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 the Reg SHO, we find that higher short interest leads to lower market shares, especially in large firms. Our Reg SHO results are also stronger in concentrated industries and industries where firms compete in strategic substitutes. Further tests show that these effects are driven by low stock price informativeness. The evidence suggests that the interaction between market power and price opacity generates incentives for overproduction, which is attenuated by short selling threats. Our results lend support to 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 via the managerial disciplining and learning channels (Chen et al., 2007; Foucault and Fresard, 2014; Boulatov et al., 2019; Tsai et al., 2021; Deng et al., 2022). 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 sale threats on firms' product market performance via a managerial monitoring channel and the role of competitive interactions. We use both historical data and a well-known regulatory change conducted by the US Securities and Exchange Commission: Regulation SHO (hereafter Reg SHO) that removed short selling constraints for a random sample of US firms. By comparing firms within the same product markets and years, we provide novel, robust evidence that short selling lead to lower market shares of sales. Interestingly, our results in both analyses stem exclusively from large firms. In our Reg SHO exercise, results are also stronger in concentrated product markets and industries where firms compete in strategic substitutes (Sundaram et al., 1996; Chod and Lyandres, 2011). Overall, our baseline analysis shows that the feedback effects from financial to product markets strongly depend on the presence of 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 selling restrictions might lead to overvaluation by limiting the transmission of negative information via stock prices, keeping the cost of capital artificially low

(Grullon et al., 2015). Hence, removing such restrictions would lead to a downward correction that reverberates in investments and output. Second, easier short selling might leave firms more exposed to bear raids that can drive stock prices down regardless of economic fundamentals by leading managers to withhold value-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., 2019). Finally, short interest can prevent managers from undertaking policies based on empire building motives and overly optimistic expectations, thus serving a monitoring and disciplining role (Fang et al., 2016; Deng et al., 2020, 2022).

While completely 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). Of note, Grullon et al. (2015) document that small firms experienced negative abnormal returns and lower investments following Reg SHO. Thus, it is unlikely that our findings are a mere byproduct of lower investments. Whereas shorting activity affects access to external capital, hampering investment (Turkiela, 2019), this is less likely to be a binding constraint on large firms, and empirical evidence shows that investment of large firms is less responsive to stock mispricing (Bakke and Whited, 2010). Since the worse product market performance of large firms in the wake of the intervention is not detrimental to their valuation (Grullon et al., 2015), our results are inconsistent with the mispricing and bear raids hypotheses.

Next, we show that our baseline effects are stronger in stocks that were less informative about firms' fundamentals at the time of Reg SHO, indicating the role of the informational content in short selling activity. Since this is consistent with both the managerial learning and disciplining hypotheses, and firms with market power have more informative stock prices (Peress, 2010), it is crucial to disentangle these channels as much as possible. Importantly, 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., 2022), especially when stock prices convey little information. In addition, it is unlikely that managerial learning would be stronger along these dimensions. Therefore, our evidence suggests that market power and price opacity amplify each other in shaping aggressive output policies, consistent with a managerial disciplining channel in which short selling threats attenuate these incentives and lead to downward adjustments in output levels.

We start our analysis by quantifying the historic association between short selling activity and firms' product market shares since 1973. Controlling for unobserved time-varying shocks specific to each sector as well as time-invariant firm characteristics, we find that shorting activity significantly predicts lower market shares. Crucially, this result stems from *large* firms, where a one standard deviation increase in short selling is associated with a 0.220-0.397 percentage point decrease in market share, depending on the measure of short selling used. These estimates represent 1.51-2.73% 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., 2022). As a result, our historical analysis could be spuriously capturing active traders' sentiment towards firms—especially large ones—through time. We address such concerns by resorting to Reg SHO. The regulation's pilot program, announced on July 2004, relaxed short selling constraints on a number of randomly selected U.S firms listed at the Russell 3000 index.[1] As the program consisted of an exogenous shock that facilitated short selling on treated firms (Grullon et al., 2015), it can be used to identify causal effects of short selling on outcomes of pilot firms. Using difference-in-differences and triple differences specifications, we estimate the overall impact of the program on pilot firms' market shares and its sensitivity to cross-sectional firm and product market characteristics at the time of the intervention.

Our results show that the suspension of short selling constraints led to a decline in market shares 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

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

pilot firms, which experienced an average 5.12% reduction in market shares as a result of 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.[2] Furthermore, we show that our baseline results are driven by firms in highly concentrated industries. Overall, our findings suggest that product market adjustments following short selling threats are larger in the presence of monopolistic rents.

We also examine the importance of the degree of strategic substitution among firms' actions. If our results reflect output adjustments from managerial disciplining, they should be amplified in industries where firms compete in strategic substitutes, where empire building motives generate more incentives to engage in aggressive output policies (Sundaram et al., 1996; Fresard and Valta, 2016; Lin et al., 2019). Conversely, the effect should be attenuated in industries where firms compete in strategic complements, as shocks to firms' sales propagate in the same direction to their peers. Consistent with this argument, we find more pronounced effects in product markets with greater degree of strategic substitution.

We inspect whether the information stemming from shorting activity is driving our baseline results by using two measures of stock prices informativeness, following Chen et al. (2007) and Parajuli (2022). The first measure, price nonsynchronicity (Roll, 1988), reflects the variation of stock returns that cannot be explained by variations in the returns of the market and the firm's respective industry. The second measure, probability of informed trading (PIN), proposed by Easley et al. (1996a,b, 1997, 1998), captures the likelihood that a stock's trading stems from informed trades. Hence, both these measures proxy for the amount of private information contained in a firm's stock price. In both cases, we find sharper decreases in market shares for firms with lower prices informativeness at the time of the intervention. More importantly, we find that treatment effect responds to *ex ante* stock price informativeness *only* in large firms, concentrated industries, and industries where firms compete in strategic substitutes. Thus, the evidence indicates that competitive aspects are crucial determinants of the sensitivity of firms' product market performance to the information released by short selling.

Finally, we address recent concerns raised by Heath et al. (2022) about the repeated use

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

of natural experiments such as the Reg SHO for causal inference. Although our baseline specifications are more rigorous than the typical Reg SHO analyses due to the inclusion of industry-year fixed effects, we conduct a series of additional tests to assess the robustness of our baseline results. Of note, we follow an alternative empirical formulation commonly used in the literature and confirm our main findings. We also show that our results obtain without the high explanatory power of firm fixed effects, showing that our findings are not particular to the highly saturated specifications that we use in our main analysis. In addition, our cross-sectional exercises rule out the main competing hypothesis: that the effects in market shares were caused by the changes in investment documented by Grullon et al. (2015). Finally, we show that our results are robust to multiple alternative specifications in the appendix.

Overall, our collective evidence is consistent with a managerial disciplining channel of short selling. Whereas product market competition and price opacity promote incentives to pursue aggressive output policies, these incentives can be alleviated by short selling threats, which is the adjustment we capture in our tests. Thus, our findings imply that short selling can be a substitute for competitive pressure in terms of modulating empire-building motives in product market strategies. Contrasting our results with previous literature suggests that these product market adjustments were were not value destroying (Grullon et al., 2015), further supporting our monitoring hypothesis.

A large body of literature examines the real effects of short selling (Goldstein and Guembel, 2008; Massa et al., 2015a; Hope et al., 2017) and, more generally, financial feedback and management disciplining (Dow and Gorton, 1997; Brav et al., 2008, 2015; Ordóñez-Calafí and Bernhardt, 2022). In addition to the direct impact of the Reg SHO on investments of small firms reported by Grullon et al. (2015), Boulatov et al. (2019) examine the relationship between short selling and investments more comprehensively. The authors provide extensive evidence that managerial learning of pessimistic sentiment by traders drives the negative effects of short selling on investments. In contrast, Deng et al. (2022) conduct a similar exercise in a non-US sample and also show that reducing short sales constraints lead to lower stock prices and investment levels. However, their results are driven by *large* firms, suggesting that short selling prevents non-US financially unconstrained firms from overinvesting. Our paper contributes to this literature by revealing how large firms can be affected by short selling threats via a channel other than investment decisions, and the role of product market aspects. Or results reinforce the idea that market performance is not a simple byproduct of corporate investment and responds to different incentives (Gutiérrez and Philippon, 2017).

We also contribute to the empirical literature on how financial phenomena affect firms' product market performance. Opler and Titman (1994) show that highly leveraged firms tend to lose market share to rivals during industry downturns. More recently, Fresard (2010) and Cookson (2017, 2018) provide evidence that financially sound firms are better able to gain market share at the expense of rivals and to deter entry of potential competitors. We contribute by unveiling how short interest can affect product market performance via competitive and informational channels. Our evidence suggests that shorting threats lead managers to internalize the product market consequences of their output policies even in concentrated industries (Hoberg and Phillips, 2010), when there is price opacity.

Short selling regulations must strike a balance between preventing manipulation (Goldstein and Guembel, 2008; Matta et al., 2023) and hampering informed short selling and its monitoring role (Karpoff and Lou, 2010; Deng et al., 2020, 2021, 2022). In contrast to the investment sensitivity to short selling previously documented by the literature, our results are stronger for firms with characteristics not typically associated with feedback effects, suggesting that short selling modulates product market competition incentives when there is price opacity. Thus, our findings lend support to policies that incentivize informed trading such as strict disclosure requirements, especially 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 our historical analysis of the correlation between the short selling and product market performance. Section 3 uses the Reg SHO intervention to estimate causal effects of short selling on market shares and establish our baseline results. On Section 4, we test whether our Reg SHO results are driven by an informational channel. We report robustness checks 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 is 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.[3] 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 relative to 3-digit SIC industries.[4] Firm-year control variables follow Boulatov et al. (2019) 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 total 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. (2019), 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*,

[3]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.

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

attempts to capture the unexpected component of short interest. Specifically, we follow Karpoff and Lou (2010) and Boulatov et al. (2019) 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 —

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 $MarketShare_{i,j,t}$ 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 absorbs 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.[5] 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:

$$Market\ Share_{i,j,t} = \alpha Small_{i,t-1} + \beta SI_{i,t-1} + \delta SI_{i,t-1} \times small_{i,t-1} + \gamma X_{i,t-1} + \mu_i + \mu_{j,t} + \epsilon_{i,j,t} \quad (2)$$

where $Small_{i,t-1}$ is an indicator that equals one when firm i is below the median firm size in year $t-1$. In these specifications, we omit $Size$ as a control variable as it is highly correlated with $Small$.[6] Here, β estimates the relationship between shorting activity and market shares of large firms, while the coefficient of the interaction, δ, estimates differential effects for small firms. A negative value β and a positive value of δ indicates that short selling negatively predicts product market performance of large firms, but less so for 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 2.6% 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 1.8% and 1.3% lower market shares, respectively.[7] All specifications in Table 2 include controls as well as firm and industry-year fixed effects.

— Place Table 2 About Here —

[5]Results are qualitatively similar if we use 3-digit SIC industries instead.

[6]Results are qualitatively similar if we include both control variables.

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

Table 3 reports estimates of β and δ in Equation (2). The estimated effect of short selling on large firms, β, is negative and statistically significant across all specifications. Results in column (1) imply that an one s.d. increase in short interest scaled by shares is associated with 0.230 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 are associated with 2.55% and 1.48% decrease in market shares, respectively. This relationship is largely attenuated—or even reversed—for small firms, as shown by the positive, significant estimates of δ.

— 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 (2022)), no association with product market performance has been previously established. In addition, consequences 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., 2022).

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 cannot be explained by firm-level characteristics or sector-specific yearly shocks. Whereas interesting from a prediction power perspective, one cannot claim causality based on these results, as they do not rule out endogeneity concerns. For instance, active traders might follow changes in firms' fundamentals over time to successfully predict worse performance relative to industry peers. In that case, our results in Section 2.2 would be reflecting 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 be reflecting 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 on relaxing a short selling constraint on a random sample of firms. The restriction revoked is usually referred to as the uptick rule, a price test that prohibited short sale orders to be 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). On 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 picked every third firm. On 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 for the purpose of estimating causal effects of short selling, which drew concerns about the validity of the results. Heath et al. (2022) argue that reusing natural experiments to estimate effects on various outcome variables 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. (2022) 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 was observed, they stress the need of 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.,Khanna and Sonti (2004); Goldstein and Guembel (2008); Goldstein et al. (2013); Edmans et al. (2015); Dow et al. (2017); Edmans et al. (2017); Terovitis and Vladimirov (2020); Matta et al. (2023)) that discuss how secondary financial markets can affect real outcomes via various channels. In addition, multiple papers lend support to 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. (2020)).

Second, when conducting new tests, one should take into account 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. (2022)) due to the inclusion of industry-year fixed effects.[8] Thus, to the extent that our estimates rely only on within industry-year variation, they're not directly comparable to previous ones. Still, considering the many instances in which the Reg SHO was used to measure causal effects of short selling, we acknowledge the reliability limitations of reusing it to gauge treatment effects on additional outcomes. This concern is partially alleviated due to our focusing mostly on cross-sectional heterogeneous effects, and the fact that these estimates are strongly significant.

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, one could argue that 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 effect of short selling that affects small and constrained firms more strongly.

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

3.2 Sample Construction

In this exercise, 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 nonpilot firms. Thus, we believe 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. (2019)) 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 $small_i$ as an indicator variable that equals one when firm i was below median assets of the Compustat universe in 2004.[9] We fix this variable at the time of the treatment of pilot firms to avoid possible confounding factors stemming for direct 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

[9]Our results are qualitative similar if we define *small* within the Russell 3000 sample of firms.

(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 interaction in each industry. Specifically, 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 this variable 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.[10]

— Place Table 4 About Here —

[10] 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.

3.3 Specification

In our first exercise with the 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.[11] $X_{i,t-1}$ 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.[12] In Section 5, 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 nonpilot firms *within the same industry*. A 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 characteristic at issue.

[11] 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)

[12] 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

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 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 0.461 p.p. in market shares, which corresponds to a 5.12% decrease in the market share of the average large firm.

— 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 *small*. 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 0.374 p.p. market share, which corresponds to a 4.17% decrease relative to the mean of large firms. On the other hand, the positive coefficients of the triple differences term imply strong attenuation of this effect on small firms. In fact, 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 3.4). 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 the Appendix, 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 A.3 and A.4. The estimates confirms 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.

4 Price Informativeness

Our results on Section 3.4 are unlikely to reflect overvaluation or the threat of bear raids, as 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 threats, 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 and stock prices.

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, as the experiment increased the threat of short selling for treated firms (Fang et al., 2016), we should observe stronger effects where prices had less private content up to the treatment, enabling unpunished overreach.

To test this hypothesis, we follow Chen et al. (2007) and Parajuli (2022) 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 a market-related variation, an 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 variations.

The second proxy for price informativeness is *Probability of Informed Trading, (PIN)*, developed by Easley et al. (1996a,b, 1997, 1998). The measure is based on a structural market microstructure model where each trade might have originated at noise traders or informed traders. Simply put, the underlying reasoning is that, as uninformed buy and sell orders arrive independently, stocks with low informed trading will have a relatively balanced number of buy and sell orders within a trading day. Conversely, high fluctuations of daily buy and sell orders are more likely to reflect informed trading. Based on this notion, the authors build a likelihood function for a sequence of trading days and derive a stock-level probability of informed trading. This measure was later extended by Venter and De Jongh (2006), where the authors consider that informed trading might have an impact on uninformed trade patterns. As the authors show, this extension improves the measure's fit to empirical data, and is therefore the version we use in our tests.[13] In our sample, the mean value of PIN is 15.17%.

We explore cross-sectional variation in $(1 - R^2)$ and PIN at the time of the experiment in various tests. First, we conduct a heterogeneity analysis similar to that of Section 3.4, 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

[13]Results are qualitatively similar if we use the original measure

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 PIN is associated with 0.20 p.p. lower market shares.

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.[14] In Table 10 and Table 11, we report results across small and large firms for $(1-R^2)$ and PIN, 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 PIN is commensurate with 0.22 p.p lower market shares. We find no responsiveness to price informativeness across small firms.

Finally, we split our sample by product market characteristics. Table 12 and Table 13 report the results for $(1-R^2)$ and PIN 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 PIN. For concentrated industries, a one s.d. decrease in $(1-R^2)$ and PIN is associated with 0.82 and 0.52 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 PIN, these estimates are 0.24 p.p. for both measures of CSM.

Our collective evidence implies that price informativeness plays a meaningful role in how

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

short selling interacts with product market performance. These findings suggest that the sensitivity of market shares to short interest is driven by 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. Since there is no reason to expect that a managerial learning channel 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.

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, we address the high explanatory power of firms' fixed characteristics in our main regressions by replicating our baseline results without firm fixed effects.

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 important to ensure that our baseline results obtain in the whole period of the intervention as a way to gauge Reg SHO's overall short run impact on product market composition. To do this, 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. (2019); Chu et al. (2021)).

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 14. 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 3.4. The results show that the removal of short selling constraints lead to an average decrease of 0.141 p.p. in market shares relative to firms with price tests in place. This effect corresponds to 2.11% 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 0.324 p.p. in their market shares relative to large firms with price tests in place. This estimate corresponds to 3.49% of the average market share of large firms in this sample.

The results reported in Table 14 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 result stems from small firms, which experienced a decrease in investments of roughly three times that of large firms. Hence, our main results obtain when considering the whole period of the intervention. In addition, the fact that we replicate results previously documented in the literature when considering both the first wave and the whole period of the Reg SHO further validates our empirical approach.

5.2 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 a 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 variation in market shares is left to be explained by shorting activity.

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 15 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.[15]

Table 16 reports the output of the estimation of Equation (2). Again, we find a strong negative relationship between short short selling and product market performance of large firms. The estimates 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.

Next, we estimate Equation (3) without including firm fixed effects. We perform the same heterogeneity analysis as of Section 3.4, 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 17.[16] 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 firm after the first wave of price tests suspension. This effects corresponds to 3.33% lower market shares of the average firm. In

[15]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.

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

column (4), where we also report the coefficient of the triple interaction with $small$, we can see that the results are indeed driven by large firms, with small firms experiencing virtually no effect of the Reg SHO as compared to their control counterparts. The point estimates reported imply that large firms saw a decrease of 4.93% in their average market share.

Finally, Table 18 reports results of the heterogeneity analysis by HHI and CSM, as in Section 3.4. 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 subsequently.

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 Reg SHO, we establish that shorting activity negatively impacts firms' output relative to their industry peers in the form of lower shares of sales. Next, we show that the sensitivity of market shares to short selling stems from market power and strategic substitution among product market rivals, suggesting that our baseline results are not driven by downward stock price corrections or bear raids.

We show that the decrease in market shares of treated firms following Reg SHO was sharper for firms with lower stock price informativeness at the time of the intervention. Interestingly, this result only holds for firms with market power in industries where firms compete in strategic substitution. Therefore, our evidence suggests that the interaction between market power and price opacity generates incentives to engage in aggressive output competition, which are attenuated by short selling threats. As a result, firms that face less product market competition experience more pronounced downsizing of output levels following short sale threats.

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 are relevant for the design of short selling regulations. In particular, our results lend support to policies that facilitate price discovery in the presence of market power such as strict disclosure requirements for large firms and concentrated industries. We believe that 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.

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Table 1: Historic 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*), which are reported in percentages. 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: Historic analysis

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 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 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: Historic analysis

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 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Small* is an indicator variable that equals one if a firm is below 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 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	-0.043^{***}		
	(0.013)		
Short interest/Shares × small	0.088^{***}		
	(0.015)		
Abnormal short interest		-0.108^{***}	
		(0.016)	
Abnormal short interest × small		0.091^{***}	
		(0.018)	
Days-to-cover			-0.035^{***}
			(0.012)
Days-to-cover × small			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 outcome variables is *Market share*, which is relative to 3-digit SIC codes, reported in percentages. The table reports descriptive statistics across pilot and control groups. The last column reports p-values of t tests for differences of 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* in percentage points, computed relative to 3-digit SIC industries total sales (Compustat's *sale*), and *Investment*, which is Compustat's *capx* scaled by total assets. 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 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 small if it was below median total assets relative to the Compustat 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**	0.050	−0.461***	−0.666**	−1.449***	−0.304
	(0.103)	(0.064)	(0.147)	(0.263)	(0.530)	(0.333)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	10,673	3,872	6,751	10,575	3,823	6,702
R^2	0.991	0.995	0.993	0.732	0.758	0.794

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 *Small*. *Treated* is an indicator that equals one if the firm was included in the original pilot group, *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004, and *Small* in an indicator that the firm was below median total assets relative to the Compustat 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 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^{**}	-0.437^{***}	-0.208^{**}	-0.374^{***}
	(0.103)	(0.147)	(0.098)	(0.143)
Treated×Post×Small		0.586^{***}		0.386^{**}
		(0.178)		(0.169)
Controls			✓	✓
Firm FE	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	10,673	10,623	9,649	9,605
R^2	0.991	0.991	0.993	0.993

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 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 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 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 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 PIN

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and probability of informed trade (PIN) . 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 pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. PIN is computed as in Venter and De Jongh (2006). Control variables are *Q*, *Size*, and *Cash flow*. Controls are lagged 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	-0.710^{***}	-0.596^{***}
	(0.199)	(0.197)
Treated×Post×PIN	3.675^{***}	2.830^{***}
	(1.177)	(1.081)
Controls		✓
Firm FE	✓	✓
Industry-Year FE	✓	✓
Observations	9,490	8,682
R^2	0.994	0.996

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 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 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. *Small* in an indicator that the firm was below median total assets relative to the Compustat sample in 2004. Control variables included are Q, *Size*, and *Cash flow*. Controls are lagged 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^{***}	0.066	-2.040^{***}
	(0.456)	(0.171)	(0.604)
Treated×Post×$(1-R^2)$	2.142^{***}	-0.014	2.858^{***}
	(0.616)	(0.208)	(0.893)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	9,187	3,229	5,927
R^2	0.994	0.997	0.995

Table 11: Reg SHO and market shares by PIN

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and probability of informed trade (PIN). 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 pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. PIN is computed as in Venter and De Jongh (2006). *Small* in an indicator that the firm was below median total assets relative to the Compustat sample in 2004. Control variables included are *Q*, *Size*, and *Cash flow*. Controls are lagged 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	−0.596***	0.027	−0.641**
	(0.197)	(0.070)	(0.257)
Treated×Post×PIN	2.830***	0.142	3.115*
	(1.081)	(0.352)	(1.787)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	8,682	3,228	5,421
R^2	0.996	0.998	0.997

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 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 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. Control variables included are Q, *Size*, and *Cash flow*. Controls are lagged 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 PIN

This table reports output from the estimation triple differences specifications where we interact *Treated*, *Post*, and probability of informed trade (PIN). 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 pilot group, and *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004. PIN is computed as in Venter and De Jongh (2006). 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. Control variables included are Q, *Size*, and *Cash flow*. Controls are lagged 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	0.040	−1.471***	−0.374	−0.684***	−0.417	−0.725***
	(0.108)	(0.482)	(0.345)	(0.234)	(0.281)	(0.258)
Treated×Post×PIN	−0.474	7.333***	1.141	3.381***	2.100	3.309**
	(0.564)	(2.662)	(2.107)	(1.270)	(1.711)	(1.392)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	4,481	4,201	2,488	6,180	3,108	5,546
R^2	0.985	0.996	0.997	0.995	0.996	0.995

Table 14: 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 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* in percentage points, computed relative to 3-digit SIC industries total sales (Compustat's *sale*), and *Investment*, which is Compustat's *capx* scaled by total assets. 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 small if it was below median total assets relative to the Compustat 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
SHO	-0.141^{*}	0.036	-0.324^{***}	-0.535^{**}	-0.939^{*}	-0.313
	(0.081)	(0.053)	(0.114)	(0.219)	(0.482)	(0.232)
Firm FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓	✓	✓	✓	✓
Observations	13,834	5,017	8,817	13,720	4,966	8,754
R^2	0.988	0.994	0.989	0.723	0.741	0.790

Table 15: Short interest and market shares: Historic analysis. No firm fixed effects

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 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 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 16: Short interest and market shares by size: Historic analysis. No firm fixed effects.

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 3-digit SIC industries, in percentage points. Our short selling variables are *Short interest/Shares*, *Abnormal short interest* and *Days-to-cover*. *Small* is an indicator variable that equals one if a firm is below 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 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	-0.321^{***}		
	(0.037)		
Short interest/Shares × small	0.480^{***}		
	(0.042)		
Abnormal short interest		-0.107^{***}	
		(0.026)	
Abnormal short interest × small		0.088^{**}	
		(0.035)	
Days-to-cover			-0.306^{***}
			(0.034)
Days-to-cover × small			0.364^{***}
			(0.038)
Controls	✓	✓	✓
Industry-Year FE	✓	✓	✓
Observations	80,097	80,070	80,080
R^2	0.708	0.704	0.707

Table 17: 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 *Small*. *Treated* is an indicator that equals one if the firm was included in the original pilot group, *Post* is an indicator that equals one when the firm's fiscal year includes at least seven months after July 2004, and *Small* in an indicator that the firm was below median total assets relative to the Compustat 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 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 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*	−0.442***	−0.214*	−0.443***
	(0.113)	(0.162)	(0.126)	(0.169)
Treated × Post × Small		0.618***		0.535**
		(0.197)		(0.258)
Controls			✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	10,673	10,623	9,649	9,605
R^2	0.684	0.712	0.780	0.783

Table 18: 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 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 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

Additional Results

Table A.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 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 A.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*. *Small* is an indicator variable that equals one if a firm is below 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 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		
	(1)	(2)	(3)
Short interest/Shares	-0.040^{***}		
	(0.015)		
Short interest/Shares×Small	0.125^{***}		
	(0.018)		
Abnormal short interest		-0.127^{***}	
		(0.019)	
Abnormal short interest×Small		0.110^{***}	
		(0.022)	
Days-to-cover			-0.032^{**}
			(0.015)
Days-to-cover×Small			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 A.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 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 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 A.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 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. 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 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 A.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 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 small if it was below median total assets relative to the Compustat sample 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	0.050	−0.465***	0.042	−0.419**
	(0.077)	(0.170)	(0.082)	(0.185)
Controls			✓	✓
Industry-Year FE	✓	✓	✓	✓
Observations	3,872	6,751	3,770	6,663
R^2	0.959	0.754	0.961	0.836