Short Selling Governance and Intrafirm Resource Allocation*

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Abstract

We exploit Regulation SHO as a natural experiment to investigate the effects of short selling threats on intrafirm capital allocation. Using detailed data on the foreign operations of multinationals, we find that the marginal effect on aggregate investment masks a significant effect on intrafirm reallocation. Managers reallocate investment and R&D expenditures across borders toward productive subsidiaries and R&D centers, respectively. Treated firms shifted 30% more capital toward foreign subsidiaries with strong recent performance. These results provide new evidence on the scope and potential benefits of governance by short sellers and demonstrate the importance of cross-border spillovers of capital markets regulation.

JEL Classifications: D22, G23, G28, G34, M5
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1 Introduction

As an external governance mechanism, short selling can reduce agency conflicts between managers and shareholders (Massa, Zhang, and Zhang 2015), particularly as they relate to investment (Gilchrist, Himmelberg, and Huberman 2005; Campello and Graham 2013). Theoretically, short-selling constraints can lead to overvaluation by preventing negative information from being incorporated in prices (Miller 1977; Harrison and Kreps 1978; Allen, Morris, and Postlewaite 1993; Chen, Hong, and Stein 2002; Sheinkman and Xiong 2003; Hong and Stein 2003). Past empirical work has shown that short-sellers impact corporate financing and investment policies through two governance activities: they identify overvalued firms (Karpoff and Lou 2010; Fang, Huang, and Karpoff 2016) and provide a mechanism for prices to reflect negative information (Grullon, Michenaud, and Weston 2015). With these direct and indirect effects on prices, short-sellers can reduce agency conflicts and encourage first-best capital allocation.

Recent survey evidence suggests that the scope for overinvestment exists–almost half of CEOs surveyed say that they unilaterally make internal capital allocation decisions (Graham, Harvey, and Puri 2015). Given the prevalence and level of stock-based compensation, these unilateral decision-makers, in the face of overvalued equity,

also have the motive to pursue inefficient investment policies (Jensen 2005; Benmelech, Kandel, and Veronesi 2010). Because internal capital markets determine capital allocation (Stein 1997; Lamont 1997) and within-firm dispersion in capital returns exceeds cross-firm dispersion (Kehrig and Vincent 2016), we investigate whether the scope of short-seller governance extends to intrafirm capital allocation.

In this paper, we study the effect of short-sellers on intrafirm reallocations of capital and labor. In so doing, we face two critical challenges: internal capital and labor allocation data is scarce and short-sellers endogenously select target firms in a way that may relate to the efficiency of capital allocation. We address these challenges with unique data on the foreign subsidiaries of U.S. multinational firms from the Bureau of Economic Analysis (BEA) and a randomized experiment based on the pilot program of Regulation SHO. Because our dataset on internal capital allocation is comprised of foreign subsidiaries, our results also speak to the literature on cross-border investment flows.

We contribute three novel findings to the literature on the scope of governance by short-sellers. First, we provide evidence that although the increased threat of short selling does not affect aggregate investment, on average, it does affect the allocation of investment across subsidiaries. This finding suggests that management reallocates

capital in response to short-selling pressure and reconciles previous findings on the effects of short-selling threats on firm-level innovation and investment (He and Tian 2017; Grullon, Michenaud, and Weston 2015). Second, we show that managers subject to short-selling pressure reallocate capital to high performing subsidiaries, which suggests that short-selling pressure disciplines managerial decisions. We also find that these managers reallocate research and development (R&D) expenditures toward R&D centers, allowing them to take advantage of economies of scale in R&D activities (Romer 1986). These findings are consistent with, for example, external governance reducing empire-building or quiet-life strategies by managers. Moreover, because our focus is the foreign subsidiaries of multinational corporations, these results suggest that short-selling pressure incentivizes managers to invest costly effort to oversee distant subsidiaries (Harford, Wang, and Zhang 2016). Third, we provide evidence that subsidiaries that receive additional capital experience no decrease in productivity, suggesting that the marginal product of capital is not decreasing in investment for these subsidiaries. A benefit of this test is that it implicitly eliminates measurement error and mean reversion in productivity as alternative explanations for our reallocation results, which means that capital reallocation induced by short-selling pressure solves within-firm misallocation problems.

The pilot program of Regulation SHO provides an ideal natural experiment to study the causal effects of short-sellers because it randomly selected a set of firms, known as pilot firms, to trade with fewer short sale restrictions.[1] For our purposes, the regulation provides relevant variation in the threat of short selling precisely because (i) it reduced the cost of short-selling, thereby increasing the latent propensity of short-selling, and (ii) firms in the pilot program of Regulation SHO experienced an increase in short-selling activity (Alexander and Peterson 2008; Diether, Lee, and Werner 2009; Grullon, Michenaud, and Weston 2015). We use a difference-in-differences estimator to study the effect of inclusion in the Regulation SHO pilot program on capital and labor reallocation. The microdata on the foreign subsidiaries of multinationals allows us to observe subsidiaries at the country-year level, so we identify treated subsidiaries as those whose parent companies were randomly selected as pilot firms and control firms as those whose parent companies were not.

We first investigate the effect of short-selling pressure on aggregate investment and employment at the firm level. We find no evidence that short-selling pressure impacts aggregate investment, which corroborates baseline evidence in Grullon, Michenaud, and Weston (2015), or employment. These null effects could mask any

[1] Rule 202T of Regulation SHO removed the uptick rule, which previously restricted short sales from following price declines. The uptick rule was implemented as SEC policy to reduce price manipulation and constrain short sales from contributing to price declines.

disciplining effects of short-selling threats if, for example, capital markets frictions produce heterogeneity in over- and under-investment among treated firms. Because discipline from short-selling pressure could in principle lead firms that overinvest to invest less and firms that underinvest to invest more, the effect on aggregate investment is ex ante unclear. To delineate among these alternative explanations, we investigate within-firm variation in investment. Specifically, we study within-firm dispersion in investment at the subsidiary level, and find that short-selling pressure significantly increases capital reallocation. Despite the random selection of pilot companies, one may still be concerned that treated and control subsidiaries may differ in trend on various characteristics before assignment to the pilot program. We investigate the parallel trends assumption using a dynamic specification of our difference-in-differences estimator, and find that the assumption of parallel trends is not rejected at any conventional significance level in the three years before Regulation SHO.

We next investigate how disciplined managers reallocate capital across subsidiaries. Compared to productive subsidiaries at control firms, productive subsidiaries at treated firms experience 30% higher investment. This corresponds to a 200% increase in the productivity-sensitivity of investment. We find that, on average, the productivity of these subsidiaries did not decrease despite receiving a significant

increase in capital, which suggests that the reallocation due to short-selling threats was efficient.

Our results indicate that short selling governance improves the allocation of resources within multinational firms. In this sense, market governance relates to the profitability of U.S. multinational firms' operations, including their operations abroad. Policies, such as Regulation SHO, that provide scope for investors to influence the management of multinational firms therefore plays a role in the puzzling direct investment returns differential (Curcuru, Dvorak, and Warnock (2008) and Curcuru and Thomas (2014)).

This paper also contributes to the literatures that study the governance effects of short sellers, intrafirm resource allocation, and financial markets regulation. The extant literature on the external governance role of short sellers suggests that short seller activism and short-selling threats both discipline managers. In particular, both Karpoff and Lou (2010) and Dyck, Morse, and Zingales (2010) find that short sellers play an important role in the detection of fraud. A long literature documents the negative impact of short selling on stock prices (Jones and Lamont 2002; Saffi and Sigurdsson 2010; Bris, Goetzmann, and Zhu 2007; Boehmer and Wu 2013) which, in theory, leads to a reduction in investment (Gilchrist, Himmelberg, and Huberman 2005). To combat

the identification challenge that short sellers select targets of activism events, several other papers have also used the Regulation SHO pilot program setting. Fang, Huang, and Karpoff (2015) and Massa, Zhang, and Zhang (2015) show that short selling threats lead to a reduction in earnings management. On the contrary, Li and Zhang (2015) find that short selling pressure causes managers to reduce the precision of bad news forecasts and the readability of their annual reports, and Hope, Hu, and Zhao (2016) find an increase in audit fees and an increase in the incidence of auditor switching.

To the literatures that study intrafirm resource allocation, we contribute novel international evidence that managers reallocate capital across borders in response to an external governance stimulus. Previous research in this area has focused on domestic resource allocation by divisional managers (Duchin and Sosyura 2013; Duchin, Goldberg, and Sosyura 2016), across plants (Bertrand and Mullainathan 2003; Ersahin, Irani, and Le 2015; Giroud and Mueller 2015), and across states (Jayaratne and Strahan 1996), due to changes in investment opportunities, taxes, or deregulation. We differ from the existing literature in that we study a shock to external governance and reallocation across borders. Lastly, our work also contributes to the literature that studies financial markets regulation; we find that financial markets regulation has cross-border spillover effects through intrafirm resource allocation.

2 Data

We construct a panel on the direct investment activities of U.S. multinationals using data collected through BEA's annual surveys on U.S. Direct Investment Abroad.[2] For the purposes of BEA's surveys, and consistent with international conventions, direct investment is defined as the ownership or control, direct or indirect, by a legal person of 10 percent or more of the voting securities of an incorporated foreign business enterprise, or an equivalent interest in an unincorporated foreign business enterprise. A multinational is the combination of a single legal entity that undertakes the direct investment, termed the parent company, of at least one foreign business enterprise, known as the foreign affiliate.

BEA's surveys are conducted pursuant to the International Investment and Trade in Services Act (hereafter the Act). The Act stipulates that the "use of an individual company's data for tax, investigative, or regulatory purposes is prohibited." Willful noncompliance with the Act may result in imprisonment for up to one year. For these reasons, in addition to their monitoring of corporate actions and a system of internal data integrity checks, BEA believes the surveys accurately capture virtually complete data on the universe of all U.S. direct investment abroad.

[2] These data are collected for the purpose of producing publicly available aggregate statistics on the activities of multinational enterprises.

The surveys provide detailed data on respondents' financial and operating characteristics.[3] Among other items, these include information on the balance sheet, income statement, employment, and R&D activities of the respondent.[4] Data are reported in accordance with U.S. generally accepted accounting principles and any currency translation adjustments are made in accordance with Financial Accounting Standard 52 (Foreign Currency Translation).[5]

The sample is selected as follows. We begin with the members of the Russell 3000 in 2004. Excluded from this list are stocks that went public or had spin-offs after April 30, 2004 and stocks not previously subject to price tests because they were listed on an exchange other than the Nasdaq, NYSE, and AMEX. These firms were matched to BEA's data using their names, industry codes, and consolidated total assets, sales, and net income. Our analysis requires changes in subsidiary level financials at an annual frequency, as well as observations prior to, during, and after the implementation of

[3] Subsidiaries may report on a consolidated basis if they are residents of the same country, as determined by their physical location, and are classified within the same four-digit International Surveys Industry. International Surveys Industry classifications are similar to NAICS codes.

[4] The reporting requirement thresholds for survey questions vary according to the size of the subsidiary and the ownership stake of the parent; rules for specific years can be found in BEA's benchmark data reports, such as BEA (2013). BEA imputes values for some data items of some subsidiaries to calculate direct investment universe totals. Imputed data comprise a minuscule portion of direct investment activity. In the 2004 data, for example, 99.8 percent of subsidiary net income was reported. Nevertheless, to rule out concerns related to the data estimated by BEA, the analysis in this paper relies only on the reported data.

[5] A further discussion of BEA's data on multinational firms can be found in Mataloni (1995). More detailed information is included in the methodology sections of BEA's various benchmark data reports; BEA (2013) is the most recent finalized version.

Regulation SHO. This leaves a sample of 376 firms. These firms have 5,575 subsidiaries, or roughly 15 subsidiaries per firm. Summary statistics are contained in Table 1.

3 Identification

We face two key challenges in identifying the effect of short selling on resource allocation within firms. First, the paucity of data on intrafirm resource allocation makes measuring resource allocation within firms difficult. We focus on the allocation of capital and labor among foreign subsidiaries of multinational firms because detailed microdata is available from the Bureau of Economic Analysis. Furthermore, foreign operations constitute an ideal setting to explore corporate governance for at least three reasons. First, monitoring and bonding costs are high (Doukas and Travlos 1988). Multinationals have complex organizational structures (Creal, Robinson, Rogers, and Zechman 2014). They also face information frictions from distance (Schroff, Verdi, and Yu 2014) and contracting frictions due to the reliance on local market knowledge (Edlin and Reichelstein 1995). Second, due to financial reporting regulation, disclosure about foreign operations is opaque, making internal and external monitoring difficult (Hope and Thomas 2008). Third, foreign operations have greater variation in productivity than domestic operations (Lucas 1990), which suggests that not only are foreign operations

an ideal setting to explore governance effects but also that there is enough within-firm variation to do so.

Consistent with the notion that foreign operations are a bastion of agency problems, foreign operations constitute a significant focus of short sellers. For example, in the recent short campaign by Muddy Waters against American Tower Corporation, wasteful foreign investment was one of the key arguments. In its publicly available research, Muddy Waters suggested that American Tower had engaged in a "value destroying investment binge overseas" which had destroyed "at least $1B of value."[6]

The second challenge we face is that causal inference is typically limited due to the lack of exogenous variation in short-selling activity. Without such exogenous variation, one may be concerned that, for example, managers anticipate discipline by short-sellers or that unobservable characteristics that determine external governance by short-sellers simultaneously affect governance by shareholders or regulators. We address this challenge by focusing on exogenous variation in the cost of short-selling as a shock to short-selling threats. We exploit the pilot program of Regulation SHO as a natural experiment that reduced the cost of short-selling by eliminating short-sale price tests for a randomly selected set of firms (Diether, Lee, and Werner 2009). As the cost of short-

[6] http://www.muddywatersresearch.com/research/amt/initiating-coverage-amt-slide-deck/

selling decreased for pilot firms relative to nonpilot firms, short-selling threats increased for pilot firms relative to nonpilot firms.

The structure of the Regulation SHO's pilot program provides an ideal setting to study the effect of short-selling threats on corporate policies. First, the program, which focused exclusively on Russell 3000 index members, selected pilot firms based on exchange listing status and trading volume. In particular, from each of the NYSE, NASDAQ, and AMEX, the pilot program chose every third firm based on trading volume rank. Selection by rank within each exchange ensures that Russell 3000 index members were unable to manipulate their way into or out of the pilot program and that pilot firms are a stratified subsample based on trading volume. Moreover, prior studies that have utilized the Regulation SHO setting for identification have demonstrated that pilot and nonpilot firms are balanced on observable characteristics (Diether, Lee, and Werner 2009; Grullon, Michenaud, and Weston 2015) and that potential participants did not lobby for or against participation (Fang, Huang, and Karpoff 2016). To support a causal interpretation of our findings, we illustrate that the behavior of nonpilot firms represent a valid counterfactual for pilot firms with a dynamic test of the parallel trends assumption.

Second, the starting and ending dates of the pilot program were unlikely to have been anticipated by potential participants. The official start and end dates for the program were May 2, 2005 and August 6, 2007, respectively, but these differed from the originally announced and scheduled dates of January 3, 2005 and December 31, 2005.[7] Third, because the pilot program had an official end date, it provides us with an opportunity to confirm our findings by studying whether the effects reverse after the pilot program ends. Because short selling may have transient or permanent effects on internal governance, a test of whether the behavior of pilot firms reverts to that of nonpilot firms once the program ends is also informative about the influence of short-selling threats on internal governance. We may expect more permanent effects if short-selling threats affect board membership or information acquisition by shareholders, for example. Our evidence suggests that the difference in firm behavior between pilot and nonpilot groups goes away after the program ends, which supports the view that, as an external governance mechanism, short selling threats have a transient effect on corporate policies. This transience further suggests that the underlying governance problems are persistent and systematic features of the corporate environment.

[7] Securities Exchange Act Release No. 50104, July 28, 2004, and Securities Exchange Act Release No. 53684, April 20, 2006.

4 Results

4.1 Regulation SHO and Consolidated Investment and Employment

We begin by exploring the relationship between Regulation SHO and firms' consolidated investment and employment. Specifically, we estimate the following regression specification:

$$Inv_{i,t} = \alpha_i + \alpha_t + \beta \times SHO_i \times Post_t + \gamma' X_{it} + \varepsilon_{it}$$

The unit of observation is the firm-period, where firm-years are averaged within the period prior to the institution of Regulation SHO, and the period after the regulation is in place. $Inv_{i,t}$ is the period average of the natural log of annual consolidated firm investment. The employment outcome variable, which is a count of the number of employees at the consolidated firm, is measured analogously. The terms α_i and α_t denote firm and period fixed effects, respectively. SHO_i is an indicator variable that equals one if a firm is a member of the Regulation SHO pilot group. $Post_t$ is an indicator variable that equals one once Regulation SHO has been implemented. X_{it} represents the age control, which is measured as the natural log of the number of years since the firm first appears in the data and ε_{it} is the usual error term. Reported standard errors are robust (heteroscedasticity-consistent).

Table 2 presents our results on the effects of Regulation SHO on consolidated investment and employment. Column (1) shows that pilot firms experience a statistically insignificant increase in investment relative to control firms (those which are not in the pilot group). In column (2), we show a similarly statistically insignificant effect of short-selling threats on employment. These results are in keeping with the aggregate results in Grullon, Michenaud, and Weston (2015), and show that, on net, the Regulation SHO pilot program does not yield strong evidence for an effect of short-selling threats on overall investment or employment at the firm level.

4.2 Dispersion in Investment and Employment Growth

Having established that Regulation SHO does not have a statistically significant association with either consolidated investment or consolidated employment, we turn to the issue of reallocation. Specifically, we estimate the following regression equation using ordinary least squares.

$$\sigma_{\Delta Inv_{i,t}} = \alpha_i + \alpha_t + \beta \times SHO_i \times Post_t + \gamma' X_{it} + \varepsilon_{it}$$

Terms matching those in the consolidated specification are unchanged. $\sigma_{\Delta Inv_{i,t}}$ measures the dispersion in investment changes, and is calculated as follows. First, we take the first difference of investment for each subsidiary. Then, for all the firm's subsidiaries, we calculate the standard deviation of the changes in a given year. Finally, we average the

standard deviations – observed at the firm-year level – over each period. The employment dispersion measure is calculated analogously. Reported standard errors are robust (heteroscedasticity-consistent).

Table 3 presents our results on dispersion in investment, or the effect on intrafirm investments among subsidiaries. In column (1) we look at dispersion in the change in investment, whereas in column (2) we investigate the dispersion in change in employees among subsidiaries. We find that pilot firms see an economically significant increase in the dispersion of their intrafirm investments among subsidiaries. While this could be due to various choices these firms are making, this is evidence that their investment strategies are changing substantially relative to other firms. Pilot firms may face more scrutiny, and so appear to reimplement capital expenditure plans across subsidiaries. While intrafirm capital allocation policy appears to be responsive to the pilot program, hiring and firing decisions across subsidiaries does not. This is evidence that while changing capital allocation at the subsidiary level may be feasible for firms, it appears as though changing employment practices faces greater frictions at the subsidiary level.

4.3 Dynamic specification

Next we turn to the dynamic impact of Regulation SHO on investment. This step also provides insight into whether treated and untreated firms behaved similarly prior to treatment, the key identifying assumption. To evaluate this issue, we estimate the following specification using ordinary least squares.

$$\begin{aligned}\sigma_{\Delta Inv_{i,y}} = \beta_{11} \times SHO_i \times \mathbb{I}_{2004} + \beta_{21} \times SHO_i \times \mathbb{I}_{2005} \\ +\beta_{31} \times SHO_i \times \mathbb{I}_{2006}\beta_{41} \times SHO_i \times \mathbb{I}_{2007} + \\ +\beta_{51} \times SHO_i \times \mathbb{I}_{2008+} + \alpha_i + \alpha_y + \gamma' X_{iy} + \varepsilon_{iy}\end{aligned}$$

The unit of observation is the firm-year. The definitions of terms also appearing in the static specification are unchanged. α_y are year (not period) fixed effects. $\mathbb{I}_{2004}$ is an indicator variable that equals one in 2004. $\mathbb{I}_{2005}$, $\mathbb{I}_{2006}$, and $\mathbb{I}_{2007}$ are defined analogously. $\mathbb{I}_{2008+}$ is an indicator variable that equals one in 2008 and subsequently. $\sigma_{\Delta PPE_{i,t}}$ and X_{it} are no longer averaged over each period, but rather measured in a given year. Standard errors are clustered by firm.

Table 4 presents our dynamic specification for change in the dispersion of investment. One may be concerned that our results could be driven by some other trend among pilot firms. One could also investigate whether or not pilot firms appear to be affected following the pilot program. What we see, when breaking up the program into year pilot effects, there is an effect in the years of the program (2005 and 2006) but not

in the year prior, or in the years subsequent. This shows that not only is selection of pilot firms not driving our results, but that the effect is not lingering; there is a full reversal of the policy in 2007, meaning that one cannot distinguish the effects in 2007 or subsequent years, statistically, from a null effect.

4.4 Reallocation

With evidence in hand that there is greater dispersion in investment among pilot firms after Regulation SHO has been implemented, we turn to whether firms reallocate investment from underperforming to outperforming firms. We estimate the following specification.

$$Inv_{i,j,t} = \alpha_{i,j} + \alpha_t + \beta_1 \times SHO_i \times Post_t \times OutPerform_{i,j} + \beta_2 \times SHO_i \times Post_t$$
$$+\beta_3 \times Post_t \times OutPerform_{i,j} + \gamma' X_{i,j,t} + \varepsilon_{i,j,t}$$

The unit of observation is the subsidiary-period. The definitions of terms also appearing in the static specification are unchanged. j indexes subsidiaries. $\alpha_{i,j}$ denotes subsidiary fixed effects. $OutPerform_{i,j}$ is a subsidiary-level indicator variable calculated as follows. For all subsidiaries, the average ratio of net income to assets is calculated from 2000-2004. Subsidiaries for which this calculation exceeds the median relative to its parent's other subsidiaries are deemed to outperform. For these subsidiaries, $OutPerform_{i,j}$ takes the value 1. $X_{i,j,t}$ controls for age, calculated analogously to its earlier measurement. $\varepsilon_{i,j,t}$

is the usual error term. Reported standard errors are robust (heteroscedasticity-consistent).

In Table 5, we present our results on reallocation to more profitable subsidiaries. In column (1), we present subsidiary-level analyses on the effects of Regulation SHO and profitability on investment among subsidiaries. First of all, we show that investment increases by 13-14% at subsidiaries with above median ROA relative to those below the median. However, pilot firms increase investment at outperforming subsidiaries by an additional 30% on average. This means that while all firms invest more heavily in recently more profitable subsidiaries, firms under the Regulation SHO pilot program reinvest three times more in outperforming subsidiaries than do other firms. In column (2) we control for subsidiary age. Our results are quantitatively the same, meaning that our results are not driven by secular effects.

One may be interested in whether the effect of the pilot program reverses. In Table 6 we investigate this, looking at the period following the Regulation SHO pilot program (2007-2010). In column (1), using the same specification as in Table 5, we find that in this post pilot period firms invest 14-16% more in above median ROA subsidiaries, just as we found during the pilot period. However, the policy effect reverses for Regulation SHO pilot firms, as we see the incremental effect for these subsidiaries

becomes insignificant. These results are quantitatively the same if we include an age control, as we do in column (2).

We have shown that Regulation SHO pilot firms changed their investment policy across subsidiaries during the pilot period, and that these changes dissipated in the post pilot period. Moreover, we showed that increased scrutiny and the threat of downward price pressure, as introduced by the pilot program, led these changes in investment to be directed toward more profitable subsidiaries. We showed that the increase in investment to more profitable subsidiaries during Regulation SHO is three times greater for pilot firms. However, we wish to show the robustness of our results to our specification choices in Table 7. We may be interested in accounting for endogenous effects of size and investment at the subsidiary level; in column (1) we show is that our results are quantitatively the same for the increased investment in profitable subsidiaries when controlling for assets. In column (2) we do not winsorize investment, and we obtain the same coefficients in our tests. Further, if we measure subsidiary performance as above median asset turnover rather than above median ROA, we get qualitatively similar results.

If more profitable subsidiaries receive more investment, and this is particularly true for those subsidiaries held by firms in the pilot program, then we might expect that

those subsidiaries are less profitable. In Table 8, we present results that examine ROA of those subsidiaries that received increased investment during the Regulation SHO period (column 1) and following the Regulation SHO period (column 2). We find that subsidiaries of pilot program firms do not see reduced ROA when compared to subsidiaries of non-pilot firms, either during the pilot period or afterward. Given that allocating capital to high performing subsidiaries does not lead to a decrease in profitability indicates that overall the disciplining scrutiny pilot firms received was likely welfare increasing.

We have shown that potentially increased attention leads to reallocation across subsidiaries, and that this reallocation goes to more productive subsidiaries. In Table 9, column (1), we investigate the same specification as in Table 5, column (2), but instead interacting Regulation SHO with productivity within industry. We show that pilot firms invested 23% more in subsidiaries that were above the median in ROA within their industry in their country. These results lend credence to our earlier presented findings, showing that firms are reallocating taking into account relative productivity within industries, and within countries. In column (2), we look at R&D intensity, and find that pilot firms spend more on R&D particularly at subsidiaries that already were centers for R&D spending. Altogether, these findings show that increased scrutiny led to distinct

outcomes in terms of reallocation, and had implications for the reallocation of R&D spending across subsidiaries as well.

5 Conclusion

We investigate the effect of short selling threats on intrafirm capital allocation by exploiting the pilot program of Regulation SHO as a natural experiment and novel microdata on the foreign subsidiaries of U.S. multinational firms. Our primary finding is that managers subject to greater short selling threats allocate capital more efficiently within firm boundaries. This evidence suggests that (i) short sellers impose disciplining governance on corporate policies, and that (ii) there are cross-border capital markets regulation spillovers due to within firm decisions.

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Table 1. Summary Statistics

This table presents summary statistics for the parent companies and subsidiaries that comprise the sample. The data are collapsed into time periods before, during, and after Regulation SHO, as recommended in Bertrand, Duflo, and Mullainathan (2004). Consequently, the unit of observation is the parent-period and the subsidiary-period. All currency denominated variables are recorded in millions of 2009 U.S. dollars. Investment is the natural log of capital expenditures. Employees is the natural log of the number of individuals employed by the parent or subsidiary. SD(∆Investment) and SD(∆Employment) are defined precisely in the main text; loosely speaking they are the standard deviation of changes in investment and employment at a parent firm's subsidiaries. Age is measured in years since the parent or subsidiary first appears in the data. Assets is the natural log of total assets. R&D intensity is the ratio of R&D expenditures to total sales. ROA is the ratio of net income to total assets.

Panel A: Reporter Summary Statistics			
	Observations	Mean	SD
Investment	752	11.9	1.5
Employees	752	9.3	1.3
SD(∆Investment)	752	10,023.7	14,707.3
SD(∆Employees)	752	145.1	186.4
Age	752	1.4	0.4
Panel B: Subsidiary Summary Statistics			
	Observations	Mean	SD
Investment	11,150	7.0	2.6
Age	11,150	1.2	0.6
Assets	11,150	11.6	1.2
R&D intensity	11,022	0.006	0.012
ROA	11,150	0.067	0.099

Table 2. Regulation SHO Does Not Impact Consolidated Investment or Employment

This table indicates that the firms subject to Regulation SHO do not change their investment or employment policies on a consolidated basis. Consolidated figures are obtained by aggregating values from the U.S. reporter with values from the reporter's foreign subsidiaries. SHO is an indicator variable that equals one for Regulation SHO pilot firms. Post is an indicator variable that equals one beginning in 2005. The remaining variables are defined in Table 1. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Investment	Employment
	(1)	(2)
SHO x Post	0.09	0.01
	(0.06)	(0.03)
X_{it}	Yes	Yes
Parent fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	752	752
R^2	0.96	0.99

Table 3. Regulation SHO Is Associated with Large Changes in Investment Across Subsidiaries

This table provides evidence of cross-subsidiary investment reallocation among firms subject to Regulation SHO. In the case of investment, the outcome variable is the average standard deviation in the annual change in investment among a reporter's subsidiaries in each period. The employment outcome variable is calculated analogously. The other variables are defined in Tables 1 and 2. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	SD(ΔInvestment)	SD(ΔEmployees)
	(1)	(2)
SHO x Post	2,748.8**	9.5
	(1,210.9)	(14.7)
X_{it}	Yes	Yes
Parent fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	752	752
R^2	0.87	0.87

Table 4. Dynamic Specification: Changes in Investment Do Not Precede Regulation SHO

This table present evidence on the dynamic relationship between Regulation SHO and dispersion in reporters' subsidiaries' investment policies. The unit of observation is reporter-year. I_2004 is an indicator variable that equals one in 2004. The other annual indicator variables are defined analogously. The remaining variables are defined in Tables 1 and 2. Standard errors are clustered by reporter. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	SD(ΔInvestment)
SHO x I_2004	403.8
	(255.1)
SHO x I_2005	2,792.6**
	(1,199.9)
SHO x I_2006	2,221.4*
	(1,178.3)
SHO x I_2007	573.6
	(1,289.7)
SHO x I_2008	645.4
	(1,376.7)
SHO x I_2009	742.6
	(1,374.0)
SHO x I_2010	1,056.2
	(1,347.6)
X_{it}	Yes
Parent fixed effects	Yes
Year fixed effects	Yes
Observations	3,745
R^2	0.85

Table 5. Baseline: Regulation SHO Firms Shift Investment to More Profitable Subsidiaries

This table presents evidence that Regulation SHO is positively related to the reallocation of subsidiary investment toward profitable subsidiaries. The unit of observation is the subsidiary-period. Outperform is an indicator variable that equals one if a subsidiary's average ROA is above the median average ROA of a reporter's subsidiaries in 2000-2004, the five years prior to the implementation of Regulation SHO. The remaining variables are defined in Tables 1 and 2. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Investment (1)	Investment (2)
SHO x Post x Outperform	0.30**	0.30**
	(0.12)	(0.12)
SHO x Post	-0.20**	-0.21**
	(0.09)	(0.09)
Post x Outperform	0.14**	0.13**
	(0.07)	(0.06)
X_{it}	No	Yes
Subsidiary fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	11,150	11,150
R^2	0.84	0.85

Table 6. Investment Behavior Reverses After Regulation SHO

This table provides evidence that the reallocation of investment toward profitable subsidiaries is not reversed once Regulation SHO was concluded. In this case, the treatment period relies on data from 2007-2010. The variables are defined in Tables 1, 2, and 5. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Investment	Investment
	(1)	(2)
SHO x Post x Outperform	0.02	0.02
	(0.11)	(0.11)
SHO x Post	-0.14	-0.13
	(0.08)	(0.08)
Post x Outperform	0.16**	0.14**
	(0.06)	(0.06)
X_{it}	No	Yes
Subsidiary fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	13,868	13,868
R^2	0.83	0.83

Table 7. The Baseline Findings Are Robust

This table provides evidence of the robustness of the baseline reallocation findings. Column 1 includes a control for subsidiary size, which is omitted from the baseline specification due to potential endogeneity concerns between size and investment. In column 2, the outcome variable is not winsorized. In column 3, outperforming subsidiaries are identified by above median asset turnover. Asset turnover is measured as the ratio of sales to total assets and is otherwise calculated analogously to the profitability measure of outperformance. The other variables are defined in Tables 1 and 2. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Investment (1)	Investment (2)	Investment (3)
SHO x Post x Outperform	0.28**	0.30**	0.20*
	(0.12)	(0.12)	(0.12)
SHO x Post	-0.23***	-0.20**	-0.16*
	(0.09)	(0.09)	(0.09)
Post x Outperform	0.08	0.15**	0.00
	(0.07)	(0.07)	(0.06)
X_{it}	Yes	Yes	Yes
Subsidiary fixed effects	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes
Observations	11,150	11,150	11,150
R^2	0.85	0.85	0.85

Table 8. Reallocation Does Not Diminish Profitability

This table indicates that subsidiaries to which investment is shifted do not fall in profitability either during Regulation SHO (column 1) or afterward (column 2). The variables are defined in Tables 1, 2, and 5. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	ROA	ROA
	(1)	(2)
SHO x Post x Outperform	0.00	-0.01
	(0.01)	(0.01)
SHO x Post	-0.00	-0.00
	(0.00)	(0.00)
Post x Outperform	-0.06***	-0.08***
	(0.00)	(0.00)
X_{it}	Yes	Yes
Subsidiary fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	11,150	13,876
R^2	0.79	0.74

Table 9. Regulation SHO Firms Shift Investment Outperforming Industries and R&D to R&D Centers

This table provides evidence that firms shift investment to outperforming industries and R&D to R&D centers. OutInd is an indicator variable that equals one if the foreign subsidiaries of U.S. firms report above median average profitability in that industry and country during 2000-2004, the years leading up to Regulation SHO. RDCent is an indicator variable that equals one if a subsidiary reports average R&D intensity during 2000-2004 above the median among the subsidiaries at its parent firm. R&D intensity is defined as the ratio of R&D expenditures to sales. The other variables are defined in Tables 1 and 2. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Investment	R&D Intensity
	(1)	(2)
SHO x Post x OutInd	0.23*	
	(0.12)	
SHO x Post x RDCent		0.001*
		(0.001)
SHO x Post	-0.17**	-0.001**
	(0.09)	(0.001)
Post x OutInd	0.04	
	(0.06)	
Post x RDCent		-0.002***
		(0.000)
X_{it}	Yes	Yes
Subsidiary fixed effects	Yes	Yes
Period fixed effects	Yes	Yes
Observations	11,006	11,006
R^2	0.85	0.89