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Short-Sale Constraints and Corporate Investment

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Abstract

In a sample of non-U.S. regulatory regime shifts, we find that expanded short selling is associated with stock price declines, reductions in capital expenditure, and lower asset growth. In a reversal of results found for U.S. stocks in a study of Regulation SHO by Grullon, Michenaud, and Weston (2015), our results are stronger for large firms than for small firms. We also show that this investment effect is stronger for firms that previously relied on outside financing. Our results suggest that short-sale policies affect corporate investment and that this effect is not driven by capital constraints.

I. Introduction

Regulators seek to reduce capital market frictions to strengthen financial markets and ultimately facilitate corporate investment. While the effect of short-sale constraints on market quality is well studied,[1] there is relatively little evidence

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[1]A representative sample includes, for example, Jennings and Starks (1986), Bris, Goetzmann, and Zhu (2007), Boehmer, Jones, and Zhang (2013), Diether, Lee, and Werner (2009), Edwards and Hanley (2010), Marsh and Payne (2012), Beber and Pagano (2013), Boehmer and Wu (2013),

on the effect of short-sale constraints on corporate investment. Most notably, Grullon, Michenaud, and Weston (2015) examine changes in corporate investment around Regulation SHO (a U.S. regulatory change that relaxed some short-sale constraints) and document a decrease in investment and equity issuance, but only for smaller firms. They conclude that this link between short selling and corporate investment is related to financial constraints. As such, their results support a potentially benign view of short-sale restrictions: that short-sale restrictions benefit otherwise financially constrained firms and allow them to reach optimal investment levels (Campello and Graham (2013)).[2]

While the analysis in Grullon et al. (2015) has not been called into question, doubts have been raised as to the reliability of inferences drawn from studies using the Regulation SHO setting. The most general concern, voiced by Heath, Ringgenberg, Samadi, and Werner (2020), is that when many studies use this one setting, some spurious results are likely to be encountered. Other studies question the underlying structure of the Regulation SHO experiment, raising concerns about the timing and existence of any direct effect of this regulation on short selling or stock prices as well as the degree to which we can rely upon studies of this regulation's secondary effects, such as an impact on investment (see Bai (2008), Diether et al. (2009), and Litvak et al. (2022)).[3] Given the importance of the investment question to any short-sale policy discussion, and particularly the conclusion regarding financial constraints, analyses in new settings are needed. We provide such evidence, and new insights, by examining these effects with a series of major non-U.S. regulatory regime shifts that relaxed short-sale constraints and do not suffer from some of the concerns leveled at the Regulation SHO setting. Most notably, while our full sample results are consistent with Grullon et al. (2015), our findings with regards to firm size are reversed: we find the effect to be *positively* associated with firm size.

Directly examining the effect of short selling on investment tends to be difficult because investment decisions, firm value, and short selling are jointly determined. As with other studies, we address this challenge by exploring regulation-induced shocks to short selling. In our case, we examine five non-US economies that made substantial changes (regime shifts) on different dates that expanded short selling. In our staggered difference-in-differences analysis based on these country-level regime shifts, we are essentially using firms in countries without a contemporaneous regulation change as controls when documenting the effects of the regulation change on corporate investment. Our approach reduces the concern that results are driven by unrelated shocks that occur at the time of a regulation change (Roberts and Whited (2013)).

Engelberg, Reed, and Ringgenberg (2018), Li, Lin, Zhang, and Chen (2018), and Nishiotis and Rompolis (2019).

[2]Another possible explanation is that the effect is driven by bear raids, which have more of an effect on financially constrained firms (see Goldstein, Ozdenoren, and Yuan (2013)). Once again, however, our results offer a contrasting view.

[3]This is not to say all studies question the Regulation SHO setting. Some papers provide results (or mixed results) consistent with the assumption that Regulation SHO affected short selling and prices and might therefore have secondary impacts (see Bai (2008), Chu, Hirshleifer, and Ma (2020), Deng, Gao, and Kim (2020) and, of course, Grullon et al. (2015)). Our point is that conclusions are not yet firmly established and additional analyses are warranted.

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We find that stock prices drop around the regulatory regime shifts we examine and that both capital expenditure and growth in total assets are lower afterward. More importantly, we find that stock prices drop more for larger firms than smaller firms immediately after the regime shift, and a similar cross-sectional result holds for both capital expenditure and asset growth. We test the link between price declines and investment directly and find that magnitude of investment declines is related to the price drops. To the extent that investment should be tied to financing activities, we document a subsequent reduction in both debt and equity issues, and these effects are also more pronounced for larger firms than smaller firms.

We provide additional validation of our central results through a variety of tests.

- To address concerns that the effects may reflect already established trends (concerns as to whether the parallel-trends assumption is valid), we follow Bertrand and Mullainathan (2003) and include two lagged periods of our investment variables in our baseline regression. Including these lags does not alter our results and the loading on those prior periods is insignificant.
- The estimated treatment effects in staggered difference-in-differences analyses, such as those that establish our central results, reflect a weighting of estimates associated with various component comparisons. Goodman-Bacon (2021) notes that a few highly weighted observations can lead to conclusions inconsistent with most of the component comparisons and suggests a direct examination of component estimates to assess the robustness of any results. We do so and find no evidence suggesting our results are driven by a subset of comparisons.
- If the investment changes are driven by regulatory regime shifts, our results should not be isolated to the period immediately after the regulation change. In addition to omitting the year in which a change occurred to ensure transient effects do not drive our results, we look at the dynamics of investing decisions and find that investment levels are lowered, relative to years prior to the regulatory changes, for at least 5 years after the regulatory changes. Furthermore, we find no evidence of a shift in investment for smaller firms, only for larger firms.
- We construct a placebo test in which we examine changes in investment around randomly generated false regulatory changes. The coefficients on the true changes are clearly statistically different from the placebo coefficients based on the distribution of placebo coefficients.
- Three of our regime shifts result from the introduction of short selling for all stocks in the country, whereas two regime shifts (China and Hong Kong) involved the largest expansion of short selling for a list of stocks available for shorting. This list was revised at times prior to that major expansion and revised subsequently as well. Thus, for China and Hong Kong, we can exploit within-country variation in changes to short-sale restrictions and build a difference-in-differences test around those changes.[4] In such tests, we again observe a decline in investment associated with expanded short selling and, though slightly weaker, evidence that the effect is more pronounced for larger firms.

[4]Note that in our main tests, we limit our sample to the firms affected by the regime shift in a given country and use firms in other countries as the control set. In the Hong Kong and China tests, on the other hand, we keep all firms in those two countries and use unaffected firms as the control set.

- In our main tests, we excluded nine countries from our analysis that had regulation regime shifts that allowed short selling, but where short selling remained largely infeasible for other reasons (see Bris et al. (2007), Chang, Luo, and Ren (2014)). We include these countries in an alternate specification that tests whether the reduction in investment associated with regulatory changes is more pronounced for the economies where short selling is not only expanded but also feasible than it is for economies where short selling is infeasible despite a regulatory change. We find this to be the case.

Given that Grullon et al. (2015) find results are stronger for small firms, they suggest the investment effect is related to financial constraints. We consider this possibility directly by looking at the financing activities of firms *before* the regime shift and find that those firms that accessed external capital more frequently see the largest decline in investment. Thus, as with the reversal on the size results, our analysis indicates the investment effect is not related to financial constraints.[5]

Our study contributes directly to the debate on whether short-sale restrictions impact corporate investment and, more specifically, whether such an effect is beneficial or harmful. As noted, we confirm the Grullon et al. (2015) result that relaxing short-sale constraints lower investment, but reverses their results on firm size. This novel reversal of results has implications for our assessment of the desirability of short-sale restrictions. As Campello and Graham (2013) note in their study of the technology bubble, there can be positive externalities associated with overly high stock prices, and higher prices need not result in overinvestment. Given that short-sale restrictions lead to higher prices, if short-sale restrictions facilitate capital raising by capital-constrained (smaller) firms, the result might be more optimal investment levels. If, on the other hand, short-sale restrictions facilitate capital raising by unconstrained (larger) firms, the result is more likely to be overinvestment.[6] Our results, in conjunction with concerns raised about the Regulation SHO setting employed by Grullon et al. (2015), support the latter conclusion. In this respect, our results are related to Massa, Wu, Zhang, and Zhang (2015), who explore the effect of short selling on managerial myopia and provide evidence that an increased threat of short selling (short-sale potential) reduces underinvestment and encourages investing for long-term benefits.[7] While the actual effect of short selling on investment differs in our two studies, both suggest short selling leads to more appropriate investment levels (in our case, by reducing overinvestment; in their case, by encouraging long-term investments).

[5]While we follow earlier studies and partition by firm size within our sample, we note that our results are similar if we use cutoffs consistent with the Grullon et al. (2015) sample.

[6]Grullon et al. (2015) provide an alternative argument that does not rely on capital constraints but arrives at a similar conclusion. As noted in Goldstein and Guembel (2008), bear raids drive down stock prices and may reduce corporate investment if managers assume stock prices are informative about future prospects. This suggests that restrictions on short-sale constraints, which hamper the bear raids commonly seen in small firms, would again lead to more optimal (higher) investment levels for those firms.

[7]They argue the beneficial effect of short selling arises from two related effects: monitoring of firm actions by short sellers and firms relying on the more informative market prices that short selling might induce.

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Many studies look at short selling-induced price effects to explore questions related to market efficiency.[8] Initial work documented a link between prices and corporate decisions (e.g., Morck, Shleifer, Vishny, Shapiro, and Poterba (1990), Blanchard, Rhee, and Summers (1993), Chirinko and Schaller (2001), Gilchrist, Himmelberg, and Huberman (2005), Goldstein and Guembel (2008), Polk and Sapienza (2009), and Bond, Edmans, and Goldstein (2012)), while some specifically documented an impact on investment (e.g., Campello, Ribas, and Wang (2014), Massa et al. (2015), and He and Tian (2016)). Many of these studies propose and explore mechanisms that might drive such links, generally through the effect of short selling on price informativeness. For example, relaxing short-sale constraints might lead to more informative stock prices which, in turn, could reduce the cost of capital and expand the set of profitable investments. Relaxed limits on short selling would thereby increase investing. Alternatively, the higher prices that result from limits on short selling may either encourage well-intentioned managers to invest when they should not have invested or encourage managers to knowingly issue overpriced equity and invest the resulting proceeds. Relaxed limits on short selling would thereby decrease investing. These mechanisms, along with others, are not mutually exclusive and all are likely to play a role, as suggested in Campello et al. (2014).[9]

As for generating direct insights on the aforementioned mechanisms in our setting, we did explore the link through the cost of equity and found the cost of equity estimates to be quite noisy and unrelated to investment (they were, in fact, unrelated to *any* of the typical dependent variables of interest in such studies).[10] We also explored the link between market-to-book ratios, often used as a signal of future investment profitability, and investment levels and found weak evidence of a positive link. However, other studies have already established how more accurate information drives investment in other (more compelling) settings (see, e.g., Bris et al. (2007), Chang, Cheng, and Yu (2007), and Boehmer and Wu (2013)) and we would simply be reaffirming that work. The central purpose of our study, then, is to use our setting to provide needed new evidence with respect to the very existence of an investment effect and its cross-sectional properties with respect to firm size. Our analysis suggests future research on mechanisms should explore why the effect is more pronounced for larger firms.

Subsequent sections discuss our sample of regulatory regime shifts; describe our data; present our central results; present our robustness tests; and draw conclusions from the analysis.

[8]Early work documenting price distortions arising from short-selling restrictions includes Miller (1977), Diamond and Verrecchia (1987), Levine (1991), and Holmström and Tirole (1993), among others. Later work typically confirms the price effect and then explores what Litvak, Black, and Yoo (2022) characterize as the "indirect effects" of those distortions. Such effects include changes to corporate innovation (Massa et al. (2015), He and Tian (2016)), disciplining earnings management (Massa, Zhang, and Zhang (2015), Fang, Huang, and Karpoff (2016)), and incentive contracting (De Angelis, Grullon, and Michenaud (2017)).

[9]These mechanisms are discussed in detail in Baker, Stein, and Wurgler (2003), Chen, Goldstein, and Jiang (2007), Campello and Graham (2013), and Massa, Wu, Zhang, and Zhang (2015), among others.

[10]Cost of equity estimates are commonly generated from residual income models (see Claus and Thomas (2001), Gebhardt, Lee, and Swaminathan (2001), Gode and Mohanram (2003), and Dhaliwal, Krull, and Li (2007)). We employed the method of Francis, Khurana, and Pereira (2005) since we, as in that paper, are looking at a global setting.

II. Sample of Regulation Regime Shifts

To generate an initial list of countries that made significant changes (regime shifts) that broadly permitted short selling, we use three academic papers (Bris, Goetzmann, and Zhu (2007), Charoenrook and Daouk (2009), and Jain, Jain, McInish, and McKenzie (2013)). From these papers, we identified 24 countries where regime shifts occurred after 1990.[11] Prior research is not always fully aligned on when these regime shifts occurred, so we contacted regulators in these 24 countries for more information.[12] We then deleted countries i) whose regulators informed us that short selling was actually allowed before 1990 (Spain, New Zealand, and Hungary); ii) that were covered in only one of the above-noted three papers and where there was no information forthcoming from the regulators (Luxembourg, Fiji, Greece, Peru, Taiwan, and Namibia); and iii) that reversed their decision to allow short selling within 3 years of having allowed it (Malaysia). This left us with a study sample of 14 countries (we use the term *country* to refer to either countries or distinct economies within a country such as Hong Kong). These 14 countries span three continents (South America, Asia, and Europe) and various degrees of economic development.

While countries may shift regulations to broadly allow short selling, prior studies emphasize that this does not always result in short selling becoming feasible: tax rules, frictions, market laws (Bris et al. (2007)), and high costs (Chang et al. (2014)) may be such that short selling is not feasible. Using the information in Bris et al. (2007) and Charoenrook and Daouk (2009) on feasibility and information in Jain et al. (2013) on short-sale usage, we identify five countries with regulatory shifts that expanded the number of firms that could be shorted and where shorting was also feasible: China, Hong Kong, Norway, South Korea, and Sweden.[13] For the rest, short selling, while broadly allowed, was not feasible: these countries were Argentina, Chile, Finland, India, Indonesia, Philippines, Poland, Thailand, and Turkey. Table 1 contains information on the regulation change for each of these countries and our information source. Our baseline methodology is a staggered difference-in-differences analysis based on country-level regime shifts for those countries that had regime shifts and where short selling was also feasible: we look at firm-year observations with an indicator identifying those firm-years that are after

[11] As long as at least one paper specifies that short selling becomes legal in a country after 1990, we consider that country for our sample.

[12] In some cases, we are not able to obtain the date from the regulator, and so use the date from the literature. These countries are Argentina, Finland, Norway, and Poland. For the remaining countries, the dates we obtain from regulators/exchanges match at least one of the three academic studies we cite above (except for China and India, for which regulations change after the sample period covered in the three studies).

[13] Regarding feasibility, we are able to obtain information from regulators for China, India, South Korea, and Sweden. Jain et al. (2013) report scaled borrowing ratio (SBR). SBR is the daily average outstanding dollar borrowing during the period from July 2006 to Jan. 2010, divided by the country's total stock market capitalization at the end of the previous year. A large number of regulation changes in our sample happen before 2006, however, if this statistic is low for years after the regulation change, then it is likely that short selling never became feasible after the regulation change. We classify short selling as unfeasible if SBR ratio is below 0.03.

TABLE 1

Short-Sale Regulation Changes Around the World

Table 1 contains information on the countries that had major shifts in regulation toward allowing short selling. We obtain information from each country's regulator or exchange, and from Bris, Goetzmann, and Zhu (BGZ) (2007), Charoenrook and Daouk (CD) (2009), and Jain, Jain, McInish, and McKenzie (JJMM) (2013). For each country, we present the date of the regulation shift, the number of short-sale-eligible firms with available accounting data, details on the regulation shift and source of information. Panel A lists those countries with regulation shifts in which short selling was also feasible (those in our main tests), while Panel B lists those countries with regulation shifts in which short selling, though allowed, was infeasible (those are used as additional controls in an expanded test).

Countries	Date	No. of Firms	Institutional Details
Panel A. Study Sample (Countries Where Short Selling Is Feasible)			
China	1/31/2013	267	In China, the Chinese Securities Regulatory Commission (CSRC) first allowed short selling for a list of 90 blue-chip stocks in Mar. 2010. The list was subsequently revised. According to the short-sale data provided by CSMAR (China Stock Market and Accounting Research Database) and Chang, Luo, and Ren (2014), short selling becomes widely practiced following short-selling list revision on 1/31/2013, which added 276 stocks to the list
Hong Kong	5/1/1997	56	Hong Kong first allowed short selling for a list of 17 stocks in Jan. 1994. The list was subsequently revised. On 5/1/1997, the Stock Exchange of Hong Kong (SEHK) made its first major revision to the short-selling list and added 129 new stocks to the short-selling designated list. This is the date we use in our study. Short selling becomes feasible after 1997 as per CD and BGZ
Norway	9/1/1999	187	According to CD, BGZ, and JJMM, Norway allowed short selling in 1992, and short selling becomes feasible in Sept. 1999, as per CD
South Korea	1/1/2000	613	According to BGZ and JJMM, short selling is first allowed on 9/1/1996, but does not become widely practiced as a direct result of this regulation change (see CD and BGZ). However, according to the regulator, the Korea Securities Depository implemented some significant regulatory changes in 2000, making short-sale transactions active
Sweden	8/1/1991	207	We obtain information from Sweden's financial regulator: Finansinspektionen. Although short selling was not banned for nonfinancial stocks, it was very difficult to short stocks before 1991. The law changed on Aug. 1, 1991, making short selling feasible for all market participants
Panel B. Secondary Control Sample (Countries Where Short Selling Is Not Feasible)			
Argentina	9/6/1999	74	We get information on the date of the regulation change and feasibility from CD, BGZ, and JJMM
Chile	10/1/1999	13	According to Superintendency of Securities and Insurance, Chile first allowed short selling in 10/1/1999, and short selling initially opened for 23 stocks. We classify short selling as not feasible as per CD and BGZ
Finland	1998	183	We get information on the date of the regulation change and feasibility from CD, BGZ, and JJMM
India	4/21/2008	128	SEBI (Capital Market Regulator in India) started regulated Short selling vide Circular – MRD/DoP/SE/Dep/Cir-14/2007 on Apr. 21, 2008. There are 221 securities traded in the F&O segment eligible for short selling, but only 130 of those securities remain in the list through 2011. SEBI stated that short selling is not yet widely practiced, which is confirmed by the low security lending ratio (less than 0.1%)
Indonesia	6/30/2008	562	Bapepam-LK (Indonesian Capital Market and Financial Institution Supervisory Agency) started regulated short selling in June 2008 (see Bapepam Decree No. Kep-258/BL/2008 dated June 30, 2008). Short selling is not feasible, according to CD, BGZ, and JJMM
Philippines	1998	234	Philippine Stock Exchange allowed short selling in 1998. CD and BGZ agree that short selling does not become feasible after this regulatory change
Poland	1/1/2000	74	According to CD, BGZ, and JJMM, short selling is first allowed on 1/1/2000. Both CD, and BGZ state that short selling is not widely practiced after this regulatory change
Thailand	1/3/2001	41	On Jan. 3, 2001, the Stock Exchange of Thailand implemented a new regulation to allow stocks in SET 50 to be shorted (Bor.Sor./Khor. 01–00). However, securities lending was not yet developed (the security lending ratio is less than 0.1% in 2002)
Turkey	4/2/1995	24	According to Istanbul Stock Exchange (ISE), short selling is first allowed on 4/2/1995 for stocks part of ISE National 100. However, securities lending and short selling were negligible in the subsequent years (the security lending ratio is less than 0.1% in 2002)

the regulatory regime shift in the firm's home country.[14] In a robustness check, we include firms in all 14 countries and construct a test as follows: we include an indicator variable for the regime shift in every country, but focus our attention on an interaction term between that indicator and an indicator that short selling was feasible (an indicator of the countries in our main tests). This test, which also effectively expands the number of controls, addresses the possibility that the changes we observe are due entirely to some aspect of short-sale regulatory changes that do not actually affect short selling.

The list of countries where short selling was feasible includes two countries where short selling had been allowed for an expanding list of designated firms: China and Hong Kong. In our baseline tests, we include only the firms that were affected by the largest expansion of those lists, and as noted previously, the control set for the difference-in-differences test consists of firms in the other countries that were not affected at the time of the expansion. In an alternate test, we exploit the smaller (though not inconsequential), within-country changes in the China and Hong Kong lists.[15] In those tests, we include all firms in those two countries, and the unaffected firms comprise the control set.

III. Data and Sample

We obtain data for accounting measures and stock market returns from Thomson Reuters Datastream from 1990 to 2018. We look at regulation changes between 1990 and 2018 because data is scarce in earlier periods. The country that made the earliest regulation change in our sample is Sweden, which changed in 1991. China is the last country in our sample to have changed regulations, it changed in 2013.

Our analysis includes all firms with information available on Datastream except financial firms, which are excluded. We use Datastream's list of active and dead firms to avoid survivorship bias. Table 1 includes the number of firms that comprise our sample in each country: 1,330 firms from countries where short selling becomes broadly allowed and also feasible (our baseline tests) and 1,333 firms from the remaining sample (used as an expanded control sample in a robustness check). Table 2 reports summary statistics for the sample of firms used in our baseline tests. We define all variables in Appendix A.

We calculate stock returns using the datastream variable TOTAL_RETURN_INDEX. We filter out holidays and nontrading days by deleting dates with low-frequency data on nonzero returns. For each country-day, we count the number of stocks with nonzero returns. We then compare the number of nonzero returns for

[14]Put option markets existed in three of these five countries at the time of the short-selling regulation change: Hong Kong, Norway, and Sweden. Though put option markets contribute to the negative information content of stock prices, there is evidence that short selling contributes more (see, e.g., Hao, Lee, and Piqueira (2013), Deng, Gao, and Kemme (2018)). Thus, we do not expect put option markets to crowd out the short-selling effects we investigate in this article.

[15]The number of stocks affected by the list revisions in China is 90 (Mar. 2010), 189 (Dec. 2011), 276 (Jan. 2013, our regime shift in the main tests), 206 (Sept. 2013), and 218 (Sept. 2014). For Hong Kong, the number of stocks affect is 17 (Jan. 1994), 94 (Mar. 1996), 129 (May 1997, our regime shift in the main tests), 69 (Jan. 1998), 15 (Mar. 1998), 25 (Nov. 1998), 7 (Mar. 1999), 3 (Sept. 1999), 24 (Feb. 2000), 7 (May 2000), 32 (Aug. 2000), 15 (Feb. 2001), 6 (May 2001), and 9 (Aug. 2001).

TABLE 2

Descriptive Statistics

The sample in Table 2 contains descriptive statistics for firms that were affected by the regime shifts in countries where short selling is feasible over our study period, 1990 to 2018. All variables are defined in Appendix A.

	No. of Obs.	Mean	Median	Std. Dev.	P25	P75
ASSET_GROWTH	17,114	5.301	2.288	11.92	−2.062	9.572
CAPEX	17,720	5.666	3.719	5.691	1.415	7.904
CASH_GROWTH	17,698	1.692	0.438	7.777	−2.365	4.416
EQUITY_ISSUANCE	17,900	2.020	−0.342	8.862	−2.013	1.592
DEBT_ISSUANCE	17,520	3.058	0.403	11.06	−3.328	7.716
TOTAL_ASSETS	17,942	3,395	397.3	13,347	131.0	1,573
LEVERAGE	17,634	28.57	27.20	19.69	12.26	42.42
CASH_FLOW	17,942	6.653	6.441	7.811	2.545	11.18
PROFITABILITY	17,931	4.183	4.149	6.589	1.036	7.717
GDP_GROWTH	17,942	4.672	4.020	3.525	2.679	6.847
COUNTRY_RET	17,942	0.242	0.309	0.645	−0.091	0.659
COUNTRY_VOL	17,942	4.040	3.521	1.888	2.793	4.892

each day with that month's average. If the number of nonzero returns is less than 5% of the month's average, we consider that date a holiday and delete the data for that country-date from the sample. Datastream retains the values of TOTAL_RETURN_INDEX for a long time after a firm is delisted. Following Ince and Porter (2006), we get each firm's last nonzero-return day, and set to missing all the zero-return dates that follow. We follow their method for filtering outliers as well.[16] Daily stock and market returns are trimmed at the 1st and 99th percentiles. Accounting variables are winsorized at the 5th and 95th percentiles.

IV. Effect of Short Selling on Stock Prices and Corporate Investment

A. Stock Market Reaction to Short-Sale Regulation Changes

In this section, we investigate the stock market reaction to countries' regulation regime shifts using traditional event study techniques. We compute cumulative abnormal returns (CARs) from abnormal returns where the abnormal return is the difference between a firm's return and the market index. We examine returns during trading days −60 to +120 relative to the date of a regime shift.[17] Results are shown in Table 3. The first part of the table tabulates the CARs over various windows. We test whether CARs over the [0, 60] treatment window are reliably more negative than would be expected by comparing CARs during that window to CARs over a [−60, −1] pre-treatment window and a [61, 120] post-treatment window. We find that the treatment-window CARs are reliably more negative than the CARs over either of the other windows.[18] In untabulated tests, we also find no reliable

[16] They remove returns greater than 300% and smaller than −50%.

[17] For robustness, we use two additional models to estimate CARs: the market model with world index returns to proxy for country market returns; and a variation of market-adjusted abnormal returns where we proxy for market returns using a regional index. Results remain unchanged. We also note that results are similar for each individual country.

[18] Regarding anticipation of the changes just before implementation, we note that there is no statistically reliable cumulative return over the 10 days just prior to the regulation shift.

TABLE 3

Daily Cumulative Abnormal Returns Around Short-Sale Regulation Changes

Table 3 and Figure 1 report cumulative abnormal returns (CARs) around short-sale regulation changes for sample firms. Abnormal returns are market adjusted and are computed as the individual stock return for each stock minus the equal-weighted market returns. We document CARs for various event windows, where day 0 is the effective date of a regulation change. We present mean CARs and *t*-statistics in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	[−60, −1]	[0,60]	[61,120]	Diff. ([0, 60] to [−60, −1])	Diff. ([0, 60] to [61, 120])
Whole sample	−0.055***	−0.088***	−0.058***	−0.033**	−0.030***
	(−5.55)	(−9.89)	(−8.85)	(−2.51)	(−2.73)
Small firms	−0.053***	−0.042***	−0.054***	0.011	0.012
	(−3.35)	(−3.28)	(−5.58)	(0.054)	(0.078)
Large firms	−0.057***	−0.143***	−0.062***	−0.086***	−0.081***
	(−5.28)	(−12.14)	(−7.39)	(−5.40)	(−5.60)
Large minus small	−0.004	−0.102***	−0.008	−0.098***	−0.094***
	(−0.22)	(−5.64)	(−0.60)	(−3.67)	(−4.26)

difference between the CARs over the [−60, −1] pre-treatment window and over the [60, 120] post-treatment window. In fact, the CARs over these two windows are almost identical in magnitude.

Of particular importance to our analysis is the observation that small firms are less affected by regulatory shifts than large firms. This is also explored in Table 3. For small firms, we find no reliable difference in CARs over the treatment and control windows. Therefore, unsurprisingly, for large firms, we find reliable differences more than twice as great as the whole sample. Direct tests of the difference between large and small firms' CARs are not reliably different between small and large firms over the pre-treatment and post-treatment windows, but significantly more negative for large firms over the treatment window. These results confirm other studies that document a decline in stock prices associated with enhanced short selling.[19] Our novel contribution in this regard is to show that in a non-US setting, this effect is more pronounced for larger firms. The statistical results are readily observed in Figure 1, which shows the difference between the two subsamples and the subsamples individually.

The effect of our regime shifts on stock prices might be regarded as an indirect effect since the regulations are intended to directly impact short selling, not necessarily prices. We do not have data on short selling around all of our events, only around the regime shift in China. Figure 2 shows the changes in short-sale volume for the two calendar years around the regime shift in China, which occurred on Jan. 31, 2013. The figure shows both share volume, rising to about 140,000 shares per day per stock, and share volume as a percentage of daily volume, rising to about 0.80% per day per stock. Thus, as expected, allowing short selling did, in fact, result in an increase in short selling.[20]

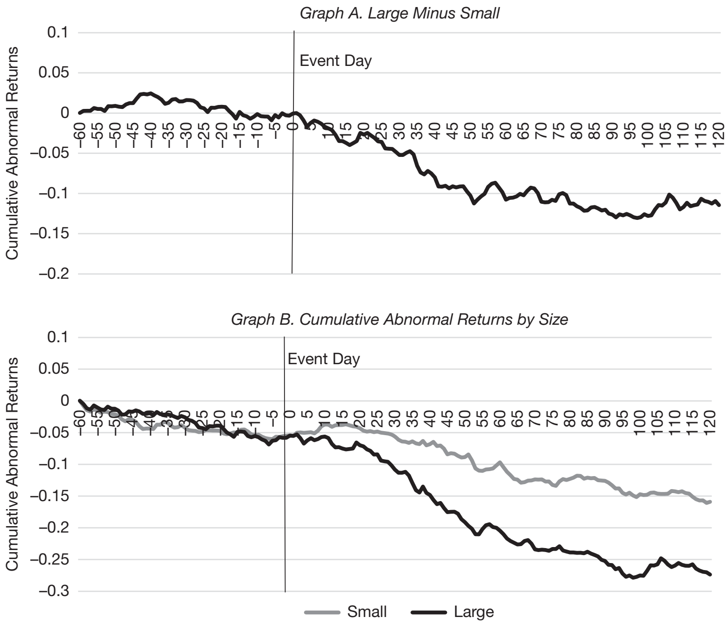
[19]For empirical evidence that prices drop as short-sale constraints decline, see Cohen, Diether, and Malloy (2007), Jones and Lamont (2002), Ofek and Richardson (2003), Ofek, Richardson, and Whitelaw (2004), Chang, Cheng, and Yu (2007), Chang, Luo, and Ren (2014), and Grullon, Michenaud, and Weston (2015).

[20]While we do not have data around the regime changes for other economies, we do have data on short-sale lending starting some years after the events. In unreported analysis, we find there is short-sale

FIGURE 1

Cumulative Abnormal Returns Around Short-Sale Regulation Changes

Figure 1 depicts average cumulative abnormal returns (CARs) around the date regulation changed to allow short selling. The sample is composed of shortable stocks from countries where short selling became feasible. Day 0 is the effective date of the regulation change. Graph A depicts the difference in CARs between large and small firms, and Graph B depicts CARs for large and small firms separately.



B. Corporate Investment

In this section, we present our baseline analyses investigating the effect of short-sale constraints on corporate investment. Table 4 presents our results for all firms. We run panel regressions where the dependent variable is either capital expenditure (CAPEX), annual growth in long-term assets in percent (ASSET_GROWTH), or annual growth in cash in percent (CASH_GROWTH). The first two variables are most closely related to the activity we are investigating: firms making capital investments. It is the focus on capital investment that motivates our focus on long-term assets (total assets less current assets) in ASSET_GROWTH.[21] We include cash growth to test a secondary hypothesis suggested by Stein (1996): that firms may respond to higher equity prices by issuing equity and keeping the amounts in cash (rather than overinvesting).

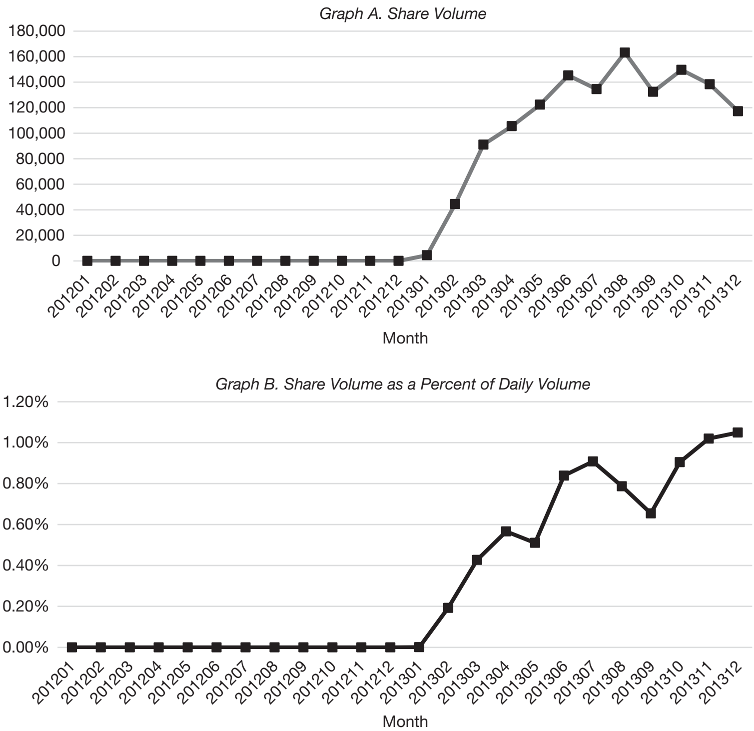
lending equal to about 6% of shares outstanding for firms for which short selling was made feasible when that data does become available. While we cannot contrast this with a pre-event period, we can contrast this with countries where short selling is not feasible. In those countries, short-sale lending is about 0.10% of shares outstanding. A variety of tests, not surprisingly, find a statistically reliable difference between these two samples.

[21]The ASSET_GROWTH variable, notably, includes the effect of long-term assets other than property, plant, and equipment, such as goodwill and assets listed as "other long-term assets."

FIGURE 2

The Effect of the Short-Sale Expansion in China on Short-Sale Activity

Figure 2 shows the average level of daily short-sale activity in China for the firms that were impacted by the expansion in short selling on Jan. 31, 2013. The graph presents monthly averages (across days) of the daily averages (across firms) of short-sale volume in shares (Graph A) and as a percentage of daily trading volume (Graph B).



The independent variable of interest is TREATMENT, an indicator variable for firm-year observations, for a given country, that is after a regulation shift. As noted earlier, the economies in our study sample change regulations in different years, and our estimation essentially uses firms in countries without a regulation shift as controls (see Bertrand and Mullainathan (2003)). We omit the year in which a regulation change occurs so that our results are not driven by short-term effects. Control variables are typical for investment studies and capture the effects of capital constraints (CASH_FLOW), scale (SIZE), availability of firm projects (PROFITABILITY), and the desirability of investment due to macro factors (GDP_GROWTH, COUNTRY_RET, and COUNTRY_VOL). Control variables are defined in Appendix A and are lagged 1 year except for cash flow, which is included to control for internally generated cash flows. All regressions include firm fixed effects to capture time-invariant heterogeneity across firms, and year fixed effects to capture time variation in investment. Standard errors account for firm-level clustering.

Regression 1 shows that the growth rate in CAPEX is reduced by about 1.5% relative to levels before a regulation shift. The mean in our sample is about 6%, so this coefficient implies a reduction of more than 25%. Regression 2 shows that asset

TABLE 4

Corporate Investment and Short-Sale Regulation Changes

Table 4 presents the effect of regulation regime shifts on three measures of corporate investment: capital expenditure (CAPEX), growth in total assets excluding short-term assets (ASSET_GROWTH), and growth in cash (CASH_GROWTH). The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after a regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions have firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX	ASSET_GROWTH	CASH_GROWTH
	1	2	3
TREATMENT	−1.499***	−3.073***	0.261
	(0.266)	(0.487)	(0.263)
CASH_FLOW	0.122***	0.455***	0.312***
	(0.009)	(0.020)	(0.016)
SIZE	−0.811***	−2.926***	−1.843***
	(0.111)	(0.230)	(0.140)
PROFITABILITY	0.112***	0.197***	−0.034**
	(0.011)	(0.024)	(0.017)
GDP_GROWTH	−0.054*	−0.172***	−0.002
	(0.029)	(0.062)	(0.042)
COUNTRY_RET	0.221**	−0.203	0.023
	(0.103)	(0.260)	(0.171)
COUNTRY_VOL	−0.422***	−0.703***	0.170**
	(0.054)	(0.119)	(0.080)
N	17,720	17,114	17,698
Adj. R^2	0.440	0.214	0.094

growth is lower by 3% while the sample mean is 5%, a reduction of at least 40% in the rate of growth. We find no statistically reliable effect of regulation changes on growth in cash. Thus, it would appear that in response to limits on short selling, firms are investing more than they would have (possibly even overinvesting) rather than holding cash. The control variables are of the predicted signs and consistent with prior studies: firms invest more when they have resources, can grow faster when they are small, invest more when opportunities are profitable, and invest less when faced with volatility. The only unusual investment result is that capital expenditure and asset growth are slightly lower when GDP growth is high.

Firm size results are presented in Table 5. Firm size partitions are based on the median firm size within a given country. We show results for both CAPEX and ASSET_GROWTH, and within those two we have four variations: separate regressions for large and small firms with TREATMENT in each, a regression of all firms with an interaction term between TREATMENT, and an indicator that a firm is larger than the median (IND_L), and a regression of all firms with an interaction term between TREATMENT and SIZE. The conclusion in every case is quite clear: we see a significantly larger investment effect for large firms. In fact, for small firms, the CAPEX and ASSET_GROWTH effects are less than half the magnitude of those for large firms. While the regressions with interaction terms constrain the coefficients on control variables to be the same for large and small firms, it is notable that the interaction effect with the indicator is about equal to the difference in coefficients between the large and small firm regressions. Furthermore, the interaction between short selling and size measured continuously is negative in

TABLE 5

Investment Effect with Size Partitions

Table 5 presents the effect of regulation regime shifts on CAPEX and ASSET_GROWTH, with a focus on the difference between larger and smaller stocks. We present three analyses: separate regressions for the sample partitioned into two groups based on SIZE (within-country sorts) and an indicator for the post-regulation change time period (TREATMENT); a regression of the whole sample with TREATMENT interacted with an indicator for firms in the larger size grouping (IND_L); and a regression of the whole sample with TREATMENT interacted with SIZE. The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after a regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions include firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX				ASSET_GROWTH			
	Small	Large	Size Indicator	Size Value	Small	Large	Size Indicator	Size Value
	1	2	3	4	5	6	7	8
TREATMENT	−0.771**	−2.146***	−0.870***	0.611	−1.690**	−3.978***	−1.555***	12.790***
	(0.381)	(0.348)	(0.324)	(1.275)	(0.682)	(0.690)	(0.590)	(2.380)
TREATMENT × IND_L			−1.166***				−2.881***	
			(0.358)				(0.651)	
TREATMENT × SIZE				−0.127*				−0.958***
				(0.074)				(0.137)
CASH_FLOW	0.102***	0.157***	0.121***	0.122***	0.412***	0.536***	0.454***	0.455***
	(0.010)	(0.015)	(0.009)	(0.009)	(0.026)	(0.034)	(0.020)	(0.020)
SIZE	−1.080***	−0.554***	−0.823***	−0.855***	−3.500***	−2.449***	−2.951***	−3.253***
	(0.150)	(0.153)	(0.110)	(0.112)	(0.316)	(0.339)	(0.230)	(0.239)
PROFITABILITY	0.133***	0.083***	0.114***	0.114***	0.217***	0.189***	0.201***	0.206***
	(0.014)	(0.017)	(0.011)	(0.011)	(0.030)	(0.040)	(0.024)	(0.024)
GDP_GROWTH	−0.097**	−0.020	−0.060**	−0.056*	−0.167*	−0.228***	−0.187***	−0.183***
	(0.040)	(0.040)	(0.029)	(0.029)	(0.089)	(0.086)	(0.062)	(0.062)
COUNTRY_RET	0.252*	0.166	0.249**	0.275***	−0.263	−0.143	−0.140	0.202
	(0.146)	(0.146)	(0.103)	(0.102)	(0.354)	(0.369)	(0.260)	(0.265)
COUNTRY_VOL	−0.373***	−0.448***	−0.421***	−0.438***	−0.543***	−0.834***	−0.702***	−0.831***
	(0.080)	(0.073)	(0.054)	(0.055)	(0.169)	(0.167)	(0.118)	(0.120)
N	9,511	8,209	17,720	17,720	9,543	7,571	17,114	17,114
Adj. R^2	0.383	0.502	0.441	0.440	0.192	0.250	0.216	0.218

both cases. All told, the investment effect seems to be greater in magnitude for larger firms.

Our results on the size partition are the reverse of what was documented by Grullon et al. (2015) in their study of Regulation SHO in the United States. One possibility is that the firms classified as small firms in Grullon et al.'s (2015) sample are classified as large firms when we look at our international sample. We replicated our tests using size cutoffs that mirror the Grullon et al. (2015) partitions. The results are unchanged.

We have documented that larger firms see a greater price drop immediately after the regulation change and that the larger firms have a more significant investment effect. The direct driver of any short selling effect should be the drop in prices created by changes in short-sale activity. We directly test this link by partitioning our sample based on the price effect associated with the regulation change. This analysis is shown in Table 6. Here we replicate Table 5, but partition on whether firms have a higher or lower CAR in the 0 to +60 window.[22] Here again,

[22]In an earlier version of this article, we partitioned based on CARs in the (−10 to +10) range. We included the earlier 10 days since Grullon, Michenaud, and Weston (2015) had observed an anticipation of the price effect by that amount of time. Results are similar to those presented here, which use the longer windows we added to this draft to address referee comments.

TABLE 6

Investment Effect with Cumulative Return Partitions

Table 6 presents the effect of regulation regime shifts on CAPEX and ASSET_GROWTH, with a focus on the difference between stocks with relatively larger or small CARs in the period of 0 to +60 days relative to a regulation change. The table presents four regressions for both CAPEX and ASSET_GROWTH: separate regressions for the sample partitioned into two groups based on CAR with an indicator for the post-regulation change time period (TREATMENT); a regression of the whole sample with TREATMENT interacted with an indicator for the more negative CAR grouping (IND_{CAR}); and a regression of the whole sample with TREATMENT interacted with CAR. The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after a regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions have firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX				ASSET_GROWTH			
	High CAR	Low CAR	CAR Indicator	CAR Value	High CAR	Low CAR	CAR Indicator	CAR Value
	1	2	3	4	5	6	7	8
TREATMENT	−1.409***	−2.408***	−1.281***	−1.587***	−2.495***	−4.573***	−1.815***	−2.466***
	(0.413)	(0.403)	(0.357)	(0.296)	(0.708)	(0.884)	(0.613)	(0.531)
TREATMENT × IND_{CAR}			−1.027***				−3.376***	
			(0.389)				(0.700)	
TREATMENT × CAR				1.832**				9.340***
				(0.763)				(1.289)
CASH_FLOW	0.111***	0.129***	0.119***	0.119***	0.489***	0.400***	0.443***	0.435***
	(0.013)	(0.013)	(0.009)	(0.009)	(0.031)	(0.032)	(0.022)	(0.022)
SIZE	−0.870***	−0.803***	−0.841***	−0.843***	−2.865***	−3.321***	−3.086***	−3.223***
	(0.171)	(0.164)	(0.120)	(0.119)	(0.346)	(0.364)	(0.253)	(0.248)
PROFITABILITY	0.110***	0.114***	0.113***	0.114***	0.190***	0.232***	0.213***	0.218***
	(0.019)	(0.015)	(0.012)	(0.012)	(0.039)	(0.034)	(0.026)	(0.026)
GDP_GROWTH	−0.086**	−0.032	−0.065**	−0.066**	−0.283***	−0.231**	−0.278***	−0.272***
	(0.040)	(0.054)	(0.031)	(0.031)	(0.090)	(0.111)	(0.065)	(0.065)
COUNTRY_RET	0.293*	0.109	0.204*	0.212*	0.087	−0.403	−0.149	−0.034
	(0.163)	(0.187)	(0.114)	(0.114)	(0.421)	(0.458)	(0.278)	(0.281)
COUNTRY_VOL	−0.444***	−0.358***	−0.422***	−0.423***	−0.750***	−0.720***	−0.740***	−0.761***
	(0.079)	(0.114)	(0.060)	(0.060)	(0.182)	(0.231)	(0.132)	(0.130)
N	7,695	7,750	15,445	15,445	7,504	7,311	14,815	14,998
Adj. R^2	0.411	0.437	0.425	0.425	0.219	0.217	0.219	0.216

we test the difference directly with an indicator (IND_{CAR} for firms with more negative cumulative returns) and with an interaction with cumulative returns itself (CAR). Note that since we believe a more negative cumulative return drives lower investment, we would anticipate a positive sign in this interaction. Once again, our results are clear: firms with a more negative cumulative return see a greater drop in investment.

One of the key concerns about difference-in-differences studies is a possible violation of the parallel-trends assumption: that in the absence of a treatment, observable and unobservable factors that drive differences between treatment and control groups are constant over time. There are a variety of ways to address this. Most common, following Bertrand and Mullainathan (2003), is to include indicators for two pre-treatment periods along with our treatment indicators. This is done in Table 7 for the whole sample and in Table 8 for size partitions. Note that since these regressions contain the year of the regulation change, the sample sizes are larger than our earlier regressions. Consistent with our prior results, the coefficients on pre-treatment indicators are not statistically different from zero, consistent with the parallel-trends assumption.

TABLE 7

Testing for Pre-Treatment Trends

Table 7 presents the effect of regulation regime shifts on CAPEX and ASSET_GROWTH around short-sale regulation changes. We expand the baseline model to include indicators for pre- and post-regulation levels for various time periods before and after a regulation change: indicators for the second year before a regulation change (TREATMENT (−2)), for the first year before a regulation change (TREATMENT (−1)), for the year of a regulation change (TREATMENT (0)), for the first year after a regulation change (TREATMENT (1)), and years subsequent to the first year after a regulation change (TREATMENT (2+)). The results are from OLS regressions of firm-year observations for 1990 to 2018. We exclude any firms that were still not allowed to have short selling after a regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions include firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX	ASSET_GROWTH
	1	2
TREATMENT (−2)	−0.503	−0.554
	(0.319)	(0.712)
TREATMENT (−1)	−0.139	0.327
	(0.339)	(0.699)
TREATMENT (0)	−0.958***	−1.895***
	(0.296)	(0.608)
TREATMENT (1)	−1.586***	−2.870***
	(0.333)	(0.638)
TREATMENT (2+)	−1.581***	−3.022***
	(0.336)	(0.622)
CASH_FLOW	0.124***	0.457***
	(0.008)	(0.019)
SIZE	−0.828***	−2.965***
	(0.109)	(0.233)
PROFITABILITY	0.112***	0.189***
	(0.011)	(0.023)
GDP_GROWTH	−0.045	−0.159**
	(0.029)	(0.063)
COUNTRY_RET	0.090	−0.522**
	(0.104)	(0.258)
COUNTRY_VOL	−0.427***	−0.732***
	(0.054)	(0.117)
N	18,822	18,190
Adj. R^2	0.476	0.269

It is common in a standard difference-in-differences setting to present graphs of variables for inspection of pre-event trends. Following Cremers, Guernsey, and Sepe (2019), we included 11 indicators centered on our events in the full regression model and graph those indicators. The results are presented in Figure 3. The filled line markers represent coefficients that are significant at the 5% level. The figures include the whole sample and then small and large firms separately. Consistent with all our results so far, we see a drop in capital expenditure and asset growth subsequent to the regime shifts in the whole sample, and there is no indication that these levels were anticipated, nor any indication that the changes were short-lived. Comparing large firms to small ones, we see no significant shift for small firms but a clear and substantial shift for large firms.[23] Clearly, these graphs show a shift downward in the level of investment in the broad sample and, more importantly, that this shift is a feature of the larger firms.

[23]For small firms, we observe no significant coefficients before or after our regime shifts with the exception of the indicator for year −5 (and before) in the case of ASSET_GROWTH, which is significantly negative. That said, the evidence suggests little or no decrease in investment brought about by our regime changes for small firms.

TABLE 8

Testing for Pre-Treatment Trends with Size Partitions

Table 8 presents the effect of regulation regime shifts on CAPEX and ASSET_GROWTH around short-sale regulation changes partitioned by size. We expand the baseline model to include indicators for pre- and post-regulation levels for various time periods before and after a regulation change: indicators for the second year before a regulation change (TREATMENT (−2)), for the first year before a regulation change (TREATMENT (−1)), for the year of a regulation change (TREATMENT (0)), for the first year after a regulation change (TREATMENT (1)), and years subsequent to the first year after a regulation change (TREATMENT (2+)). IND_L is an indicator for firms in the larger size grouping. The results are from OLS regressions of firm-year observations for 1990 to 2018. We exclude any firms that were still not allowed to have short selling after the regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions include firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX	ASSET_GROWTH
	1	2
TREATMENT (−2) × IND_L	−0.406 (0.530)	1.375 (1.168)
TREATMENT (−1) × IND_L	−0.163 (0.536)	1.071 (1.105)
TREATMENT (0) × IND_L	−1.110** (0.466)	−1.363 (0.954)
TREATMENT (1) × IND_L	−0.974* (0.497)	−2.053** (0.993)
TREATMENT (2+) × IND_L	−1.154*** (0.424)	−2.234*** (0.761)
TREATMENT (−2)	−0.239 (0.441)	−1.214 (0.973)
TREATMENT (−1)	0.003 (0.431)	−0.052 (0.908)
TREATMENT (0)	−0.317 (0.384)	−1.031 (0.778)
TREATMENT (1)	−1.009** (0.414)	−1.691** (0.819)
TREATMENT (2+)	−0.946** (0.400)	−1.814** (0.741)
With controls	Yes	Yes
N	18,822	18,190
Adj. R^2	0.477	0.270

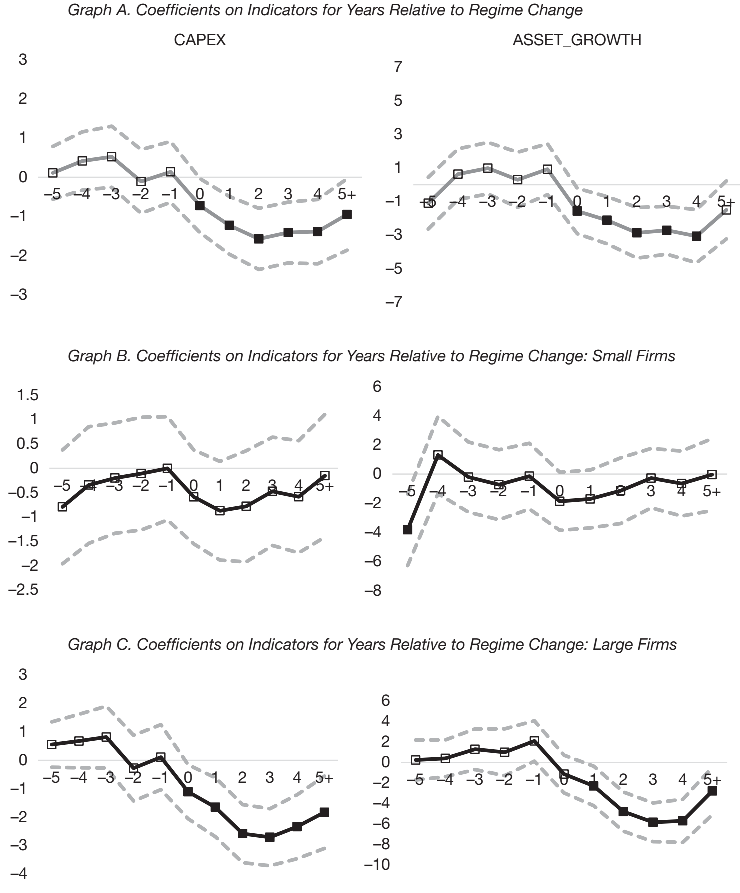
Another potential concern with staggered difference-in-differences analyses is highlighted in Goodman-Bacon (2021), who notes that treatment effects may not be consistent across time and that the structure of traditional staggered difference-in-differences analyses implicitly assigns weights to various treatment comparisons. In an extreme case, a heavily weighted comparison may drive results in a direction inconsistent with most comparisons. A decomposition of the difference-in-differences treatment estimate into various comparison groups can identify such problems. Appendix B provides a detailed discussion of how we adapted our setting to generate the balanced panel needed for a decomposition and the results we obtained. These results do not suggest a small set of unusual comparisons are driving our results.

Essential to our argument is the idea that a change in short selling activity will impact future price levels and therefore investment activity. While we should expect a price change to occur only in a period near the regulation change, the change in short selling behavior should be permanent. Thus, relative to pre-regime-change levels, capital expenditure and asset growth rates should be permanently lowered. Admittedly, over longer horizons, any change that might have occurred would be

FIGURE 3

Dynamics of Investment Effects

Figure 3 illustrates the effect of short-sale regulation change on capital expenditure and asset growth. The *x*-axis shows the time (in years) relative to the regime shifts. The graph presents the coefficient estimates on annual dummy variables from the full baseline regression explaining CAPEX (left) and ASSET_GROWTH (right). The dashed lines correspond to the 95% confidence intervals of the coefficient estimates. Graph A presents the whole sample, Graph B the results for small firms, and Graph C the results for large firms. Confidence intervals are calculated from standard errors clustered by firm and filled boxes indicate significant coefficients.



harder to detect from a statistical point of view, but we also certainly would not expect the effect to be short-lived. It is important to note that in these graphs we see no dissipation in the effects.

C. Financing Activities

As noted, if the reduced levels of investment we document are related to equity pricing, this could result from two effects. First, firms may be taking advantage of the mispricing to issue overvalued equity and, as a consequence, invest more than they would have otherwise. Alternatively, firms may interpret the artificially higher prices as true indicators of future firm prospects and maybe investing accordingly.

TABLE 9

Short Selling Regime Shifts and Future Financing Activity

Table 9 presents the effect of regulation regime shifts on two measures of financing activity: EQUITY_ISSUES (the change in owner's equity net of income effects) and DEBT_ISSUES (the change in total debt). The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after the regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. This specification also includes lagged firm leverage (LEVERAGE). All regressions have firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	EQUITY_ISSUES	DEBT_ISSUES
	1	2
TREATMENT	−0.117	−2.517***
	(0.375)	(0.491)
CASH_FLOW	0.085***	0.013
	(0.017)	(0.021)
SIZE	−3.474***	0.010
	(0.210)	(0.249)
PROFITABILITY	−0.071***	0.059**
	(0.020)	(0.024)
LEVERAGE	0.106***	−0.218***
	(0.007)	(0.010)
GDP_GROWTH	−0.047	−0.007
	(0.048)	(0.063)
COUNTRY_RET	0.156	−0.458*
	(0.193)	(0.254)
COUNTRY_VOL	0.378***	−0.961***
	(0.101)	(0.113)
N	17,615	17,520
Adj. R^2	0.244	0.159

We follow Grullon et al. (2015) and others to look at whether changes in short-sale constraints are associated with changes in financing activities. Finding such a relation would not rule out firms also responding to signals, but would be a necessary condition for any exploitation of mispricing. In our tests, we look at both debt and equity for completeness. Any change in equity issues would also be associated with changes in debt issues if firms are maintaining an optimal capital structure. It is also possible that debt would be mispriced (to some degree) along with any equity mispricing if markets use equity price information to determine yields on debt.

Table 9 presents the results for the whole sample. The first column presents results for the dependent variable EQUITY_ISSUES, which is the annual percentage change in owner's equity net of any income effects. While we observe no statistically reliable change in equity issuance for the whole sample, we note that standard errors are large, suggesting a lack of power, and the observed coefficient is negative. The second column presents results for the dependent variable DEBT_ISSUES, which is the annual percentage change in total debt. Here we see a reduction. The more important results are presented in Table 10. In that table we distinguish, once again, between larger and smaller firms and, as in Table 5, formally test the difference with both an indicator variable (IND_L) and a continuous variable (SIZE). With regards to both equity and debt issues, we see that large firms are employing less outside financing (relative to pre-shift levels) after the regime shift.

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TABLE 10

Investment Effect and Future Financing Activity by Size

Table 10 presents the effect of regulation regime shifts on two measures of financing activity: EQUITY_ISSUES (the change in owner's equity net of income effects) and DEBT_ISSUES (the change in total debt), with a focus on the difference between larger and smaller stocks. We present three analyses: separate regressions for the sample partitioned into two groups based on SIZE (within-country sorts) and an indicator for the post-regulation change time period (TREATMENT); a regression of the whole sample with TREATMENT interacted with an indicator for firms in the larger size grouping (IND_L); and a regression of the whole sample with TREATMENT interacted with SIZE. The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after the regulation change. Column headings indicate the dependent variables and the corresponding subsamples/regressions. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. This specification also includes lagged firm leverage (LEVERAGE). All regressions have firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	EQUITY_ISSUES				DEBT_ISSUES			
	Small	Large	Size Indicator	Size Value	Small	Large	Size Indicator	Size Value
	1	2	3	4	5	6	7	8
TREATMENT	0.993*	−0.997**	1.186**	5.799***	−2.032***	−2.483***	−1.707***	17.714***
	(0.578)	(0.464)	(0.501)	(2.018)	(0.714)	(0.715)	(0.636)	(2.468)
TREATMENT × IND_L			−2.401***				−1.485**	
			(0.525)				(0.743)	
TREATMENT × SIZE				−0.356***				−1.215***
				(0.114)				(0.145)
CASH_FLOW	0.044**	0.147***	0.085***	0.085***	−0.018	0.063*	0.012	0.013
	(0.022)	(0.027)	(0.017)	(0.017)	(0.025)	(0.036)	(0.021)	(0.020)
SIZE	−4.459***	−2.439***	−3.509***	−3.592***	−0.452	0.166	−0.012	−0.395
	(0.287)	(0.309)	(0.212)	(0.220)	(0.323)	(0.385)	(0.250)	(0.256)
PROFITABILITY	−0.033	−0.107***	−0.066***	−0.068***	0.076***	0.060	0.062***	0.072***
	(0.026)	(0.030)	(0.020)	(0.020)	(0.029)	(0.041)	(0.023)	(0.023)
LEVERAGE	0.126***	0.080***	0.108***	0.105***	−0.237***	−0.194***	−0.217***	−0.221***
	(0.010)	(0.010)	(0.007)	(0.007)	(0.013)	(0.015)	(0.010)	(0.010)
GDP_GROWTH	−0.087	−0.060	−0.059	−0.052	0.005	−0.065	−0.014	−0.024
	(0.071)	(0.067)	(0.048)	(0.049)	(0.086)	(0.091)	(0.062)	(0.062)
COUNTRY_RET	0.423	−0.103	0.214	0.309	−0.492	−0.447	−0.424*	0.051
	(0.297)	(0.245)	(0.193)	(0.198)	(0.345)	(0.366)	(0.253)	(0.257)
COUNTRY_VOL	0.263*	0.464***	0.377***	0.330***	−0.458***	−1.440***	−0.962***	−1.127***
	(0.154)	(0.129)	(0.101)	(0.100)	(0.154)	(0.166)	(0.113)	(0.114)
N	9,465	8,150	17,615	17,615	9,376	8,144	17,520	17,520
Adj. R^2	0.276	0.197	0.246	0.245	0.134	0.192	0.159	0.167

In our next tests, we provide additional evidence as to whether the investment effect is related to firms' ability to access equity. Specifically, we examine whether firms that have *historically* relied upon equity are relatively more affected. Again, for completeness, we include past debt reliance as well.

The results are presented in Table 11, with Panel A showing results for past equity issues and Panel B showing results for past debt issues. These regressions are identical to those in Table 5, but instead of conditioning on firm size, we are conditioning on past reliance on outside capital. To measure that reliance, we use the variable EQUITY_RELIANCE, which is the average annual percentage change in equity (not including net income effects) over the 5 years prior to a regulation change. In addition to the continuous variable, we use an indicator for firms with above median reliance, IND_E in the interaction test. The variables DEBT_RELIANCE and IND_D are defined analogously.

We find that both high- and low-equity-reliance firms see a decline in investment, but the effect is more pronounced for those that rely more heavily on equity

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TABLE 11
Investment Effect and Past Financing Activity

Table 11 presents the effect of regulation regime shifts on CAPEX and ASSET_GROWTH, with a focus on the difference between firms that regularly accessed equity and debt financing. Panel A presents three analyses: separate regressions for the sample partitioned into two groups based on EQUITY_RELIANCE, which is the average annual percentage change in equity (not including net income effects) over the 5 years prior to a regulation change, and an indicator for the post-regulation change time period (TREATMENT); a regression of the whole sample with TREATMENT interacted with an indicator for firms with relatively larger reliance on past equity issues (IND_E); and a regression of the whole sample with TREATMENT interacted with EQUITY_RELIANCE. Panel B provides the same analysis, but looking at past debt issues, as measured by DEBT_RELIANCE, which is the average annual percentage change in outstanding debt over the 5 years prior to a regulation change. The indicator of a larger reliance on debt issues is IND_D. The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after the regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. All regressions include firm and year fixed effects. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Past Equity Reliance

	CAPEX				ASSET_GROWTH			
	Less Equity Reliance	More Equity Reliance	Equity Reliance Indicator	Equity Reliance	Less Equity Reliance	More Equity Reliance	Equity Reliance Indicator	Equity Reliance
	1	2	3	4	5	6	7	8
TREATMENT	−1.131***	−1.701***	−1.140***	−1.285***	−2.350***	−4.337***	−1.534***	−1.843***
	(0.390)	(0.402)	(0.304)	(0.296)	(0.638)	(0.773)	(0.558)	(0.522)
TREATMENT × IND_E			−0.635*				−3.980***	
			(0.359)				(0.636)	
TREATMENT × EQUITY_RELIANCE				−0.035				−0.366***
				(0.033)				(0.058)
CASH_FLOW	0.113***	0.142***	0.131***	0.131***	0.476***	0.483***	0.483***	0.484***
	(0.015)	(0.013)	(0.010)	(0.010)	(0.033)	(0.032)	(0.023)	(0.023)
SIZE	−0.844***	−0.804***	−0.813***	−0.823***	−2.895***	−2.473***	−2.572***	−2.539***
	(0.195)	(0.157)	(0.124)	(0.123)	(0.321)	(0.360)	(0.256)	(0.252)
PROFITABILITY	0.134***	0.103***	0.115***	0.115***	0.227***	0.187***	0.200***	0.201***
	(0.016)	(0.020)	(0.013)	(0.013)	(0.035)	(0.039)	(0.026)	(0.027)
GDP_GROWTH	−0.079*	−0.006	−0.046	−0.044	−0.280***	−0.060	−0.197***	−0.188***
	(0.041)	(0.046)	(0.030)	(0.031)	(0.084)	(0.100)	(0.065)	(0.066)
COUNTRY_RET	0.246	0.262	0.258**	0.250**	−0.191	−0.389	−0.302	−0.359
	(0.154)	(0.164)	(0.112)	(0.112)	(0.371)	(0.412)	(0.278)	(0.278)
COUNTRY_VOL	−0.366***	−0.423***	−0.396***	−0.395***	−0.739***	−0.671***	−0.738***	−0.736***
	(0.078)	(0.082)	(0.056)	(0.056)	(0.164)	(0.187)	(0.123)	(0.123)
N	7,179	7,013	14,192	14,192	6,752	6,974	13,726	13,726
Adj. R^2	0.432	0.440	0.438	0.438	0.196	0.247	0.228	0.229

Panel B. Past Debt Reliance

	CAPEX				ASSET_GROWTH			
	Less Debt Reliance	More Debt Reliance	Debt Reliance Indicator	Debt Reliance	Less Debt Reliance	More Debt Reliance	Debt Reliance Indicator	Debt Reliance
	1	2	3	4	5	6	7	8
TREATMENT	−1.127***	−2.112***	−0.778**	−0.784***	−1.155*	−5.455***	−1.037*	−1.274**
	(0.385)	(0.398)	(0.308)	(0.294)	(0.683)	(0.766)	(0.568)	(0.557)
TREATMENT × IND_D			−1.769***				−4.577***	
			(0.350)				(0.622)	
TREATMENT × DEBT_RELIANCE				−0.176***				−0.410***
				(0.027)				(0.051)
CASH_FLOW	0.096***	0.158***	0.127***	0.128***	0.421***	0.539***	0.479***	0.481***
	(0.012)	(0.016)	(0.010)	(0.010)	(0.032)	(0.034)	(0.023)	(0.023)
SIZE	−0.875***	−0.800***	−0.800***	−0.765***	−2.928***	−2.515***	−2.662***	−2.581***
	(0.184)	(0.164)	(0.123)	(0.123)	(0.362)	(0.358)	(0.256)	(0.250)
PROFITABILITY	0.123***	0.121***	0.115***	0.115***	0.192***	0.240***	0.198***	0.198***
	(0.018)	(0.019)	(0.013)	(0.013)	(0.034)	(0.041)	(0.026)	(0.026)
GDP_GROWTH	−0.099**	0.011	−0.044	−0.050	−0.271***	−0.148	−0.202***	−0.214***
	(0.042)	(0.046)	(0.031)	(0.031)	(0.090)	(0.097)	(0.066)	(0.066)
COUNTRY_RET	0.236	0.213	0.235**	0.241**	−0.440	−0.355	−0.386	−0.374
	(0.149)	(0.170)	(0.114)	(0.113)	(0.358)	(0.434)	(0.282)	(0.281)
COUNTRY_VOL	−0.414***	−0.384***	−0.395***	−0.385***	−0.701***	−0.842***	−0.749***	0.728***
	(0.077)	(0.087)	(0.058)	(0.058)	(0.160)	(0.198)	(0.125)	(0.124)
N	7,297	6,515	13,812	13,812	6,984	6,372	13,356	13,356
Adj. R^2	0.396	0.467	0.442	0.443	0.187	0.257	0.230	0.232

financing. Specifically, those firms with higher-than-median reliance see a larger decrease in both capital expenditure and asset growth. This is true for the indicator variable (which effectively tests the difference in the effect between the partitioned samples) for both capital expenditure and asset growth. It is also true for the continuous variable for asset growth, though we see no significant effect in this specification looking at CAPEX. Results for past debt reliance are qualitatively identical to those for equity reliance.

V. Robustness

In Section V, we validate our central results on the investment effect and the difference between large and small firms with a variety of tests.

A. Placebo Tests

To ensure that the results we document are not driven by longer-term trends or other changes not associated with the regulation regime shifts, we conduct placebo regressions for falsification tests (Slusky (2017)). For each country, we randomly assign a pseudo year for the regulation change that is not within 3 years of the actual regulation change. Using these counterfactual years, we estimate the regression models reported in Table 4, the baseline regression showing the effect of regime changes with the variable TREATMENT, and Table 5, the specifications 3 and 7 that capture the difference in the effect of larger firms through the variable TREATMENT $\times$ IND_L. We repeat this procedure 1,000 times. Table 12 presents the results. The average placebo run coefficient and its *t*-statistic are presented first. In no case is their statistical significance. The reported coefficients from the earlier tables are then presented along with where these would sit in the distribution of coefficients created by the placebo runs. The actual coefficients are in the 99th percentile of every placebo run distribution.

TABLE 12

Placebo Tests

Table 12 presents results of a placebo test of the dates of the regulation regime shifts. For each country, we randomly assign a pseudo year for the regulation shift that is between 1990 and 2018 and is not within 3 years of the actual regulation shift. We then estimate the baseline regressions (the regressions with the variable TREATMENT as shown in Table 4) and size-effect regressions (the regressions with the larger firm indicator variable IND_L interacted with TREATMENT as shown in Table 5) based on the counterfactual event-years. We do so 1,000 times for both CAPEX and ASSET_GROWTH as dependent variables. Results are summarized for the variable TREATMENT (baseline) and TREATMENT $\times$ IND_L (size effect). We report the mean coefficient of the placebo runs along with a *t*-statistic of its significance based on the distribution of the placebo run outcomes. We also report the earlier regression coefficients and the percentile in which that regression coefficient sits within the placebo distribution.

	Average Placebo Run Coefficient	*t*-Statistic for Average Placebo Run Coefficient (Based on Placebo Distribution)	Reported Regression Coefficient	Percentile of Reported Regression Coefficient in the Placebo Run Distribution
Baseline regressions (Table 4): Coefficient on TREATMENT				
CAPEX	0.065	0.81	−1.499	99th
ASSET_GROWTH	0.131	0.64	−3.073	99th
Firm size regressions (Table 5): Coefficient on TREATMENT × IND_L				
CAPEX	0.051	0.35	−1.166	99th
ASSET_GROWTH	0.501	1.38	−2.881	99th

FIGURE 4

Coefficients Distribution of Placebo Tests

Figure 4 illustrates the histogram of the coefficient estimates from the placebo runs reported in Table 12. The arrow shows the location of the reported baseline regression coefficients in Tables 4 and 5, relative to the placebo run distribution. Graph A presents results for the coefficient on TREATMENT and Graph B presents results for the coefficient on TREATMENT x INDL.

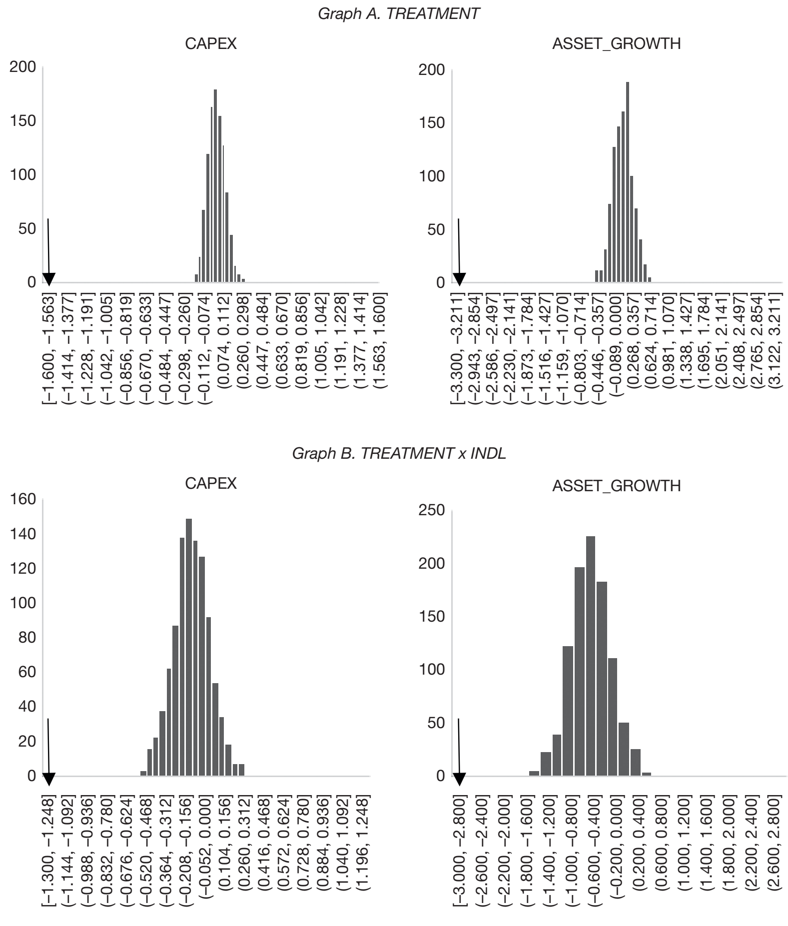

Figure 4 plots the distribution of the placebo run coefficients. In these figures, the coefficient from Tables 4 and 5 regressions is indicated. The figures clearly show that our results stand out relative to the placebo run distributions, suggesting our results are driven by the events we identify.

B. Sample Adjustments

Our empirical structure is a staggered difference-in-differences analysis where we have five shocks to regulation associated with five economies. The power of the test comes from the implicit comparison of all firms in the economy that have had a regulatory regime shift with all the firms in the remaining four economies. In this section, we explore two alternative approaches that adjust the samples used in our

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TABLE 13

Including Countries Where Short Selling Is Not Feasible

The sample in Table 13 includes all short-sale-eligible firms from the 14 countries that changed regulation to allow short selling between 1990 and 2018. The variable TREATMENT indicates the post-regulation regime for all countries. The variable FEASIBLE is an indicator for those countries where short selling is feasible (the countries in our main tests). The results are from OLS regressions of firm-year observations for 1990 to 2018. We omit the event year in which the regulations are changed and exclude any firms that were still not allowed to have short selling after the regulation change. Column headings indicate the dependent variables. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX			ASSET_GROWTH		
	Investment Effect	Investment and Size Effect		Investment Effect	Investment and Size Effect	
	1	2	3	4	5	6
TREATMENT	−0.012 (0.207)	0.071 (0.265)	−0.213 (0.979)	0.349 (0.358)	1.222*** (0.455)	6.117*** (1.633)
TREATMENT × FEASIBLE	−0.833*** (0.261)	−0.283 (0.371)	1.277 (1.647)	−2.428*** (0.463)	−1.889*** (0.646)	2.134 (2.805)
TREATMENT × IND_L		−0.211 (0.336)			−1.920*** (0.561)	
TREATMENT × SIZE			0.010 (0.054)			−0.331*** (0.091)
TREATMENT × IND_L × FEASIBLE		−0.948* (0.498)			−0.701 (0.858)	
TREATMENT × SIZE × FEASIBLE			−0.124 (0.094)			−0.274* (0.161)
CASH_FLOW	0.131*** (0.007)	0.131*** (0.007)	0.131*** (0.007)	0.412*** (0.016)	0.412*** (0.016)	0.413*** (0.016)
SIZE	−0.871*** (0.091)	−0.882*** (0.091)	−0.892*** (0.092)	−2.775*** (0.174)	−2.822*** (0.175)	−2.913*** (0.179)
PROFITABILITY	0.129*** (0.009)	0.130*** (0.009)	0.130*** (0.009)	0.203*** (0.018)	0.206*** (0.018)	0.206*** (0.018)
GDP_GROWTH	0.086*** (0.017)	0.084*** (0.017)	0.086*** (0.017)	−0.038 (0.036)	−0.041 (0.036)	−0.032 (0.036)
COUNTRY_RET	0.195*** (0.069)	0.205*** (0.069)	0.209*** (0.069)	−0.126 (0.162)	−0.101 (0.162)	−0.026 (0.163)
COUNTRY_VOL	−0.198*** (0.036)	−0.196*** (0.036)	−0.203*** (0.036)	−0.157** (0.078)	−0.153** (0.078)	−0.189** (0.078)
N	31,641	31,641	31,641	32,026	32,026	32,026
Adj. R^2	0.418	0.418	0.418	0.211	0.212	0.213

tests. A third approach, mentioned in the introduction, is to look at changes in the sample of firms affected by short selling rules (list revisions) in the two countries that had a number of such adjustments: China and Hong Kong. This analysis is presented separately in Appendix C.

As noted, not all economies that experience regulatory regime changes are ones where short selling is feasible. The effect of short-sale regulation changes in these countries should be much smaller, if at all significant. Furthermore, the firms in these countries could also be used as controls and would capture other (exogenous) changes in trading environments. In Table 13, we add the firms in the other nine countries to our baseline specification. The variable TREATMENT in this test is associated with the regime change in all 14 countries, not just our main sample countries. To test for an investment effect, we include an interaction term between an indicator, FEASIBLE, that denotes those firms in our main tests, and the variable TREATMENT. The results are presented in column 1 for CAPEX and column 4 for ASSET_GROWTH. The interaction term is significant and negative for both

CAPEX and ASSET_GROWTH.[24] As for the effect of TREATMENT itself, it is insignificant in all specifications, confirming that only where short selling is both feasible and permitted do we see an investment effect.

We also use this expanded sample to explore the existence of a size effect. In this case, we rely upon a triple interaction between TREATMENT, FEASIBLE, and both an indicator of large firms and size itself. The results are presented in columns 2 and 3 for CAPEX and in columns 5 and 6 for ASSET_GROWTH. The coefficients on the triple interaction are always of the right sign, but significant for CAPEX with the indicator for size and significant for ASSET_GROWTH with the continuous size value. Thus, this set of tests provides more qualified support for the size effect, though it strongly confirms the basic investment effect.

VI. Summary and Conclusions

We use major regulatory regime shifts in five non-US economies between 1990 and 2018 to investigate the effect of reducing short-sale constraints on corporate investment. We find that policies that relax short-sale restrictions are associated with a drop in stock prices and a reduction in investment. What is unique to our results, and distinguishes us from the Grullon et al. (2015) study of Regulation SHO in the United States, is that our results are more pronounced for larger firms than for smaller firms. In addition, we find that the effects are more pronounced for firms that relied previously on outside financing.

Our results provide much-needed evidence to address two unresolved questions. First, given the concerns raised in a variety of studies on the reliability of evidence based on Regulation SHO, which underlies many studies of short selling effects, evidence from other settings is needed. This is particularly true for studies of secondary effects of short-sale policy changes, which are those effects that arise not from the policy-induced changes in short-sale behavior themselves (the primary effect), but from the policy changes' indirect impact on other actions, such as, in our case, corporate investment. Second, among the more important questions is whether the investment effect is primarily observed in financially constrained firms. If short-sale restrictions allow stock prices to remain too high and this, in turn, allows financially constrained firms to access capital they would otherwise not be able to access, then short-sale restrictions arguably improve corporate investing. On the other hand, if the high stock prices encourage financially unconstrained firms to invest more than they would have otherwise invested, corporate investing is higher than optimal.

We confirm the impact of short-sale policies on investment and, more importantly, we reverse the earlier finding that the effect is more pronounced for small firms. In fact, we find little evidence of an effect on small firms. Our results suggest that, if anything, policies that restrict short selling are more likely to promote overinvestment than restore investing to optimal levels.

[24]The variable FEASIBLE is not included in the tests on its own since it is subsumed under the fixed effects.

Appendix A. Key Variable Definitions

ASSET_GROWTH: Change in nonshort-term total assets (total assets minus cash minus noncash current assets) divided by lagged total assets, and multiplied by 100 (WC0299, WC02001, and WC02201).

CAPEX: Capital expenditure (WC04601) divided by lagged assets, and multiplied by 100.

CAR: Cumulative abnormal returns for the event window [0, 60].

CASH_FLOW: Net income before extraordinary items/preferred dividends (WC01551) plus depreciation (WC01151) scaled by lagged total assets and multiplied by 100.

DEBT_ISSUANCE: Change in total debt scaled by lagged total assets, and multiplied by 100. Total debt is the sum of long-term debt (WC03251) and short-term debt (WC03051).

EQUITY_ISSUANCE: Change in total shareholder equity (WC03995) minus net income divided by lagged total shareholder equity, and multiplied by 100.

FEASIBLE: Indicator variable for short selling becoming feasible after the regulation change.

GDP_GROWTH: Yearly national GDP growth, obtained from World Bank Open Data.

CASH_GROWTH: Change in cash and short-term investment (WC02001) scaled by lagged total assets, and multiplied by 100.

LEVERAGE: Long-term debt (WC03251) plus short-term debt (WC03051) scaled by total assets. All multiplied by 100.

COUNTRY_RET: Country-level average weekly returns over the past 52 weeks.

COUNTRY_VOL: Country-level average standard deviation of weekly returns over the past 52 weeks.

PROFITABILITY: Ratio of operating income (WC01250) divided by total assets, and multiplied by 100.

SIZE: Natural log of total assets in millions of original currency (WC0299).

SHORTABLE: A firm-year indicator for years wherein a given firm was allowed to be shorted in China or Hong Kong.

TREATMENT: Indicator variable for years after the regime shift that result in a major increase in firms in a country being allowed to be shorted.

Appendix B. Goodman-Bacon Decomposition Analysis

Goodman-Bacon (2021) notes that treatment effects may not be consistent across time and the structure of a traditional staggered difference-in-differences analysis implicitly assigns weights to various treatment comparisons. To be specific, the difference-in-differences treatment effect is, according to Goodman-Bacon, a "weighted average of all possible two-group/two-period difference-in-differences estimators in the data," where the weights are affected by the variance of the treatment effects and the timing of the treatments. In an extreme case, one heavily weighted comparison may drive all the results. In this appendix, we discuss diagnostics available to evaluate whether such a problem might impact our results.

In the context of our study, the treatment estimate in our staggered difference-in-differences analysis is a weighted average of comparisons between firms in a country making a regime change and: i) firms in those countries that have not yet had a regime change (referred to as "early treatment vs. later control"), ii) firms in those countries that have already had a regime change (referred to as "later treatment vs. earlier control"), iii) firms in those countries that had a regime change before the study period (referred to as "treatment vs. already treated"), and iv) firms in those countries who have never had a regime change (referred to as "treatment vs. never treated").[25]

One complication is that the diagnostics require a balanced panel. Thus, we can only include firms in our analysis that have observations for every year of our study. To get a balanced sample covering the largest number of regime changes, we must restrict ourselves to the 1995 to 2018 sample period, which means that Sweden's regime shift will have occurred prior to the analysis. Firms in Sweden will, therefore, be included as always treated in the diagnostics. We have verified that our results in this subsample are quite similar (even in magnitude) to the results for the whole sample even though we reduce our sample from about 17,000 to about 2,000 observations.

The diagnostics generate results both with and without control variables. The analysis with controls allows, of course, a rich set of control variables to be included, but generates a more limited decomposition. In particular, given the nature of multivariate regressions, when controls are included, there is no distinction drawn between the first and second comparisons, which are referred to together as "Timing Comparisons." Thus, when we have controls, the diagnostics present the impact of three effects: a timing effect, a treated versus always treated effect, and the effect of control variables. The analysis without controls, in contrast, provides the impact of all four comparisons (without, of course, an effect from control variables). In addition, the output provides a scatter plot of all the underlying implicit comparisons (the individual 2×2 difference-in-differences estimates associated with every pair of countries).

B.1. Goodman-Bacon Analysis, With Controls

Table B1 presents the basic Goodman-Bacon breakdown for CAPEX and ASSET_GROWTH with controls. This analysis addresses the reliability of the coefficients in Table 4. The first two estimates are the key comparisons in the analysis. The third, which has an extremely small weight and (despite a large estimate) a very small impact on the results, reflects the effects of the control variables.[26] Note that the weights on the estimates will be the same for CAPEX and ASSET_GROWTH since the weights are a function of the number and timing of observations in the sample. We see that for both CAPEX and ASSET_GROWTH, the Treated firms have reduced investments regardless of the comparison that is made.[27] Thus, no one comparison seems to be

[25]As we have structured our analyses, the Never Treated firms are those where short selling was never feasible despite the ostensible change in regulation.

[26]In the language of decompositions, the term "estimate" refers to a component, that is, weighted to arrive at the two-way fixed effects difference-in-difference stimate of a treatment effect, which is the "coefficient" of interest in such a regression.

[27]The weighted average of the effects is equal to the coefficient generated from a difference-in-differences analysis of our sample. These are, as noted, similar in magnitude to those in our presented regressions.

driving our results.[28] When comparing large and small firms, the point of our article is that the effect is more pronounced for the large firms (and may not even exist for small firms). For the large firm sample, we also see that all comparisons have the same (negative) sign. For small firms, where we have already documented that the investment effect is lower, we do see that treated versus always treated is positive for CAPEX. This is not a concern, of course, since we are not trying to validate an effect in small firms.

TABLE B1

Goodman-Bacon Breakdown With Controls

Table B1 presents a decomposition of the estimated difference-in-differences coefficients for a balanced panel of countries using data from 1995 to 2018. Results are presented for both CAPEX and ASSET_GROWTH and are generated by the bacondecomp module of STATA with control variables included. The decomposition highlights the effect of comparing firms experiencing a regime change to those that have not yet experienced a regime change as well as those that have experienced a regime change during the sample period (timing comparisons), comparing firms experiencing a regime change to firms that have experienced a regime change that occurred before the sample period (treatment vs. always treated), and the effect of control variables.

		CAPEX		ASSET_GROWTH	
	Weights (W) (%)	Estimate (E)	W × E	Estimate (E)	W × E
All firms					
Timing comparisons	25.55	−1.66	−0.42	−2.27	−0.58
Treatment vs. already treated	74.02	−1.54	−1.14	−5.85	−4.33
Effect of control variables	0.43	−67.47	−0.29	−204.26	−0.88
Weighted-average effect			−1.85		−5.79
Small firms					
Timing comparisons	15.77	−1.17	−0.18	−0.98	−0.15
Treatment vs. already treated	84.02	0.32	0.27	−4.27	−3.59
Effect of control variables	0.21	−84.2	−0.17	−316.30	−0.66
Weighted-average effect			−0.09		−4.40
Large firms					
Timing comparisons	33.23	−1.90	−0.63	−3.29	−1.09
Treatment vs. already treated	66.44	−2.59	−1.72	−6.80	−4.52
Effect of control variables	0.32	−0.12	−0.01	59.77	0.19
Weighted-average effect			−2.35		−5.41

B.2. Goodman-Bacon Analysis, Without Controls

As noted in Section B.1, when controls are not included, the bacondecomp module will generate a scatter plot showing the estimates and weights for every country pair that drives the overall treatment effect. These plots, shown in Figure B1, present the estimates on the vertical axis and the weights on the horizontal axis. The associated estimated effects from each of the possible comparison groups described above are presented in Table B2.

In Graph A of Figure B1 (all firms), we see that there are (as expected) both positive and negative estimates. For the purpose of this analysis, the key observation is that there are many negative estimates, not just a few highly weighted observations or a few with extreme values. The negative values are also generally larger in magnitude. We do find that one observation in each case has a large weighting (though modest in magnitude) and this is associated with the effect of comparing treated to always treated. Based on the summary statistics in Table B2, we see that even without this comparison

[28]The Goodman-Bacon analysis does not provide tests of conjectures. It simply provides a decomposition of the coefficients that would arise from a difference-in-differences analysis. It is intended, as we have used it, to be a diagnostic tool for identifying possible problems. Similarly, it does not provide a comparison of small and large results, just the diagnostics for each subsample.

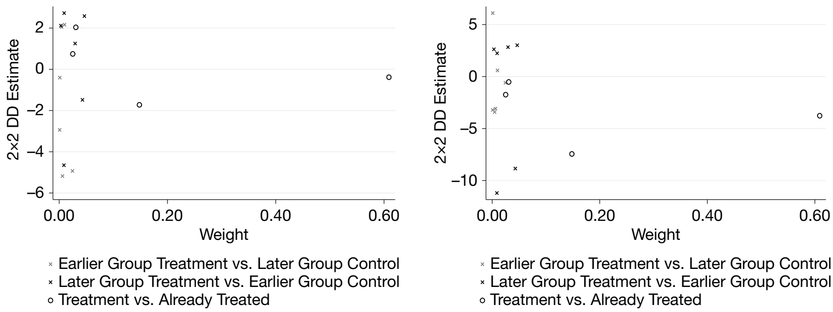
FIGURE B1

Goodman-Bacon Plots, Analysis Without Controls

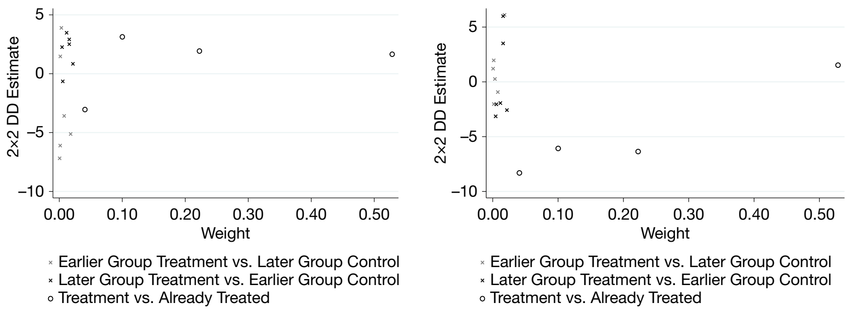
Figure B1 presents the output scatter plots from the bacondecomp module of STATA. It shows the estimated treatment effect for every possible country pair and the weight associated with that estimate. The pairs are classified in a manner consistent with the tabulated group output in which firms experiencing a regime change are compared to firms that have not yet experienced a regime change (earlier vs. later), firms that have experienced a regime change during the sample period (later vs. earlier), and firms that have experienced a regime change that occurred before the sample period (treatment vs. always treated).

CAPEX | *ASSET_GROWTH*

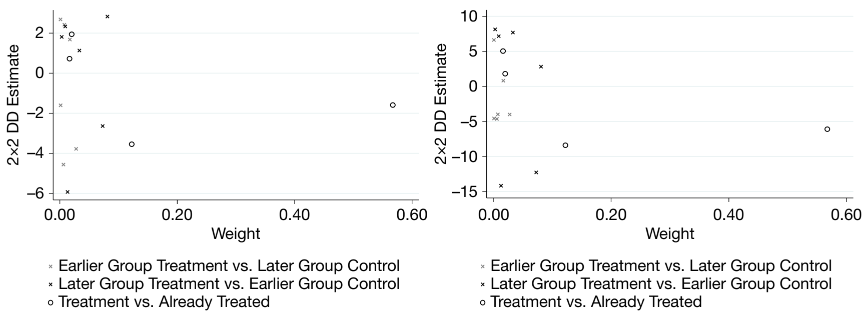
Graph A. All Firms



Graph B. Small Firms



Graph C. Large Firms



group, the effect of treatment is negative. As with the earlier tests, there is nothing to suggest that the overall results are driven by a few extreme comparisons.

Graphs B and C of Figure B1 present the same plots as Graph A, but generated separately for small and large firms. Once again, our main concern is the robustness of

TABLE B2

Goodman-Bacon Breakdown Without Controls

Table B2 presents a decomposition of the estimated difference-in-differences coefficients for a balanced panel of countries using data from 1995 to 2018. Results are presented for both CAPEX and ASSET_GROWTH and are generated by the bacondecomp module of STATA without control variables. The decomposition highlights the effect of comparing firms experiencing a regime change to firms that have not yet experienced a regime change (earlier vs. later), firms that have experienced a regime change during the sample period (later vs. earlier), and firms that have experienced a regime change that occurred before the sample period (treatment vs. always treated). The symbols in parentheses indicate the comparisons that are shown in Figure B1 and summarized in this table.

		CAPEX		ASSET_GROWTH	
	Weights (W) (%)	Estimate (E)	W × E	Estimate (E)	W × E
All firms					
Earlier vs. later (×)	4.70	−2.66	−0.12	−0.83	−0.039
Later vs. earlier (×)	14.00	0.60	0.08	−1.61	−0.225
Treatment vs. always treated (○)	81.30	−0.50	−0.41	−4.25	−3.456
Weighted-average effect			−0.44		−3.720
Small firms					
Earlier vs. later control (×)	3.30	−3.59	−0.12	3.17	0.10
Later vs. earlier control (×)	75.00	2.02	1.51	0.62	0.47
Treatment vs. always treated (○)	89.20	1.67	1.49	−1.75	−1.56
Weighted-average effect			2.89		−0.98
Large firms					
Earlier vs. later control (×)	6.10	−1.39	−0.08	−2.54	−0.16
Later vs. earlier control (×)	21.30	0.11	0.02	−2.37	−0.51
Treatment vs. always treated (○)	72.60	−1.77	−1.28	−6.01	−4.37
Weighted-average effect			−1.34		−5.03

the results for larger firms, and we see many estimates below zero. We do once again see a single highly weighted observation, but, also as before, the group results indicate this is not driving the overall effect.

All told, the diagnostics available from the Goodman-Bacon-based STATA module do not suggest any concerns about our staggered difference-in-differences analyses. This may not be that surprising since, as one robustness check in the article, we repeated our analysis, dropping each of the 5 countries one at a time, and obtained similar results.

Appendix C. Analysis of Regime Changes in China and Hong Kong

As described in Table 1, the evolution of short selling regulations in each country is complex. Our central tests include three countries where the regime shift most likely affected all firms in the country as of the date we identified (see Bris, Goetzmann, and Zhu (2007), Charoenrook and Daouk (2009), and Jain, Jain, McInish, and Mckenzie (2013) for detailed discussion). In two countries, China and Hong Kong, the situation is slightly different. These countries had lists of shortable stocks that changed over time and our regime shift for the main tests is the largest such list revision.[29] In this section,

[29] In China, short selling was first introduced in 2010 for the largest (blue-chip) firms with a list expansion in 2011. In Jan. 2013, the largest addition occurred and short selling became practiced (Chang, Luo, and Ren (2014); our date for the main tests) with adjustments later in 2013 and in 2014. In Hong Kong, short selling was introduced in 1994 with an expansion in 1996. In May 1997, the largest addition occurred, and short selling became feasible (Bris, Goetzmann, and Zhu (2007), Charoenrook and Daouk (2009)), with frequent adjustments through 2001. In 2002, the regulations shifted to automatic adjustments based on firm characteristics.

we use all 19 list revisions in these two countries as events and execute a staggered difference-in-differences analysis that includes all firms in the two countries. Our test is based on an indicator for firms that were on the list.[30] Thus, in contrast to our main tests where the control firms were only firms in other countries, here the controls include firms in the countries but were not affected by the list change. Note that since we now include all firms in those two countries, including the many firms for which short selling was never allowed, the sample size is greatly expanded.

The results are shown in Table C1. The variable SHORTABLE is equal to one when short selling is allowed for a given firm in any given year. As with other tests, we exclude observations for firms the year they are affected by a list change. The results are presented in the same format as Table 13. This within-country analysis provides the same conclusions as our main tests in regard to both an investment effect and size effect: short selling reduces investments and the effect is more pronounced for larger firms.[31]

TABLE C1

China and Hong Kong Analysis

The sample in Table C1 includes two economies, China and Hong Kong, where regulators introduced short selling for subsets of firms. The variable SHORTABLE is a firm-year indicator for years in which a given firm was allowed to be shorted. The sample includes all firm-year observations for 1990 to 2018, except that we omit the firm-year in which a firm is affected by the short selling change. All variables are defined in Appendix A and controls (except CASH_FLOW, which is contemporaneous) are lagged 1 year. Standard errors are in parentheses below each coefficient, and are adjusted for clustering at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	CAPEX			ASSET_GROWTH		
	Investment Effect	Investment and Size Effect		Investment Effect	Investment and Size Effect	
	1	2	3	4	5	6
SHORTABLE	−0.417***	0.129	5.270***	−1.234***	−0.140	19.582***
	(0.144)	(0.209)	(1.799)	(0.314)	(0.432)	(3.834)
SHORTABLE × IND_L		−0.771***			−2.088***	
		(0.276)			(0.550)	
SHORTABLE × SIZE			−0.358***			−1.351***
			(0.116)			(0.242)
CASH_FLOW	0.136***	0.179***	0.183***	0.511***	0.582***	0.603***
	(0.006)	(0.008)	(0.008)	(0.015)	(0.017)	(0.017)
SIZE	−0.796***	−0.680***	−0.679***	−2.877***	−2.210***	−2.120***
	(0.059)	(0.072)	(0.071)	(0.147)	(0.159)	(0.159)
PROFITABILITY	0.133***	0.166***	0.163***	0.192***	0.271***	0.266***
	(0.007)	(0.009)	(0.009)	(0.017)	(0.019)	(0.019)
GDP_GROWTH	0.015	−0.033	−0.054**	−0.100*	−0.027	−0.072
	(0.019)	(0.023)	(0.023)	(0.055)	(0.070)	(0.073)
COUNTRY_RET	0.022	0.397*	0.519**	−1.357***	−1.701***	−1.592***
	(0.178)	(0.229)	(0.228)	(0.455)	(0.601)	(0.591)
COUNTRY_VOL	0.085	0.062	0.035	0.288	0.352	0.292
	(0.072)	(0.089)	(0.092)	(0.189)	(0.223)	(0.228)
N	51,105	38,388	39,955	50,625	37,800	39,379
Adj. R^2	0.431	0.423	0.431	0.232	0.252	0.259

[30]For Hong Kong, we exclude the list changes after 2002 since they are, by construction, related to firm characteristics.

[31]We note that our main results are unchanged if we remove either China or Hong Kong from the sample. Thus, the effects we document are not driven exclusively by these two economies.

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