Laws

Table A2 reports regression results controlling for the effects of other state laws. The sample starts in 1996 and ends in 2013. The dependent variables are buy-and-hold abnormal returns (BHARs) over 6 months (i.e., 126 trading days), 3 months (i.e., 63 trading days), and 1 month (i.e., 21 trading days) after the date of an insider trade. BHARs are calculated using Carhart's four-factor model without alpha. All variables are winsorized at their 1st and 99th percentiles. *After UD Law* is an indicator variable that equals 1 if the firm is incorporated in a state that has adopted a UD law, and 0 otherwise. In Panels A1 and B1, *DD Law* (*PP Law*) {*CS Law*} [BC Law] ((FP Law)) is an indicator variable that equals 1 if a firm is incorporated in a state that has a Directors Duty law (Poison Pill law) {Control Share law} [Business Combination law] ((Fair Price law)), and 0 otherwise. Panels A2 and B2 show our baseline results excluding firms incorporated in states under the jurisdiction of the 9th circuit court. Robust standard errors clustered at the state of incorporation level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

Panel A1. Insider Sales

	BHAR6m	BHAR6m	BHAR6m	BHAR6m	BHAR6m	BHAR6m
	1	2	3	4	5	6
After UD law	−0.037***	−0.039***	−0.033**	−0.034**	−0.036***	−0.033**
	(0.011)	(0.011)	(0.013)	(0.013)	(0.012)	(0.013)
DD law	0.029**					−0.000
	(0.011)					(0.023)
PP law		0.036***				0.046**
		(0.010)				(0.020)
CS law			−0.011			−0.069***
			(0.017)			(0.022)
BC law				−0.027		−0.028
				(0.025)		(0.025)
FP law					0.041**	0.025
					(0.019)	(0.023)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	291,677	291,677	291,677	291,677	291,677	291,677
Adj. R^2	0.629	0.629	0.629	0.629	0.629	0.629

Panel B1. Insider Purchases

	BHAR6m	BHAR6m	BHAR6m	BHAR6m	BHAR6m	BHAR6m
	1	2	3	4	5	6
After UD law	0.083	0.067	0.065	0.081	0.079	0.047
	(0.058)	(0.062)	(0.063)	(0.058)	(0.059)	(0.066)
DD law	−0.012					−0.402***
	(0.064)					(0.103)
PP law		0.081				0.337***
		(0.058)				(0.091)
CS law			0.093			0.139
			(0.109)			(0.116)
BC law				0.047		0.105
				(0.122)		(0.104)
FP law					0.040	−0.007
					(0.065)	(0.085)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	70,432	70,432	70,432	70,432	70,432	70,432
Adj. R^2	0.824	0.824	0.824	0.824	0.824	0.824

Panel A2. Insider Sales after Dropping Ninth Circuit Firms

	BHAR6m	BHAR3m	BHAR1m
	1	2	3
After UD law	−0.029**	−0.024***	−0.025***
	(0.012)	(0.005)	(0.003)
Control variables	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes

(continued on next page)

TABLE A2 (continued)

Controlling for the Effects of Other Laws

	BHAR6m	BHAR3m	BHAR1m
Panel A2. Insider Sales after Dropping Ninth Circuit Firms (continued)			
	1	2	3
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	262,040	262,040	262,040
Adj. R^2	0.636	0.601	0.473
Panel B2. Insider Purchases after Dropping Ninth Circuit Firms			
	BHAR6m	BHAR3m	BHAR1m
	1	2	3
After UD law	0.105	0.016	0.029
	(0.067)	(0.051)	(0.037)
Control variables	Yes	Yes	Yes
Firm × month FE	Yes	Yes	Yes
HQ state × time FE	Yes	Yes	Yes
Industry × time FE	Yes	Yes	Yes
No. of obs.	63,155	63,155	63,155
Adj. R^2	0.838	0.784	0.620

TABLE A3

Firm Characteristics Before and After UD Laws

Table A3 reports summary statistics of the firm characteristics used in this study before and after UD laws separately for the purchase and sales samples. The sample consists of firms in which D&O make open-market trades in common stock of firms listed on NYSE, Amex, or Nasdaq, excluding financial and utility firms, during 1996–2013. Observations are at the firm-trade day level. All variables are winsorized at their 1st and 99th percentiles. The Appendix provides variable definitions and data sources.

Variables	Mean	SD	25%	50%	75%	No. of Obs.
Panel A1. Firm Characteristics of the Insider Sales Sample before UD Laws						
Size	6.62	1.85	5.39	6.52	7.78	287,952
BEME	−1.19	0.81	−1.66	−1.13	−0.63	283,067
Leverage	0.22	0.51	0.00	0.04	0.21	287,952
Ret (*t*-1)	0.05	0.16	−0.04	0.03	0.12	293,655
Ret (*t*-12, *t*-1)	0.20	0.51	−0.06	0.19	0.46	279,680
ROA	0.01	0.19	0.00	0.05	0.10	293,232
PPENT	0.21	0.20	0.06	0.14	0.29	293,247
Cash	0.17	0.17	0.04	0.12	0.24	291,757
Spread	0.21	0.52	0.02	0.06	0.23	289,944
Panel A2. Firm Characteristics of the Insider Sales Sample after UD Laws						
Size	6.28	1.72	5.14	6.30	7.28	39,108
BEME	−1.00	0.74	−1.41	−0.95	−0.51	38,734
Leverage	0.24	0.50	0.00	0.08	0.26	39,108
Ret (*t*-1)	0.04	0.15	−0.04	0.03	0.11	39,546
Ret (*t*-12, *t*-1)	0.20	0.45	−0.03	0.19	0.42	38,782
ROA	0.04	0.15	0.02	0.06	0.10	39,520
PPENT	0.25	0.20	0.09	0.19	0.34	39,414
Cash	0.13	0.14	0.03	0.09	0.19	39,100
Spread	0.23	0.52	0.02	0.06	0.24	39,043
Panel B1. Firm Characteristics of the Insider Purchase Sample before UD Laws						
Size	5.07	1.91	3.66	4.89	6.30	85,666
BEME	−0.77	0.94	−1.32	−0.71	−0.15	81,990
Leverage	0.59	1.34	0.01	0.15	0.54	85,666
Ret (*t*-1)	−0.04	0.19	−0.15	−0.04	0.05	87,547
Ret (*t*-12, *t*-1)	−0.20	0.64	−0.55	−0.14	0.20	83,279
ROA	−0.09	0.30	−0.11	0.01	0.06	87,110
PPENT	0.25	0.22	0.08	0.18	0.34	87,196
Cash	0.14	0.18	0.02	0.07	0.20	86,848
Spread	0.77	1.22	0.11	0.38	0.97	84,615

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A3 (continued)

Firm Characteristics Before and After UD Laws

Variables	Mean	SD	25%	50%	75%	No. of Obs.
Panel B2. Firm Characteristics of the Insider Purchase Sample after UD Laws						
Size	4.94	1.82	3.63	4.75	6.24	13,115
BEME	−0.64	0.85	−1.14	−0.60	−0.10	12,848
Leverage	0.56	1.14	0.03	0.19	0.58	13,115
Ret (*t*-1)	−0.04	0.18	−0.13	−0.04	0.05	13,388
Ret (*t*-12, *t*-1)	−0.16	0.60	−0.48	−0.10	0.21	12,876
ROA	−0.03	0.26	−0.03	0.03	0.07	13,338
PPENT	0.26	0.21	0.10	0.20	0.38	13,358
Cash	0.11	0.15	0.02	0.05	0.14	13,259
Spread	0.84	1.45	0.13	0.43	1.06	12,932

TABLE A4

Timeline of the Adoption of UD Laws

Table A4 reports the timeline of the adoption of the UD laws by 23 U.S. states and DC.

Source: Appel (2019) and Jung et al. (2021)

Year	State
1989	GA
	MI
1990	FL
1991	WI
1992	MT
	VA
	UT
1993	NH
	MS
1995	NC
1996	AZ
	NE
1997	CT
	ME
	PA
	TX
	WY
1998	ID
2001	HI
2003	IA
2004	MA
2005	RI
	SD
2011	DC

TABLE A5

Summary of Google Scholar Case Search Results

Table A5 summarizes the findings from a search conducted on Google Scholar Case Law in August 2024. The search used the terms: "insider trading" + "derivative action" OR "derivatively" OR "derivative litigation," without specifying purchases or sales. The search scope included all state courts and covered cases from 1996 to 2013. Out of the top 20 search results sorted by relevance, we eliminated three duplicate entries.

Case Title	Primary Allegation	Insider Trading Allegation
In re Oracle Corp. Derivative Litigation	Insider trading by executives based on nonpublic info.	Yes, selling
In re Citigroup Inc. Shareholder Litigation	Directors failed to monitor subprime risks; insider trading in a consolidated case.	Yes, selling
Pfeiffer v. Toll	Insider trading by selling stock while possessing nonpublic info.	Yes, selling
Grosset v. Wenaas	Insider trading related to a secondary offering.	Yes, selling

(continued on next page)

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

TABLE A5 (continued)

Summary of Google Scholar Case Search Results

Case Title	Primary Allegation	Insider Trading Allegation
Beam ex rel. M. Stewart Living v. Stewart	Breach of fiduciary duty related to selling ImClone stock.	Yes, selling
Kahn v. Kolberg Kravis Roberts Co., L.P.	Insider trading by purchasing preferred stock based on nonpublic info.	Yes, buying
In re Walt Disney Co. Derivative Litigation	Breach of fiduciary duty related to executive compensation.	No
Marx v. Akers	Allegations of corporate waste and breaches of fiduciary duty.	No
Desimone v. Barrows	Backdating stock options and financial misstatements.	No
Friese v. Superior Court	Insider selling with knowledge of misleading accounting.	Yes, selling
In re Caremark International Inc.	Breaches of fiduciary duty leading to legal risks.	No
Biondi v. Scrushy	Insider selling by directors before adverse info was public.	Yes, selling
Stone v. Ritter	Failure to monitor employee compliance with regulations.	No
Guttman v. Huang	Insider trading based on improper accounting practices.	Yes, selling
Braddock v. Zimmerman	Insider trading by directors using nonpublic information.	Yes, selling
Scrushy v. Tucker	Insider selling by concealing impact of a new policy.	Yes, selling
In re American International Group, Inc.	Insider trading by selling stock based on nonpublic fraud info.	Yes, selling

References

American Bar Association. *Model Business Corporation Act: Official Text with Official Comment and Statutory Cross–References, Revised Through December 2007*. Chicago, IL (2008).

Aboody, D., and B. Lev. "Information Asymmetry, R&D, and Insider Gains." *Journal of Finance*, 55 (2000), 2747–2766.

Agrawal, A., and S. Chadha. "Corporate Governance and Accounting Scandals." *Journal of Law and Economics*, 48 (2005), 371–406.

Agrawal, A., and T. Cooper. "Insider Trading Before Accounting Scandals." *Journal of Corporate Finance*, 34 (2015), 169–190.

Agrawal, A., and J. F. Jaffe. "Does Section 16b Deter Insider Trading by Target Managers?" *Journal of Financial Economics*, 39 (1995), 295–319.

Agrawal, A.; J. F. Jaffe; and J. M. Karpoff. "Management Turnover and Corporate Governance Changes Following the Revelation of Fraud." *Journal of Law and Economics*, 42 (1999), 309–342.

Agrawal, A., and T. Nasser. "Insider Trading in Takeover Targets." *Journal of Corporate Finance*, 18 (2012), 598–625.

Aldredge, D. M., and D. C. Cicero. "Attentive Insider Trading." *Journal of Financial Economics*, 115 (2015), 84–101.

Ali, U., and D. Hirshleifer. "Opportunism as a Firm and Managerial Trait: Predicting Insider Trading Profits and Misconduct." *Journal of Financial Economics*, 126 (2017), 490–515.

Appel, I. "Governance by Litigation." Working Paper, University of Virginia (2019).

Appel, I. R.; T. A. Gormley; and D. B. Keim. "Passive Investors, not Passive Owners." *Journal of Financial Economics*, 121 (2016), 111–141.

Baker, T., and S. J. Griffith. "How the Merits Matter: Directors' and Officers' Insurance and Securities Settlements." *University of Pennsylvania Law Review*, 157 (2008), 755.

Banerjee, A., and E. W. Eckard. "Why Regulate Insider Trading? Evidence from the First Great Merger Wave (1897–1903)." *American Economic Review*, 91 (2001), 1329–1349.

Barber, B. M., and J. D. Lyon. "Detecting Long-run Abnormal Stock Returns: The Empirical Power and Specification of Test Statistics." *Journal of Financial Economics*, 43 (1997), 341–372.

Bertrand, M., and S. Mullainathan. "Enjoying the Quiet Life? Corporate Governance and Managerial Preferences." *Journal of Political Economy*, 111 (2003), 1043–1075.

Bettis, J. C.; J. L. Coles; and M. L. Lemmon. "Corporate Policies Restricting Trading by Insiders." *Journal of Financial Economics*, 57 (2000), 191–220.

Bhattacharya, U. "Insider Trading Controversies: A Literature Review." *Annual Review of Financial Economics*, 6 (2014), 385–403.

Bhattacharya, U., and C. D Marshall. "Do They Do It for the Money?" *Journal of Corporate Finance*, 18 (2012), 92–104.

Boone, A.; E. Fich; and T. Griffin. "Shareholder Litigation Risk and the Information Environment: Revisiting Evidence from Two Natural Experiments." *Journal of Corporate Finance*, 82 (2023), 102444.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

Brav, A.; W. Jiang; and H. Kim. "The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Labor Outcomes." *Review of Financial Studies*, 28 (2015), 2723–2769.
Brochet, F., and S. Srinivasan. "Accountability of Independent Directors: Evidence from Firms Subject to Securities Litigation." *Journal of Financial Economics*, 111 (2014), 430–449.
Carhart, M. M. "On Persistence in Mutual Fund Performance." *Journal of Finance*, 52 (1997), 57–82.
Cengiz, D.; A. Dube; A. Lindner; and B. Zipperer. "The Effect of Minimum Wages on Low-Wage Jobs." *Quarterly Journal of Economics*, 134 (2019), 1405–1454.
Chen, S.; B. Miao; and T. Shevlin. "A New Measure of Disclosure Quality: The Level of Disaggregation of Accounting Data in Annual Reports." *Journal of Accounting Research*, 53 (2015), 1017–1054.
Cheng, C.S. A.; H. H. Huang; and Y. Li. "Does Shareholder Litigation Deter Insider Trading?" *Journal of Law, Finance, and Accounting*, 1 (2016), 275–318.
Cheng, Q., and K. Lo. "Insider Trading and Voluntary Disclosures." *Journal of Accounting Research*, 44 (2006), 815–848.
Choi, S. J.; K. K. Nelson; and A. C. Pritchard. "The Screening Effect of the Private Securities Litigation Reform Act." *Journal of Empirical Legal Studies*, 6 (2009), 35–68.
Coffee Jr, J. C. "Reforming the Securities Class Action: An Essay on Deterrence and its Implementation." *Columbia Law Review*, 106 (2006), 1534.
Cohen, L.; C. Malloy; and L. Pomorski. "Decoding Inside Information." *Journal of Finance*, 67 (2012), 1009–1043.
Cohen, M. "Corporate Governance Law: Firm Heterogeneity and the market for Corporate Domicile." Working Paper, Columbia University (2012).
Crane, A. D., and A. Koch. "Shareholder Litigation and Ownership Structure: Evidence from a Natural Experiment." *Management Science*, 64 (2016), 5–23.
Cziraki, P., and J. Gider. "The Dollar Profits to Insider Trading." *Review of Finance*, 25 (2021), 1547–1580.
Dai, L.; R. Fu; J. K. Kang; and I. Lee. "Corporate Governance and the profitability of Insider Trading." *Journal of Corporate Finance*, 40 (2016), 235–253.
Daniel, K.; M. Grinblatt; S. Titman; and R. Wermers. "Measuring Mutual Fund Performance with Characteristic-based Benchmarks." *Journal of Finance*, 52 (1997), 1035–1058.
Davis Jr, K. B. "The Forgotten Derivative Suit." *Vanderbilt Law Review*, 61 (2008), 387–451.
Dechow, P. M.; R. G. Sloan; and A. P. Sweeney. "Causes and Consequences of Earnings Manipulation: An Analysis of Firms Subject to Enforcement Actions by the SEC." *Contemporary Accounting Research*, 13 (1996), 1–36.
Del Guercio, D.; E. R Odders-White; and M. J. Ready. "The Deterrent Effect of the Securities and Exchange Commission's Enforcement Intensity on Illegal Insider Trading: Evidence from Run-Up before News Events." *Journal of Law and Economics*, 60 (2017), 269–307.
Deshpande, M., and Y. Li. "Who is Screened Out? Application Costs and the Targeting of Disability Programs." *American Economic Journal: Economic Policy*, 11 (2019), 213–248.
Donelson, D. C.; L. Kettell; J. McInnis; and S. Toynbee. "The Need to Validate Exogenous Shocks: Shareholder Derivative Litigation, Universal Demand Laws and Financial Reporting Decisions." *Journal of Accounting and Economics*, 73 (2022), 101427.
Embroker. Directors and Officers Insurance. (2019). https://www.embroker.com.
Erickson, J. "Corporate Governance in the Courtroom: An Empirical Analysis." *William & Mary Law Review*, 51 (2010), 1749–1831.
Ferreira, M. A., and P. Matos. "The Colors of Investors' Money: The Role of Institutional Investors Around the World." *Journal of Financial Economics*, 88 (2008), 499–533.
Ferris, S. P.; T. Jandik; R. M. Lawless; and A. Makhija. "Derivative Lawsuits as a Corporate Governance Mechanism: Empirical Evidence on Board Changes Surrounding Filings." *Journal of Financial and Quantitative Analysis*, 42 (2007), 143–165.
Fidrmuc, J. P.; M. Goergen; and L. Renneboog. "Insider Trading, News Releases, and Ownership Concentration." *Journal of Finance*, 61 (2006), 2931–2973.
Gao, F.; L. L. Lisic; and I. X. Zhang. "Commitment to Social Good and Insider Trading." *Journal of Accounting and Economics*, 57 (2014), 149–175.
Garfinkel, J. A. "New Evidence on the Effects of Federal Regulations on Insider Trading: The Insider Trading and Securities Fraud Enforcement Act (ITSFEA)." *Journal of Corporate Finance*, 3 (1997), 89–111.
Gormley, T. A., and D. A. Matsa. "Growing Out of Trouble? Corporate responses to Liability Risk." *Review of Financial Studies*, 24 (2011), 2781–2821.
Gormley, T. A., and D. A. Matsa. "Playing it Safe? Managerial Preferences, Risk, and Agency Conflicts." *Journal of Financial Economics*, 122 (2016), 431–455.
Henry, T. F. "Earnings Management and Stock Options." Working Paper, New York University (2003).

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

Heath, D.; M. C. Ringgenberg; M. Samadi; and I. M. Werner. "Reusing Natural Experiments." *Journal of Finance*, 78 (2023), 2329–2364.
Houston, J. F; C. Lin; S. Liu; and L. Wei. "Litigation Risk and Voluntary Disclosure: Evidence from Legal Changes." *Accounting Review*, 94 (2019), 247–272.
Houston, J. F.; C. Lin; and W. Xie. "Shareholder Protection and the Cost of Capital." *Journal of Law and Economics*, 61 (2018), 677–710.
Huang, S.; S. Roychowdhury; and E. Sletten. "Does Litigation Encourage or Deter Real Earnings Management?" *Accounting Review*, 95 (2020), 251–278.
Huddart, S. J., and B. Ke. "Information Asymmetry and Cross-Sectional Variation in Insider Trading." *Contemporary Accounting Research*, 24 (2007), 195–232.
Huddart, S.; B. Ke; and C. Shi. "Jeopardy, Non-Public Information, and Insider Trading Around SEC 10-K and 10-Q Filings." *Journal of Accounting and Economics*, 43 (2007), 3–36.
Jaffe, J. F. "Special Information and Insider Trading." *Journal of Business*, 47 (1974), 410–428.
Jeng, L. A.; A. Metrick; and R. Zeckhauser. "Estimating the Returns to Insider Trading: A Performance-Evaluation Perspective." *Review of Economics and Statistics*, 85 (2003), 453–471.
Johnson, M. F.; K. K. Nelson; and A. C. Pritchard. "Do the Merits Matter More? The Impact of the Private Securities Litigation Reform Act." *Journal of Law, Economics and Organization*, 23 (2007), 627–652.
Jones, R. "Rethinking Corporate Federalism in the Era of Corporate Reform." *Journal of Corporation Law*, 29 (2004), 625–663.
Jung, S.; J. Nam; and S. Shu. "Shareholder Litigation and Insider Trading: Evidence from Derivative Lawsuits." SSRN Working Paper No. 3109369 (2021).
Kallunki, J.-P.; H. Nilsson; and J. Hellström. "Why Do Insiders Trade? Evidence Based on Unique Data on Swedish Insiders." *Journal of Accounting and Economics*, 48 (2009), 37–53.
Kallunki, J.; J.-P. Kallunki; H. Nilsson; and M. Puhakka. "Do an Insider's Wealth and Income Matter in the Decision to Engage in Insider Trading?" *Journal of Financial Economics*, 130 (2018), 135–165.
Karpoff, J. M.; S. Lee, and G. Martin. "The Cost to Firms of Cooking the Books." *Journal of Financial and Quantitative Analysis*, 43 (2008), 581–611.
Karpoff, J. M, and M. D Wittry. "Institutional and Legal Context in Natural Experiments: The Case of State Antitakeover Laws." *Journal of Finance*, 73 (2018), 657–714.
Khan, M., and H. Lu. "Do Short Sellers Front-Run Insider Sales?" *Accounting Review*, 88 (2013), 1743–1768.
Kinney, T. P. "Stockholder Derivative Suits: Demand and Futility Where the Board Fails to Stop Wrongdoers." *Marquette Law Review*, 78 (1994), 172.
Korniotis, G. M., and A. Kumar. "State-level Business Cycles and Local Return Predictability." *Journal of Finance*, 68 (2013), 1037–1096.
Lakonishok, J., and I. Lee. "Are Insider Trades Informative?" *Review of Financial Studies*, 14 (2001), 79–111.
Lee, I.; M. Lemmon; Y. Li; and J. M. Sequeira. "Do Voluntary Corporate Restrictions on Insider Trading Eliminate Informed Insider Trading?" *Journal of Corporate Finance*, 29 (2014), 158–178.
Li, L. L.; G. S. Monroe; and J. Coulton. "Managerial Litigation Risk and Corporate Investment Efficiency: Evidence from Universal Demand Laws." *Journal of Empirical Legal Studies*, 20 (2023), 196–232.
Lin, C.; M. Officer; and H. Zou. "Directors' and Officers' Liability Insurance and Acquisition Outcomes." *Journal of Financial Economics*, 102 (2011), 507–525.
Marin, J. M., and J. P. Olivier. "The Dog that did not Bark: Insider Trading and Crashes." *Journal of Finance*, 63 (2008), 2429–2476.
Meulbroek, L. K. "An Empirical Analysis of Illegal Insider Trading." *Journal of Finance*, 47 (1992), 1661–1699.
Moskowitz, T.J., and M. Grinblatt. "Do Industries Explain Momentum?" *Journal of Finance*, 54 (1999), 1249–1290.
Obaydin, I.; R. Zurbruegg; M. N. Hossain; B. K. Adhikari; and A. Elnahas. "Shareholder Litigation Rights and Stock Price Crash Risk." *Journal of Corporate Finance*, 66 (2021), 101826.
Pinto, A. R., and D. M. Branson. *Understanding Corporate Law*, fourth edition. Durham, NC: Carolina Academic Press (2013).
Qi, J., and G. Pederson. "Derivative Litigation and Board Effectiveness: Evidence from Delaware's Judicially-led Reforms in 2003." Paper presented at the American Economic Association Poster Session (2019).
Ravina, E., and P. Sapienza. "What do Independent Directors Know? Evidence from Their Trading." *Review of Financial Studies*, 23 (2010), 962–1003.
Romano, R. "The Shareholder Suit: Litigation without Foundation?" *Journal of Law, Economics & Organization*, 7 (1991), 55–87.

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press

Seyhun, H. N. "Insiders' Profits, Costs of Trading, and Market Efficiency." *Journal of Financial Economics*, 16 (1986), 189–212.
Seyhun, H N. "The Effectiveness of the Insider Trading Sanctions." *Journal of Law and Economics*, 35 (1992), 149–182.
Shleifer, A., and R. W. Vishny. "A Survey of Corporate Governance." *Journal of Finance*, 52 (1997), 737–783.
Swanson, C. B. "Juggling Shareholder Rights and Strike Suits in Derivative Litigation: The ALI drops the ball." *Minnesota Law Review*, 77 (1992), 1339–1392.
Weiss, E. J., and J. S Beckerman. "Let the Money do the monitoring: How Institutional Investors can Reduce Agency Costs in Securities Class Actions." *Yale Law Journal*, 104 (1995), 2053–2127.
West, M. D. "Why Shareholders Sue: The Evidence from Japan." *Journal of Legal Studies*, 30 (2001), 351–382.
Willis. "Executive Risks". March, Issue 11 (2005).
Wizbowski, R. (2024). "Fiduciary Duties of Board Members: A Guide for Effective Governance and Reduced Liability Risk." https://www.diligent.com/resources/blog/fiduciary-duties-of-board-members.
Wu, W. "Information Asymmetry and Insider Trading." Working Paper, Texas A&M University (2018).
Xu, J. "New Evidence on the Effects of the Insider Trading Sanctions Act of 1984." SSRN Working Paper No. 1100641 (2008).

https://doi.org/10.1017/S0022109025102482 Published online by Cambridge University Press