Short Selling and Product Market Competition

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Abstract

We empirically investigate how short selling affects product market performance. We find that higher short sales of stocks lead to declines in firms' market shares. The effects are stronger in larger firms, concentrated industries, and industries where firms compete in strategic substitutes. Further results suggest that firms' competitive interactions amplify the effects of short selling on market shares via a greater sensitivity of output levels to the release of information contained in stock prices. Our findings are consistent with a managerial disciplining channel in which short interest reveals information of inefficient overreach by firms with market power, leading to downsizing and spin-offs.

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

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

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1 Introduction

Researchers in financial economics have shown a growing interest in the allocational role of secondary equity markets' prices (Bond et al., 2012). The theoretical literature shows how this role of prices is influenced by several endogenous feedback effects from financial markets to real economic activity, such as managerial disciplining (Ordóñez-Calafí and Bernhardt, 2021), managerial learning from stock prices (Dow and Gorton, 1997), stock price manipulation (Goldstein and Guembel, 2008), limits to arbitrage (Edmans et al., 2015), coordination among traders (Goldstein et al., 2013), and counterreactions to manipulative trading (Khanna and Mathews, 2012; Campello et al., 2020).

The empirical literature has paid particular attention to the effects of short selling activity on corporate policies (Grullon et al., 2015; Fang et al., 2016), in particular, via the managerial disciplining and learning channels (Brav et al., 2015; Boulatov et al., 2019). While empirical evidence supports the hypothesis that firms' managers react to the information contained in their own and their peers' stock prices (Chen et al., 2007; Foucault and Fresard, 2014; Gantchev et al., 2019), little is known about how this process is shaped by the interaction of firms in product markets. Arguably, competitive aspects of firms and industries can modulate the way managers incorporate the information released by prices. As a result, these aspects can have a first-order effect on how shorting activity feeds back into product market outcomes.

This paper investigates the effects of short sales on firms' product market performance and how they are shaped by competitive interactions. We provide novel evidence that an increase in stock short selling leads to lower shares of product market sales. Interestingly, we find stronger effects for larger firms and more concentrated product markets. Results are also stronger in industries where firms compete in strategic substitutes (Sundaram et al., 1996). Overall, our analysis shows that the feedback effects from financial to product markets depend on the presence of monopolistic rents and the nature of firms' strategic interactions. Further tests suggest that our baseline results are driven by the informational content released in short selling. Our findings are thus consistent with a managerial disciplining channel in which short interest releases information of inefficient overreach by firms with market power, leading to downsizing of output levels and spin-offs.

We start 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. Results stem 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 firm-level anticipation of poor performance relative to product market peers (Barardehi et al., 2021). As a result, our findings could be spuriously capturing active traders' sentiment towards firms—especially large ones—through time. We address such concerns by resorting to a well-known regulatory change conducted by the Securities and Exchange Comission: Regulation SHO (hereafter 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. We examine impact heterogeneity across firms and product markets based on cross-sectional characteristics at the time of the experiment.

Our results show that the suspension of short selling constraints led to a decline in market shares of pilot firms. Our estimates imply that pilot firms experienced an average decrease in market shares of 3.23% relative to control firms within the same industry and year. Consistent with our historic analysis, we find that the effects stem from large 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

[1]See Section 3.1 and Diether et al. (2009) for more detailed descriptions of the program.
[2]For validation, we confirm the results in Grullon et al. (2015) and contrast them with ours.

show that our baseline results are driven by firms in highly concentrated industries. Overall, our findings suggest that product market adjustments following the release of information are larger in the presence of monopolistic rents.

We also examine the importance of the degree of strategic substitution among firms' actions. The effect should be amplified in industries where firms compete in strategic substitutes, where empire building motives would generate more incentives to engage in aggressive output policies (Sundaram et al., 1996; 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. This suggests that the impact of short selling information on product market performance depends on the nature of the strategic reactions within industries (Fresard and Valta, 2016; Garcia-Appendini, 2018).

Next, we inspect whether the release of information stemming from shorting activity is driving our baseline results. We construct a measure of stock price informativeness across firms and years following Roll (1988) and Chen et al. (2007). This measure, *price nonsynchronicity*, reflects the variation of stock returns that cannot be explained by variations in the returns of the market and the firm's respective industry. Hence, it proxies for the amount of private information contained in a firm's stock price. We explore the cross-sectional variation in price nonsynchronicity at the time of Reg SHO and find sharper decreases in market shares for firms with lower price nonsynchronicity. Importantly, we find that the Reg SHO treatment effect responds to *ex ante* stock price informativeness *only* in large firms, firms in concentrated industries, and industries where firms compete in strategic substitutes. Thus, the evidence indicates that these competitive aspects are crucial determinants of the sensitivity of firms' product market performance to the information released by short selling.

Finally, we conduct a series of additional tests to assess the robustness of our baseline results. We follow an alternative empirical formulation commonly used in the Reg SHO literature and confirm our mains results. We also show that our results obtain without the high explanatory power of firm fixed effects, showing that our findings are not particular the highly saturated specifications that we use in our main analysis.

Overall, our collective evidence is consistent with a disciplining channel of short selling: product market competition and price opacity shape managerial incentives to pursue private benefits with inefficient overreach, which becomes clearer once the informational content of stock prices is improved by shorting activity. For large firms, highly concentrated markets, and markets with incentives for aggressive output policies, shorting activity can be particularly informative of sub-optimal product market strategies characterized by empire-building motives. Thus, our findings suggest that short selling can be a substitute for competitive pressure in terms of inducing better governance. The disciplining channel is further supported by our evidence that the impact of short selling on product market outcomes is mainly driven by firms with *ex ante* lower price informativeness.

An alternative mechanism could be that shorting activity affects firms' access to external capital, hampering investment (Turkiela, 2019). There are at least two reasons to believe this is not the channel that our results are capturing. First, access to finance is less likely to be a binding constraint on large firms. This is consistent with empirical evidence showing that investment of large firms is less responsive to stock mispricing (Bakke and Whited, 2010). Second, direct comparisons between our results and Grullon et al.'s (2015) imply that the changes in market shares of pilot firms are not driven by lower levels of corporate investment.

Our paper contributes to a large body of literature that examines the real effects of short selling (Goldstein and Guembel, 2008; Massa et al., 2015; Hope et al., 2017) and, more generally, financial feedback (Allen and Gale, 1992; Dow and Gorton, 1997) and management disciplining via hedge fund activism (La Porta et al., 2006; Brav et al., 2008, 2015; Ordóñez-Calafí and Bernhardt, 2021). In addition to the direct impact of the Reg SHO on investments 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 negative sentiment by investors is the underlying mechanism driving effects of short selling on investments.

We also contribute to the literature on how financial aspects affect firms' product market performance. In a seminal paper, Brander and Lewis (1986) theoretically investigate how firms' leverage level can lead to more aggressive output competition in product markets. From the empirical side, Opler and Titman (1994) show that highly leveraged firms tend to lose market share to financially sound product market rivals during industry downturns. Fresard (2010) provide evidence that cash-rich firms are better able to gain market share at the expense of industry rivals. We contribute to this body of research by showing how short selling can affect product market performance. Although we find that short selling shocks negatively affect firms' market shares, this does not necessarily represents a bad aspect of shorting activity, as these findings suggest better decision-making in the long run due to better monitoring of management activity.

Our analysis also has important implications for policy interventions. Despite the investment sensitivity to short selling via managerial learning previously documented in the literature, our results are stronger for firms with characteristics not typically associated with feedback effects from financial markets to managerial investment decisions. This allows us to conclude that feedback effects are amplified by firms' competitive interactions in product markets. From the perspective of short selling constraints (Barardehi et al., 2019; Campello et al., 2020), our findings suggest that market power and industry competition aspects should be considered when designing short selling regulation and blockholder disclosure thresholds (Ordóñez-Calafí and Bernhardt, 2021).

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 directly test whether our main results are driven by an informational channel. We report robustness checks on Section 5. Section 6 concludes.

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 are constructed as follows. Tobin's 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. Finally, *Profitability* is the ratio of operating income before depreciation ($oibdp$) to 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

[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

end of the month (CRSP's $SHROUT$), expressed in percentage points. Our second measure, *Abnormal short interest*, attempts to capture the unexpected component of short interest. Specifically, we follow Karpoff and Lou (2010) and Boulatov et al. (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 on 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*, as in Boulatov et al. (2019). Our coefficient of interest is β, which estimates the relationship between shorting activity and market shares in our sample. Via 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.

[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

— PLACE TABLE 2 ABOUT HERE —

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. Overall, results in Table 3 provide evidence that short selling activity predicts worse product market performance by large firms. We find no conclusive evidence of such association on small firms.

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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. However, one cannot claim causality based on these results as Equation (1) does 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 a disciplining effect.

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 Comission (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 the uptick rule would be lifted, which happened in May 2005. To construct the pilot group, the Securities and Exchange Comission (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 widely used by researchers for the purpose of estimating causal effects of short selling. Diether et al. (2009) find that firms in the pilot group experienced an increase in short selling activity, although stock returns and volatility were unaffected. Grullon et al. (2015) test different time windows around the experiment and show that pilot stocks experienced an increase in shorting activity and a decrease in prices relative to the control group. Moreover, the authors provide evidence that small firms reacted to lower prices by reducing investment and equity issuance. More recently, Boulatov et al. (2019) show that investment to short interest sensitivity increased for treated firms, consistent with the argument that less constraints to short selling can improve the informational content of shorting activity.

The recurrent use of the Reg SHO in finance empirical research also drew concerns about the validity of the results. Heath et al. (2020) 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 results, Heath et al. (2020) 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 papers such as Goldstein and Guembel (2008); Matta et al. (2021) and Terovitis and Vladimirov (2020), which lay the theoretical foundations of how short selling can influence real product market outcomes, possibly favoring some firms to the detriment of others. 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. We argue that our specifications are more rigorous than those previously used in the literature (including Heath et al. (2020)) 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.

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 that 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

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

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 (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 as of 2004 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

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

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.

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 —

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

[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.

[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)

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. A negative estimate indicates that pilot firms lost market share after the exemption of price tests relative to firms for which the tests remained in place. Importantly, our specification ensures that identification stems from comparing firms within the same sector and year. Finally, to assess heterogeneous effects, we estimate tripe differences models where we interact the cross-sectional variable of interest with $Treated_i$ and $Post_{i,t}$. The triple differences estimator in these specifications measures the sensitivity of the treatment effect to the variable of interest.

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 results previously documented by Grullon et al. (2015). Second, it allows us to contrast our results to theirs, who established that pilot firms decreased investment, especially small ones.

We report results from this exercise in Table 5. We find a significant negative effect 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

[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

most of the effect on investment is driven by small firms—as reported by Grullon et al.—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 0.399 p.p. market share, which corresponds to a 4.44% 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 by the time of the program is associated with 0.692 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 36.5% 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 strategies. Specifically, results in column (4) imply that a one s.d. lower CSM is associated with 0.766 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 73.3% 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

In this section, we investigate whether our results are driven by the informational content of short interest and stock prices. The findings of Brav et al. (2015) and others suggest that active trading of hedge funds have a disciplining effect on managers. Hence, the removal of short selling restrictions can release new information about inefficient overreach by large firms, leading managers to adjust accordingly with lower output levels relative to similar industry peers.

If managerial discipline is driving our main results on product market performance, we should observe stronger effects where price had less private content up to the treatment. As the experiment increased short interest for treated firms (Grullon et al., 2015), our baseline effects should be more pronounced on firms with higher *ex ante* opacity in stock prices, characterized by lower levels of price informativeness at the time of the intervention.

To test this hypothesis, we follow Roll (1988) and Chen et al. (2007) and construct a firm-year measure of the amount of firm-specific information contained in stock prices. The idea is 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. To construct this measure, we first estimate the following regression for each firm-year observation in our sample:

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

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. As in Chen et al. (2007), this variable is negatively correlated with firms' size.

We use $(1 - R^2)$ in several tests. We explore cross-sectional variation in $(1 - R^2)$ at the time of the experiment in our main regressions of product market performance. We conduct a heterogeneity analysis similar to that of Section 3.4, with a triple interaction term that includes $(1 - R^2)$ at the time of the treatment. We report estimates of the coefficients of interest in Table 8. Results suggest that the lower the price nonsynchronicity the larger the market share loss due to the treatment. More precisely, column (2) suggest 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.

Next, we explore how firms and product market characteristics shape the sensitivity of market shares to price informativeness during the Reg SHO. First, we estimate triple differences models similar to the those on Table 8 on the sample of small and large firms.[13] We report results in Table 9. Estimates show that the treatment effect responds to $(1 - R^2)$ in large firms only. For these firms, 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 firms. No such relation exists for small firms, where we find an overall null effect of the intervention, in consonance with our baseline results.

Next, we break down our sample by product market characteristics. Table 10 reports the results for the samples of low versus high concentration based on median HHI, and by whether firms compete in strategic substitutes or complements, as measured by the sign of the CSM. For low concentrations industries and product markets where firms compete in complements we find no overall treatment effect nor a significant response to price informativeness. For high concentration industries, a one s.d. in $(1 - R^2)$ is associated with 0.82 p.p. higher market shares. For industries with negative $CSM3$ and $CSM4$, a one s.d. smaller $(1 - R^2)$ implies 0.43 and 0.5 p.p. smaller market shares, respectively.

Our collective evidence implies that price informativeness plays a meaningful role in how short selling affects product market performance. We show that pilot firms with *ex ante* lower levels of private information in prices experienced sharper decreases in market shares following the suspension of price tests. Crucially, this result stems from large firms, concentrated industries and industries where firms compete in strategic substitutes, providing strong evidence that our baseline results are driven by a informational mechanism. These findings suggest that firms and product markets' competitive characteristics affect the sensitivity of market shares to short interest via the informational content of prices. This is consistent with a managerial disciplining channel in which large firms scale back output relative to industry peers in the wake of information release.

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

[13] We favor splitting the sample in this framework to avoid using quadruple interactions in our specifications.

whole period of the experiment, not just the first part. Second, we address the 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 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 to be 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 11. 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 11 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, we replicate our main results of the Reg SHO exercise 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 main results in these analyses.

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 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 12 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.[14]

[14]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

Table 13 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.[15] 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 14.[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 0.354 p.p. decrease in market shares relative to control firm after the first wave of price tests suspension. This effects corresponds to 5.52% lower market shares of the average firm. In 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 6.17% in their average market share.

Finally, Table 15 reports results of the heterogeneity analysis by HHI and CSM, as in Section 3.4. The coefficients are consistent with those on Table 7. The only visible difference is that the coefficients of the triple interaction with the CSM measures fail to reach 10% statistical significance when control variables are included.

unexpected component of short interest.

[15]In that case, we also include $Treated_i$ as an explanatory variable, since it is not washed up by the firm fixed effects.

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

6 Concluding Remarks

We study the effects of short selling on firms in the context of product market competition. We establish two main results. First, that shorting activity negatively impact firms' performance relative to their industry peers in the form of lower market shares. Second, that the sensitivity of market shares to short selling is stronger in the presence of market power and strategic substitution among product market rivals.

We find extensive evidence that these baseline effects are driven by a managerial disciplining channel, where shorting activity releases information about sub-optimal output policies, particularly for firms with market power and firms in industries with more incentives to engage in aggressive output competition. As a result, the downsizing of output levels is more pronounced in firms and product markets with these features.

Our results imply that product market competition aspects are meaningful in shaping financial feedback effects, and thus are relevant for the design of short selling regulations. We believe that this intersection is promising and relatively unexplored, and future research might provide us with a better understanding of how product and financial markets 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)
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)
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	2.399	1.884	1.616	574	2.466	1.830	2.198	1,114	−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)
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***	−0.230**	−0.399***
	(0.103)	(0.147)	(0.098)	(0.142)
Treated×Post×Small		0.586***		0.396**
		(0.178)		(0.170)
Controls			✓	✓
Observations	10,673	10,623	9,830	9,789
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***	0.400**	−0.090	−0.055	−0.139	−0.123
	(0.184)	(0.095)	(0.111)	(0.095)	(0.103)	(0.090)
Treated×Post×HHI	−5.685***	−4.801***				
	(1.801)	(1.560)				
Treated×Post×CSM3			7.883**	11.012***		
			(3.617)	(3.679)		
Treated×Post×CSM4					5.804**	8.314***
					(2.732)	(2.841)
Controls		✓		✓		✓
Observations	10,673	9,830	10,655	9,814	10,644	9,804
R^2	0.991	0.991	0.991	0.993	0.991	0.992

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 industry. 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		✓
Observations	10,001	9,187
R^2	0.993	0.994

Table 9: 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)
Observations	9,187	3,229	5,927
R^2	0.994	0.997	0.995

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. 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)
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 11: 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)
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 12: 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)
Observations	80,097	80,070	80,080
R^2	0.750	0.748	0.750

Table 13: 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)
Observations	80,097	80,070	80,080
R^2	0.708	0.704	0.707

Table 14: 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***	−0.354**	−0.554**
	(0.113)	(0.162)	(0.164)	(0.218)
Treated×Post×Small		0.618***		0.544*
		(0.197)		(0.278)
Controls			✓	✓
Observations	10,673	10,623	10,582	10,534
R^2	0.684	0.712	0.732	0.747

Table 15: 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***	0.923***	−0.078	−0.289	−0.126	−0.340*
	(0.203)	(0.308)	(0.123)	(0.203)	(0.114)	(0.198)
Treated×Post×HHI	−5.515***	−9.564***				
	(1.988)	(2.727)				
Treated×Post×CSM3			7.732*	5.548		
			(3.982)	(5.847)		
Treated×Post×CSM4					5.595*	4.629
					(3.012)	(4.439)
Controls		✓		✓		✓
Observations	10,673	9,830	10,655	9,814	10,644	9,804
R^2	0.692	0.753	0.676	0.740	0.668	0.734

7 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)
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)
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	-0.230^{**}	-0.470^{**}	-0.026
	(0.098)	(0.208)	(0.062)
Controls	✓	✓	✓
Observations	9,830	4,875	4,955
R^2	0.993	0.992	0.975

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	−0.231**	−0.162	−0.260**	−0.111	−0.303**
	(0.098)	(0.195)	(0.115)	(0.142)	(0.131)
Controls	✓	✓	✓	✓	✓
Observations	9,814	2,752	7,062	3,478	6,326
R^2	0.993	0.993	0.992	0.993	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^{***}	-0.036	-0.673^{**}
	(0.077)	(0.170)	(0.080)	(0.282)
Controls			✓	✓
Observations	3,872	6,751	3,660	6,129
R^2	0.959	0.754	0.962	0.811