Mispricing and Anomalies: An Exogenous Shock to Short Selling from the Dividend Tax Law Change*

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Abstract

We study the *causal* effect of short selling on asset pricing anomalies by exploiting a novel exogenous shock to short selling. After the Job and Growth Tax Relief Reconciliation Act (JGTRRA) of 2003, equity lenders are reluctant to lend shares around the dividend record dates because substitute dividends that they would receive are taxed at ordinary income rates while qualified dividends are taxed at 15 percent, thus creating a negative shock to short selling. Using arguably the most comprehensive set of anomalies to date and the difference-in-differences (DID) regression framework, we find that anomalies become stronger after the dividend record months in the post-JGTRRA periods, driven by stronger mispricing in the dividend record months. We further show that the effect mainly comes from the overpriced stocks. Overall, our results provide strong evidence that most anomalies are likely due to mispricing, with valuation anomalies as exception.

Keywords: Exogenous short-selling shock; mispricing; anomalies; market efficiency; dividend taxation; difference-in-differences

JEL Classification Codes: G00, G12, G14

1 Introduction

Asset pricing literature documents that many firm characteristics can predict future stock returns (see e.g., Nagel, 2005; Harvey et al., 2016; Hou et al., 2017), yielding anomalies that standard asset pricing models cannot explain. A fundamental question of the literature is what causes anomalies. Despite many studies on anomalies, researchers still disagree on the source of return predictability. The literature offers two main explanations. First, return predictability could be a result of cross-sectional variations in risk (e.g., Fama and French, 1992, 1998). Second, it could reflect mispricing (e.g., Barberis and Thaler, 2003; Hirshleifer et al., 2012; Engelberg et al., 2018). In the presence of limits to arbitrages, such as short-sale constraints, it would be difficult for investors to quickly exploit overpricing. Therefore, overpriced stocks generate lower future returns due to slow correction and contribute to the return predictability.

Recent studies try to disentangle these two explanations by focusing on how short-sale constraints affect the profitability of anomalies. If anomalies indeed reflect mispricing, the cross-sectional return predictability would be stronger among more short-sale constrained stocks (Nagel, 2005). To test this conjecture, researchers rely on proxies of short-sale constraints such as firm size, breadth of ownership, institutional ownership, short interests and lending fees.[1] However, these proxies often arise endogenously and could be correlated with measures of risk (e.g., Lam and Wei (2011), and Reed (2015)) .

In this paper, we alleviate these concerns by exploiting a novel exogenous shock to short selling and investigate the *causal* relationship between short-sale constraints and arguably the most comprehensive set of anomalies. The shock stems from the differential taxation on dividends faced by equity lenders. It occurs each time a firm pays a dividend, four times a year. In general, short sellers need to borrow shares to sell to potential buyers, and equity holders lend shares to short sellers for a fee. If a stock loan is open over the dividend record

[1]For details on the use of those proxies, see Jones and Lamont (2002), Geczy et al. (2002), Chen et al. (2002), Ali et al. (2003), Asquith et al. (2005), Nagel (2005), Hirshleifer et al. (2011), Israel and Moskowitz (2013), Drechsler and Drechsler (2014), and Han et al. (2019).

date, the short seller reimburses the lender the amount of dividends (substitute dividends) because the buyer in the short position is the legal shareholder of record. After the Jobs and Growth Tax Relief Reconciliation Act (henceforth JGTRRA) of 2003, those substitute dividends are taxed at ordinary income rates while qualified dividends are taxed at 15 percent. To that end, equity lenders would increase their fees and decrease their lending quantities around the dividend record dates. Thornock (2013) first documents this dividend taxation effect on short selling. We confirm this negative tax-driven shock to short selling in months when dividends are recorded (henceforth dividend record months), and find that JGTRRA has a more negative impact on the relative short interests in the dividend record months than in the other months, while we observe no significant difference in the relative short interests between them before JGTRRA.

We investigate how shocks to short selling activities in the dividend record months affect mispricing and anomalies from July 1985 to December 2019. To measure mispricing, we select 182 significant anomalies from a comprehensive set of 355 individual anomalies from the existing literature. Inspired by Stambaugh et al. (2015) and Engelberg et al. (2018), we construct a cross-sectional aggregated mispricing measure, *net overpriced score, (NOPS)*. Stocks with the highest values of *NOPS* are the most "overpriced", whereas those with the lowest values are the most "underpriced". We examine two main hypotheses. (1) Mispricing is stronger in the dividend record months compared to the other months after the JGTRRA of 2003, and as a result, anomalies are stronger in the subsequent months. (2) The effect mainly comes from the overpriced stocks.

To investigate the causal effect of short selling on mispricing and anomalies, we use a stock-level difference-in-differences (DID) panel regression framework. Specifically, we regress future one-month stock returns on *NOPS*, dividend record month dummy and *JGTRRA* dummy variables, the interaction terms between *NOPS* and each of two dummy variables, and finally a three-way interaction term between *NOPS*, *DivR*, and *JGTRRA*. The coefficient on the three-way interaction term measures the DID effect, namely, the difference between after and before the enacting of JGTRRA of 2003 of the differences in the predictive effect between the dividend record months and non-dividend record months. In other words, it captures the differential responses of anomalies to JGTRRA between following the dividend record months and following the other months. We show that the coefficients on the three-way interaction term, $NOPS \times DivR \times JGTRRA$, are significantly negative at the 1% level for various fixed effects and clustering methods. These results indicate that after JGTRRA, anomalies become stronger following the dividend record months than following the other months, because stocks become more mispriced in the dividend record months when it is harder for arbitrageurs to short sell stocks.

Chu et al. (2019) is the first to examine the causal effect of short-sale constraints with 11 well-known anomalies by exploring the SEC's Regulation SHO (Reg SHO). However, there are studies that raise concerns about whether Reg SHO has a meaningful impact on short selling. For example, Diether et al. (2009) show that Reg SHO only has a marginal effect on short selling volume and no effect on returns or volatility. They also find that the remove of the uptick rule has little impact on NASDAQ firms.[2] We confirm these results by showing that there are no significant differences between the pilot and non-pilot stocks in the relative short interests during Reg SHO program periods. In addition, Heath et al. (2019) show that the repeated use of the Reg SHO increases the likelihood of false discoveries. Furthermore, given that Reg SHO lasts only for two years, and Chu et al. (2019) consider only 11 anomalies, it is unclear whether Reg SHO is powerful enough to affect a comprehensive set of anomalies.

We show that the tax-driven shock to short selling is persistent and its impact on short-sale constraints is more powerful than that o Reg SHO. First, our results still hold after the Reg SHO program periods, when the uptick rule is removed for all stocks. Second, we repeat the DID analysis in Chu et al. (2019) using *NOPS*, and find an insignificant difference in the return predictability of *NOPS* for pilot and non-pilot stocks during the Reg SHO program periods, suggesting that Reg SHO may not have a significant impact on the comprehensive set of anomalies.

[2]Due to this marginal impact of Reg SHO on short selling, Black et al. (2019) reassess three recently published papers: Fang et al. (2016), Grullon et al. (2015) and Hope et al. (2017), and claim that their results are not robust.

To test the hypothesis that the effects of shocks to short selling come mostly from the overpriced stocks, we extend our DID analysis by adding *High NOPS*, *Low NOPS* and their corresponding interaction terms. If our conjecture is true, then we expect that the coefficient on *High NOPS*$\times$*DivR*$\times$*JGTRRA* would be significantly negative, while the coefficient on *Low NOPS*$\times$*DivR*$\times$*JGTRRA* would be insignificant. Indeed, we find that the coefficient on *High NOPS*$\times$*DivR*$\times$*JGTRRA* is -0.827 with a t-stats of -2.43, whereas the coefficient on *Low NOPS*$\times$*DivR*$\times$*JGTRRA* is positive and insignificant. In summary, our results are consistent with the mispricing explanation for anomalies. This tax-driven exogenous shock to short selling prevents arbitrageurs from exploiting overpricing and thereby amplifying anomalies.

We further demonstrate that our results are unlikely driven by risk or data-mining. Following Engelberg et al. (2018), we find that our results are robust to controlling of various dynamic risk factors including market risk and five macroeconomic risk factors of Chen et al. (1986). Moreover, we conduct various placebo tests to address the data-mining concern. We change the timing of JGTRRA to various periods and re-estimate our DID regressions. We find that these fictitious acts have no impact on anomalies between dividend record months and other months. Next, we randomly create pseudo dividend record months without changing the timing of JGTRRA and then repeat our DID analysis. We find that the coefficient of the three-way interaction term is always statistically insignificant.

We conduct a battery of robustness checks. First, we show that our results hold in a portfolio-level DID frameworks similar to Chu et al. (2019). Economically, in response to JGTRRA, the change in the anomaly returns is on average 1.677% higher after the dividend record months than after the other months, indicating a stronger response of anomalies after the dividend record months. Second, we find that the effect of shocks to short selling on mispricing is more pronounced in high investor sentiment periods and is stronger for stocks with younger age, smaller size, higher idiosyncratic volatility, higher relative short interests, and lower size-adjusted institutional ownership. Lastly, we show in the appendix that our results are robust to alternative sample period, regression specification, mispricing measure, etc.

Equipped with the large number of anomalies, we separate them into four groups: event, market, fundamentals and valuation, and construct the mispricing measure (*NOPS*) for each of the anomaly groups following Engelberg et al. (2018). We then conduct the DID analysis and find that the effect of the shock is significant for event, market, and fundamentals anomalies but is insignificant for the valuation anomalies. Similar results are obtained if we separate the anomalies into momentum, profitability, investment, intangible, trading frictions, and value & growth according to Hou et al. (2017). The effect is highly significant in all but the anomalies in value & growth.

Finally, it is worth noting that the DID framework used in this paper only requires a rather weak exogeneity condition that the decision to issue dividend does not affect the evolution of anomalies over time, which is likely to be true as firms rarely change their dividend policies. In particular, it does not require that dividend stocks and non-dividend stocks are indistinguishable. In other words, dividend stocks can differ from non-dividend stocks in systematic ways as long as the differences do not depend on some time-varying unobservable that affect the anomalies because of the double difference approach. Nevertheless, we test the robustness of our results using only firms that issue dividends so as to have a completely matched sample. In addition, Chetty and Saez (2005) report that a number of firms initiate dividends immediately after the enactment of the law, which in itself should not affect the evolution of anomalies over time since it is a one-time change. We nevertheless further exclude all firms that initiate dividend payouts after the JGTRRA of 2003 to mitigate the potential concern that firms may initiate dividends in response to the act. An additional benefit of this exclusion is that we now have the same firms as treated (dividend record months) and control (other months) before and after JGTRRA. In other words, we have a setting that resembles the controlled experiment that is often available in biology, physics, or other natural science disciplines, but rarely available to finance or economics. Our results are robust in both samples.

Our paper makes significant contributions to the literature of market efficiency and anomaly. First, it proposes a novel exogenous shock to short selling and provides strong evidence for a *causal effect* of short selling on anomalies. Second, our paper constructs arguably the most comprehensive set of anomalies containing 355 individual firm, stock, or option attributes identified in the literature. Third, this paper sheds new light on the source of return predictability for anomalies. Using this exogenous shock to short selling, we are able to disentangle the mispricing explanation from the risk-based explanation.[3] The large number of anomalies allow us to divide them into different types and investigate each type separately. Our findings suggest that most anomalies are likely driven by mispricing and limits-to-arbitrage, whereas the valuation anomalies are not.

There is one caveat for our findings. We provide strong evidence that overall, anomalies are likely driven by mispricing. Yet, if we separate the anomalies into different types, valuation anomalies do not seem to be affected by mispricing. Therefore, it is possible that some individual anomalies in the other three types may not be driven by mispricing. We only measure the effect on the anomalies as a group and do not rule out that possibility.

The remainder of the paper is organized as follows. Section 2 introduces JGTRRA and its effect on short selling. Section 3 discusses sample and research design. Section 4 presents the stock-level differences-in-difference regression results. Section 5 differentiates various explanations for our results. Section 6 provides various robustness checks. Section 7 investigate different type of anomalies. Section 8 conducts further analysis including using only dividend stocks, etc. Section 9 concludes.

2 JGTRRA Dividend Tax Cut

The Jobs and Growth Tax Relief Reconciliation Act (JGTRRA) of 2003 was a tax law, passed by the United States Congress on May 23, 2003. This law reduced the maximum federal tax

[3]The literature also offers a third explanation – anomalies could be a result of data-mining (e.g., Harvey et al., 2016; Linnainmaa and Roberts, 2018). However, Chen (2020) argues that it is virtually impossible to attribute all the anomalies to p-hacking. Our results also cast doubts on this explanation. Since the dividend record dates of different firms are mostly random (symmetric with the median at the middle of the month), if anomalies are driven by data-mining, it is difficult to explain why they become stronger following the dividend record months after JGTRRA.

rate on qualified dividends from 38.6% to 15%.[4] This tax cut remains effective for taxpayers whose income does not exceed the thresholds set for the highest income tax.[5]

JGTRRA provides a new opportunity for examining the effects of dividend taxation on financial market. First, this tax cut was largely a surprise to the market prior to 2003 as it moved from initial proposal to signed law in under 5 months. Consequently, researcher can consider it as an exogenous event. Second, JGTRRA was free of other major changes to the tax law that might confound empirical analysis of its effects. The literature links the dividend tax cut of JGTRRA to corporate payout policies (Chetty and Saez, 2005; Brown et al., 2007; Brav et al., 2008; Hanlon and Hoopes, 2014), capital structure (Lin and Flannery, 2013), mutual fund tax clienteles (Sialm and Starks, 2012), and return comovement (Hameed and Xie, 2019).

The JGTRRA dividend tax cut also substantially affects short selling. Thornock (2013) first documents the effect of dividend taxation on short selling around the dividend record date using a proprietary short lending data between 2005 and 2007. He argues that dividend taxation can affect short selling via two channels. The first channel, *loan effect*, stems from the different tax treatment for qualified and unqualified dividends. If a short seller borrows a stock over the dividend record date, then she repays the amount of dividend to the lender because the buyer in the short sale is the legal shareholder of record. This repayment refers to the substitute dividend, which is taxed at ordinary income rate rather than the rate of qualified dividends.

The following numerical example explains the above tax effect. A mutual fund in the 35% marginal tax bracket who owns 100,000 shares of a firm with a price of $100 and dividend payment of $0.20. After JGTRRA of 2003, this dividend of $20,000 could be taxed at 15% and therefore paying $3,000 taxes. However, if the fund lends the shares, it would pay $7,000

[4]JGTRRA also reduced the statutory long-term capital gains tax rate from 20% to 15% and the top marginal tax rate on ordinary income from 38.6% to 35%.

[5]The American Taxpayer Relief Act of 2012 was passed by the Congress on January 1, 2013. Under this law, the maximum federal tax rate for qualified dividends was lifted to 20% for taxpayers whose income exceed the highest income threshold ($400,000 for single filers; $425,000 for heads of households; $450,000 for joint filers; $11,950 for estates and trusts).

taxes. This tax differential of $4,000 is economically large. Consequently, tax-sensitive equity lenders would increase their fees and decrease their lending quantities around the dividend record dates. Another adverse effect of dividend taxation on short selling is associated with dividends received deduction (DRD) from corporate income. The DRD allows for a 70% deduction on dividends received from other corporations. However, substitute dividends are not qualified for the DRD.

The second channel is called *reimbursement effect*. It is known that on the ex-dividend date, stocks prices do not drop by the full amount of the dividend. As a result, the substitute dividend, which is the cost of a short seller, is greater than the price drop. This reimbursement effect can also adversely affect short selling around dividend record dates. Collectively, Thornock (2013) shows that lending fees spike on average by 24% over the median rate and loan quantities for tax-sensitive lenders decrease by 18% over the median quantities before the dividend record dates. For tax-neutral lenders, he finds that lending fees also increase but to a lesser degree, but there is no change in lending shares.

Relatedly, Dixon et al. (2019) also observe a significant tightening of the equity lending market around dividend record days. They find that an expansion of demand and a contraction of supply contribute to the tightening effect around dividend record days. Blocher et al. (2013) find that prices of hard-to-borrow stocks surge around ex-dividend dates due to a decline in short selling supply driven by the dividend taxation. Nevertheless, none of these studies directly test a causal relation between JGTRRA on short selling. Using JGTRRA as an exogenous event, we empirically confirm the adverse effect of the dividend taxation on short selling during the dividend record months in Section 8.

3 Data and Research Design

In this section, we discuss the data used in this paper, the construction of the aggregated mispricing measure based on a comprehensive set of anomalies, and the difference-in-differences panel regression framework used to detect the impact of shocks to short selling on mispricing and the strength of anomalies.

3.1 Sample

We collect dividend information, prices, and monthly returns from the Center for Research in Security Prices (CRSP) between July 1965 and December 2019. One concern is that from 1954 to 1984, a dividend income exemption was introduced that initially started at \$50, and a 4% tax credit for dividends above the exemption. After 1985, dividends were fully taxed under ordinary income rates, without any exemption, until the JGTRRA of 2003.[6] To that end, we restrict our main analysis using the sample from July 1985 to December 2019. We also restrict our sample to ordinary taxable cash dividends (CRSP distribution code = 1232) of \$0.01 or greater that are paid by ordinary common shares listed on the NYSE, AMEX, or NASDAQ exchanges. We exclude stocks with prices below \$5 per share on the cum-dividend dates.

We define $DivR_{i,t-1}$ as a dummy variable that equals to one if stock i reports a dividend record date in month $t-1$ and zero otherwise. Panel A Table 1 provides descriptive statistics of $DivR_{i,t-1}$ for our sample. In total, we obtain 1,588,481 firm-month observations with a mean $DivR$ of 14.20%. We obtain firm information from CRSP/Compustat Merged annual and quarterly files, IBES, Thompson Reuter's 13F database, and OptionMetrics to construct anomaly variables.

3.2 Stock mispricing: net overpriced score

We use a comprehensive set of anomalies to construct the mispricing measure. Our initial anomaly pool consists of 355 individual anomaly variables. These variables are primarily drawn from Hou et al. (2017), Chen and Zimmermann (2020), and Engelberg et al. (2019), Engelberg et al. (2018), Green et al. (2017), McLean and Pontiff (2016), and Harvey et al. (2016).[7] We follow Green et al. (2017) to exclude variables that are insignificant in predicting future

[6] A brief history of dividend tax rates in U.S. can be found in `https://www.dividend.com/taxes/a-brief-history-of-dividend-tax-rates/`

[7] Chen and Zimmermann (2020) covers all independent anomalies in Hou et al. (2017); 98% of the portfolios in McLean and Pontiff (2016); 90% of the characteristics from Green et al. (2017); and 90% of the firm-level predictors in Harvey et al. (2016). We thank Andrew Chen for making their data available. We also obtain additional variables from Han et al. (2020), Wu and Xu (2018), Avramov et al. (2020) and among others.

returns and end up with 182 significant ones.[8] Specifically, for each anomaly, we sort stocks into ten decile portfolios each month and then compute the value-weighted returns for the long-short portfolio. If the average long-short portfolio return of an anomaly is insignificant at the 10% level, we then drop this variable from our pool of anomalies.

Inspired by Stambaugh et al. (2015) and Engelberg et al. (2018), we construct a cross-sectional aggregated mispricing measure, *net overpriced score, (NOPS)*. Stocks with the highest values of *NOPS* are the most "overpriced", whereas those with the lowest values are the most "underpriced". We construct the mispricing measure as follows. Each month, we sort stocks into ten decile portfolios based on each anomaly characteristics. We use the extreme deciles to define the long or short side for each anomaly. Next, for each firm and month, we sum the number of short-side and long-side anomalies that the firm belongs to. Doing so will produce *NShort* and *NLong*. Finally, The cross-sectional mispricing measure, *NOPS* is defined as $NShort - NLong$. Panel B Table 1 provides the descriptive statistics of *NOPS*. On average, a stock is in 12.54 short portfolios and 14.83 long portfolios. *NOPS* has a mean value of -2.29, a standard deviation of 10.12, a maximum value of 63, and a minimum value of -61.

3.3 Difference-in-differences regressions

Ideally, the best approach to identify a *causal* effect is a controlled (random) experiment often done in physics, biology, and other natural science disciplines. In finance and economics, the best available situation most of the times is a quasi-experiment such as JGTRRA or Reg SHO. In this case, the validity of the approach crucially depends on the identification assumption. An instrumental variable approach such as 2-step Least Square is often used if an exogenous variable that only affects the treated can be identified. Alternatively, if conditional exogeneity assumption (or selection on observables) holds, a simple difference approach or propensity score matching approach would suffice. However, if instead a weaker exogeneity condition holds such as exogeneity of selection to changes in outcomes, the appropriate approach is the difference-in-differences (DID) approach that compares the difference from before and after

[8] We also drop nine dividend related anomalies such as dividend initiation, dividend omission, and dividend yield, etc.

the enact of a law for a treated group to the same difference for a control group. In particular, DID approach does not require the treated and the control groups to be matched or the same. Instead, it only requires that the selection does not change over time or if it changes it won't affect the changes in the outcome. Since a firm's dividend policy rarely changes, it seems that the DID approach is appropriate to study the effect of JGTRRA.[9]

In this paper, we investigate the effect of the differential tax-driven shock to short selling on mispricing and anomalies in a stock-level DID panel regression framework. Specifically, we estimate the following regression equation,

$$\begin{aligned} ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} \\ + b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}, \qquad (1) \end{aligned}$$

where $ret_{i,t}$ is the percentage return of stock i in month t. $DivR_{i,t-1}$ is the dummy variable indicating the dividend record months. $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003 (after the JGTRRA of 2003). $JGTRRA_{t-1}$ itself is subsumed by the time fixed effect, and thus is dropped from the regression. α_t is the time fixed effect that captures the common factor and/or market-wide or economy-wide trends that drive the stock returns in both dividend record months and other months. α_i is the firm fixed effect to mitigate the potential omitted variable bias.

The three-way interaction term, $NOPS \times DivR \times JGTRRA$ captures the moderating effect of the JGTRRA of 2003 and the dividend record months on the predictive power of $NOPS$. The DID coefficient b_6 is the coefficient of interest, capturing the difference between the dividend record months and non-dividend record months in their respective changes of $NOPS'$ predictive power between after and before the enactment of JGTRRA of 2003. In other words, it captures the differential response of anomalies to JGTRRA between the dividend record months and the non-dividend record months. If our hypothesis is true, we would expect b_6

[9] Even if a firm's dividend policy does change occasionally, for example, a firm initiating dividend payouts coincidentally after JGTRRA, it should not affect the evolution of anomaly over time, as long as it does not constantly change its policy over time. JGTRRA also allows for a situation resembling the controlled experiment, which will be discussed in details in Section 8.

to be significantly negative because of stronger mispricing in the dividend record months.

We also investigate the causal effect of short selling on anomalies in a portfolio-level DID panel regression framework similar to Chu et al. (2019). For stocks that report dividend record dates in the previous months, we sort them into ten decile portfolios based on *NOPS* and then compute the monthly equal-weighted portfolio returns for the long, short, and long-short portfolios. Then we repeat the procedure for stocks that do not report dividend record dates in the previous months. Next, we conduct the following DID panel regression,

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{t-1} + \beta_1 Treated_{t-1} \times JGTRRA_{t-1} + \epsilon_{i,t}, \tag{2}$$

where α_t is the time fixed effect. $y_{i,t}$ is the equal-weighted monthly return of portfolio i, which can be the long side, short side, or the long-short portfolio in month t; $Treated_{t-1}$ is a dummy variable which is equal to one if portfolio i is formed on stocks which report dividend record dates in month $t-1$. The DID coefficient, β_1, is the coefficient of interest, which captures the difference between the dividend record months and the other months in their respective differences in portfolio returns after versus before the JGTRRA of 2003. In other words, it captures the differential impact of JGTRRA on anomaly returns after the dividend record months versus after the non-dividend record months.

4 Shocks to Short Selling and Mispricing

In this section, we examine the hypothesis that mispricing is stronger in dividend record months compared with other months after the JGTRRA of 2003 periods. We also investigate whether our hypothesis holds after Regulation SHO program periods. Finally, we test whether the effect of Regulation SHO on mispricing is robust using *NOPS*.

4.1 Stock-level difference-in-differences analyses

In this subsection, we report the effect of the tax-driven shock to short selling on mispricing and anomalies in a stock-level DID panel regression framework as discussed in subsection 3.3. Note that the three-way interaction term in Equation (1), $NOPS \times DivR \times JGTRRA$ captures the moderating effect of the JGTRRA of 2003 and the dividend record months on the predictive power of *NOPS*, and thus its coefficient, b_6, represents the differential impacts of JGTRRA on the predictive power of *NOPS* between the dividend record months and the non-dividend record months. If our hypothesis is true, we would expect b_6 to be significantly negative.

Table 2 reports the coefficients of Equation (1), from b_1 to b_6 for specifications with various fixed effects and clustering methods. We find that our overpricing measure, *NOPS*, is highly significantly negative in each column. In the last column, the coefficient of *NOPS* is -0.100 with a t-statistic of -11.12, confirming the strong negative return predictability for *NOPS*. Interestingly, we also observe significantly negative coefficients of $DivR_{i,t-1}$, indicating a negative return after dividend record month before JGTRRA. This result is consistent with Hartzmark and Solomon (2013), who find a positive premium in dividend month, while observing a substantial reversal in the 40 days after the ex-dividend day. Additionally, the interaction term between the overpricing score and the dividend record month dummy variable is always significantly positive. This finding implies that before JGTRRA, return predictability of *NOPS* becomes weaker after the dividend record months. It is likely that the differences between the dividend stocks and non-dividend stocks largely account for this result. Next, we show a strong positive coefficient for $NOPS \times JGTRRA$ in each column, consistent with the findings that anomalies are weaker in the recent two decades (see, e.g, Chordia et al., 2014).

Consistent with our prediction, the coefficients on the three-way interaction term are significantly negative across all columns. For example, in the last column with firm and time fixed effects and double clustering standard errors, b_6 is -0.028 with a t-stat of -2.89, which is significant at the 1% level. These results indicate that a significant increase of the predictive power of *NOPS* to future returns in response to the enactment of JGTRRA for the dividend record months relative to the other months. It is worth noting that the results do not indicate that the predictive power of *NOPS* is higher in the dividend record months than in the non-dividend record months. In fact, the predictive power of *NOPS* is similar in both the dividend record months and the other months after JGTRRA, while it is much weaker for the dividend record months before JGTRRA.

We also conduct several robustness checks. First, we re-run the regression by extending the sample period up to July 1965. Second, we replace firm fixed effect with industry fixed effect (3 digit SIC codes) and then re-estimate our regressions. Finally, we repeat our analyses using an alternative proxy for mispricing, which is the mispricing score proposed by Stambaugh et al. (2015).[10] These results are presented in Appendix A1 - A3, respectively. All these results deliver the same message as Table 2. Collectively, we demonstrate that this tax-driven shock to short selling tightens short-sale constraints and thereby causing stocks to be more overpriced in the dividend record months than in the non-dividend record months.

4.2 Does this effect hold after the Reg SHO program periods?

Researchers consider Reg SHO program, which randomly selects pilot stocks to remove the uptick rule, as an exogenous shock to the relaxation of short-sale constraints. This program was in effective from May 2, 2005 to July 6, 2007. After the program period, the SEC eliminated short-sale price tests for all exchange-listed stocks. Consequently, the short-sale constraint imposed by the up-tick rule was removed for all stocks. In this subsection, we investigate whether the effect of the differential tax-driven shock to short selling on mispricing and anomalies still prevails after the Reg SHO program periods. Doing so could validate the robustness of this dividend taxation shock to short-sale constraints.

We exclude sample periods between June 2003 and June 2007 and redo our DID analysis. Table 3 reports our regression results. We find that the coefficients on the three-way interaction term are always negatively significant. For example, using firm and time fixed effects and double clustering method, the coefficient of $NOPS \times DivR \times JGTRRA$ is -0.022 with a t-statistic of -2.64 . Our results indicate that the effect of the dividend taxation shock to short selling on mispricing and anomalies is powerful even after the Reg SHO, when the short-sale price tests are eliminated for all stocks.

[10] *MISP* is constructed using 11 anomalies studied in Stambaugh et al. (2012). The data of *MISP* is available on Robert Stambaugh's website http://finance.wharton.upenn.edu/~stambaug/.

4.3 Does Reg SHO affect net overpriced score?

In this subsection, we test the robustness of the impact of Reg SHO on anomalies using our mispricing measure constructed using 182 anomalies. We run the following stock-level difference-in-differences panel regression:

$$
\begin{aligned}
ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 Pilot_i \times During_{t-1} + b_3 NOPS_{i,t-1} \times Pilot_i \\
+ b_4 NOPS_{i,t-1} \times During_{t-1} + b_5 NOPS_{i,t-1} \times Pilot_i \times During_{t-1} + \varepsilon_{i,t},
\end{aligned} \quad (3)
$$

where $Pilot_i$ is a dummy variable equal to one if stock i is a pilot stock, and zero otherwise; $During_{t-1}$ is a dummy variable equal to one if month $t-1$ is between July 2005 and June 2007 (i.e., during the pilot period of Reg SHO). Similarly, α_t and α_i capture the time and firm fixed effects, respectively. $Pilot_i$ is subsumed by the firm fixed effect and $During_t$ is subsumed by the time fixed effect. Our sample consists of pilot and nonpilot stocks listed on NYSE/AMEX exchanges in Reg SHO program. The sample period is between July 1985 and June 2007 because Reg SHO program ended on July 6, 2007. In Appendix Table A4, we follow Chu et al. (2019) and reproduce the portfolio-level DID results of using the equal-weighted and gross-return weighted portfolio returns. We also consider various sample periods as robustness tests.

Similarly, the DID coefficient, b_5, captures the difference-in-differences in anomaly return predictability between pilot and non-pilot stocks during the program period, compared with the periods before Reg SHO. Table 4 displays the results with different fixed effects and clustering methods. We show that for each column, the coefficient on the the three-way interaction term, $NOPS \times Pilot \times During$, is positive but statistically insignificant. Our portfolio-level DID results in Appendix Table A4 deliver a consistent message. We find that anomaly returns generated by the pilot stocks are statistically indifferent from those by the nonpilot ones. These results are robust to alternative sample periods and different weighting methods. Collectively, we find that the effect of Reg SHO on anomalies is weak after using a comprehensive set of anomalies. Two reasons could account for this insignificant result: (1)

the impact of Reg SHO on short-sale constraints is marginal; (2) given that Reg SHO only spans for two years, its testing power could be weak.

5 Explanations for the Effect of the Tax-Driven Shock

In this section, we explore various explanations for our results. We first examine whether the effect of the tax-driven shocks to short selling on anomalies mainly comes from the overpriced stocks. Next, we investigate whether risks can explain why anomalies become stronger following the dividend record months after JGTRRA. Finally, we address the concern that our results could be spurious using various placebo tests.

5.1 Overpricing from the tax-driven shock to short selling

Anomalies could reflect mispricing. In the presence of limits-to-arbitrage such as short-sale constraints, negative information could be slowly incorporated into stock prices. Therefore, overpriced stocks earn lower future stock returns and contribute to the return predictability. Because this tax-driven shock to short selling tightens short-sale constraints, its effect on anomalies should be mainly manifested on the overpriced stocks, which are concentrated in the short leg of the anomalies.

We construct two dummy variables, *Low NOPS* and *High NOPS*, based on the decile rank of *NOPS* each month. *Low NOPS* identifies the most underpriced stocks, while *High NOPS* represents the most overpriced stocks. In other words, *High NOPS* represents stocks in the short side of anomalies, whereas *Low NOPS* reflects stocks in the long side of anomalies. Next, we separately add *High NOPS* or *Low NOPS*, along with the interactions between $JGTRRA$, *DivR* and each of the dummy variables to our DID regression in Table 2. We also put these dummy variables and interaction terms together and then re-run the DID regressions. The specification is as follows,

$$
\begin{aligned}
ret_{i,t} = {} & \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t-1} + b_2 DivR_{i,t-1} \times JGTRRA_{t-1} + b_3 NOPS_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR_{i,t-1} \\
& + b_5 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_6 Low\ NOPS_{i,t-1} + b_7 High\ NOPS_{i,t-1} + b_8 Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \\
& + b_9 Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} + b_{10} High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{11} High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{12} Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{13} High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}.
\end{aligned} \tag{4}
$$

The results are presented in Table 5. Consistent with our prediction, the effect mainly comes from the overpriced stocks. The first column reports the regression results for *Low NOPS* alone. The coefficient on the three-way interaction term, $Low\ NOPS \times DivR \times JGTRRA$, is positive but statistically insignificant. This result imply that underpriced stocks play little role in driving the difference in return predictability of anomalies across the dividend record months and the other months after the JGTRRA. The second column presents the result for *High NOPS* alone. The coefficient on the three-way interaction term, $High\ NOPS \times DivR \times JGTRRA$, is -0.854 with a t-stats of -2.49, which is significant at the 1% level. The last column reports the result in Equation (4) after considering *Low NOPS* and *High NOPS* effects together. We obtain similar results. The coefficient on $Low\ NOPS \times DivR \times JGTRRA$ is insignificant, while the coefficient of $High\ NOPS \times DivR \times JGTRRA$ is significantly negative. These results indicate that after JGTRRA, stocks in the short leg of anomalies become substantially overpriced in the dividend record month than in the other months. Consequently, this pattern drives the effect of the tax-driven shock to short selling on the strength of the anomalies.

5.2 The risk-based explanation

In this subsection, we examine whether risk-based explanations can explain why anomalies become stronger following the dividend record months after JGTRRA.

We set up a dynamic risk premia framework by considering an arbitrage pricing theory (APT) model with different sources of risks. Consider that the expected return of *NOPS* can be explained by its exposure to multiple systematic risk factors. We write the expected return for a stock in a typical dynamic-multi-factor model such that,

$$E[r_{i,t}] = r_f + \beta_{i,j,t} \times E[f_{j,t}], \tag{5}$$

where $E[f_{j,t}]$ denotes the risk-premium with respect to the risk factor f_j at time t.

Our results could potentially hold if either (1) risk premia change in the dividend record months or (2) betas change in the dividend record months. For example, an increase in risk premium would lead to a higher long-short portfolio returns for *NOPS* during the dividend record months. Alternatively, around the dividend record dates, betas increase (decrease) for *Low* (*High*) *NOPS* stocks.

We consider various sources of risk factors in the APT model. We use the CRSP value-weighted index return as a proxy for the market risk factor and five macroeconomic risk factors of Chen et al. (1986), including log-change in monthly industrial production index (MP), unexpected inflation (UI), change in expected inflation (DEI), change in term premium (UTS), and change in default premium (UPR).[11]

To examine whether risk can explain our results, we re-estimate our stock-level DID regression in the previous section by adding risk factors and their corresponding interaction terms. We modify Equation (4) by interacting each source of risk with the *High* or *Low NOPS*, the $JGTRRA$, the *DivR*, and also include the four-way interactions between each of the *High* and *Low NOPS*, the $JGTRRA$, the *DivR*, and each source of the risks. In Appendix, we provide an example of the regression specification with the market risk factor. Table 6 displays our results. We find similar results to Table 5 after controlling for the effects of the various risk factors. These coefficients of *High NOPS*×*DivR*×*JGTRRA* are always significantly negative in all columns, whereas those coefficients of *Low NOPS*×*DivR*×*JGTRRA* are mostly insignificant. We obtain similar results after adding Fama and French (2015) five-factor betas to our regression in Equation (4), as a robustness. The results can be found in Appendix A5. Over-

[11]The first three factors data used in Liu and Zhang (2008) can be downloaded from Laura Liu's website: `http://lauraxiaoleiliu.gsm.pku.edu.cn/en_research.htm`. The data for term premium and default premium are obtained from Amit Goyal's website `http://www.hec.unil.ch/agoyal/`.

all, our findings suggest that risk is unlikely to explain the findings that overpriced stocks largely contribute to the effect of the tax-driven shock on anomalies.

5.3 Placebo tests

Data-mining and repeated use of the same data have always been a concern in finance (see, e.g., Harvey et al., 2016; McLean and Pontiff, 2016; Linnainmaa and Roberts, 2018). For instance, Heath et al. (2019) show that the repeated use of the Reg SHO pilot program increases the likelihood of false discoveries. We alleviate this concern by exploiting a novel exogenous shock to short selling, and thus it is less likely to be spurious. However, to further guard against spurious results, we conduct several falsification tests for our main DID analysis.

First, we conduct various placebo tests by changing the timing of JGTRRA, while maintaining dividend record dates for each stock. We use the timing before and after 2003 for pseudo enactment of JGTRRA including July of 1997 and 2013, and January of 1999 and 2005. To avoid the actual effect of JGTRRA, we use two sample periods: (1) between 1985:7 and 2003:5; (2) between 2003:6 and 2019:12 for our placebo tests. We run the difference-in-differences regression as follows,

$$\begin{aligned} ret_{i,t} =& \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} \\ &+ b_4 NOPS_{i,t-1} \times PseudoJGTRRA_{t-1} + b_5 DivR_{i,t-1} \times PseudoJGTRRA_{t-1} \\ &+ b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times PseudoJGTRRA_{t-1} + \varepsilon_{i,t}, \end{aligned} \tag{6}$$

where $PseudoJGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after each of these four pseudo JGTRRA periods and zero otherwise.

Table 7 reports these placebo tests. For pseudo events before 2003 , these coefficients of $NOPS \times DivR \times PseudoJGTRRA$ are significantly positive, while those coefficients are statistically insignificant for pseudo events after 2003.

Next, we conduct several placebo tests on the dividend record months. We consider two testing samples: (1) excluding the dividend record months; (2) non dividend-paying stocks only. For each sample, in each month, we randomly choose 14% of firm-months observations (based on summary statistics in Table 1) to be the dividend record months. Consequently, we create a pseudo dividend record month dummy variable for each sample. We run the following regressions using different simulation seeds,

$$\begin{aligned} ret_{i,t} = &\alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 PseudoDivR_{i,t-1} + b_3 NOPS_{i,t-1} \times PseudoDivR_{i,t-1} \\ &+ b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_5 PseudoDivR_{i,t-1} \times JGTRRA_{t-1} \\ &+ b_6 NOPS_{i,t-1} \times PseudoDivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}, \end{aligned} \tag{7}$$

where $PseudoDivR_{i,t-1}$ is a dummy variable indicating the pseudo dividend record month of $t-1$ for stock i.

The results are presented in Table 8. We show that the coefficient of interest is essentially zero and statistically highly insignificant in each column. In sum, our placebo tests indicate that our results are not spurious that anomalies become stronger following the dividend record months after JGTRRA.

6 Portfolio-Level DID and Subsample Analyses

In this section, we provide portfolio-level DID analyses to further confirm our findings in the previous sections. We first conduct portfolio-level DID regressions to re-examine our hypotheses. Next, we provide a series of subperiod or subsample analyses to further confirm the causal effect of short selling on anomalies.

6.1 Portfolio-level DID analyses

In this subsection, we investigate the causal effect of short selling on mispricing and anomalies in a portfolio-level DID panel regression framework. Similar to Section 5, we also examine whether risk can explain our portfolio-level results by adding six risk factors into Equation (2), and interacting each factor with *Treated* and *Treated* $\times$ $JGTRRA$.

Table 9 reports the results of the portfolio-level DID regressions. The coefficients of interest are always significantly positive for the long-short portfolios at 1% level. These results further confirm our first hypothesis that anomalies become stronger after the dividend record months compared with the change after the other months in response to the JGTRRA of 2003. For example, in our baseline results, β_1 of the long-short portfolio is 1.677 with a t-stat of 4.04, which is sizeable. This result indicates that in response to JGTRRA, the change in the anomaly returns is on average 1.677% higher after the dividend record months than after the other months. It is worth noting that it does not mean that the anomaly return is higher after the dividend record months than after the other months. It merely signifies that JGTRRA has a stronger impact on anomalies following the dividend record months, and this is because mispricing is stronger in the dividend record month due to the tax-driven shock to short selling. Indeed, β_0, the coefficient of $Treated$, is -2.083, highly significant and larger than β_1 in magnitude, suggesting that anomalies are much weaker after the dividend record months before JGTRRA. This result is consistent with that reported in Table 2. In contrast, anomalies are much less so following the dividend record months after JGTRRA.

We also find that the short side dominates the long side portfolio in driving the causal effect of short selling on anomalies, confirming our previous findings. In the baseline DID results, the coefficient on $Treated \times JGTRRA$ in the short side is -1.284 compared with 0.393 in the long side. Economically, after JGTRRA, the short side contributes to around 77% of difference in anomaly profit change between the dividend record months and the other months. Although the coefficient on $Treated \times JGTRRA$ for the long side is significant in the baseline DID, it becomes insignificant after controlling for the dynamic risk factors.[12] For the short side, β_1 is always negatively significant at 1% level in each column. Additionally, the DID coefficient of $Treated \times JGTRRA$ becomes stronger in the presence of the dynamic risk factors.

We also conduct portfolio-level DID analyses for our placebo tests. Table 10 reports these placebo tests on tax code change. Similar to our stock-level DID placebo tests, we change the

[12] This coefficient and β_0 for the short side become insignificant in Table 14 where we use dividend stocks only, suggesting that the significance is due to the differences between dividend and non-dividend stocks.

timing of JGTRRA to various time periods including July of 1997, January of 1999, January of 2005 and July of 2013. Next, we separately re-estimate the DID regressions with Pseudo event dummies. Consistent with our stock-level DID results, we find the coefficients of $Treated \times PseudoEvent$ are always statistically insignificant.

Overall, our portfolio-level results confirm our findings in the stock-level DID analyses. We document that this dividend taxation shock imposes a greater constraints to short selling in the dividend record months and thereby causing more overpricing in the short legs of the anomalies. As a result, anomalies become stronger following the dividend record months compared with the other time periods.

6.2 Investor sentiments

In this subsection, we examine how investor sentiment impacts the relation between short selling and anomalies. Stambaugh et al. (2012) argue that when investor sentiment is high, overpricing becomes more prevalent and thereby anomalies become stronger. If this short selling and anomalies relation is driven by mispricing, then this effect should be more pronounced in the high investor sentiment periods and should be concentrated in the short legs of anomalies.

We use two orthogonalized investor sentiment indices from Baker and Wurgler (2006) and Huang et al. (2015) to identify high and low sentiment periods.[13] We obtain the means of each index using an expanding-window approach with at least twenty-four monthly observations. A high (low) sentiment month is the one in which the value of sentiment index at the end of previous month is above (below) the estimated mean value. Next, for the long, short, and long-short sides, we re-run the baseline portfolio-level DID regressions in Table 9 over the high and low sentiment periods, respectively. Table 11 reports the coefficient of interest, β_1, for each regression.

Left panel describes the results using Baker and Wurgler (2006)'s sentiment index. For the

[13]The investor sentiment data in Baker and Wurgler (2006) is on Jeffrey Wurgler's website: `http://people.stern.nyu.edu/jwurgler/`. And the data in Huang et al. (2015) can be obtained on Guofu Zhou's website: `http://apps.olin.wustl.edu/faculty/zhou/`. Both indices end in December 2018.

long-short portfolios, the coefficient on $Treated \times JGTRRA$ is 3.195 (t-stat = 3.48) in the high sentiment periods compared with 0.909 (t-stat = 2.20) in the low sentiment periods. These results accord well with our hypotheses. For the short side, the high sentiment periods display a significantly negative coefficient on $Treated \times JGTRRA$, while the low sentiment periods accompany an insignificant β_1. After JGTRRA, when the investor sentiment is high, stocks in the short legs of the anomalies become more overpriced in the dividend record months than in the other months. In contrast, we do not observe such pattern in the low investor sentiment periods. Furthermore, we find little variation in β_1 between the high and low sentiment periods for the long side. It appears that investor sentiment plays little role in the underpriced stocks. We find qualitatively similar results using the sentiment index of Huang et al. (2015). In an unreported analysis, we also identify the high or low sentiment periods using the full-sample median value and find similar results. For the long-short portfolios, β_1 is 1.858 (t-stat =3.08) in the high sentiment periods as opposed to 0.819 (t-stat =1.69) in the low sentiment periods.

Overall, we obtain a substantial variation in the effect of the shock to short selling on the anomalies in the investor sentiment. These results also strengthen our second hypothesis that the effect mainly comes from the overpriced stocks.

6.3 Subsample

We further explore the effect of the tax-driven shock to short selling on anomalies across stocks with various degrees of limits-to-arbitrage. If our results are driven by overpricing due to the negative shock to short selling, then the effect should be stronger for stocks with higher limits-to-arbitrage.

We consider various proxies for limits-to-arbitrage including firm age, firm size, idiosyncratic volatility, size-adjusted institutional ownership and relative short interests. Stocks of young and small firms are faced with greater limits-to-arbitrage as they are more costly and difficult to arbitrage (Israel and Moskowitz, 2013). Idiosyncratic volatility could reflect risks that deter arbitrage (Stambaugh et al., 2015). Asquith et al. (2005) use institutional ownership

(IO) as a proxy for shorting supply, and relative short interest (RSI) as a measure of shorting demand. RSI is defined as short interests scaled by total number of shares outstanding. Stocks are short selling constrained when there is a strong demand to sell short and a limited supply of shares to borrow. Furthermore, Nagel (2005) shows that short-sale constraints are most likely to be binding among stocks with low size-adjusted IO. As IO is highly correlated with firm size, we follow Nagel (2005) to calculate size-adjusted IO.[14]

For each of the limits-to-arbitrage proxies except for RSI, we define high or low groups based on tercile portfolios. We use the monthly median value to define high or low RSI stocks because around one-third of our firm-month observations have missing RSI. Then we conduct the portfolio-level DID analysis for each subsample. Table 12 Panel A reports the results for firm size, firm age and idiosyncratic volatility. Consistent with our expectation, we observe substantially larger coefficients on $Treated \times JGTRRA$ in smaller and younger stocks and stocks with higher idiosyncratic volatility for the long-short portfolios. For example, the coefficient of $Treated \times JGTRRA$ is 1.510 (t-stat = 3.78) for small stocks compared with 0.833 (t-stat = 2.54) for large stocks. We also find that for the short legs of the anomalies, the DID coefficients are considerable more negative for smaller and younger stocks and stocks with higher idiosyncratic volatility. For instance, the coefficient of $Treated \times JGTRRA$ is -1.299 (t-stat = -3.06) for small stocks compared with -0.543 (t-stat = -1.35) for large stocks. For the long legs of anomalies, we do not observe a significant difference across these subsamples. Panel B reports the results for IO and RSI. We find the effect of short-sale constraints and anomalies is stronger in stocks with lower size-adjusted IO and greater RSI. The coefficient of $Treated \times JGTRRA$ is 0.988 (t-stat = 2.87) for high RSI stocks compared with 0.155 (t-stat = 0.54) for low RSI stocks.

In summary, the evidence in Table 12 provides additional support for the causal effect of short selling on anomalies.

[14]To calculate size-adjusted IO, we first obtain the logit of IO and then run cross-sectional regressions of the logit(IO) on the logarithm of firms size and squared logarithm of firms size each quarter. The residual in the regressions is the size-adjusted IO.

7 Anomaly Types

In this section, we investigate whether different types of anomalies respond differently to the shock. We categorize the 182 anomalies based on four types in Engelberg et al. (2018) and McLean and Pontiff (2016): (1) Event, (2) Market, (3) Fundamentals, and (4) Valuation. Specifically, *Event* anomalies are based on corporate events and changes in performance, such as share issues and investment growth. *Market* anomalies are constructed using only market data such as price momentum and idiosyncratic volatility. *Fundamentals* anomalies are firm accounting attributes. Finally, *Valuation* anomalies consist of accounting fundamentals scaled by market information, such as book-to-market and earnings-to-price ratios. In total, we have 52 *Event* anomalies, 62 *Market* anomalies, 51 *Fundamentals* anomalies, and 17 *Valuation* anomalies. Besides these four types of anomalies, we consider the 11 anomalies from Stambaugh et al. (2012). We label this type of anomalies as SYY.

We first construct *NOPS* separately for each of these five groups of anomalies. Next, we re-run our portfolio-level DID regressions. Table 13 presents the results separately for each anomaly type. We find consistent results for all but *Valuation* anomalies. The coefficients on $Treated \times JGTRRA$ are always significantly positive for the long-short portfolios, confirming our hypothesis that anomalies respond to JGTRRA more strongly after the dividend record months compared with the other months. Furthermore, the coefficients of $Treated \times JGTRRA$ are substantially negative for the short legs, whereas these coefficients are insignificant for the long legs. These results are consistent with our conjecture that the effect mainly comes from the overpriced stocks.

Our findings that most anomalies including event, fundamentals, and market type anomalies are likely driven by mispricing are surprising and yet important. Mispricing is often associated with behavioral biases, and thus our findings highlight the prevalence of behavioral biases in stock markets. A few fundamental anomalies such as accruals (Hirshleifer et al., 2012, 2011) and asset growth (Lam and Wei, 2011; Lipson et al., 2011) are argued to be related to mispricing in the prior literature. Additionally, Yan and Zheng (2017) find that many fundamental-based anomalies are stronger following higher sentiment periods and among stocks with greater limits to arbitrage. They argue that these anomalies are likely to be driven by mispricing rather than random chance or data mining. We provide more definite evidence supporting this argument.

We also follow Hou et al. (2017) to categorize our 182 anomalies into six types: (1) Momentum, (2) Value & Growth, (3) Profitability, (4) Investment, (5) Intangibles, and (6) Trading Frictions. The results can be found in Appendix A6. We find that our results remain strong for Momentum, Profitability, Investment and Intangibles, marginally significant for Trading Frictions, but insignificant for Value & Growth.

One possible reason could account for why the tax-driven shock to short selling does not affect *Valuation* or *Value & Growth* anomalies. These *Valuation* anomalies are mainly book-to-market ratios or anomalies that are highly correlated with book-to-market ratio such as sales-to-price, earnings-to-price and enterprise value-to-price. These anomalies could be explained by risk. The existing literature offers various risk-based explanations for the value premium. One explanation is that value stocks earn a positive premium due to a greater financial distress risk (see, e.g., Fama and French, 1995; Chen and Zhang, 1998). One strand of literature focuses on investment-based models for explaining the value premium (see, e.g., Berk et al., 1999; Gomes et al., 2003; Carlson et al., 2004; Zhang, 2005; Cooper, 2006). In the presence of investment irreversibility and countercyclical price of risk, value stocks tend to be risker than growth stocks, especially in bad times (Zhang, 2005). Another strand of literature links the value premium with consumption risk (see, e.g., Lettau and Ludvigson, 2001; Parker and Julliard, 2005; Yogo, 2006). Value stocks have higher consumption betas than growth stocks and thereby demanding a positive premium. Additionally, the value premium may reflect aggregate cash flow risk (see, e.g., Campbell and Vuolteenaho, 2004; Da and Warachka, 2009; Campbell et al., 2010). Value stocks have greater cash flow betas than growth stocks and thus earn a value premium. Finally, cash flow duration may explain the value anomaly (Lettau and Wachter, 2007). Short-horizon equity (value stocks) is more risky than long-horizon equity (growth stocks).

Overall, we demonstrate that the causal effect of short selling to mispricing is robust to various type of anomalies. Our results also suggest that *Valuation* or *Value & Growth* anomalies are unaffected by the shocks to short selling and thereby casting doubt on the mispricing explanation of the value premium (e.g., Ali et al., 2003).

8 Further Analysis

In this section, we first investigate the effect of this tax-drive shock to short selling on anomalies using only dividend-paying stocks. Second, we verify the effect of JGTRRA tax cut on the relative short interests in a DID framework. Finally, we examine whether the different days of a month for the dividend record dates affects the effect of the taxation shock on anomalies.

8.1 Dividend Stocks Only

One of the advantages of a DID regression framework is the weak requirement of exogeneity. For example, it only requires that the selection into dividend record months does not affect the evolution of anomalies over time. Since firms rarely change their dividend policy, this exogeneity condition should hold.

Nevertheless, we test the robustness of our results using only firms that issue dividends so as to have a completely matched sample. Specifically, in this sample, the treated and control observations are all from the same dividend stocks. Panel A of Table 14 reports the results. The DID coefficient, β_1, is 0.747 (t-stat = 3.04) for the long-short portfolio, -0.532 (t-stat = -2.42) for the short side, and 0.215 (t-stat = 1.41) for the long side, respectively. These results are consistent with the evidence in Table 9.

One potential issue about the exogeneity of JGTRRA is that some firms may start to pay dividends because of the dividend tax cut. For example, Chetty and Saez (2005) show an increase in dividend initiations immediately after JGTRRA for non-financial and non-utility firms. In contrast, Brav et al. (2008) argue that the tax cut merely imposes a marginal effect on a firm's dividend policy. They show that dividend initiations indeed temporary spike after the act, but then return to pre-JGTRRA levels.[15] In addition to the low number of tax cut induced dividend initiations, this one-time change should not affect the evolution of anomalies over time, and thus it does not violate the weak exogeneity requirement of the DID analysis. Nevertheless, further excluding these stocks creates an interesting setting resembling a controlled experiment – identical samples are randomly chosen either to be the treated (dividend record months) or the control (other months) before the shock.[16] We repeat the above analysis by keeping only stocks that pay dividends before JGTRRA. We assume that it takes three months for a firm to change its dividend policy after JGTRRA. Thus, we drop stocks that initiated dividends after September 2003. Panel B of Table 14 reports the DID results. We observe virtually the same results: β_1 is 0.517 (t-stat = 2.01) for the long-short portfolio and -0.514 (t-stat = -2.54) for the short side while it is only 0.03 (t-stat = 0.02) for the long side. It is worth noting that β_0, the coefficient of the *treated* on the short side is significant in either panels, contrary to the significant coefficients in Table 9, confirming that the treated and the control behave the same before the act.[17]

Overall, the results in this subsection further strengthen the causal effect of the differential tax-driven shock to short selling on mispricing and anomalies.

8.2 Dividend tax cut and short selling

Our results are built on the evidence that the dividend tax cut imposes a negative shock to short selling documented in the previous studies (Thornock, 2013; Dixon et al., 2019; Blocher et al., 2013). As a robustness check, in this subsection, we validate the effect of the dividend taxation on short selling using the DID panel regression framework. As a comparison, we also study the effect of Reg SHO on short selling using the pilot and non-pilot stocks listed on NYSE/AMEX exchanges in Reg SHO program.We run the following two DID panel re-

[15]They find that between 2002 and 2005, 76 out of 265 firms initiated dividends after the act. Only a few firms occasionally mention the dividend tax cut as the reason for their initiations in their press releases. In our sample, we observe an increase of 66 dividend stocks to a total of 1555 from 2003Q3 to 2004Q3.

[16]Despite the above stated benefits of using this sample, there are also potential issues such as survivorship bias and too few observations. Dividend stocks only account for about 14% of the stocks in our sample.

[17]The reversal after the dividend record month (Hartzmark and Solomon, 2013) may account for the negative β_0 coefficient for the long sides in either panels.

gressions:

$$RSI_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t} + b_2 DivR_{i,t} \times JGTRRA_t + X^T c + \varepsilon_{i,t}, \tag{8}$$

$$RSI_{i,t} = \alpha_0 + \alpha_t + \alpha_i + c_2 Pilot_i \times During_t + X^T c + \varepsilon_{i,t}, \tag{9}$$

where $RSI_{i,t}$ is the relative short interests for firm i at month t. Control variables include firm size, contemporaneous return, idiosyncratic volatility, momentum, earnings momentum, and ROE. We add both firm and month fixed effects and cluster standard errors on both firm and month. In the regression related to Reg SHO, the $During$ term is subsumed by the time fixed effect.

The coefficient on the interaction term, b_2, captures the different changes of the relative short interests in response to the enactment of JGTRRA of 2003 between the dividend record months and the non-dividend record months. In contrast, b_1 captures the difference in the relative short interest between the dividend record months and the non-dividend record months before JGTRRA. If the dividend taxation effect on short selling is true, we would expect that the dividend record months should experience more negative changes in the relative short interest in response to the law and thus b_2 to be significantly negative. Likewise, c_2 captures the different responses of the pilot and non-pilot stocks in terms of the relative short interests during the enactment of Reg SHO. If Reg SHO affects shorting activities, we would expect relatively more short selling on the pilot stocks and thus c_2 to be significantly positive.

Table 15 reports our regression results. The first two columns display the results for the dividend taxation effect and the last two columns report the results for the Reg SHO. Consistent with our prediction, we observe a significantly negative b_2 coefficient. For example, after controlling for various firm characteristics, b_2 is -0.117 (t-stat = -2.80). This result indicates that JGTRRA has a more negative impact on the relative short interests in the dividend record months than in the non-dividend record months. It is also worth noting that b_1 is insignificant at 0.033 (t-stat = 1.25), indicating that before JGTRRA, there is no significant difference in the relative short interest between the dividend months and the non-dividend months. In contrast, we observe an insignificant c_2 on the interaction term, $Pilot \times During$, in Panel B. This result suggests that although Reg SHO relaxes the short-sale constraints, it does not significantly affect the short selling activities. Overall, our results provide additional evidence for the adverse effect of the dividend taxation on short selling.

8.3 Do different dividend record dates in a month matter?

In this subsection, we explore whether it can impact the effect of this tax-driven shock to short selling on anomalies that the dividend record date falls onto different days of a month. Figure 1 shows that the distribution of the record dates within a dividend record month is almost symmetric and is concentrated around Days 15, 1, 10, and 30. It can be argued that short sellers could start to increase their short selling activities several days after the dividend record dates to compensate for the low short-selling activities during the record dates. Consequently, the effect of the dividend tax cut on short selling may be weaker for stocks with the dividend record dates early in the month than for stocks with late dividend record dates.

To test the above conjecture, we re-produce our panel DID regressions by separating the dividend record months into two subperiods. Specifically, we construct two dummy variables, $DivR1$ and $DivR2$, based on the calendar day of the dividend record months, and $DivR1_{i,t-1}$ ($DivR2_{i,t-1}$) is a dummy variable that equals one if stock i reports a dividend record date in the first (second) half of month $t-1$ and zero otherwise. We then run the following regression,

$$\begin{aligned} ret_{i,t} = {} & \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR1_{i,t-1} + b_3 DivR2_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR1_{i,t-1} \\ & + b_5 NOPS_{i,t-1} \times DivR2_{i,t-1} + b_6 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_7 DivR1_{i,t-1} \times JGTRRA_{t-1} \\ & + b_8 DivR2_{i,t-1} \times JGTRRA_{t-1} + b_9 NOPS_{i,t-1} \times DivR1_{i,t-1} \times JGTRRA_{t-1} \\ & + b_{10} NOPS_{i,t-1} \times DivR2_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}. \end{aligned} \tag{10}$$

As there is a substantial number of stocks recording dividends on the 15th day of a month, we consider three different definitions of the dummy variables. We set $DivR2$ ($DivR1$) equals to one for stocks with the 15th day of dividend record months in the first (second) regression. Additionally, we run a third regression after excluding stocks with the dividend record dates occurring on the 15th day in a month. Table 16 reports our results. We find that the coefficients on the three-way interaction terms are always statistically significantly negative in all of these three columns. These results suggest that it does not matter which day of a month that a dividend record date falls on for the impact of the dividend taxation shock to short selling on anomalies.

9 Conclusions

Despite numerous studies on anomalies in the asset pricing literature, academics still disagree about their causes. In this study, we investigate the causal effect of short selling on mispricing and anomalies using a robust and plausibly exogenous shock to short selling in the dividend record months after the JGTRRA of 2003.

Our study lends support to the explanation that anomalies reflect mispricing. Using arguably the most comprehensive set of 355 anomalies and the DID regression frameworks, we find that mispricing become stronger in dividend record months after the JGTRRA of 2003, and as a result, anomalies are stronger after dividend record months. As expected, we show that the effect mainly comes from the overpriced stocks. Our findings are robust after controlling for various risk factors, and our various falsification tests indicate that data-mining is unlikely to drive our results.

We further divide anomalies into four types and examine each type separately. We find that the effect of the shock is significant in all but valuation anomalies, suggesting that most anomalies are driven by mispricing while valuation anomalies are likely driven by risks. Furthermore, taking advantage of the unique setting of the shock, we consider dividend stocks only so that the treated and control are the same firms and find virtually the same results.

Finally, we demonstrate that our results are stronger during high investor sentiment periods when overpricing is more prevalent and in more short-sale constrained stocks. Taken together, our analysis offers a novel test of the *causal* effect of short selling on mispricing and anomalies, and provides strong evidence that most anomalies reflect mispricing while a few valuation anomalies may reflect risk.

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Table 1: Summary statistics

Panel A describes our sample in terms of dividend record dates. To be included in our sample, a stock must have a price at least \$5. We define $DivR_{i,t-1}$ as an indicator variable which is equal to one if a stock i satisfies the following three conditions: 1) it reports a dividend record date in month $t-1$; 2) the dividend distributed in that month is ordinary taxable cash dividends (CRSP distcd = 1232); 3) the dividend is at least \$0.01 per share. We obtain the stock price, return, and dividend record dates from CRSP. The sample period is 1985:7 to 2019:12.

Panel B provides descriptive statistics for the aggregated misprcing measure which is the average of those at the stock level. The *net overpiced score (NOPS)* for each stock is defined as *NShort - NLong*, where *NShort* and *NLong* are the total numbers the stock is in the *decile* short-legs or long-legs of 182 anomalies, respectively. The sample period is 1985:7 to 2019:12.

Panel A: Firm-month observations with dividend record dates

	DivR = 1	*DivR* = 0	Total
# of firm-month observations	225,631	1,362,850	1,588,481
Percentage	14.20%	85.80%	100%

Panel B: Summary statistics of NOPS

	Mean	Std.Dev	Min	p25	p50	p75	Max
NShort	12.54	8.94	0	6	10	16	77
NLong	14.83	7.88	0	9	14	19	66
NOPS	-2.29	10.12	-61	-8	-2	3	63

Table 2: Difference-in-differences results

This table reports results from our main difference-in-differences regression,

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1}$$
$$+ b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t},$$

where monthly return, the dependent variable, is multiplied by 100. $JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Firm & Month	Firm & Month
S.E. Clusters	Month	Firm & Month	Month	Firm & Month
NOPS	-0.095***	-0.095***	-0.100***	-0.100***
	(-9.02)	(-9.00)	(-11.07)	(-11.12)
DivR	0.077	0.077	-0.273***	-0.273***
	(0.37)	(0.37)	(-2.78)	(-2.77)
NOPS×*DivR*	0.046***	0.046***	0.031***	0.031***
	(4.55)	(4.55)	(4.76)	(4.76)
NOPS×*JGTRRA*	0.055***	0.055***	0.062***	0.062***
	(4.56)	(4.55)	(5.76)	(5.79)
DivR×*JGTRRA*	-0.148	-0.148	0.081	0.081
	(-0.62)	(-0.62)	(0.43)	(0.43)
NOPS×*DivR*×*JGTRRA*	-0.035***	-0.035***	-0.028***	-0.028***
	(-2.83)	(-2.82)	(-2.89)	(-2.89)

Table 3: DiD results after Regulation SHO

This table provides DID results for testing whether the effect of short-selling supply on anomalies is robust after the Reg SHO. We run the following regression:

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}.$$

$JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Firm & Month	Firm & Month
S.E. Clusters	Month	Firm & Month	Month	Firm & Month
NOPS	-0.089***	-0.089***	-0.098***	-0.098***
	(-12.88)	(-12.86)	(-17.22)	(-17.26)
DivR	-0.056	-0.056	-0.300***	-0.300***
	(-0.52)	(-0.52)	(-6.32)	(-6.19)
NOPS×*DivR*	0.031***	0.031***	0.021***	0.021***
	(5.33)	(5.29)	(4.86)	(4.82)
NOPS×*JGTRRA*	0.052***	0.052***	0.071***	0.071***
	(5.12)	(5.11)	(8.69)	(8.72)
DivR×*JGTRRA*	0.063	0.063	0.140	0.140
	(0.35)	(0.35)	(0.99)	(0.99)
NOPS×*DivR*×*JGTRRA*	-0.019*	-0.019*	-0.022***	-0.022***
	(-1.87)	(-1.86)	(-2.66)	(-2.64)

Table 4: Regulation SHO Pilot Program as an alternative exogenous shock

In this table, we investigate whether the effect of Regulation SHO on mispricing hold using NOPS. The regression is as follows:

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 Pilot_i \times During_t + b_3 NOPS_{i,t-1} \times Pilot_i + b_4 NOPS_{i,t-1} \times During_t$$
$$+ b_5 NOPS_{i,t-1} \times Pilot_i \times During_t + \varepsilon_{i,t}.$$

$Pilot_i$ is a dummy variable which is equal to one if stock i is a pilot stock, and zero otherwise. $During_t$ is a dummy variable, which equals one if month t is between July 2005 and June 2007 (i.e., during the pilot period of Regulation SHO). t-statistics are presented in parentheses. The sample period is 1985:7 to 2007:7. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Firm & Month	Firm & Month
S.E. Clusters	Month	Firm & Month	Month	Firm & Month
NOPS	-0.043***	-0.043***	-0.060***	-0.060***
	(-6.45)	(-6.25)	(-9.10)	(-7.22)
Pilot	0.045	0.045		
	(0.86)	(0.93)		
Pilot×*During*	0.212**	0.212**	0.248**	0.248*
	(2.36)	(2.04)	(2.15)	(1.76)
NOPS×*Pilot*	0.003	0.003	0.005	0.005
	(0.47)	(0.47)	(0.71)	(0.69)
NOPS×*During*	0.009	0.009	0.009	0.009
	(0.65)	(0.64)	(0.65)	(0.62)
NOPS×*Pilot*×*During*	0.019	0.019	0.024	0.024
	(1.38)	(1.29)	(1.52)	(1.30)

Table 5: Overpricing from the tax-driven shock to short selling

This table tests whether the mispricing effect comes from the long or short side. We create two dummy variables, *High NOPS* and *Low NOPS* , based on the decile rank of *NOPS* each month. *High NOPS* and *Low NOPS* represent stocks concentrated in the short and long sides, respectively.

$$\begin{aligned} ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t-1} + b_2 DivR_{i,t-1} \times JGTRRA_{t-1} + b_3 NOPS_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR_{i,t-1} \\ + b_5 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_6 Low\ NOPS_{i,t-1} + b_7 High\ NOPS_{i,t-1} + b_8 Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \\ + b_9 Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} + b_{10} High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{11} High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\ + b_{12} Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{13} High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}. \end{aligned}$$

Monthly return is multiplied by 100. $DivR_{i,t-1}$ is a dummy variable that equals one if stock i records dividends in month $t-1$ and zero otherwise. $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003. We add firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

DivR	-0.325***	-0.366***	-0.358***
	(-3.54)	(-4.01)	(-3.88)
DivR×JGTRRA	0.186	0.229	0.217
	(1.09)	(1.40)	(1.29)
NOPS	-0.108***	-0.090***	-0.100***
	(-9.68)	(-11.44)	(-9.07)
NOPS×DivR	0.023***	0.014***	0.018***
	(4.21)	(3.31)	(3.25)
NOPS×JGTRRA	0.065***	0.053***	0.061***
	(5.11)	(6.03)	(5.02)
Low NOPS	-0.455***		-0.344**
	(-3.19)		(-2.51)
High NOPS		-0.496***	-0.348***
		(-4.04)	(-3.31)
Low NOPS×DivR	0.159		0.088
	(1.27)		(0.73)
Low NOPS×JGTRRA	0.316*		0.260
	(1.77)		(1.56)
High NOPS×DivR		0.693***	0.608***
		(3.35)	(2.99)
High NOPS×JGTRRA		0.345**	0.219
		(1.99)	(1.51)
Low NOPS×DivR×JGTRRA	0.198		0.168
	(1.27)		(1.09)
High NOPS×DivR×JGTRRA		-0.854**	-0.827**
		(-2.49)	(-2.43)

Table 6: Controlling for dynamic risk factors

This table investigates whether the effect of short-selling supply on anomalies comes from the short legs after controlling for dynamic risk factors. We consider six factors including market systematic risk which is the CRSP value-weighted index returns, and the five macroeconomic risk factors from Chen et al. (1986): the growth rate of industrial production (MP), unexpected inflation (UI), change in expected inflation (DEI), term premium (UTS), and default premium (UPR). We interact each source of risk with the *High* or *Low NOPS*, the $JGTRRA$, the $DivR$, and also include four-way interactions between each of the *High* and *Low NOPS*, the $JGTRRA$, the $DivR$, and each source of risk. We run the following regressions,

$$\begin{aligned}
ret_{i,t} = {} & \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t-1} + b_2 DivR_{i,t-1} \times JGTRRA_{t-1} + b_3 NOPS_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR_{i,t-1} \\
& + b_5 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_6 Low\ NOPS_{i,t-1} + b_7 High\ NOPS_{i,t-1} + b_8 Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \\
& + b_9 Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} + b_{10} High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{11} High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{12} Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{13} High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} \\
& + \ addtional\ interaction\ terms\ with\ risk\ factors + \varepsilon_{i,t}.
\end{aligned}$$

We add firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Market	Macroeconomic Risk Factors					All Factors
		MP	UI	DEI	UTS	UPR	
DivR	-0.083 (-0.78)	-0.434*** (-3.68)	-0.333*** (-3.52)	-0.367*** (-4.02)	-0.364*** (-4.00)	-0.348*** (-3.78)	-0.067 (-0.52)
DivR×JGTRRA	0.090 (0.58)	0.284 (1.54)	0.185 (1.08)	0.216 (1.27)	0.215 (1.27)	0.189 (1.14)	0.037 (0.21)
NOPS	-0.098*** (-9.46)	-0.100*** (-9.25)	-0.100*** (-9.10)	-0.100*** (-9.14)	-0.100*** (-9.25)	-0.099*** (-9.09)	-0.099*** (-9.85)
NOPS×DivR	0.019*** (3.43)	0.019*** (3.38)	0.019*** (3.40)	0.019*** (3.46)	0.019*** (3.45)	0.019*** (3.35)	0.021*** (3.75)
NOPS×JGTRRA	0.059*** (5.13)	0.060*** (4.99)	0.058*** (4.78)	0.058*** (4.76)	0.059*** (4.87)	0.059*** (4.90)	0.056*** (4.99)
Low NOPS	-0.226 (-1.61)	-0.389*** (-2.94)	-0.338** (-2.48)	-0.347** (-2.54)	-0.354*** (-2.65)	-0.339** (-2.48)	-0.220* (-1.66)
High NOPS	-0.857*** (-4.08)	-0.255 (-1.29)	-0.351*** (-3.26)	-0.345*** (-3.28)	-0.333*** (-2.99)	-0.352*** (-3.24)	-1.051*** (-4.44)
Low NOPS×RecMon	-0.044 (-0.36)	0.128 (0.98)	0.077 (0.65)	0.120 (0.99)	0.117 (0.96)	0.091 (0.74)	-0.038 (-0.28)
Low NOPS×JGTRRA	0.176 (1.01)	0.286* (1.75)	0.257 (1.53)	0.264 (1.57)	0.274* (1.65)	0.256 (1.53)	0.147 (0.86)
High NOPS×DivR	0.743*** (3.33)	0.634** (2.59)	0.615*** (3.02)	0.597*** (2.97)	0.583*** (2.88)	0.595*** (2.89)	0.954*** (3.51)
High NOPS×JGTRRA	0.484** (2.07)	0.177 (0.79)	0.218 (1.42)	0.223 (1.48)	0.224 (1.46)	0.218 (1.41)	0.783*** (2.98)
Low NOPS×DivR×JGTRRA	0.288* (1.85)	0.158 (0.94)	0.178 (1.15)	0.134 (0.86)	0.140 (0.90)	0.174 (1.12)	0.309* (1.79)
High NOPS×DivR×JGTRRA	-0.958*** (-2.68)	-1.019*** (-2.71)	-0.908*** (-2.62)	-0.892** (-2.57)	-0.863** (-2.57)	-0.848** (-2.46)	-1.393*** (-3.70)

Table 7: Placebo tests with pseudo JGTRRA

This table reports various placebo tests with changing the timing of tax code change. We use the timing before and after 2003 for pseudo enactment of JGTRRA including July of 1997 and 2013, and January of 1999 and 2005. Consequently, we use two sample periods: (1) between 1985:7 and 2003:5; (2) between 2003:6 and 2019:12. We consider $PseudoJGTRRA_t$, a dummy variable which equals to one if month t is after the pseudo date specified, and zero otherwise. The regression is as follows:

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times PseudoJGTRRA_{t-1} + b_5 DivR_{i,t-1} \times PseudoJGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times PseudoJGTRRA_{t-1} + \varepsilon_{i,t}.$$

We add firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are presented in parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	1985:7 - 2003:5		*2003:6 - 2019:12*	
	1997:7	*1999:1*	*2005:1*	*2013:7*
NOPS	-0.090***	-0.093***	-0.019	-0.034***
	(-12.48)	(-12.04)	(-1.19)	(-5.22)
DivR	-0.334***	-0.291**	-0.581*	-0.387***
	(-2.69)	(-2.35)	(-1.80)	(-4.38)
NOPS×DivR	0.013**	0.016**	-0.015	-0.000
	(2.10)	(2.41)	(-0.77)	(-0.03)
NOPS×PseudoJGTRRA	-0.050**	-0.058**	-0.013	0.011
	(-2.52)	(-2.30)	(-0.77)	(1.04)
DivR×PseudoJGTRRA	0.322	0.248	0.410	0.463***
	(0.87)	(0.47)	(1.13)	(2.76)
NOPS×DivR×PseudoJGTRRA	0.048***	0.055***	0.021	0.013
	(3.03)	(2.77)	(1.00)	(1.22)

Table 8: Placebo tests with pseudo dividend record months

This table reports placebo tests with pseudo dividend record dates. We consider two testing samples: (1) stocks without recording dividends over the previous months; (2) non-dividend-paying stocks. For each sample, in each month, we randomly choose 14% of firm-months observations (based on summary statistics in Table 1) to be stocks with dividend record dates over the previous month. $PseudoDivR_{i,t-1}$ is a dummy variable that equals one if stock i has a dividend record date in month $t-1$, and zero otherwise. *Simulation 1* and *Simulation 2* use two different simulation seeds. Then we run the following DID regression:

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 PseudoDivR_{i,t-1} + b_3 NOPS_{i,t-1} \times PseudoDivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_5 PseudoDivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times PseudoDivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}.$$

We add firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are presented in parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	***Drop if DivR* = 1**		***Drop Dividend Stocks***	
	Simulation 1	*Simulation 2*	*Simulation 1*	*Simulation 2*
NOPS	-0.051***	-0.053***	-0.113***	-0.113***
	(-8.66)	(-8.97)	(-11.58)	(-11.38)
PseudoDivR	-0.027	0.063	0.109	-0.080
	(-0.35)	(0.88)	(1.33)	(-1.13)
NOPS×*PseudoDivR*	-0.006	0.008	0.002	-0.001
	(-0.65)	(0.84)	(0.26)	(-0.14)
NOPS×*JGTRRA*	0.022**	0.022**	0.074***	0.073***
	(2.32)	(2.32)	(6.26)	(6.19)
PseudoDivR×*JGTRRA*	0.010	0.094	-0.092	0.184**
	(0.09)	(0.86)	(-0.89)	(1.97)
NOPS×*PseudoDivR*×*JGTRRA*	0.007	0.007	0.002	0.004
	(0.46)	(0.48)	(0.29)	(0.43)

Table 9: Portfolio-level DiD

This table reports the portfolio-level DID results. We run portfolio-level DiD regression in the following specification. For stocks that report dividend record dates in previous month, we sort them into ten decile portfolios based on NOPS and then compute the monthly equal-weighted portfolio returns for the long, short, and long-short portfolios. Then we repeat the procedure for stocks that do not report dividend record dates in previous month. Next, we run the following regression:

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{t-1} + \beta_1 Treated_{t-1} \times JGTRRA_{t-1} + \textit{ addtional interaction terms with risk factors} + \epsilon_{i,t},$$

where dependent variable, $y_{i,t}$, is the equally-weighted monthly return of a portfolio, which can be the long side, short side, and the long-short portfolio, in month t. $Treated_{t-1}$ is a dummy variable which is equal to one if the portfolio is formed on stocks whose $DivR_{i,t-1} = 1$. $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003. We add the time fixed effect and report the Robust t-statistics in the parentheses. We also incorporate the dynamic risk framework as we did in Table 6. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Long-Short	*Long Side*	*Short Side*	*Long-Short*	*Long Side*	*Short Side*
	Baseline DiD			***Dynamic Risk DiD***		
Treated	-2.083***	-0.545***	1.539***	-2.771***	-0.403***	2.368***
	(-6.21)	(-3.67)	(3.73)	(-7.15)	(-2.75)	(5.30)
Treated$\times$*JGTRRA*	1.677***	0.393**	-1.284***	2.443***	0.306	-2.137***
	(4.04)	(2.12)	(-2.68)	(5.11)	(1.65)	(-4.09)
Constant	2.351***	1.476***	-0.875***	2.408***	1.509***	-0.900***
	(15.82)	(22.23)	(-5.06)	(17.41)	(23.48)	(-5.76)

Table 10: Portfolio-level DiD Placebo tests

This table reports the first portfolio-level placebo test results with changing the timing of tax code change. We consider the same two testing periods and *PseudoEvents* as in Table 7.

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{t-1} + \beta_1 Treated_{t-1} \times PseudoEvent_{t-1} + \epsilon_{i,t},$$

$Treated_{t-1}$ is a dummy variable which is equal to one if the portfolio is formed on stocks whose $DivR_{i,t-1} = 1$. We add the time fixed effect and report the Robust t-statistics in the parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Long-Short	*Long Side*	*Short Side*	*Long-Short*	*Long Side*	*Short Side*
Panel A: Testing sample 1985:07 - 2003:05						
		1997:7			***1999:1***	
Treated	-1.600***	-0.405***	1.195***	-1.639***	-0.402***	1.237***
	(-7.19)	(-2.91)	(4.53)	(-7.56)	(-2.96)	(4.80)
Treated×*PseudoEvent*	-1.496	-0.397	1.099	-1.846	-0.545	1.300
	(-1.60)	(-1.05)	(0.95)	(-1.54)	(-1.19)	(0.87)
Constant	3.270***	1.714***	-1.556***	3.270***	1.714***	-1.556***
	(13.83)	(16.28)	(-5.33)	(13.87)	(16.32)	(-5.33)
Panel B: Testing sample 2003:06 - 2019:12						
		2005:1			***2013:7***	
Treated	-0.632	-0.493	0.139	-0.223	-0.263*	-0.039
	(-0.89)	(-1.01)	(0.19)	(-0.80)	(-1.73)	(-0.14)
Treated×*PseudoEvent*	0.249	0.375	0.126	-0.464	0.281	0.745
	(0.33)	(0.75)	(0.16)	(-0.88)	(1.29)	(1.42)
Constant	1.360***	1.202***	-0.158	1.360***	1.202***	-0.158
	(7.88)	(15.34)	(-0.91)	(7.90)	(15.36)	(-0.92)

Table 11: Investor sentiment

In this table we use two orthogonalized investor sentiment indices from Baker and Wurgler (2006) and Huang et al. (2015) to identify high or low sentiment periods. We first obtain the mean of each investor sentiment index using a recursive-window with at least twenty-four monthly observations. A high-sentiment month is the one in which the value of sentiment index at the end of previous month is above the mean value in the recursive-window, or vice versa. We re-run the DiD regression for two subperiods, respectively, and report the main DiD coefficient, β_1.

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{t-1} + \beta_1 Treated_{t-1} \times JGTRRA_{t-1} + \epsilon_{i,t}.$$

$Treated_{t-1}$ is a dummy variable which is equal to one if the portfolio is formed on stocks whose $DivR_{i,t-1} = 1$; $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003 (after the JGTRRA of 2003). We add the time fixed effect and report the Robust t-statistics in the parentheses. The sample period ends in 2018:12 due to the availability of sentiment indices. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Baker and Wurgler (2006)		*Huang et al. (2015)*	
	High Sent	*Low Sent*	*High Sent*	*Low Sent*
β_1 (*Long-Short*)	3.195***	0.909**	3.424***	0.795**
	(3.48)	(2.20)	(3.45)	(2.14)
β_1 (*Long Side*)	0.303	0.402*	0.860*	0.174
	(0.80)	(1.90)	(1.92)	(0.92)
β_1 (*Short Side*)	-2.892***	-0.507	-2.565**	-0.621
	(-2.77)	(-1.12)	(-2.14)	(-1.64)

Table 12: Subsamples of Limits-to-arbitrage proxies

In this table, we divide the entire sample into subgroups based on idiosyncratic volatility ($IVOL$), firm age, firm size, size-adjusted institutional ownership (IO) and relative short interest (RSI). For each of short-sale constraint proxies except for RSI, we define high or low groups based on tercile portfolios. We use the monthly median value to define high or low RSI stocks because around one-third of our firm-month observations have missing RSI. For each subsample, we re-run our portfolio-level DiD regression:

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{t-1} + \beta_1 Treated_{t-1} \times JGTRRA_{t-1} + \epsilon_{i,t}.$$

In which $Treated_{t-1}$ is a dummy variable which is equal to one if the portfolio is formed on stocks whose $DivR_{i,t-1} = 1$; $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003 (after the JGTRRA of 2003). We add the time fixed effect and report the Robust t-statistics in the parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Firm size, firm age, and idiosyncratic volatility

	Large Firm	*Small Firm*	*Mature Firm*	*Young Firm*	*High IVOL*	*Low IVOL*
β_1 (*Long-Short*)	0.833**	1.510***	0.729***	1.840***	1.594***	0.502***
	(2.54)	(3.78)	(3.36)	(3.48)	(2.86)	(2.80)
β_1 (*Long Side*)	0.289	0.211	0.231	0.381	0.520	0.143
	(1.55)	(0.96)	(1.38)	(1.30)	(1.47)	(1.14)
β_1 (*Short Side*)	-0.543	-1.299***	-0.497**	-1.459**	-1.073*	-0.359**
	(-1.35)	(-3.06)	(-2.09)	(-2.50)	(-1.86)	(-2.14)

Panel B: Institutional ownership and short interest

	High IO	*Low IO*	*High RSI*	*Low RSI*
β_1 (*Long-Short*)	1.044***	1.351***	0.988***	0.155
	(2.96)	(3.28)	(2.87)	(0.54)
β_1 (*Long Side*)	0.241	0.380*	0.296	0.125
	(1.09)	(1.67)	(1.54)	(0.74)
β_1 (*Short Side*)	-0.803**	-0.972*	-0.692*	-0.030
	(-2.19)	(-1.86)	(-1.90)	(-0.11)

Table 13: Types of anomalies

We split our 182 significant anomalies into the four groups based on Engelberg et al. (2018) and McLean and Pontiff (2016): (1) *Event*, (2) *Market*, (3) *Fundamentals*, and (4) *Valuation*. We separately compute *NOPS* for each of these four types of anomalies. We also consider *NOPS* constructed from 11 anomalies studied in Stambaugh et al. (2012). We labeled it as *NOPS SYY*. We then re-run our portfolio-level DiD regression for each of five misprcing measures,

$$y_{j,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{j,t-1} + \beta_1 Treated_{j,t-1} \times JGTRRA_{t-1} + \epsilon_{j,t}.$$

where $Treated_{j,t-1}$ is a dummy variable which is equal to one if the portfolio j is formed on stocks whose $DivR_{i,t-1} = 1$ sorted by type j of *NOPS*; $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003. We add the time fixed effect and report the Robust t-statistics in the parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

NOPS by Type:	*SYY*	*Event*	*Market*	*Fundamentals*	*Valuation*
	11	52	62	51	17
Panel A: Long-Short					
Treated	-1.589***	-1.085***	-1.964***	-1.924***	-0.693
	(-6.29)	(-5.36)	(-6.00)	(-6.14)	(-1.56)
Treated×*JGTRRA*	1.258***	1.092***	1.064***	1.440***	0.296
	(3.89)	(4.13)	(2.81)	(3.60)	(0.55)
Constant	1.617***	1.128***	2.194***	1.605***	1.206***
	(14.00)	(11.97)	(16.03)	(11.23)	(6.26)
Panel B: Long Side					
Treated	-0.459**	-0.215	-0.423***	-0.498***	-0.110
	(-2.44)	(-1.01)	(-2.60)	(-2.69)	(-1.03)
Treated×*JGTRRA*	0.342	0.221	0.024	0.356	0.001
	(1.60)	(0.88)	(0.12)	(1.62)	(0.01)
Constant	1.133***	1.057***	1.327***	1.215***	1.095***
	(14.62)	(11.68)	(18.62)	(15.37)	(18.23)
Panel C: Short Side					
Treated	1.130***	0.870***	1.541***	1.427***	0.583
	(3.33)	(3.13)	(3.85)	(3.38)	(1.19)
Treated×*JGTRRA*	-0.916**	-0.871***	-1.040**	-1.085**	-0.295
	(-2.26)	(-2.64)	(-2.31)	(-2.17)	(-0.52)
Constant	-0.483***	-0.072	-0.866***	-0.390**	-0.111
	(-3.31)	(-0.60)	(-5.31)	(-2.17)	(-0.54)

Table 14: Dividend stocks only

This table investigate the effect of short-selling supply on anomalies for dividend-paying stocks. In Panel A, we exclude non-dividend-paying stocks during the entire sample period. In Panel B, we exclude both non-dividend-paying stocks and stocks that initiated dividends after September, 2003. We choose September of 2003 because we assume that it might take three months for a firm to react to the act. We re-run our portfolio-level DiD regression.

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{i,t-1} + \beta_1 Treated_{i,t-1} \times JGTRRA_{t-1} + \epsilon_{i,t}.$$

$Treated_{i,t-1}$ is a dummy variable which is equal to one if the portfolio i is formed on stocks whose $DivR_{i,t-1} = 1$; $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003. We add the time fixed effect and report the Robust t-statistics in the parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dividend-paying stocks only			
	Long-Short	*Long Side*	*Short Side*
Treated	-0.710***	-0.492***	0.218
	(-3.99)	(-4.63)	(1.25)
Treated×JGTRRA	0.747***	0.215	-0.532**
	(3.04)	(1.41)	(-2.42)
Constant	1.423***	1.509***	0.085
	(16.32)	(27.94)	(1.08)
Panel B: Dividend-paying stocks before JGTRRA only			
Treated	-0.470***	-0.348***	0.122
	(-3.05)	(-3.62)	(0.97)
Treated×JGTRRA	0.517**	0.003	-0.514**
	(2.01)	(0.02)	(-2.54)
Constant	1.309***	1.430***	0.121*
	(14.52)	(25.69)	(1.71)

Table 15: Dividend tax cut and Reg SHO on relative short interests

This table compares the effect of JGTRRA dividend tax cut and Reg SHO on the relative short interests. Panel A reports the DID regression results for the effect of JGTRRA dividend tax cut on relative short interests. We run the following regression:

$$RSI_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t} + b_2 DivR_{i,t} \times JGTRRA_t + X^T c + \varepsilon_{i,t},$$

where $DivR_{i,t}$ is a dummy variable that equals one if stock i reports a dividend record date in month t and zero otherwise. $JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). The sample period is 1985:7 to 2019:12.

Panel B reports the DID regression results for the effect of Reg SHO on the relative short interests. We run the following regression:

$$RSI_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_2 Pilot_i \times During_t + X^T c + \varepsilon_{i,t},$$

where $Pilot_i$ is a dummy variable which is equal to one if stock i is a pilot stock, and zero otherwise. $During_t$ is a dummy variable, which equals one if month t is between July 2005 and June 2007 (i.e., during the pilot period of Regulation SHO). The sample period is 1985:7 to 2007:6. We add both firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are presented in parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: Taxation Shock		***Panel B: Reg SHO***	
	All	Dividend-paying	All	No NASDAQ
DivR	0.033	0.040*		
	(1.25) (1.81)			
DivR×JGTRRA	-0.117***	-0.093***		
	(-2.80)	(-2.74)		
Pilot×During			0.096	-0.275
			(0.521)	(-1.301)
Firm Size	-0.133**	-0.453***	-0.716***	-1.105***
	(-2.08)	(-6.51)	(-13.788)	(-15.399)
Return	-0.978***	-0.237	-1.610***	-0.877*
	(-6.94)	(-1.42)	(-4.686)	(-1.779)
IVOL	0.298***	0.313***	1.303***	1.085***
	(16.78)	(12.69)	(16.684)	(10.171)
Momentum	-0.543***	-0.343***	-0.538***	-0.548**
	(-11.16)	(-6.04)	(-2.779)	(-2.068)
SUE	-0.032***	-0.030***	-0.094***	-0.138***
	(-4.96)	(-4.18)	(-2.947)	(-3.809)
ROE	-0.643***	-0.690*	-1.679	0.619
	(-3.15)	(-1.70)	(-1.425)	(0.411)

Table 16: DID results with different dividend record dates of the months

This table explores whether the days of dividend record months matter in the effect of the tax-driven shock to short selling and anomalies. We run the following regression,

$$\begin{aligned} ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR1_{i,t-1} + b_3 DivR2_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR1_{i,t-1} \\ + b_5 NOPS_{i,t-1} \times DivR2_{i,t-1} + b_6 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_7 DivR1_{i,t-1} \times JGTRRA_{t-1} \\ + b_8 DivR2_{i,t-1} \times JGTRRA_{t-1} + b_9 NOPS_{i,t-1} \times DivR1_{i,t-1} \times JGTRRA_{t-1} \\ + b_{10} NOPS_{i,t-1} \times DivR2_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}, \end{aligned}$$

where $DivR1_{i,t-1}$ ($DivR2_{i,t-1}$) is a dummy variable that equals one if stock i reports a dividend record date in the first (second) half of month $t-1$ and zero otherwise. We set $DivR2$ ($DivR1$) equals to one for stocks with the dividend record dates on the 15th day of the months in the first (second) regression. In the third column, we drop stocks reporting the dividend record dates on the 15th day of the months. We add both firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are presented in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Day 15 in 2nd-half	Day 15 in 1st-half	Drop Day 15
NOPS	-0.100***	-0.100***	-0.100***
	(-11.12)	(-11.12)	(-11.12)
DivR1	-0.359***	-0.327***	-0.354***
	(-3.41)	(-3.06)	(-3.42)
DivR2	-0.190*	-0.197*	-0.191*
	(-1.79)	(-1.93)	(-1.91)
NOPS×DivR1	0.030***	0.031***	0.029***
	(4.00)	(4.37)	(3.96)
NOPS×DivR2	0.032***	0.031***	0.031***
	(4.30)	(3.98)	(3.95)
NOPS×JGTRRA	0.061***	0.061***	0.061***
	(5.79)	(5.79)	(5.77)
DivR1×JGTRRA	0.109	0.077	0.110
	(0.55)	(0.39)	(0.56)
DivR2×JGTRRA	0.059	0.097	0.097
	(0.29)	(0.49)	(0.50)
NOPS×DivR1×JGTRRA	-0.028**	-0.031***	-0.028**
	(-2.59)	(-2.93)	(-2.55)
NOPS×DivR2×JGTRRA	-0.028**	-0.024**	-0.023**
	(-2.48)	(-2.01)	(-1.97)

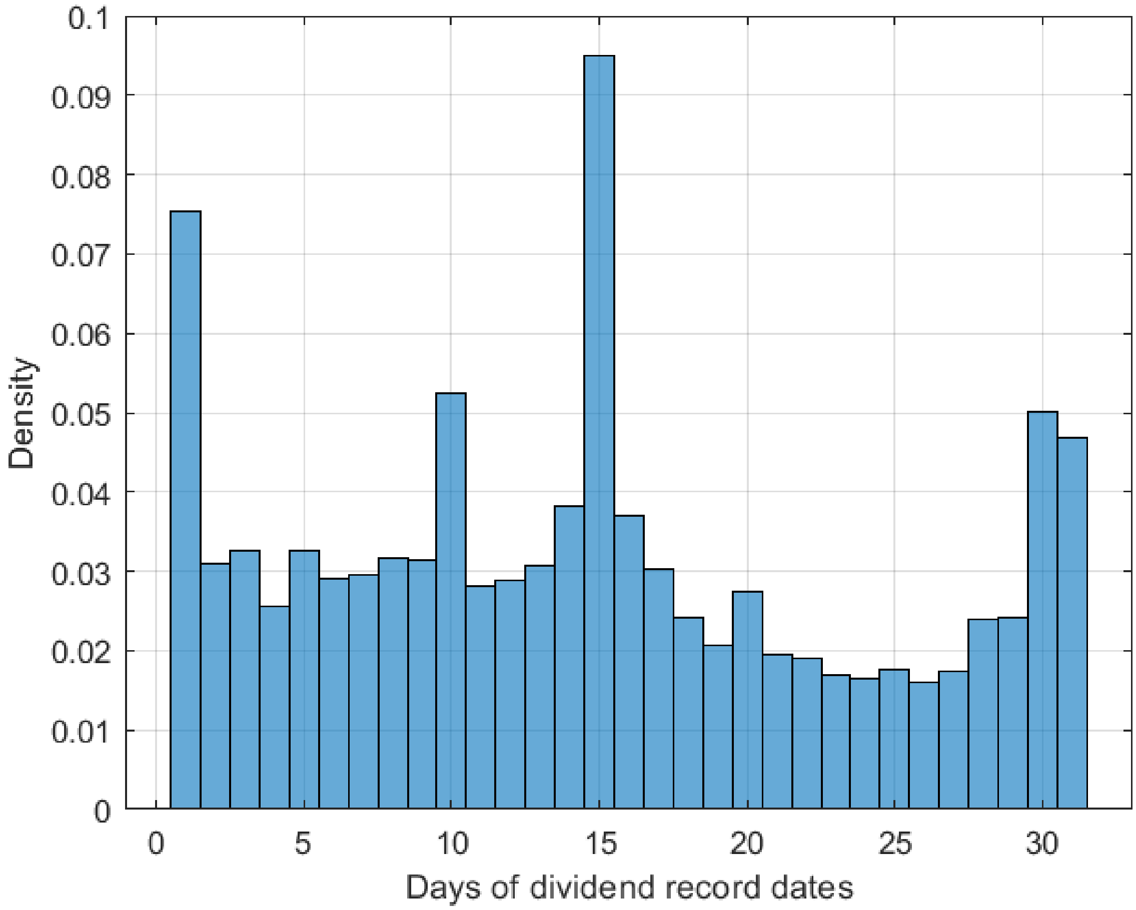

Figure 1: Distribution of dividend record dates within a month

This figure plots the distribution of the dividend record dates during a dividend record month.

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Appendix

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To examine whether risk can explain away our results, we re-estimate our stock-level DiD regressions in the previous section by adding risk factors and their corresponding interaction terms. We add each factor one by one to our stock-level regression in Equation (4), and interact each factor with the existing variables. Take market risk factor as an example, we estimate the following regression specification,

$$\begin{aligned}
ret_{i,t} = {} & \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t-1} + b_2 DivR_{i,t-1} \times JGTRRA_{t-1} + b_3 NOPS_{i,t-1} \\
& + b_4 NOPS_{i,t-1} \times DivR_{i,t-1} + b_5 NOPS_{i,t-1} JGTRRA_{t-1} + b_6 Low\ NOPS_{i,t-1} \\
& + b_7 High\ NOPS_{i,t-1} + b_8 Low\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_9 Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{10} High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{11} High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{12} Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{13} High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{14} MKT \times DivR_{i,t-1} + b_{15} MKT \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{16} NOPS_{i,t-1} \\
& + b_{17} MKT \times NOPS_{i,t-1} \times DivR_{i,t-1} + b_{18} MKT \times NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{19} MKT \times Low\ NOPS_{i,t-1} + b_{20} MKT \times High\ NOPS_{i,t-1} \\
& + b_{21} MKT \times Low\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{22} MKT \times Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{23} MKT \times High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{24} MKT \times High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{25} MKT \times Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} \\
& + b_{26} MKT \times High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t},
\end{aligned}$$

where MKT denote the CRSP value-weighted index return.

Table A1: DID results over a longer sample 1965:7 to 2019:12

This table reports results from our main DID regression over a longer sample period 1965:7 to 2019:12.

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t},$$

Monthly return, the dependent variable, is multiplied by 100. $JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Firm & Month	Firm & Month
S.E. Clusters	Month	Firm & Month	Month	Firm & Month
NOPS	-0.089***	-0.089***	-0.096***	-0.096***
	(-12.88)	(-12.86)	(-16.74)	(-16.78)
DivR	-0.056	-0.056	-0.304***	-0.304***
	(-0.52)	(-0.52)	(-5.84)	(-5.75)
NOPS×*DivR*	0.031***	0.031***	0.020***	0.020***
	(5.33)	(5.29)	(4.75)	(4.71)
NOPS×*JGTRRA*	0.049***	0.049***	0.058***	0.058***
	(5.40)	(5.39)	(7.04)	(7.07)
DivR×*JGTRRA*	-0.015	-0.015	0.123	0.123
	(-0.09)	(-0.09)	(0.82)	(0.82)
NOPS×*DivR*×*JGTRRA*	-0.020**	-0.020**	-0.018**	-0.018**
	(-2.18)	(-2.17)	(-2.21)	(-2.19)

Table A2: DID results with industry fixed effect

This table reports results from our main DID regression by replacing firm fixed effect with industry fixed effect (3-digit SIC codes),

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_j + b_1 NOPS_{i,t-1} + b_2 DivR_{i,t-1} + b_3 NOPS_{i,t-1} \times DivR_{i,t-1} + b_4 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}.$$

Monthly return, the dependent variable, is multiplied by 100. α_j denotes the industry fixed effect. $JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Ind & Month	Ind & Month
S.E. Clusters	Month	Ind & Month	Month	Ind & Month
NOPS	-0.095***	-0.095***	-0.099***	-0.099***
	(-9.02)	(-8.46)	(-10.22)	(-9.54)
DivR	0.077	0.077	0.010	0.010
	(0.37)	(0.38)	(0.07)	(0.07)
NOPS×DivR	0.046***	0.046***	0.042***	0.042***
	(4.55)	(4.53)	(5.21)	(4.89)
NOPS×JGTRRA	0.055***	0.055***	0.056***	0.056***
	(4.56)	(4.38)	(4.91)	(4.65)
DivR×JGTRRA	-0.148	-0.148	-0.110	-0.110
	(-0.62)	(-0.60)	(-0.47)	(-0.44)
NOPS×DivR×JGTRRA	-0.035***	-0.035***	-0.033***	-0.033***
	(-2.83)	(-2.76)	(-2.76)	(-2.59)

Table A3: DID results with *MISP*

We re-run our main DID regression by replacing $NOPS$ with $MISP$ proposed by Stambaugh et al. (2015),

$$ret_{i,t} = \alpha_0 + \alpha_t + \alpha_i + b_1 MISP_{i,t-1} + b_2 DivR_{i,t-1} + b_3 MISP_{i,t-1} \times DivR_{i,t-1} + b_4 MISP_{i,t-1} \times JGTRRA_{t-1} + b_5 DivR_{i,t-1} \times JGTRRA_{t-1} + b_6 MISP_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + \varepsilon_{i,t}.$$

Monthly return, the dependent variable, is multiplied by 100. $JGTRRA_t$ is a dummy variable which equals to one if month t is after May 2003 (after the JGTRRA of 2003). $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. The sample period ends in 2018:12 due to the availability of $MISP$ data . *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

Fixed Effects	Month	Month	Firm & Month	Firm & Month
S.E. Clusters	Month	Firm & Month	Month	Firm & Month
MISP	-0.049***	-0.049***	-0.041***	-0.041***
	(-8.54)	(-8.53)	(-5.98)	(-6.00)
DivR	-1.767***	-1.767***	-1.171***	-1.171***
	(-8.09)	(-8.00)	(-5.63)	(-5.57)
MISP×*DivR*	0.034***	0.034***	0.017***	0.017***
	(5.75)	(5.73)	(3.98)	(3.97)
MISP×*JGTRRA*	0.034***	0.034***	0.039***	0.039***
	(4.38)	(4.38)	(5.35)	(5.37)
DivR×*JGTRRA*	1.372***	1.372***	1.255***	1.255***
	(4.56)	(4.58)	(4.20)	(4.23)
MISP×*DivR*×*JGTRRA*	-0.029***	-0.029***	-0.024***	-0.024***
	(-3.68)	(-3.69)	(-3.67)	(-3.69)

Table A4: Portfolio-level DID using Reg SHO

In this table we adopt the Regulation SHO Pilot Program as an alternative exogenous shock to repeat our main DID results under portfolio levels. Each column uses a different sample period suggested by Chu et al. (2019). In Panel A, we form equal-weighted portfolios, and in Panel B, we follow Chu et al. (2019) to form gross-return-weighted portfolios.

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Pilot_i + \beta_1 Pilot_i \times During_t + \epsilon_{i,t},$$

Dependent variable, $y_{i,t}$, is the monthly return of a long-short portfolio i in month t. Portfolio returns are multiplied by 100. $Pilot_i$ is a dummy variable which is equal to one if the portfolio is formed by pilot stocks, and zero otherwise; $During_t$ is a dummy variable, which equals one if month t is between July 2005 and June 2007 (i.e., during the pilot period of Regulation SHO). The sample consists of pilot and nonpilot stocks listed on NYSE/AMEX. June 2007 is the ending period for all of these tests. We add the time fixed effect and report the Robust t-statistics in the parentheses. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

	Equal-weighted Portfolio				***Gross-return-weighted Portfolio***			
	1985-2007	*1980-2007*	*1990-2007*	*2000-2007*	*1985-2007*	*1980-2007*	*1990-2007*	*2000-2007*
Pilot×During	-0.79	-1.03**	-0.64	-0.93	-0.78	-1.04**	-0.66	-1.16*
	(-1.51)	(-2.00)	(-0.84)	(-1.36)	(-1.46)	(-1.98)	(-0.85)	(-1.67)
Pilot	-0.02	0.22	-0.18	0.12	-0.04	0.22	-0.16	0.34
	(-0.10)	(1.06)	(-0.49)	(0.24)	(-0.16)	(1.05)	(-0.42)	(0.69)
Constant	1.30***	1.45***	1.32***	1.11***	1.33***	1.48***	1.34***	1.02***
	(8.71)	(10.55)	(8.02)	(4.15)	(8.67)	(10.58)	(7.82)	(3.77)

Table A5: Anomaly returns and Fama-French betas on dividend record days

We calculate betas from Fama and French (2015) five-factor model using daily stock returns. For each firm in each month, we require at least 15 daily observations. We add five betas into Equation (4) as control variables.

$$\begin{aligned} ret_{i,t} = {} & \alpha_0 + \alpha_t + \alpha_i + b_1 DivR_{i,t-1} + b_2 DivR_{i,t-1} \times JGTRRA_{t-1} + b_3 NOPS_{i,t-1} + b_4 NOPS_{i,t-1} \times DivR_{i,t-1} \\ & + b_5 NOPS_{i,t-1} \times JGTRRA_{t-1} + b_6 Low\ NOPS_{i,t-1} + b_7 High\ NOPS_{i,t-1} + b_8 Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \\ & + b_9 Low\ NOPS_{i,t-1} \times JGTRRA_{t-1} + b_{10} High\ NOPS_{i,t-1} \times DivR_{i,t-1} + b_{11} High\ NOPS_{i,t-1} \times JGTRRA_{t-1} \\ & + b_{12} Low\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} + b_{13} High\ NOPS_{i,t-1} \times DivR_{i,t-1} \times JGTRRA_{t-1} \\ & + a_1 \beta^{MKT}_{i,t} + a_2 \beta^{SMB}_{i,t} + a_3 \beta^{HML}_{i,t} + a_4 \beta^{RMW}_{i,t} + a_5 \beta^{CMA}_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

Monthly return, the dependent variable, is multiplied by 100. $DivR_{i,t-1}$ is a dummy variable that equals one if stock i reports a daily dividend record date in month $t-1$ and zero otherwise. t-statistics are presented in parentheses. $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after June 2003 (after the JGTRRA of 2003). We add firm and month fixed effects and cluster standard errors on both firm and time. t-statistics are in parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

DivR	-0.353*** (-3.82)	-0.354*** (-3.83)	-0.356*** (-3.85)	-0.356*** (-3.85)	-0.356*** (-3.85)	-0.352*** (-3.82)
DivR×*JGTRRA*	0.207 (1.24)	0.209 (1.25)	0.214 (1.28)	0.214 (1.28)	0.214 (1.28)	0.206 (1.23)
NOPS	-0.100*** (-9.14)	-0.100*** (-9.13)	-0.100*** (-9.11)	-0.100*** (-9.11)	-0.100*** (-9.11)	-0.101*** (-9.15)
NOPS×*DivR*	0.018*** (3.26)	0.018*** (3.27)	0.018*** (3.28)	0.018*** (3.28)	0.018*** (3.27)	0.018*** (3.26)
NOPS×*JGTRRA*	0.061*** (5.06)	0.061*** (5.07)	0.061*** (5.06)	0.061*** (5.06)	0.061*** (5.06)	0.061*** (5.06)
Low NOPS	-0.346** (-2.52)	-0.345** (-2.51)	-0.343** (-2.50)	-0.342** (-2.50)	-0.342** (-2.49)	-0.347** (-2.53)
High NOPS	-0.339*** (-3.21)	-0.338*** (-3.20)	-0.339*** (-3.21)	-0.339*** (-3.21)	-0.338*** (-3.20)	-0.340*** (-3.22)
Low NOPS×*DivR*	0.087 (0.72)	0.087 (0.71)	0.087 (0.72)	0.087 (0.71)	0.086 (0.71)	0.086 (0.71)
Low NOPS×*JGTRRA*	0.266 (1.59)	0.264 (1.58)	0.261 (1.57)	0.261 (1.56)	0.261 (1.56)	0.266 (1.60)
High NOPS×*DivR*	0.605*** (2.96)	0.606*** (2.97)	0.600*** (2.94)	0.601*** (2.94)	0.602*** (2.94)	0.606*** (2.97)
High NOPS×*JGTRRA*	0.214 (1.47)	0.210 (1.45)	0.214 (1.47)	0.213 (1.46)	0.213 (1.46)	0.213 (1.46)
Low NOPS×*DivR*×*JGTRRA*	0.173 (1.11)	0.174 (1.12)	0.173 (1.12)	0.174 (1.12)	0.173 (1.12)	0.175 (1.13)
High NOPS×*DivR*×*JGTRRA*	-0.816** (-2.41)	-0.816** (-2.41)	-0.814** (-2.40)	-0.813** (-2.40)	-0.814** (-2.40)	-0.814** (-2.40)
β^{MKT}	0.088*** (4.61)					0.076*** (3.38)
β^{SMB}		0.047*** (3.58)				0.026* (1.78)
β^{HML}			0.015* (1.75)			0.003 (0.24)
β^{RMW}				-0.002 (-0.29)		-0.016** (-2.00)
β^{CMA}					0.006 (0.78)	0.000 (0.05)

Table A6: Results from different types of anomalies by Hou et al. (2017)

We split our 182 significant anomalies into six groups based on Hou et al. (2017): (1) *Momentum*, (2) *Value & Growth*, (3) *Profitability*, (4) *Investment*, (5) *Intangibles*, and (6) *Trading Frictions*. The details of each anomaly types can be found in the Appendix of Hou et al. (2017). We sort portfolios based on *NOPS* computed with each of six subcategories of anomalies. We then re-run our portfolio-level DiD regression,

$$y_{i,t} = \alpha_0 + \alpha_t + \beta_0 Treated_{i,t-1} + \beta_1 Treated_{i,t-1} \times JGTRRA_{t-1} + \epsilon_{i,t},$$

Dependent variable, $y_{i,t}$, is the equally-weighted monthly return of a portfolio i, which can be the long side, short side, and the long-short portfolio, in month t. Portfolio returns are multiplied by 100. $Treated_{i,t-1}$ is a dummy variable which is equal to one if the portfolio i is formed on stocks whose $DivR_{i,t-1} = 1$; $JGTRRA_{t-1}$ is a dummy variable which equals to one if month $t-1$ is after May 2003 (after the JGTRRA of 2003). We add the time fixed effect and report the Robust t-statistics in the parentheses. The sample period is 1985:7 to 2019:12. *, **, and *** denote two-tail statistical significance at the 10%, 5%, and 1% levels, respectively.

NOPS by Type:	*Momentum*	*Value & Growth*	*Profitability*	*Investment*	*Intangibles*	*Trading Frictions*
	21	21	27	30	44	39
Panel A: Long-Short						
Treated	-1.619***	-0.825*	-2.036***	-0.838***	-1.236***	-1.660***
	(-5.54)	(-1.85)	(-6.18)	(-4.03)	(-6.53)	(-4.10)
Treated×JGTRRA	1.371***	0.364	1.460***	0.959***	1.255***	0.760*
	(3.88)	(0.67)	(3.39)	(3.56)	(4.81)	(1.68)
Constant	1.735***	1.218***	1.475***	0.744***	1.260***	1.760***
	(13.67)	(6.21)	(9.61)	(7.75)	(13.62)	(10.72)
Panel B: Long Side						
Treated	-0.670**	-0.135	-0.453**	-0.017	-0.376	-0.246**
	(-2.20)	(-1.28)	(-2.24)	(-0.08)	(-1.39)	(-2.47)
Treated×JGTRRA	0.450	-0.102	0.283	0.164	0.287	-0.091
	(1.30)	(-0.59)	(1.24)	(0.66)	(0.90)	(-0.67)
Constant	1.322***	1.114***	1.086***	0.813***	1.179***	1.035***
	(10.57)	(18.39)	(13.08)	(9.04)	(10.26)	(21.52)
Panel C: Short Side						
Treated	0.948***	0.690	1.583***	0.820***	0.860***	1.414***
	(2.92)	(1.44)	(3.43)	(2.96)	(4.10)	(3.21)
Treated×JGTRRA	-0.922**	-0.465	-1.177**	-0.795**	-0.968***	-0.850*
	(-2.40)	(-0.82)	(-2.13)	(-2.45)	(-3.68)	(-1.73)
Constant	-0.413***	-0.103	-0.388*	0.069	-0.081	-0.725***
	(-2.98)	(-0.50)	(-1.95)	(0.59)	(-0.86)	(-4.06)