Short Selling and Product Market Competition

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Abstract

We empirically investigate how short selling affects firms' product market performance via a managerial monitoring channel. Using both historical data and exogenous shocks to short selling, we find robust evidence that short interest negatively impacts market shares, especially in large firms. Our Reg SHO results are stronger in concentrated industries and industries where firms compete in strategic substitutes. Further tests show that these effects are driven by low *ex-ante* stock price informativeness. The evidence suggests that the interaction between market power and price opacity generates incentives for overproduction, which short selling attenuates. Our results support policies that facilitate price discovery in the presence of market power.

Keywords: Short sales, product market competition, financial feedback, price informativeness.

JEL classification: G14, G23, G34, D43, D82, D84.

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1 Introduction

The allocational role of secondary equity markets' prices has become a central topic in financial economics (Bond et al., 2012). The theoretical literature shows how equity prices might shape several feedback effects from financial markets to real economic activity, such as managerial disciplining (Holmstrom and Tirole, 1993; Strobl, 2014), managerial learning (Dow and Gorton, 1997), and stock price manipulation (Allen and Gale, 1992; Goldstein and Guembel, 2008). Concurrently, the empirical literature has paid particular attention to the effects of short selling on corporate investment (Grullon et al., 2015), providing strong evidence that investment levels respond to the information in stock prices.[1] However, since firms' overall performance may depend less on investment than on competition and market power (Gutiérrez and Philippon, 2017), competitive aspects of product markets can modulate how short interest feeds back into production decisions.

This paper investigates the effects of short selling on firms' product market performance via a managerial monitoring channel and the role of competitive interactions. We use historical data and two different regulatory changes as plausibly exogenous shocks to shorting activity: Reg SHO and the Job and Growth Tax Relief Reconciliation Act (JGTRRA). By comparing firms within the same product markets and years, we provide novel, robust evidence that short selling leads to lower market shares of sales, particularly for *large* firms. Going deeper into our main Reg SHO exercise, we show that results are stronger in concentrated product markets and industries where firms compete in strategic substitutes. Overall, our baseline analysis shows that the feedback effects from financial to product markets strongly depend on monopolistic rents and the nature of firms' strategic interactions.

There are multiple channels through which short selling could materialize into lower market shares. First, short sale restrictions might lead to overvaluation by limiting the transmission of negative information via stock prices, keeping the cost of capital artificially low (Jones and Lamont, 2002; Chang et al., 2007; Autore et al., 2015). Hence, removing such restrictions would lead to a downward correction, reverberating investments and output. Second, easier short selling might expose firms to bear raids that can drive stock prices down regardless of economic fundamentals by leading managers to withhold value-

[1] See, e.g., Chen et al. (2007); Foucault and Fresard (2014); Tsai et al. (2021); Deng et al. (2023); Boulatov et al. (2023)

creating projects (Goldstein and Guembel, 2008). Third, managers might learn from increased price discovery about inefficient levels of operations and scale down accordingly (Boulatov et al., 2023). Finally, short interest can prevent managers from undertaking policies based on empire-building motives and overly optimistic expectations, thus serving a disciplining role (Fang et al., 2016; Deng et al., 2020, 2023).

While entirely disentangling these mechanisms is challenging, our cross-sectional tests rule out alternatives. Crucially, both upward mispricing and bear raids are more likely in small, financially constrained firms (Campello and Graham, 2013; Goldstein et al., 2013). Whereas shorting activity affects access to external capital, hampering investment (Turkiela, 2019), this is less likely to be a binding constraint on large firms. Accordingly, Grullon et al. (2015) show that small firms decreased investments following Reg SHO. In general, short selling is a strong negative predictor of returns (Rapach et al., 2016; Boehmer et al., 2022; Gorbenko, 2023). However, improved price discovery can increase long investors' demand for the asset by either reducing perceived uncertainty (Nezafat et al., 2017), or by leading them to revise their valuations upward due to better resource allocation, resulting in positive price effects in the longer run. Notably, Grullon et al. (2015) document negative abnormal returns following Reg SHO, particularly for small firms. Reciprocally, we find evidence of positive abnormal returns for large firms over a longer period. In summary, our baseline results are inconsistent with the mispricing and bear raids hypotheses.

Next, we explicitly explore the role of informational content in short selling activity. First, we show that our cross-sectional results are more pronounced in stocks that were less informative about firms' fundamentals at the time of Reg SHO. Whereas this is consistent with both the managerial learning and disciplining hypotheses, we show that the estimates are sensitive to price informativeness only for large firms and product markets with high concentration and strategic substitution. These are the firms with greater incentives and ability for overreach (Deng et al., 2023), especially when stock prices convey little information. In addition, managerial learning is unlikely to be stronger along these dimensions. Second, we show that lifting the short selling restrictions led to increased stock price informativeness only in large firms, in consonance with our baseline results in market shares.

Although we cannot completely rule out alternative mechanisms, our collective evidence supports a managerial disciplining channel of short selling. Whereas market power and price opacity promote incentives to pursue aggressive output policies, short selling can alleviate these incentives, leading to downward adjustments in output levels and more informative stock prices. Thus, our findings imply that short selling can substitute competitive pressure in terms of modulating empire-building motives in product market strategies.

We start our analysis by quantifying the historical association between short selling and firms' product market shares since 1973. Controlling for unobservable sector-level time-varying shocks and time-invariant firm characteristics, we find that shorting activity predicts lower market shares. This result stems from large firms, where a one standard deviation (s.d.) increase in short selling is associated with a 0.216-0.371 percentage point (p.p.) decrease in market share, depending on the measure of short selling. These estimates represent 1.48-2.55% of the average market share of large firms. We find no evidence of such empirical association in small firms.

A natural concern with our historical analysis is the endogenous nature of shorting activity. For example, stock trading might reflect investors' anticipation of firm performance relative to product market peers (Barardehi et al., 2024). Thus, our historical results could be spuriously capturing changes in active traders' sentiment towards large firms. We address such concerns by resorting to two regulatory changes that affected short selling, with our main exercise based on the Reg SHO. The regulation's pilot program, announced in July 2004, relaxed short selling constraints on randomly selected U.S. firms listed in the Russell 3000 index.[2] As the program consisted of an exogenous shock that facilitated short selling on treated firms (Grullon et al., 2015), it can be used to gauge the causal effects of short selling on outcomes of pilot firms.

Identifying effects from variation within the same industry and year, we estimate that pilot firms experienced an average decrease in market shares of 3.23% relative to control firms. Consistent with our historical analysis, we find that the effects stem from large pilot firms, which experienced an average 4.21% reduction in market shares due to the program. This evidence suggests that our results are not driven by the decrease in corporate investment documented by Grullon et al. (2015), which comes mostly from *small* firms.[3]

If our results reflect output adjustments from managerial disciplining, they should be amplified in the presence of monopolistic rents and in industries with competition in strategic

[2] See Section 3.1 and Diether et al. (2009a) for more detailed descriptions of the program.

[3] For validation, we confirm the results in Grullon et al. (2015) and contrast them with ours.

substitutes, where empire-building motives generate incentives to engage in aggressive output policies (Sundaram et al., 1996; Fresard and Valta, 2016; Lin et al., 2019). Accordingly, we find more pronounced effects in highly concentrated industries and product markets with a greater degree of strategic substitution as measured by industry-level HHI and Competitive Strategy Measure *(CSM)* (Sundaram et al., 1996; Chod and Lyandres, 2011), respectively.

To investigate the role of the information stemming from shorting activity in driving our baseline results, we resort to two measures of stock price informativeness. The first measure, price nonsynchronicity, reflects the variation of stock returns that cannot be explained by variations in the returns of the market and the firm's industry (Roll, 1988; Chen et al., 2007). The second measure, intensity of informed trading (ITI), detects informed trades by tracking empirical patterns in days with Schedule 13D transactions (Bogousslavsky et al., 2023). In both cases, we find sharper decreases in market shares for firms with lower price informativeness at the time of the intervention. More importantly, we find that the treatment effect responds to *ex-ante* stock price informativeness only in large firms, concentrated industries, and industries where firms compete in strategic substitutes. We also show that price nonsynchronicity increased after the experiment exclusively in large firms.

Crucially, we validate our main findings by resorting to an alternative regulatory intervention, the Job and Growth Tax Relief Reconciliation Act (JGTRRA) of 2003. By implementing a differential tax treatment to qualified dividends, the law consisted of a negative shock to the supply of lendable shares of dividend-paying firms, limiting short selling activity (Thornock, 2013; Han et al., 2024). Consistent with our baseline results, we find that these firms gained market share after 2003 and that large firms entirely drove this effect. Importantly, these findings are robust to the exclusion of all the Reg SHO firms from the JGTRRA sample, ensuring that the Reg SHO treatment effects do not confound these results. In summary, the JGTRRA provides an alternative, symmetric empirical framework with an entirely different sample where we find strong support for our baseline findings.

Our JGTRRA exercise and a series of additional tests to assess the robustness of our results broadly address recent concerns raised by Heath et al. (2023) about the repeated use of natural experiments such as the Reg SHO for causal inference. Of note, we find that our results are associated with actual shorting activity, as in Grullon et al. (2015).

That we find no association between these product market adjustments and lower firm value further supports our monitoring hypothesis.

A vast body of literature studies short selling and its regulatory framework.[4] Generally, theory and evidence suggest that short sellers are informed traders (e.g., Diamond and Verrecchia (1987); Comerton-Forde et al. (2016); Chague et al. (2019)). As such, shorting activity produces information that can improve market efficiency and trigger multiple real effects (e.g., Goldstein and Guembel (2008); Massa et al. (2015a); Hope et al. (2017)). Boulatov et al. (2023) provide extensive evidence that managerial learning of pessimistic sentiment by traders drives the negative effects of short selling on investments (Grullon et al., 2015). However, in a non-US sample, Deng et al. (2023) find that this negative relationship is driven by *large* firms, suggesting that short selling prevents non-US financially unconstrained firms from overinvesting.

Our paper contributes to this literature by unveiling a novel channel through which the informational content of short selling (Desai et al., 2002; Boehmer et al., 2008; Diether et al., 2009b; Boehmer and Wu, 2013) affects real corporate decisions. In particular, our findings suggest that short sellers' capacity to process information and quickly incorporate it into stock prices (Chen and Rhee, 2010; Engelberg et al., 2012; Kahraman, 2020) serves as a disciplining device for firms that engage in aggressive output policies due to market power and price opacity. By showing evidence that these output adjustments are not detrimental to firm value, our results further suggest that short interest affects small and large firms via different channels.

Our paper also relates to the empirical literature on finance and product market performance. Typically, previous findings imply that financially constrained firms' performance is more susceptible to industry downturns and product market competition (Opler and Titman, 1994; Fresard, 2010; Cookson, 2017, 2018). We contribute by unveiling how short interest can affect large firms via competitive and informational channels. Our evidence suggests that shorting activity leads managers to internalize the product market consequences of their output policies even in concentrated industries (Hoberg and Phillips, 2010) when there is price opacity. Our results further reinforce that market performance is not a simple byproduct of corporate investment and responds to different incentives (Gutiérrez and Philippon, 2017).

[4] See Jiang et al. (2022) and Edwards et al. (2024) for detailed surveys.

Short selling regulations must strike a balance between preventing manipulation (Goldstein and Guembel, 2008; Matta et al., 2023) and allowing informed short selling and its price discovery (Marsh and Payne, 2012; Duong et al., 2015) and monitoring roles (Karpoff and Lou, 2010; Deng et al., 2020, 2021, 2023). Our results are stronger for firms with characteristics not typically associated with manipulative feedback effects. Thus, our findings lend support to policies that facilitate short selling in the presence of market power, where price discovery can have a beneficial disciplining effect.

The remainder of the paper is organized as follows. Section 2 discusses the historical correlation between short selling and product market performance. Section 3 uses the Reg SHO intervention to estimate the causal effects of short selling on market shares and establish our baseline results. On Section 4, we show how our Reg SHO results are associated to stock price informativeness. We report robustness checks, including our empirical setup based on the JGTRRA, on Section 5. Section 6 concludes, and the Appendix reports additional results.

2 Historical Analysis

2.1 Data and Sample Construction

For firms' fundamentals, we use data from Compustat's North American Fundamentals Annual. Data on short sales are reported in the Supplemental Short Interest File, also available through Compustat. Information on stock trading is retrieved from the Center for Research and Security Prices (CRSP). Our baseline sample covers the years 1973-2018.[5] Following standard practice in the literature (e.g., Almeida et al. (2012)), we exclude financial institutions (SIC codes 6000-6999) and regulated utilities (SIC codes 4900-4999). We also drop firm-year observations with missing or negative values of total assets (*at*), and sales (*sale*). Variables measured in dollars are deflated to 2012 values using the yearly GDP deflator from FRED.

Our outcome variable of interest is *Market Share*, a firm's share of its industry total yearly sales expressed in percentage points (p.p.). In our main exercise, we compute market shares

[5]For the Reg SHO analysis, we restrict the sample to a shorter time window around the experiment, as we discuss in detail in Section 3.2.

relative to 3-digit SIC industries.[6] Firm-year control variables follow Boulatov et al. (2023) and are constructed as follows. Q is the ratio of total asset plus market capitalization minus common equity minus deferred taxes and investment credit ($at + prcc\_f \times csho - ceq - txditc$) to total assets ($at$). *Cash Flow* is the sum of income before extraordinary items and depreciation and amortization ($ib + dp$) to one-year lag of total assets. *Size* is the natural logarithm of total assets. All ratios are winsorized at the 1% level.

In our historical regressions, our main independent variables of interest are measures of short selling activity. Compustat's Supplemental Short Interest File reports monthly series of *Short interest* - the number of open short positions on the last business day on or before the 15th of each calendar month. Following Boulatov et al. (2023), we construct three measures of short selling activity at the monthly frequency and convert them into annual frequency by averaging them for each firm throughout its fiscal years. Our first measure, *Short interest scaled by shares* is the ratio of *Short interest* to the number of shares outstanding at the end of the month, expressed in percentage points. Our second measure, *Abnormal short interest*, attempts to capture the unexpected component of short interest. Specifically, we follow Karpoff and Lou (2010) and Boulatov et al. (2023) and define this variable as the residuals of a regression where monthly *Short interest scaled by shares* is regressed on a dummy variable for listing at NYSE plus one-year lags of Q, *Size*, *Trading volume*, and *Return on assets*. *Trading volume* is CRSP's VOL, and *Return on assets* is net income (Compustat's ni) scaled by assets (at). These regressions also include firm and month of the year fixed effects, which accounts for unobservable time-invariant firm characteristics and monthly seasonality, respectively, that can partially explain *Short interest*. Finally, our third measure, *Days-to-cover*, consists on the ratio of *Short interest scaled by shares* to the month's average daily share volume, as in Hong et al. (2016). Our final sample covers 103,594 firm-year observations. Summary statistics are reported in Table 1.

— PLACE TABLE 1 ABOUT HERE —

[6]Results are qualitatively similar if we use 4-digit SIC industries. See Table C.1

2.2 Specification

In our first exercise, we estimate historical correlations between short selling activity and product market composition by performing fixed effects regressions on our 1973-2018 sample. We regress market shares on our proxies for short interest while controlling for multiple observable and unobservable characteristics. Specifically, we estimate the following specification:

$$Market\ Share_{i,j,t} = \beta SI_{i,t-1} + \gamma X_{i,t-1} + \mu_i + \mu_{j,t} + \epsilon_{i,j,t}, \tag{1}$$

where the outcome $Market\ Share_{i,j,t}$ is firm i's market share of industry j in year t. Industry j corresponds to 3-digit SIC codes. $SI_{i,t-1}$ is the firm-level one-year lag of one of our proxies for short interest. $X_{i,t-1}$ is a vector of lagged control variables consisting of Q, $Size$, and $Cash\ Flow$. Our coefficient of interest is β, which estimates the relationship between shorting activity and market shares in our sample. Via endogenous association or causal channels, we expect β to be negative, implying that higher short interest predicts worse product market performance. We include firm fixed effects μ_i to capture any unobserved, time-invariant firm characteristics. Importantly, we also include industry-year fixed effects $\mu_{j,t}$, which absorb the effects of any sector-specific shocks over the years. For parsimony, we define industry-year fixed effects at the most granular industry classification, 4-digit SIC, in all our specifications.[7] Thus, Equation (1) explains product market composition by comparing firms in the same product market and year. Standard errors are clustered at the firm level.

Next, we assess how the relationship between short selling and market shares varies across small and large firms. Specifically, we estimate the following specification:

$$Market\ Share_{i,j,t} = \alpha Large_{i,t-1} + \beta SI_{i,t-1} + \delta SI_{i,t-1} \times Large_{i,t-1} + \gamma X_{i,t-1} + \mu_i + \mu_{j,t} + \epsilon_{i,j,t}, \tag{2}$$

where $Large_{i,t-1}$ is an indicator that equals one when firm i is above the median firm size in year $t-1$. In these specifications, we omit $Size$ as a control variable as it is highly correlated with $Large$.[8] Here, β estimates the relationship between shorting activity and market shares of small firms, while the coefficient of the interaction, δ, estimates differential

[7]Results are qualitatively similar if we use 3-digit SIC industries instead.
[8]Results are qualitatively similar if we include both control variables.

effects for large firms. A negative value of δ indicates that short selling negatively predicts product market performance of large firms relative to small firms.

2.3 Results

Table 2 reports results from the estimation of Equation (1). Across all specifications, we find negative, significant coefficients of our short selling measures. The estimated effects are economically sizeable. Our specification in column (1) shows that a one standard deviation (s.d.) increase in *Short interest scaled by shares* is associated with a 0.250 p.p. decrease in 3-digit SIC market shares, which corresponds to a 2.61% decrease in the average firm's market share. Similarly, columns (2) and (3) show that a one (s.d.) increase in *Abnormal short interest* and *Days-to-cover* translates into 1.79% and 1.34% lower market shares, respectively.[9]

— Place Table 2 About Here —

Table 3 reports estimates of β and δ in Equation (2). While the estimated effect of short selling on small firms, β, changes across specifications, the differential effect on large firms, δ, is consistently negative and significant. In fact, the total effect on large firms, $\beta + \delta$, is negative and significant at the 1% level in all models. Results in column (1) imply that an one s.d. increase in *Short interest scaled by shares* is associated with 0.216 p.p. lower 3-digit SIC market shares, which corresponds to a 1.48% decrease relative to the average market share of large firms. Similarly, an one s.d. increase in *Abnormal short interest* and *Days-to-cover* is associated with 2.55% and 1.48% decrease in large firms' market shares, respectively.

— Place Table 3 About Here —

Our results in Table 3 provide evidence that short selling activity predicts worse product market performance. Interestingly, this empirical pattern is entirely driven by large firms, as we find no conclusive evidence of such association on small firms. Whereas the literature on short selling has shown that short interest is a strong, reliable predictor of negative stock returns (e.g., Rapach et al. (2016); Boehmer et al. (2022); Gorbenko (2023)), no association with product market performance has been previously established. In addition, consequences

[9] We report results using 4-digit SIC industries in Table C.1 and Table C.2. Economic magnitudes are qualitatively similar.

of short interest are typically associated with small, financially constrained firms (Campello and Graham, 2013; Massa et al., 2015b; Grullon et al., 2015). Our results suggest that short selling can also serve as an important predictor of outcomes of large firms, in line with the investment of non-US corporations (Deng et al., 2023).

3 A Regulatory Experiment - Reg SHO

3.1 Background

Our results in Section 2.2 provide evidence that there is a negative association between short interest and market shares that firm-level characteristics or sector-specific yearly shocks cannot explain. Whereas interesting from a prediction perspective, one cannot claim causality based on these results due to endogeneity concerns. For instance, active traders might follow changes in firms' fundamentals over time to predict worse performance relative to industry peers successfully. In that case, our results in Section 2.2 would reflect stock traders' anticipation. In addition, larger firms might be more known to the general public or follow stricter disclosure practices. If so, the results in Table 3 could reflect a lower cost of acquiring information about large firms rather than disciplining or learning effects.

To alleviate anticipation and other endogeneity concerns, we exploit a regulatory experiment commonly used in the literature to gauge causal effects of short selling - Regulation SHO. The program, conducted by the Securities and Exchange Commission (SEC), consisted of relaxing a short selling constraint on a random sample of firms. The restriction revoked is known as the uptick rule, a price test that prohibited short sale orders from being placed when stock prices were declining. The rule was in place since 1938 and aimed at restricting short selling activity (Grullon et al., 2015; Fang et al., 2016). In July 2004, the SEC announced a list of 968 firms from the Russell 3000 index for which price tests would be lifted, which happened in May 2005. To construct the pilot group, the Securities and Exchange Commission (SEC) ranked stocks from the Russell 3000 index independently within each of three stock exchanges—AMEX, NASDAQ, and NYSE—by average daily trading volume and then selected every third firm. In July 2007, the SEC concluded the program and suspended price tests for all firms.

As a randomized control trial, the Reg SHO has been recurrently used by empirical finance researchers to estimate causal effects of short selling, which drew concerns about the validity of the results. Heath et al. (2023) argue that reusing natural experiments to estimate effects on various outcomes can lead to a high occurrence of false positives due to a multiple hypothesis testing problem. After applying a procedure that corrects for dependence across tests, the authors conclude that several results published as causal effects of Reg SHO could be false positives.

Based on their findings, Heath et al. (2023) provide some guidelines for authors that reuse natural experiment settings. First, to account for the possibility that researchers run multiple regressions with different dependent variables but only report those for which statistical significance appears, they stress the need for economic foundations of the empirical hypotheses made. In that aspect, our conjectures are supported by the extensive theoretical literature on feedback effects from financial markets (e.g., Goldstein and Guembel (2008); Goldstein et al. (2013); Edmans et al. (2015); Dow et al. (2017); Edmans et al. (2017); Matta et al. (2023)) that discuss how secondary financial markets can affect real outcomes via various channels. In addition, multiple papers support the hypothesis of learning and disciplining via short selling and stock prices (e.g., Chen et al. (2007); Karpoff and Lou (2010); Foucault and Fresard (2014); Fang et al. (2016); Campello et al. (2024)).

Second, when conducting new tests, one should consider that multiple hypothesis correction raises the bar of statistical significance as natural experiments are repeatedly used. In this regard, our specifications are more rigorous than those previously used in the literature (including Heath et al. (2023)) due to the inclusion of industry-year fixed effects.[10] Thus, to the extent that our estimates rely only on variation within industry-years, they are not directly comparable to previous ones. Still, Heath et al. (2023) argue that, considering the many instances in which Reg SHO was used to measure causal effects of short selling, a t-statistic of 3.41 should be used as the threshold for 5% statistical significance. We acknowledge that this is a high bar that some of our results do not reach. This concern is partially alleviated due to our emphasis on cross-sectional heterogeneous effects and our strict specifications.

Third, and perhaps most importantly, Heath et al. (2023) recommend using alternative ways to produce causal evidence. This is an important task to ensure the validity of the results

[10]To the best of our knowledge, we are the first to use such specifications.

which we undertake in Section 5.2. We use the Job and Growth Tax Relief Reconciliation Act (JGTRRA) of 2003 as a second arguably exogenous shock to short selling. As we report and discuss in detail in Section 5.2, we confirm our main results under this setup, thus providing critical support to our baseline empirical framework.

Finally, new results should reconcile exclusion restrictions with existing evidence. Whereas we do not discuss all papers that rely on Reg SHO for identification, Grullon et al. (2015) is arguably the closest one, hence warranting further justification. In particular, worse product market performance could be a direct consequence of decreased investment levels due to short selling. However, the effects on stock prices and investments documented by Grullon et al. (2015) stem from small firms, while ours are observed exclusively on large ones. Hence, it is unlikely that our results are driven by investments or any other channels of short selling that affect small and constrained firms more strongly, at least in the context of Reg SHO. Nevertheless, we acknowledge the challenge of disentangling mechanisms and recognize that we cannot completely rule out alternative explanations for our results.

3.2 Sample Construction

We focus on the first part of Reg SHO, during which only pilot stocks were exempted from short sale price tests. Therefore, our main sample ends at 2006, before the overall repeal of price tests. The reason is our interest on the role of cross-sectional characteristics on the short selling sensitivity of market shares. Specifically, the first treatment effects might compromise cross-sectional analyses if we include the second wave of treatment with confounding factors that arise if the randomness of the pilot and control groups decreases over time (Grullon et al., 2015). In addition, knowledge of the effects of the program on pilot firms might have induced active investors to anticipate likely effects of the extension to non-pilot firms. Thus, the first wave of the intervention provides us with a better-suited framework to estimate well-identified treatment effects and perform a clean cross-sectional heterogeneity analysis. Nevertheless, in Section 5, we perform a robustness test of our main results using an approach similar to that of previous research (e.g., Grullon et al. (2015); Fang et al. (2016); Boulatov et al. (2023)) where firms in the control group are considered treated after July 2007, when price tests were repealed for all firms.

We build a sample of firms listed in the Russell 3000 index as of May 2004. We merge this list of firms to Compustat's annual files and apply similar filters to those described in Section 2.1. In this exercise, the period covered spans from 2001 to 2006. Our resulting sample consists of an unbalanced panel of 10,673 firm-year observations of 1,785 firms of which 603 belong to the pilot group, and 1,182 belong to the control group. Our dependent variable is yearly 3-digit SIC market shares, which is relative to all Compustat industry peers, measured in percentage points.

To explore cross-sectional heterogeneity, we build three variables. Analogous to Section 2.2, we define $Large_i$ as an indicator variable that equals one when firm i was above median assets within the Russell 3000 sample of firms in 2004. We fix this variable at the time of the treatment of pilot firms to avoid possible confounding factors stemming from treatment effects. Our other two variables are proxies for intensity in product market interactions. First, we construct a measure of industry concentration with a Herfindahl-Hirschman Index (HHI) based on 3-digit SIC market shares. We define this variable at the industry level as of 2004.

Our third variable, the Competitive Strategy Measure (CSM), follows Sundaram et al. (1996) and Chod and Lyandres (2011) and inversely measures the intensity of competitive interactions within industries. For firm i, we compute

$$CSM_i = corr\left(\frac{\Delta\pi_i}{\Delta S_i}, \Delta S_{-i}\right),$$

where $\Delta\pi_i$ and ΔS_i are the changes in the firm's profits and sales between two periods, respectively, and ΔS_{-i} is the change in the combined sales of all product market rivals. Similar to Chod and Lyandres (2011), we calculate CSM at the firm level using values from the previous 20 quarters to compute the correlation. As Sundaram et al. (1996) explain, this measure is an empirical proxy for the cross-partial derivative of a firm's value with respect to its own and its competitors' actions. Following the literature, we take the average of this value across firms within industries to get CSM_j, a measure of competitive interaction at the product market level. For robustness, we construct CSM_j at both 3- and 4- digits SIC codes, which we refer to as $CSM3$ and $CSM4$, respectively. The resulting variable is bounded in $[-1, 1]$ and its sign measures the type of strategic interaction within an industry: negative values indicate competition in strategic substitutes, whereas positive values correspond to competition in strategic complements. The magnitude of industries' CSM measures the intensity of these interactions. Again, we fix this variable at its 2004 value for our cross-sectional heterogeneity tests.

Table 4 reports summary statistics for the firms in our Reg SHO sample in 2004. We compare mean values across pilot and control groups to ensure the variables are well-balanced. As in Grullon et al. (2015), we find no significant differences between group averages of the variables of interest, consistent with a randomized selection.[11]

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3.3 Specification

In our first exercise with Reg SHO, we test whether pilot firms lost market share relative to control firms during the pilot program. In addition, we study what product market aspects are more strongly associated with changes in composition due to short selling activity. To test our first hypothesis, we estimate the following differences-in-differences (hereafter, DID) specification:

$$Market\ Share_{i,j,t} = \beta Treated_i \times Post_{i,t} + \gamma X_{i,t-1} + \mu_i + \mu_{j,t} + \epsilon_{i,j,t}, \tag{3}$$

where $Treated_i$ is an indicator variable that equals one if firm i belongs to the pilot group, and $Post_{i,t}$ is an indicator that equals one when firm i's fiscal year includes at least seven months after July 2004, when the pilot group was announced.[12] $X_{i,t-1}$ is a vector of one-year lagged controls similar to those in Equation (1). Again we include firm and industry-year fixed effects in all specifications.[13] In Appendix B, we replicate our main historical and Reg SHO results without firm fixed effects to alleviate concerns about their high explanatory power of product market performance.

In this exercise, the coefficient of interest is β, which measures the impact of the program on pilot firms' market shares, as compared to non-pilot firms *within the same industry*. A

[11] Grullon et al. (2015, Table 1) report comparisons of several other variables for both the entire sample and small firms only and find no major differences in means.

[12] We focus on the announcement date to account for changes in expectations with respect to pilot firms when the pilot group was announced, which can potentially precede actual effects of short selling activity (Grullon et al., 2015)

[13] In Equation (3), we do not include the coefficient of $Treated$ because it is subsumed by firm fixed effects. For ease of exposition, we also don't include the coefficient of $Post$, which is estimated because it varies across firms depending on fiscal year-end. This coefficient is not statistically significant at usual levels in any of our specifications

negative estimate indicates that pilot firms lost market share after the exemption of price tests relative to peers for which the tests remained in place. To assess heterogeneous effects, we estimate triple differences models where we interact the cross-sectional variable of interest with $Treated_i$ and $Post_{i,t}$. The triple differences estimator in these specifications measures the sensitivity of the treatment effect to the characteristic at issue.

3.4 Results

First, we report univariate estimates of Equation (3) without controls on our overall sample, on a sample of small firms, and on a sample of large firms. In this exercise, we also report estimates of a specification similar to Equation (3) where the dependent variable is firms' investment, defined as capital expenditures (Compustat's *capx*) scaled by total assets. We perform this exercise for two reasons. First, it serves as validation of our empirical approach, as we show that it closely replicates the results previously documented by Grullon et al. (2015). While their dependent variable does not require within industry comparisons, we show that the inclusion of industry-year fixed effects does not affect their results substantially, which could possibly cast doubt about the novelty of our findings. Second, the replication allows us to directly contrast our results to theirs, especially with regards to firms' size.

We report results from this exercise in Table 5. We find a significant decrease on market shares of pilot firms after the price tests exemption. Specifically, market shares of these firms decreased by 0.208 p.p. relative to control firms, which corresponds to 3.23% of the overall mean in the Reg SHO sample. While this effect could be a direct consequence of the decrease in investment by pilot firms documented by Grullon et al. (2015) and which we replicate in Table 5, cross-sectional analysis of effects by firm size shows contrasting results. While most of the effect on investment is driven by small firms, decreases in market shares are only observed for *large* firms. While we find a null effect on small firms, large pilot firms experienced a decrease of 0.440 p.p. in market shares, which corresponds to a 4.21% decrease in the average market share of large firms.

— PLACE TABLE 5 ABOUT HERE —

We confirm these results in Table 6, where we report estimates of a triple differences specification where we interact *Treated*, *Post*, and *Large*. While columns (1) and (3) show that there was an overall decrease in market shares of pilot firms in the 2 years following the program announcement, columns (2) and (4) show the effects come exclusively from large firms. Again, the economic magnitudes are meaningful: based on column (4), large pilot firms lost on average 0.483 p.p. market share, which corresponds to a 4.62% decrease relative to the mean of large firms. In contrast, the DID coefficients show that changes in market shares of small pilot firms are statistically indistinguishable from zero.

— Place Table 6 About Here —

In Table 7, we report results of triple differences estimates with our product market variables. Columns (1) and (2) report heterogeneous effects by product market concentration. The negative and statistically significant coefficients of the triple interaction term suggest that the Reg SHO impact on market shares was stronger in more concentrated markets. In particular, the coefficients reported in column (2) imply that a one s.d. increase in market concentration at the time of the program is associated with 0.594 p.p. lower market shares of pilot firms after price tests exemption. These estimates also imply that a negative treatment effect is observed for pilot firms in product markets above the 36.4% quantile of the distribution of HHI within our Reg SHO sample.

— Place Table 7 About Here —

Columns (3)-(6) in Table 7 report heterogeneous effects by product market competition, as inversely proxied by industries' CSM (see Section 3.3). Columns (3) and (4) use CSM defined at the 3-digit SIC level, whereas columns (5) and (6) use 4-digit SIC industries. We find positive, statistically significant coefficients of the triple interaction terms across all specifications, suggesting that the treatment effect was stronger for pilot firms on markets with more competition in strategic substitution. Specifically, results in column (4) imply that a one s.d. lower CSM is associated with 0.714 p.p. lower market shares of pilot firms after the price tests suspension. These estimates imply negative treatment effects for pilot firms in industries below the 72.4% quantile of CSM.

In Appendix C, we revisit the results in Table 7 by performing DID regressions on samples split by the cross-sectional variables of interest, as in Table 5. For concentration, we classify industries as concentrated if their HHI is above the overall median of the Reg SHO sample.

For the CSM, we classify industries according to the sign of the measure. Industries with a positive (negative) CSM value are classified as those in which firms compete in strategic complements (substitutes), as in Chod and Lyandres (2011). We report the results of these exercises in Tables C.3 and C.4. The estimates confirm our previous results that effects of the Reg SHO on market shares were driven by firms in concentrated industries and in product market where firms compete in strategic substitutes.

To further validate and characterize our Reg SHO results, we report and discuss two more tests in Appendix A. First, we show that, in our sample, we observe positive treatment effects on our short interest variables of Section 2, allowing us to attribute our results to actual short selling rather than its threat. We also provide some evidence that the Reg SHO led to positive abnormal returns of large firms within our sample period. As we discuss in detail in Appendix A, these results help us distinguish our results from short selling consequences that are detrimental to firm value in the long run.

4 Price Informativeness

Our results on Section 3.3 are unlikely to reflect overvaluation or the threat of bear raids since these are more latent in small, financially constrained firms (Campello and Graham, 2013; Goldstein et al., 2013; Grullon et al., 2015). Instead, if the underlying mechanism is a learning or disciplining process brought about by the information released by short selling, results should be sensitive to measures of price informativeness. In this section, we investigate whether our results are driven by the informational content of short interest in the context of the Reg SHO.

The findings by Brav et al. (2008), Brav et al. (2015), Deng et al. (2020), Ordóñez-Calafí and Bernhardt (2022) and others suggest that active trading can have a disciplining effect on managers. Hence, the removal of short selling restrictions can precede the release of new information about overreach by firms with market power, leading managers to adjust accordingly with lower output levels relative to similar industry peers. If that is the case, two empirical patterns should follow. First, as the experiment increased short selling for treated firms, output adjustments should be stronger where prices had less private content up to the treatment, enabling unpunished overreach. Second, price informativeness should improve more for firms where we observe the largest treatment effects on market shares.

To test these hypothesis, we follow Chen et al. (2007) and Bogousslavsky et al. (2023) and construct two proxies for the amount of firm-specific information contained in stock prices. The first one, proposed by Roll (1988), argues that the variation in stock returns of a firm can be decomposed into market-related variation, industry-related variation, and a firm-specific component. The variable of interest, *Price Nonsynchronicity*, builds on the portion of the variation that cannot be explained by market and industry systematic fluctuations, thus conveying fundamental, private information. To construct this measure, we first estimate the following regression for each firm in our sample during the year prior to the Reg SHO announcement:

$$r_t = \alpha + \beta_m r_{m,t} + \beta_j r_{j,t} + \epsilon_t, \tag{4}$$

where r_t is the firm's daily stock return, $r_{m,t}$ is the daily CRSP value-weighted market return, and $r_{j,t}$ is the daily return of the firm's respective 3-digit SIC industry portfolio.

The measure of price nonsynchronicity is one minus the R-squared of regression (4), thus capturing the portion of a firm-year's daily stock return variation that cannot be explained by its industry and the market (Roll, 1988). For ease of exposition, we will refer to this variable as $(1 - R^2)$ henceforth. In all tables and regressions, $(1 - R^2)$ is computed in percentage points. In our sample, the average value of $(1 - R^2)$ is 65.41, showing that market and industry returns account for only about 35% of firms' stock return variation.

The second proxy for price informativeness is *Informed Trading Intensity (ITI)*, introduced by Bogousslavsky et al. (2023). The authors develop a machine learning approach to identify days with informed stock trading. For our tests, we use the authors' main measure whereby realized informed trading is detected from empirical patterns on days that informed investors trade more intensely.[14] The authors define these days as those with more 13D trades, which are informed transactions (Collin-Dufresne and Fos, 2015). As mandated by Schedule 13D, investors must disclose large transactions in a SEC filing within 60 calendar days after the trades. This allows Bogousslavsky et al. (2023) to retroactively detect informed trades and train their model to measure ITI for all stocks at a daily frequency. In

[14] These patterns relate to 41 variables deemed relevant by microstructure theory. For a list of the variables and a thorough discussion of the ITI's construction, see Bogousslavsky et al. (2023).

addition to being a novel, data-driven approach to quantify informed trading, ITI improves on measures such as the PIN measure by Easley et al. (1996), that rely on the assumption that informed traders use aggressive marketable orders.[15]

In our first battery of tests we explore cross-sectional variation in $(1 - R^2)$ and ITI at the time of the experiment. First, we conduct a heterogeneity analysis similar to that of Section 3.3, with a triple interaction term that includes each price informativeness measure. We report estimates of the coefficients of interest in Table 8 and Table 9. Results suggest that lower price informativeness at the time of the treatment led to larger market share losses. More precisely, column (2) of table Table 8 suggests that a one s.d. decrease in $(1 - R^2)$ is associated with 0.43 p.p. lower market shares after the suspension of price tests. Similarly, column (2) of table Table 9 implies that a one s.d. decrease in ITI is associated with 0.23 p.p. lower market shares.

— Place Table 8 About Here —

— Place Table 9 About Here —

Next, we explore how firms' and product markets' characteristics shape the sensitivity of market shares to price informativeness during the Reg SHO. Specifically, we estimate triple differences models similar to the those on Table 8 and Table 9 across subsamples according to our cross-section variables defined in Section 3.2.[16] In Table 10 and Table 11, we report results across small and large firms for $(1 - R^2)$ and ITI, respectively. Using both variables, the estimates show that the treatment effect responds to price informativeness in large firms only. A one s.d. decrease in $(1 - R^2)$ at the time of the treatment is associated with 0.57 p.p. smaller market shares after treatment for pilot large firms. Similarly, a s.d. decrease in ITI is commensurate with 0.42 p.p lower market shares. For small firms, we find no responsiveness of the treatment to $(1 - R^2)$ and a marginally significant sensitivity to ITI, although substantially smaller than that of large firms.

— Place Table 10 About Here —

— Place Table 11 About Here —

[15]This assumption is contradicted by recent findings by Barardehi et al. (2019) and Brogaard et al. (2019).

[16]We favor splitting the sample in this framework to avoid using interactions higher than third order in our specifications.

In our last cross-sectional exercises, we split our sample by product market characteristics. Table 12 and Table 13 report the results for $(1-R^2)$ and ITI in the samples of low versus high concentration based on median HHI, and by whether firms compete in strategic substitutes or complements, as per by the sign of the CSM. For low concentration industries and product markets where firms compete in strategic complements, we find no significant response of the treatment to the measures of price informativeness. In stark contrast, our subsamples of industries with high concentration and strategic substitution show a strong response of the treatment effect to $(1-R^2)$ and ITI. For concentrated industries, a one s.d. decrease in $(1-R^2)$ and ITI is associated with 0.82 and 0.37 p.p lower market shares, respectively. For industries with negative $CSM3$ and $CSM4$, a one s.d. smaller $(1-R^2)$ implies 0.43 and 0.50 p.p. smaller market shares, respectively. For ITI, these estimates are 0.15 p.p. and 0.18 p.p.

— Place Table 12 About Here —

— Place Table 13 About Here —

Next, we test whether lifting price tests led to an increase in price informativeness, and if this effect aligns with our baseline cross-sectional results in market shares of Section 3.3. Specifically, we estimate Equation (3) with yearly $(1-R^2)$ as dependent variable, and test for heterogeneity across small versus large firms. We report results in Table 14. In columns (1) and (3), we find no significant effect of the Reg SHO on price informativeness of overall pilot firms. However, columns (2) and (4) show significant, contrasting effects depending on size. We find an increase in $(1-R^2)$ only in large firms, with a sign reversal of the point estimate for small firms.[17] Based on column (4), price informativeness of the stocks of large firms increased by 2.63 p.p., which corresponds to 4.5% of the average for these firms.[18]

— Place Table 14 About Here —

Our collective evidence implies that stock price informativeness plays a meaningful role in how short selling interacts with product market performance. Our cross-sectional findings

[17]The total estimated effect for large firms is significant at the 1% level in both specifications.

[18]We do not report a similar exercise with ITI as dependent variable because the measure has little variation across firms in a given year. Therefore, the inclusion of year fixed effects absorbs most of the variation, making the estimation void. If we include only firm fixed effects, we find results similar to those in Table 14, albeit with small economic magnitudes. These results are available upon request.

suggest that the sensitivity of market shares to short interest depends on the informational content of prices, which is consistent with both the managerial learning and disciplining channels. Crucially, we show that this result stems solely from large firms, concentrated industries, and industries where firms compete in strategic substitutes. In addition, we find that price informativeness improved solely on large firms, further suggesting that our baseline results are driven by the informational content of stock prices. Since there is no reason to expect that managerial learning should be stronger along these dimensions, our findings are consistent with a managerial disciplining channel in which short selling modulates incentives for aggressive output policies brought by the interaction of market power and price opacity.

Our analysis in this section allows us to reconcile our baseline results with the existing literature. In the context of Grullon et al. (2015), our findings suggest that the effects of short selling in small firms' investment levels are not driven by price opacity. In addition, our evidence that stock price informativeness increased solely for large firms suggests that the results in Grullon et al. (2015) are not driven by more informed investment decisions, as in Chen et al. (2007). Instead, that Grullon et al. (2015) detect negative abnormal returns only in small firms while we find slightly positive returns for large firms in a longer period[19] further implies that these two effects are essentially different. Nevertheless, we acknowledge the challenge in disentangling output decisions from investments, since the latter can be directed to production levels and both might respond to price informativeness.

5 Robustness

In this section, we assess the robustness of our main Reg SHO results to different specifications. First, we follow related papers and estimate the impact of the Reg SHO using the whole period of the experiment, not just the first part. Second, and more importantly, we further support our baseline findings by leveraging an alternative regulatory intervention.

5.1 Reg SHO: 2001-2008 Sample

As we discuss in Section 3.2, our main regressions using the Reg SHO rely on the first phase of the experiment, when only pilot firms had price tests suspended. Nevertheless, it is

[19] See Appendix A.

important to ensure that our baseline results are obtained in the whole period of the intervention as a way to gauge Reg SHO's overall short run impact on product market composition. To that end, we follow closely other papers that estimate the causal effects of the regulatory change (e.g. Grullon et al. (2015); Fang et al. (2016); Boulatov et al. (2023)).

In this exercise, our sample covers the years of 2001 to 2008. We construct an indicator of treatment that encompasses the removal of price tests for pilot firms during the experiment and for control firms after the experiment. This variable, SHO, indicates that a firm listed in the Russell 3000 index was subject to the removal of the uptick rule for at least seven months of its fiscal year. For pilot firms, this variable equals one in the first fiscal year with at least seven months after August 2004 and onward. For control firms, SHO equals one in the first fiscal year with at least seven months after July 2007—when the repeal of the Reg SHO was announced—and onward. Otherwise, the variable is coded as zero. Hence, since control firms also had price tests lifted in the end of the experiment, they are also considered treated at that time. We use SHO to capture treatment effects in the following specification:

$$Market\ Share_{i,j,t} = \beta SHO_{i,t} + \mu_i + \mu_{j,t} + \epsilon_{i,j,t}. \tag{5}$$

We report the results of the estimation of Equation (5) in Table 15. For consistency and comparison with Grullon et al.'s (2015) results, we use both market shares and investment as dependent variables and split the sample between small and large firms, as described in Sections 3.2 and 3.3. The results show that the removal of short selling constraints lead to an average decrease of 0.148 p.p. in market shares relative to firms with price tests in place. This effect corresponds to 2.16% lower market shares relative to the sample's overall mean during the whole period of the intervention. Again, the result stems solely from large firms, which experienced a highly significant decrease of 0.349 p.p. in their market shares relative to large firms with price tests in place. This estimate corresponds to 3.34% of the average market share of large firms in this sample.

— PLACE TABLE 15 ABOUT HERE —

The results reported in Table 15 also show a significant decrease in investments following the suspension of the uptick rule. However, as in Table 5 and Grullon et al. (2015), the effect stems from small firms, further underscoring crucial differences between the results in Grullon et al. (2015) and ours.

5.2 Alternative Causal Evidence: JGTRRA

In this section, we resort to an alternative regulatory change as an identification strategy to further validate our baseline findings. The event in question is the Job and Growth Tax Relief Reconciliation Act (JGTRRA), passed by the United States Congress on May 2003, constituting a quasi-exogenous negative shock to shorting activity (Thornock, 2013).

5.2.1 Institutional Background

The JGTRRA is a law comprising tax reductions for various income sources, including capital gains. In particular, it decreased the tax rate on qualified dividends substantially and is still effective for taxpayers below the highest federal taxable income bracket (Han et al., 2024). This reduction affected the supply of lendable shares via a differential tax treatment of dividends for stocks on loan. Specifically, dividends directly paid to shareholders are qualified for the tax break. In contrast, dividends passed from stock borrowers to their respective lenders are not, thus incurring a substantially higher tax rate. As such, the regulation discourages equity lending around dividend record dates, reducing the supply of lendable shares and, in turn, limiting shorting activity (Thornock, 2013).

Han et al. (2024) argues that the JGTRRA is an appropriate setup to investigate the causal effects of short selling for two reasons. First, it went from an initial proposal to a signed law in less than five months and thus was relatively unexpected by the market when implemented. Second, there were no other major concurrent changes to the tax law that could potentially threaten identification with confounders. Therefore, the event provides us an empirical framework to investigate a shock to short selling in the opposite direction of Reg SHO.[20]

5.2.2 Sample Construction and Specification

We largely follow Han et al.'s (2024) dividend selection criterion to construct our sample. We consider firms with stock prices above $5 in the months prior to the law implementa-

[20]See Thornock (2013) and Han et al. (2024) for a more detailed discussion of JGTRRA.

tion that paid taxable cash dividends (CRSP distribution code 1232) of \$0.01 or greater to stockholders of ordinary shares listed on NYSE, AMEX, or NASDAQ exchanges. As firm's dividend policies rarely change over time, we consider a firm as treated if it paid dividends to its shareholders in the month of the JGTRRA implementation (Han et al., 2024).

Since changes to market shares take longer to materialize than stock price reactions and for consistency, we consider a period of six years, with the treatment in the fourth year, as in our Reg SHO analysis. Therefore, our sample consists of yearly Compustat observations between 2000 and 2005. We use the same specification as in Equation (3) where $Post$ indicates 2003 and the following years, and $Treated$ is defined as aforementioned. Since the JGTRRA reduced shorting activity of stocks of firms that paid dividends in 2003, we expect the DID coefficient β to be positive, indicating higher market shares for treated firms in the years after the regulatory change. We also estimate a triple difference specification where we interact the DID coefficients with $Large$, an indicator that the firm had assets above the median value of the sample as of 2003. To be consistent with our Reg SHO results, we expect the triple difference coefficient to be positive, reflecting larger gains in market shares by large firms.

A natural concern with this exercise is the time overlap with Reg SHO in 2004 and 2005. As another shock to short selling within the sample period, it can contaminate estimates of the causal effects of the JGTRRA. We consider three cases to address this and test the sensitivity of estimates to the presence of Reg SHO firms. First, we consider all firms in the JGTRRA sample, including those on the Russell 3000 index in 2004. Second, we exclude from the sample only firms in the pilot group of Reg SHO, thus cleaning our estimates of the treatment effects of Reg SHO. Finally, we exclude the Reg SHO firms altogether, whether included in the pilot group or not. The last case is our preferred specification, for it consists of an entirely different sample of firms. Thus, consistent results make a stronger case for validating our main findings. Nevertheless, we also report results using the other two samples in Appendix C for completeness and robustness.

5.2.3 Results

Table 16 reports DID results in our sample, where we exclude all firms listed in the Russell 3000 in 2004, thus having no overlap with the Reg SHO experiment. Columns (1) and (3) show strong, positive effects on market shares of treated firms. With controls included, we estimate that firms for which the dividend tax change disincentivized short selling increased their market shares by 0.938 p.p in the years that followed, which corresponds to 13.3% of the average market share in the sample. Consistent with our Reg SHO results, columns (2) and (4) show that these effects stem solely from large firms.[21] Including controls, the estimated effect corresponds to a 12.1% increase in the market shares of large firms.

— Place Table 16 About Here —

The effects reported in Table 16 are substantial. To assess their robustness, we report results in the samples with different treatments of the Reg SHO firms in Appendix C. Table C.6 excludes from the sample only the Russell 3000 firms included in the pilot group. The results are qualitatively similar with strong statistical significance, although considerably smaller in magnitude. We estimate an average increase of 9.1% in the market shares of all treated firms and 7.9% for large firms. Finally, Table C.7 considers the full sample of firms, with no exclusions. Again, the results are similar, with smaller economic magnitude and lower statistical significance. Here, we find an average increase of 4.8% in overall market shares and 4.6% for large firms.

Overall, the results using the JGTRRA as an alternative empirical strategy strongly support our baseline Reg SHO findings. In addition, they symmetrically provide evidence of our mechanism by showing that hampering short selling can cause large firms to increase output relative to industry peers. Finally, the fact that we can find such evidence on a sample of different firms supports the external validity of our findings in Section 3.4.

6 Concluding Remarks

We study the effects of short interest on firms' product market performance via a managerial monitoring channel. Using both historical data on short positions and two arguably exogenous shocks to short selling, we establish that shorting activity negatively impacts firms' output relative to their industry peers. We show that the sensitivity of market shares to short selling stems from market power and strategic substitution among product market rivals, sug-

[21] In both specifications, the total estimated effect for large firms is statistically significant at the 1% level.

gesting that our baseline results are not driven by downward stock price corrections, bear raids, or other mechanisms typically associated with small, financially constrained firms.

We show that the decrease in market shares of treated firms following Reg SHO was more pronounced for firms with lower stock price informativeness at the time of the intervention. This result only holds for firms with market power in industries where firms compete in strategic substitution. Furthermore, the intervention increased the stock price informativeness of large treated firms. This evidence suggests that the interaction between market power and price opacity generates incentives to engage in aggressive output competition, which short selling pressure attenuates. As a result, larger firms experience sharper downsizing of output levels and improvements in price discovery.

Following previous work, we provide additional evidence that short selling can serve a monitoring role. By emphasizing the context of product market competition, our results lend support to policies that facilitate price discovery in the presence of market power. The intersection between financial feedback effects and product market competition is promising and relatively unexplored, and future research might provide us with a better understanding of how they are intertwined. In this context, two types of analyses are warranted. First, an unified approach that encompasses multiple channels through which shorting activity operates and their respective stock price dynamics. Second, how financial phenomena might affect firms' competitive positions via channels other than investment levels and how to disentangle pure competitive aspects from investment opportunities.

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Table 1: Historical analysis summary Statistics

This table reports summary statistics for the variables used in our historical analysis. The sample covers 103,594 firm-year observations over the period 1973-2018. Our outcome variables are *Market share* (*3-digit SIC*) and *Market share* (*4-digit SIC*), in percentage points. Our proxies for short selling are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*, which are computed monthly and averaged over the fiscal year period. For details on variables construction, see Section 2.1.

Statistic	Mean	Median	St. Dev.	N
Market share (3-digit SIC, %)	9.554	1.389	19.335	103,593
Market share (4-digit SIC, %)	14.775	3.038	24.986	103,587
Short interest/Shares (%)	3.031	1.069	5.121	103,035
Abnormal short interest (%)	−0.142	−0.176	3.445	98,696
Days-to-cover	4.995	2.956	6.078	103,001
Q	1.857	1.368	1.530	98,968
Size	6.338	6.263	2.075	103,594
Cash flow	0.043	0.084	0.215	93,295

Table 2: Short interest and market shares: Historical analysis

This table reports output from the estimation of Equation (1), which measures the historical relationship between short selling activity and market shares. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. Control variables are *Q*, *Size*, and *Cash flow*. See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (3-digit SIC, %)		
	(1)	(2)	(3)
Short interest/Shares	−0.049***		
	(0.010)		
Abnormal short interest		−0.050***	
		(0.010)	
Days-to-cover			−0.021***
			(0.006)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,097	80,070	80,080
R^2	0.962	0.962	0.962

Table 3: Short interest and market shares by size: Historical analysis

This table reports output from the estimation of Equation (2), which measures the historical relationship between short selling activity and market shares across large versus small firms. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Large* is an indicator variable that equals one if a firm is above the median total assets in period $t-1$. Control variables are Q and *Cash flow.* See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (3-digit SIC, %)		
	(1)	(2)	(3)
Short interest/Shares	0.045***		
	(0.009)		
Short interest/Shares × Large	−0.088***		
	(0.015)		
Abnormal short interest		−0.017*	
		(0.010)	
Abnormal short interest × Large		−0.091***	
		(0.018)	
Days-to-cover			0.008
			(0.007)
Days-to-cover × Large			−0.043***
			(0.014)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,097	80,070	80,080
R^2	0.959	0.959	0.959

Table 4: Reg SHO summary statistics

This table reports summary statistics of our Reg SHO sample as of 2004, when the SEC announced the pilot group of Russell 3000 firms that would be exempted from short selling price tests (see Section 3.1). The sample covers a total of 1,885 firms, 603 of which are in the pilot group and 1,182 are in the control group. Our dependent variable is *Market share*, which is relative to 3-digit SIC codes, in percentage points. The table reports descriptive statistics across pilot and control groups. The last column reports *p*-values of t-tests for differences in means. For details on variable definitions and sample constructions, see Section 3.2.

	Pilot group				Control group					
Statistic	Mean	Median	St. Dev.	N	Mean	Median	St. Dev.	N	Diff	p-value
Market share	6.262	1.364	12.326	603	6.476	1.024	13.833	1,182	−0.21	0.74
Q	2.124	1.634	1.598	571	2.190	1.553	2.212	1,109	−0.07	0.48
Total assets	3,130	783	7,612	603	3,464	744	8,508	1,182	−333	0.40
Cash flow	8.340	10.423	18.041	602	7.888	10.150	21.871	1,181	0.45	0.64
HHI	0.158	0.110	0.154	603	0.148	0.109	0.138	1,182	0.01	0.17
CSM3	−0.015	−0.022	0.067	603	−0.012	−0.022	0.071	1,179	0.00	0.43
CSM4	−0.007	−0.015	0.087	602	−0.014	−0.019	0.085	1,178	0.01	0.13

Table 5: Short interest and market shares: Reg SHO

This table reports output from the estimation of Equation (3). The dependent variables are *Market share*, computed relative to 3-digit SIC industries total sales (Compustat's *sale*), and *Investment*, which is Compustat's *capx* scaled by total assets. Both dependent variables are reported in percentage points. The table reports estimates of the differences-in-differences coefficient β. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. No controls are used in these specifications. See Section 3.2 for detailed variables construction. For each dependent variable, we run a regression on the whole sample, on the sample of large firms, and on the sample of small firms. We classify a firm as large if it was above median total assets within the Russell 3000 sample in 2004. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:					
	Market share (%)			Investment (%)		
	All	Small	Large	All	Small	Large
Treated × Post	-0.208^{**}	0.082	-0.440^{***}	-0.666^{**}	-1.517^{***}	−0.476
	(0.103)	(0.111)	(0.131)	(0.263)	(0.423)	(0.363)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	10,673	5,319	5,354	10,575	5,181	5,394
R^2	0.991	0.993	0.995	0.732	0.789	0.800

Table 6: Short interest and market shares by size: Reg SHO

This table reports output from the estimation of Equation (3) and triple differences specifications where we interact *Treated*, *Post*, and *Large*. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004, and *Large* in an indicator that the firm was above median total assets within the Russell 3000 firms in 2004. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are Q, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. Columns (1) and (3) report DID specifications, and columns (2) and (4) reports the triple differences estimates. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)			
	(1)	(2)	(3)	(4)
Treated × Post	−0.208**	−0.068	−0.208**	−0.050
	(0.103)	(0.133)	(0.098)	(0.124)
Treated × Post × Large		−0.392**		−0.434**
		(0.198)		(0.186)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	10,673	10,466	9,649	9,460
R^2	0.991	0.992	0.993	0.994

Table 7: Reg SHO and market shares by product market characteristics.

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and the product market variable of interest. *Treated* is an indicator that equals one if the firm was included in the original pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. In the specifications reported in columns (1) and (2) we use an Herfindahl-Hirschman index (HHI) to measure product market concentration. In columns (3) to (6) our variable of interest in the Competitive strategy measure (CSM) by Sundaram et al. (1996), which measures the degree of complementarity among the actions of firms within an industry (see Section 3.2). In columns (3) and (4) this variable is computed at the 3-digit SIC level, whereas in columns (5) and (6), at the 4-digit SIC level. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.545***	0.336**	−0.090	−0.049	−0.139	−0.097
	(0.184)	(0.148)	(0.111)	(0.093)	(0.103)	(0.090)
Treated × Post × HHI	−5.685***	−4.129***				
	(1.801)	(1.480)				
Treated × Post × CSM3			7.883**	10.260***		
			(3.617)	(3.565)		
Treated × Post × CSM4					5.804**	8.356***
					(2.732)	(2.794)
Controls		✓		✓		✓
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	10,673	9,649	10,655	9,634	10,644	9,623
R^2	0.991	0.993	0.991	0.993	0.991	0.993

Table 8: Reg SHO and market shares by price nonsynchronicity

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and price nonsynchronicity. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. Price nonsynchronicity $(1 - R^2)$ represents firm-years' portion of variation in daily stock returns that is not explained by variation in market returns and firms' 3-digit SIC industries. Control variables are Q, *Size*, and *Cash flow.* Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)	
	(1)	(2)
Treated × Post	−1.665***	−1.606***
	(0.540)	(0.456)
Treated × Post × $(1 - R^2)$	2.235***	2.142***
	(0.724)	(0.616)
Controls		✓
Firm FE	✓	✓
Industry-Year FE	✓	✓
Observations	10,001	9,187
R^2	0.993	0.994

Table 9: Reg SHO and market shares by ITI

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and informed trade intensity (ITI) (Bogousslavsky et al., 2023). The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. ITI is computed as in the day of the Reg SHO announcement. Control variables are Q, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market Share	
	(1)	(2)
Treated × Post	-0.619^{***}	-0.575^{***}
	(0.181)	(0.177)
Treated × Post × ITI	1.545^{***}	1.528^{***}
	(0.506)	(0.498)
Controls		✓
Firm FE	✓	✓
Industry-Year FE	✓	✓
Observations	10,130	9,178
R^2	0.992	0.994

Table 10: Reg SHO and market shares by price nonsynchronicity

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and price nonsynchronicity. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. Price nonsynchronicity $(1 - R^2)$ represents firm-years' portion of variation in daily stock returns that is not explained by variation in market returns and firms' 3-digit SIC industry. We classify a firm as large if it was above median total assets within the Russell 3000 sample in 2004. Control variables included are Q, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share		
	All	Small	Large
Treated × Post	−1.606***	−0.556	−1.936**
	(0.456)	(0.452)	(0.985)
Treated × Post × $(1 - R^2)$	2.142***	0.671	2.579**
	(0.616)	(0.728)	(1.291)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	9,187	4,427	4,714
R^2	0.994	0.996	0.995

Table 11: Reg SHO and market shares by ITI

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and informed trade intensity (ITI) (Bogousslavsky et al., 2023). The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. ITI is computed as in the day of the Reg SHO announcement. We classify a firm as large if it was above median total assets within the Russell 3000 sample in 2004. Control variables included are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share		
	All	Small	Large
Treated × Post	-0.575^{***}	-0.074	-1.033^{***}
	(0.177)	(0.118)	(0.286)
Treated × Post × ITI	1.528^{***}	0.514^{*}	2.710^{***}
	(0.498)	(0.309)	(0.860)
Controls	✓	✓	✓
Observations	9,178	4,572	4,580
R^2	0.994	0.996	0.996

Table 12: Reg SHO and market shares by price nonsynchronicity

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and price nonsynchronicity. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. Price nonsynchronicity ($1 - R^2$) represents firm-years' portion of variation in daily stock returns that is not explained by variation in market returns and firms' 3-digit SIC industries. We consider concentrated industries those with above median Herfindahl-Hirschman index of the sample in 2004. We split our sample according to the sign of the Competitive strategy measure (CSM) by Sundaram et al. (1996), which gauges the nature and intensity of firms interactions within an industry. We split our sample according to CSM values in 2004. As in Chod and Lyandres (2011), we consider industries with positive (negative) CSM values as product markets where firms compete in strategic complements (substitutes). See Section 3.2 for details on the construction of the CSM. We compute this variable at both 3- and 4-digits SIC codes (CSM3 and CSM4, respectively). Control variables included are Q, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market shares					
	Concentration		CSM3		CSM4	
	Low	High	Positive	Negative	Positive	Negative
Treated × Post	−0.354	−3.028***	−0.870	−1.650***	−0.395	−1.891***
	(0.280)	(0.923)	(1.142)	(0.486)	(0.914)	(0.520)
Treated × Post × ($1 - R^2$)	0.444	4.089***	1.043	2.148***	0.383	2.500***
	(0.366)	(1.288)	(1.658)	(0.638)	(1.248)	(0.692)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	4,554	4,633	2,655	6,519	3,362	5,798
R^2	0.979	0.994	0.994	0.994	0.994	0.994

Table 13: Reg SHO and market shares by ITI

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and informed trade intensity (ITI) (Bogousslavsky et al., 2023). The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. ITI is computed as in the day of the Reg SHO announcement. We consider concentrated industries those with above median Herfindahl-Hirschman index of the sample in 2004. We split our sample according to the sign of the Competitive strategy measure (CSM) by Sundaram et al. (1996), which gauges the nature and intensity of firms interactions within an industry. We split our sample according to CSM values in 2004. As in Chod and Lyandres (2011), we consider industries with positive (negative) CSM values as product markets where firms compete in strategic complements (substitutes). See Section 3.2 for details on the construction of the CSM. We compute this variable at both 3- and 4-digits SIC codes (CSM3 and CSM4, respectively). Control variables included are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market shares					
	Concentration		CSM3		CSM4	
	Low	High	Positive	Negative	Positive	Negative
Treated × Post	−0.232	−0.983***	−0.077	−0.480**	−0.215	−0.570**
	(0.183)	(0.348)	(0.293)	(0.198)	(0.255)	(0.224)
Treated × Post × ITI	0.733	2.427***	−0.038	0.994**	0.955	1.146**
	(0.590)	(0.882)	(0.867)	(0.489)	(0.832)	(0.578)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	4,618	4,560	2,654	6,567	3,312	5,840
R^2	0.980	0.994	0.995	0.994	0.995	0.993

Table 14: Reg SHO and price nonsynchronicity

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and *Large*. The dependent variable is price nonsynchronicity $(1-R^2)$, computed as firm-years' portion of variation in daily stock returns that is not explained by variation in market returns and firms' 3-digit SIC industry, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004 and *Large* in an indicator that the firm was above median total assets within the Russell 3000 firms in 2004. Control variables included are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 3.2 and Section 4 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	$1-R^2$ (%)			
	(1)	(2)	(3)	(4)
Treated × Post	−0.042	−2.808***	0.166	−2.121**
	(0.683)	(0.853)	(0.662)	(0.829)
Treated × Post × Large		5.574***		4.749***
		(1.111)		(1.108)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	10,004	9,989	9,198	9,185
R^2	0.813	0.814	0.834	0.835

Table 15: Short interest and market shares: Reg SHO. Sample 2001-2008.

This table reports output from the estimation of a specification where we expand our Reg SHO sample to include 2001-2008. As in Grullon et al. (2015), we consider non-pilot firms to be treated after the repeal of price tests for all firms, on July 2007. Specifically, SHO is an indicator variable that equals one if (*i*) the firm was in the original Reg SHO pilot group and was subject to the suspension of prices tests for at least seven months of its fiscal year, starting from August 2004; or (*ii*) the firm was listed in the Russell 3000 index as of May 2004 and had at least seven months of its fiscal year after July 2007, when the repeal of the program was announced (See Section 3.1 and Section 3.2). The dependent variables are *Market share*, computed relative to 3-digit SIC industries total sales (Compustat's *sale*), and *Investment*, which is Compustat's *capx* scaled by total assets. Both dependent variables are reported in percentage points. See Section 3.2 and Section 5.1 for detailed variables construction. For each dependent variable, we run a regression on the whole sample, on the sample of large firms, and on the sample of small firms. We classify a firm as large if it was above median total assets within the Russell 3000 firms in 2004. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:					
	Market share (%)			Investment (%)		
	All	Small	Large	All	Small	Large
SHO	-0.148^{*}	0.085	-0.349^{***}	-0.534^{**}	-1.370^{***}	-0.182
	(0.081)	(0.099)	(0.114)	(0.219)	(0.341)	(0.342)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	13,899	6,898	7,001	13,784	6,734	7,050
R^2	0.988	0.991	0.992	0.723	0.787	0.779

Table 16: Short interest and market shares by size: JGTRRA

This table reports output from the estimation of Equation (3) and triple differences specifications where we interact *Treated*, *Post*, and *Large* in the context of JGTRRA. *Treated* is an indicator that equals one if the firm paid dividends in 2003. *Post* is an indicator that equals one on and after 2003, and *Large* in an indicator that the firm was above median total assets relative to the Compustat sample in 2003. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are Q, *Size*, and *Cash flow.* Controls are lagged by one period. See Section 2.1, Section 3.2, and Section 5.2 for details on variable construction. Columns (1) and (3) report DID specifications, and columns (2) and (4) reports the triple differences estimates. The sample excludes all the firms that were listed in the Russell 3000 index in 2005. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:			
	Market share (%)			
	(1)	(2)	(3)	(4)
Treated × Post	1.067***	−0.711	0.938***	−1.112
	(0.232)	(0.773)	(0.235)	(0.764)
Treated × Post × Large		1.819**		2.224***
		(0.829)		(0.830)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	12,606	12,606	10,888	10,888
R^2	0.981	0.982	0.984	0.984

Appendix

A Reg SHO, Short Selling and Stock Prices

Using Reg SHO to gauge causal effects of short selling relies on the premise that the intervention actually affected shorting activity. Otherwise, effects might result from reactions to the *threat* of short selling (Fang et al., 2016). In our context, the threat of increased shorting activity could be enough to trigger managerial reactions, which could drive our baseline results and increase price informativeness, as documented in Table 14.

In addition to actual short selling, any impact of the intervention on stock prices is also of interest, as it can help disentangle mechanisms with opposite pricing effects. In this regard, Grullon et al. (2015) document an increase in abnormal short selling as well as negative abnormal returns after the announcement date of Reg SHO, especially for small firms. Still, a similar exercise is important in our context for three reasons. First, to test whether our results are driven by actual shorting activity or the threat of it. Second, because our proposed mechanism might not have negative price effects in the longer run, as opposed to downward correction of mispricing or bear raids. Finally, the variation we capture on our highly saturated regression models might be associated with different effects from what was previously established by the literature.

First, we test for significant changes in short selling after the implementation date of Reg SHO. We estimate a DID model with the proxies for short interest from Section 2 as dependent variables. We use monthly data to better capture the timing associated with the removal of the uptick rule. The control variables are the same as in our main analysis, but at quarterly frequency and lagged by one quarter. We include firm and industry by year-month fixed effects.

We report results on short interest on Table A.1. Columns (1), (3), and (5) show evidence of increased shorting activity after the suspension of price tests. Moreover, columns (2), (4), and (6) show no significant differential effects between small and large firms. These null results somewhat contrast with Grullon et al. (2015), who find typically larger point estimates in their sample of small firms compared to their full sample. Two things could potentially explain the differences across the studies. First, Grullon et al. (2015) center their study around the announcement of the pilot test and do not include the actual implementation on their sample. Thus, it is possible that short selling of large firm stocks' caught up to small firms over time, either by increasing in a lower pace or by responding to the implementation instead of the announcement. Second, there are a number of differences in the specifications used, such as the inclusion of granular fixed effects and firm-level controls.

— Place Table A.1 About Here —

Next, we test for effects on stock prices within our sample. We estimate a DID model with daily abnormal returns—stock returns in excess of CRSP value-weighted portfolio returns—in percentage points as the dependent variable. We include lagged quarterly control variables and firm and industry-by-day fixed effects.

We report results on Table A.2. Column (1) shows no significant changes in stock returns in the full sample. However, column (2) shows that large firms outperformed small firms significantly, generating average daily abnormal returns of 2.9 basis points. These results are consistent with Grullon et al.'s (2015) long-horizon analysis of abnormal returns around Reg SHO, except that we focus on the implementation of the program itself.

— Place Table A.2 About Here —

While there is ample evidence that short interest depresses firm value in the short run, a mechanism that improves resource allocation might lead investors to reassess the value of firms upward in the longer run. Alternatively, short selling might decrease risk-averse, long investors' perceived uncertainty, increasing their demand for the security (Nezafat et al., 2017). Arguably, such mechanism would take longer to manifest in prices than overpricing correction or bear raids. Altogether, the results in Table A.1 and Table A.2 speak to allocational improvements. Whereas we detect increased short selling activity across both small and large firms following the removal of a short selling constraint, the value of equity responded differently. We find that large firms' gained value (at worst, did not lose value), while small firms did not, consistent with Grullon et al. (2015). In practice, we recognize that multiple channels might operate simultaneously. Completely disentangling the channels through which shorting activity operates and their respective dynamic pricing effects is a task we do not undertake in this paper.

Table A.1: Reg SHO and short selling

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and *Large*. The dependent variables are monthly *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one after May of 2005. *Large* is an indicator that the firm was above median total assets within the Russell 3000 firms in 2004. Control variables included are quarterly *Q*, *Size*, and *Cash flow*. Controls are lagged one quarter. See Section 2.1 and Section 3.2 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year-month fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:					
	Short interest/Share (%)		Abnormal short interest (%)		Days-to-cover	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.478**	0.529*	0.492**	0.546*	0.423*	0.291
	(0.195)	(0.280)	(0.195)	(0.280)	(0.253)	(0.344)
Treated × Post × Large		0.002		−0.003		0.416
		(0.406)		(0.407)		(0.498)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year-Month FE	✓	✓	✓	✓	✓	✓
Observations	88,655	88,655	88,655	88,655	88,651	88,651
R^2	0.737	0.739	0.563	0.567	0.475	0.477

Table A.2: Reg SHO and abnormal returns

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and *Large*. The dependent variable is *Abnormal Returns*, which is stocks' daily returns net of the CRSP value-weighted returns, in percentage points. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one after May 2, 2005. *Large* in an indicator that the firm was below median total assets within the Russell 3000 firms in 2004. Control variables included are quarterly *Q*, *Size*, and *Cash flow*. Controls are lagged one quarter. See Section 2.1, Section 3.2, and Appendix A for details on variable construction. The regressions are estimated via OLS and include firm fixed and industry-day fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:	
	Abnormal Returns (%)	
	(1)	(2)
Treated × Post	0.005	−0.013
	(0.010)	(0.017)
Treated × Post × Large		0.041**
		(0.017)
Controls	✓	✓
Firm FE	✓	✓
Industry-Day FE	✓	✓
Observations	3,314,261	3,251,348
R^2	0.045	0.047

B Robustness: Firm Fixed Effects

So far, we included firm fixed effects in all our specifications to control for unobservable firm-level, time-invariant characteristics. On one hand, controlling for such factors is important to avoid confounding the estimates. However, the fixed effects have high explanatory power on our baseline regressions, suggesting that market shares tend to be stable within firms and across years. Thus, it is important to ensure that our results are not driven by saturated specifications, where only a small fraction of the variation in market shares is left to be explained by the treatment.

To assess the robustness of our results with respect to the explanatory power of firm-level dummies, we estimate our main historical and Reg SHO specifications without firm fixed effects. Table B.1 reports the estimation of Equation (1) for our three measures of shorting activity. The results confirm the negative association between shorting interest and product market performance. The estimates in column (1) imply that a one s.d. in *Short interest scaled by shares* is associated with a 1.09 p.p. lower market share, which corresponds to a 11.4% decrease of its average value. In addition, a one s.d. increase in *Abnormal short interest* and *Days-to-cover* are associated with 1.68% and 7.37% lower market shares, respectively.[22]

— Place Table B.1 About Here —

Table B.2 reports the output of the estimation of Equation (2). Again, we find inconclusive evidence of a relationship between short short selling and product market performance of small firms. In contrast, the interaction terms point to a significantly more negative association for large firms. Again, we find that the total estimates for large firms are statistically different from zero at the 1% level in all specifications. The coefficients suggest that a one s.d. increase in *Short interest scaled by shares* is associated with 11.3% lower market shares of large firms. For *Abnormal short interest* and *Days-to-cover*, these figures are 2.53% and 12.8%, respectively.

— Place Table B.2 About Here —

[22] Note that the estimated effect of changes in *Abnormal short interest* is largely unaffected by removing firm fixed effects on the specifications. This is due to the fact that firm fixed effects are used to capture the unexpected component of short interest.

Next, we estimate Equation (3) without including firm fixed effects. We perform the same heterogeneity analysis as of Section 3.3, where we interact $Treated \times Post$ with our variables of interest to assess how our baseline effect responds to firms' product market competitive characteristics.

We report our main results and the heterogeneity by firm's size in Table B.3.[23] The results are qualitatively similar to those where we include firm fixed effects. In column (3), where we report the DID estimator with controls included, we estimate that pilot firms saw an average 0.214 p.p. decrease in market shares relative to control firms after the first wave of price tests suspension. This effects corresponds to 3.21% lower market shares of the average firm. In column (4), where we also report the coefficient of the triple interaction with $Large$, we can see that the results are indeed driven by large firms. The point estimates reported imply that large firms saw a decrease of 8.93% in their average market share.

— Place Table B.3 About Here —

Finally, Table B.4 reports results of the heterogeneity analysis by HHI and CSM, as in Section 3.3. Overall, the coefficients are consistent with those on Table 7, albeit the point estimates of the triple interactions with CSM measures are smaller in magnitude and statistical significance. The coefficients on column (2) imply that a one s.d. in concentration is associated to 0.615 p.p. lower market shares following treatment. The estimate in column (4) suggest that a one s.d. lower $CSM3$ by the time of the experiment led to 0.714 p.p. lower market shares.

— Place Table B.4 About Here —

[23] For completeness and robustness purposes, Table C.5 report estimates of Equation (3) by splitting the sample between small and large firms.

Table B.1: Short interest and market shares: Historical analysis. No firm fixed effects

This table reports output from the estimation of Equation (1), which measures the historical relationship between short selling activity and market shares. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. Control variables are *Q*, *Size*, and *Cash flow.* See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (3-digit SIC %)		
	(1)	(2)	(3)
Short interest/Shares	-0.216^{***}		
	(0.021)		
Abnormal short interest		-0.046^{***}	
		(0.018)	
Days-to-cover			-0.116^{***}
			(0.015)
Controls	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,097	80,070	80,080
R^2	0.750	0.748	0.750

Table B.2: Short interest and market shares by size: Historical analysis. No firm fixed effects.

This table reports output from the estimation of Equation (2), which measures the historical relationship between short selling activity and market shares across large versus small firms. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Large* is an indicator variable that equals one if a firm is above the median total assets in period $t-1$. Control variables are Q and *Cash flow.* See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (3-digit SIC, %)		
	(1)	(2)	(3)
Short interest/Shares	0.158***		
	(0.024)		
Short interest/Shares × Large	−0.480***		
	(0.042)		
Abnormal short interest		−0.019	
		(0.024)	
Abnormal short interest × Large		−0.088**	
		(0.035)	
Days-to-cover			0.058***
			(0.016)
Days-to-cover × Large			−0.364***
			(0.038)
Controls	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,097	80,070	80,080
R^2	0.708	0.704	0.707

Table B.3: Short interest and market shares by size: Reg SHO. No firm fixed effects.

This table reports output from the estimation of Equation (3) and triple differences specifications where we interact *Treated*, *Post*, and *Large*. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004, and *Large* in an indicator that the firm was above median total assets within the Russell 3000 firms in 2004. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. Columns (1) and (3) report DID specifications, and columns (2) and (4) reports the triple differences estimates. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)			
	(1)	(2)	(3)	(4)
Treated × Post	−0.192*	0.553*	−0.214*	0.476
	(0.113)	(0.310)	(0.126)	(0.302)
Treated × Post × Large		−1.651***		−1.362***
		(0.509)		(0.440)
Controls			✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	10,673	10,465	9,649	9,454
R^2	0.684	0.744	0.780	0.791

Table B.4: Reg SHO and market shares by product market characteristics. No firm fixed effects.

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and the product market variable of interest. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. In the specifications reported in columns (1) and (2) we use an Herfindahl-Hirschman index (HHI) to measure product market concentration. In columns (3) to (6) our variable of interest in the Competitive strategy measure (CSM) by Sundaram et al. (1996), which measures the degree of complementarity among the actions of firms within an industry (see Section 3.2). In columns (3) and (4) this variable is computed at the 3-digit SIC level, whereas in columns (5) and (6), at the 4-digit SIC level. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. The regressions are estimated via OLS and include industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.537***	0.392*	−0.078	−0.116	−0.126	−0.156
	(0.203)	(0.218)	(0.123)	(0.134)	(0.114)	(0.126)
Treated × Post × HHI	−5.515***	−4.273**				
	(1.988)	(1.910)				
Treated × Post × CSM3			7.732*	6.967*		
			(3.982)	(3.837)		
Treated × Post × CSM4					5.595*	5.111*
					(3.012)	(2.956)
Controls		✓		✓		✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	10,673	9,649	10,655	9,634	10,644	9,623
R^2	0.692	0.788	0.676	0.778	0.668	0.772

C Additional Tables

Table C.1: Short interest and market shares: Historic analysis. 4-digit SIC market shares.

This table reports output from the estimation of Equation (1), which measures the historic relationship between short selling activity and market shares. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 4-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. Control variables are *Q*, *Size*, and *Cash flow*. See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (4-digit SIC, %)		
	(1)	(2)	(3)
Short interest/Shares	-0.049^{***}		
	(0.012)		
Abnormal short interest		-0.051^{***}	
		(0.012)	
Days-to-cover			-0.019^{**}
			(0.008)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,095	80,068	80,078
R^2	0.963	0.963	0.963

Table C.2: Short interest and market shares by size: Historic analysis. 4-digit SIC market shares

This table reports output from the estimation of Equation (2), which measures the historic relationship between short selling activity and market shares across large versus small firms. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 4-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Large* is an indicator variable that equals one if a firm is above the median total assets in period $t-1$. Control variables are Q and *Cash flow*. See Section 2.1 for details on variable construction. All explanatory variables are lagged by one period. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (4-digit SIC, %)		
	(1)	(2)	(3)
Short interest/Shares	0.085***		
	(0.012)		
Short interest/Shares × Large	−0.125***		
	(0.018)		
Abnormal short interest		−0.017	
		(0.013)	
Abnormal short interest × Large		−0.110***	
		(0.022)	
Days-to-cover			0.018**
			(0.009)
Days-to-cover × Large			−0.049***
			(0.017)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,095	80,068	80,078
R^2	0.959	0.959	0.959

Table C.3: Reg SHO and market shares by industry concentration.

This table reports output from the estimation of Equation (3) on samples of high and low concentration industries. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. We consider concentrated industries those with above median Herfindahl-Hirschman index of the sample in 2004. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)		
	Product market concentration		
	All	High	Low
Treated × Post	−0.208**	−0.424**	−0.021
	(0.098)	(0.203)	(0.060)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	9,649	4,788	4,861
R^2	0.993	0.993	0.976

Table C.4: Reg SHO and market shares by Competitive Strategy Measure.

This table reports output from the estimation of Equation (3) on samples of industries in which firms compete in strategic substitutes versus industries where firms compete in strategic complements. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. We split our sample according to the sign of the Competitive strategy measure (CSM) by Sundaram et al. (1996), which gauges the nature and intensity of firms interactions within an industry. We split our sample according to CSM values in 2004. As in Chod and Lyandres (2011), we consider industries with positive (negative) CSM values as product markets where firms compete in strategic complements (substitutes). See Section 3.2 for details on the construction of the CSM. We compute this variable at both 3- and 4-digits SIC codes (CSM3 and CSM4, respectively). The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1 and Section 3.2 for details on variable construction. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share (%)				
		CSM3		CSM4	
	All	Positive	Negative	Positive	Negative
Treated × Post	−0.208**	−0.162	−0.257**	−0.057	−0.308**
	(0.098)	(0.191)	(0.114)	(0.144)	(0.129)
Controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓
Observations	9,649	2,724	6,910	3,420	6,203
R^2	0.993	0.995	0.993	0.994	0.992

Table C.5: Short interest and market shares: Reg SHO. No firm fixed effects.

This table reports output from the estimation of Equation (3). The dependent variables are *Market share* in percentage points, computed relative to 3-digit SIC industries total sales (Compustat's *sale*) The table reports estimates of the differences-in-differences coefficient β. *Treated* is an indicator that equals one if the firm was included in the original Reg SHO pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. See Section 3.2 for detailed variables construction. For each dependent variable, we run a regression on the whole sample, on the sample of large firms, and on the sample of small firms. We classify a firm as large if it was above median total assets within the Russell 3000 firms in 2004. The regressions are estimated via OLS and include industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Market share, (%)			
	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Treated × Post	0.249	-0.991^{***}	0.225	-0.866^{**}
	(0.184)	(0.305)	(0.189)	(0.355)
Controls			✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	5,313	5,360	4,925	4,724
R^2	0.922	0.795	0.935	0.876

Table C.6: Short interest and market shares by size: JGTRRA. No Reg SHO treated firms.

This table reports output from the estimation of Equation (3) and triple differences specifications where we interact *Treated*, *Post*, and *Large* in the context of JGTRRA. *Treated* is an indicator that equals one if the firm paid dividends in 2003. *Post* is an indicator that equals one on and after 2003, and *Large* in an indicator that the firm was above median total assets relative to the Compustat sample in 2003. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1, Section 3.2 and Section 5.2 for details on variable construction. Columns (1) and (3) report DID specifications, and columns (2) and (4) reports the triple differences estimates. The sample excludes all the firms that were on Reg SHO's pilot group in 2004. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:			
	Market share (%)			
	(1)	(2)	(3)	(4)
Treated × Post	0.531***	−0.364	0.520***	−0.492
	(0.143)	(0.358)	(0.148)	(0.361)
Treated × Post × Large		0.901**		1.111***
		(0.395)		(0.404)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	19,865	19,865	17,330	17,330
R^2	0.985	0.985	0.987	0.987

Table C.7: Short interest and market shares by size: JGTRRA. All firms

This table reports output from the estimation of Equation (3) and triple differences specifications where we interact *Treated*, *Post*, and *Large* in the context of JGTRRA. *Treated* is an indicator that equals one if the firm paid dividends in 2003. *Post* is an indicator that equals one on and after 2003, and *Large* in an indicator that the firm was above median total assets relative to the Compustat sample in 2003. The dependent variable is *Market share*, computed as the share of a firm's sales (Compustat's *sale*) relative to their 3-digit SIC industries, in percentage points. Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged by one period. See Section 2.1, Section 3.2 and Section 5.2 for details on variable construction. Columns (1) and (3) report DID specifications, and columns (2) and (4) reports the triple differences estimates. The regressions are estimated via OLS and include firm and industry-year fixed effects, where industries are defined as 4-digit SIC codes. Standard Errors clustered at the firm level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable:			
	Market share (%)			
	(1)	(2)	(3)	(4)
Treated × Post	0.277**	−0.350	0.240**	−0.450*
	(0.112)	(0.255)	(0.116)	(0.264)
Treated × Post × Large		0.632**		0.771**
		(0.290)		(0.303)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	23,529	23,529	20,651	20,651
R^2	0.983	0.983	0.984	0.984