Target the untargeted: Essays in unconventional disclosures and policies

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Target the Untargeted:
Essays in Unconventional Disclosures and Policies

Essays over ongebruikelijke aankondigingen en beleid

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Chicago, South Bend, Singapore, Miami, Milan, Seattle, Paris, Austin, Glasgow, Vienna, Zurich, Charlotte, Mannheim, Dallas...

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Chapter 1

Introduction

This dissertation on targeting the untargeted examines whether economic actors'
conventional disclosures or decisions can have unconventional intentions from different
research angles. Specifically, I examine whether firms use disclosures to capital markets
to change the the behavior of (potential) competitors and whether the central banks
provide financing to large firms in order to boost bank financing for small and middle
enterprises.

In the context of financial accounting, researchers typically consider (potential)
shareholders to be the primary targeted audience of firm disclosure. Ever since Ball and
Brown (1968), which catalyzed research in accounting, researchers have investigated
the capital market effects and determinants of disclosure.[1] However, firm disclosure
can also be informative to other economic actors and, consequently, help to improve
the profitability of firms. Different streams of research show that firm disclosure can,
for example, affect corporate investments and have market wide outcomes (for a
review see Leuz and Wysocki, 2016).

Recently, there has been a push to understand the role that disclosures play in
product markets. A traditional view in the accounting literature is that (potential)
competitors could have a dampening effect on firm disclosure through proprietary
costs (Verrecchia, 1983; Dye, 1985). When firms provide information voluntarily to
the market, it could also be used by competitors in a way that brings harm to the
firm's prospects. However, empirical evidence on the proprietary cost hypothesis is

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[1]See, for example: Lang and Lundholm (1993); Botosan (1997); Core (2001); Healy and Palepu
(2001); Lambert, Leuz, and Verrecchia (2007); Francis, Nanda, and Olsson (2008); Bischof and Daske
(2013); Balakrishnan, Billings, Kelly, and Ljungqvist (2014); Leuz and Wysocki (2016); Dyer, Lang,
and Stice-Lawrence (2016); Guay, Samuels, and Taylor (2016); Schoenfeld (2017); Gow, Larcker, and
Zakolyukina (2019).

mixed (for a review see Beyer, Cohen, Lys, and Walther, 2010). An important reason for this is that competition could also be a driver of firm disclosure. Prior studies show that disclosures could be used as strategic devices to improve firms' competitive positioning (e.g., Tomy, 2017; Bloomfield, 2018; Burks, Cuny, Gerakos, and Granja, 2018; Bourveau, She, and Zaldokas, 2019; Glaeser and Landsman, 2019; Kepler, 2019). In this dissertation, I examine two different ways in which firms could use disclosures in such a way.

In chapter 2, I investigate whether firms use disclosures in order to tacitly collude with their competitors and identify an important and previously under-explored effect that may limit this behavior: antitrust oversight. Theory suggests that firms facing competition from only a few competitors could use disclosure to tacitly coordinate with their competitors (e.g. Fried, 1984; Bertomeu and Liang, 2015). Specifically, firms may use future product price disclosures to induce their competitors to raise their prices above competitive levels (Corona and Nan, 2013). I find evidence that firms in concentrated industries provide more future price increase disclosures, consistent with the notion that these firms use future price increase disclosures to coordinate prices. The results in my study suggest that antitrust oversight could be an effective way to limit this behavior by firms. However, it may come with the unintended consequence that it becomes more difficult for investors to be informed.

In chapter 3, which is joint work with Matthew Bloomfield, we explore whether firms use voluntary disclosures as part of their entry deterrence strategies. Analytical work on industrial organization suggests that firms can deter entry by investing in capacity expansions (e.g., Spence, 1977; Dixit, 1980; Tirole, 1988; Ellison and Ellison, 2011), but this is only effective when observable to potential entrants. Disclosures may therefore have an important role as they can inform potential competitors of a firm's expansion before they make the decision to enter the market. We provide evidence that firms issue capacity expansion announcements, strategically, to ensure that potential entrants are aware of ongoing capacity investments. Consistent with our predictions, larger firms are more likely to respond in this fashion, while more opaque firms—that that plausibly have more private information—are less likely to respond in the fashion. Finally, capital expansion announcements appear to be effective at deterring entry.

The second setting in which I investigate whether economic actors can target a particular group while seemingly targeting another is the banking industry. Small- and medium-sized enterprises (SMEs) are the backbone of an economy and rely heavily on bank financing. Especially during the financial crisis and after, SME credit access

contracted substantially to great concern of policymaker and regulators (Ferrando, Popov, and Udell, 2017; Bord, Ivashina, and Taliaferro, 2018; Cortés, Demyanyk, Li, Loutskina, and Strahan, 2018). The regular approach to encourage more lending to SME's is to, for example, lower interest rate, engage in risk-sharing or provide direct credit guarantees to increase banks' willingness to lend to small businesses by making this type of lending more attractive to banks (Beck, Klapper, and Mendoza, 2010). Recently, the European Central bank took a new approach by using regulator-led financial disintermediation in non-SME credit to enhance financial intermediation in the SME sector. The idea of this approach is that banks could extend more credit to small businesses if large corporate loans become less attractive and the opportunity cost of lending to SMEs decreases.

In chapter 4, which is joint work with Anya Kleymenova and Aytekin Ertan, we study whether there are indeed are spillover effects of financial disintermediation on the supply of credit to SMEs. We find that direct central bank lending to large corporations induces banks to increase lending to SMEs by 8 to 12 percent. This effect is stronger for liquidity-constrained banks. SMEs with relationship banks affected by disintermediation borrow approximately €77,750 more relative to SMEs in the same country and industry. We verify that these inferences are not due to changing economic fundamentals or selection in central bank financing. Despite documenting positive effects, we also find that they disappear in the long term, casting some doubt on the structural efficacy of financial disintermediation as a tool to enhance bank lending to SMEs.

Chapter 2

Shall we talk price increases? The fine line between disclosure and antitrust oversight

2.1 Introduction

A vast stream of literature finds that firms facing strong competition provide less disclosure, presumably for proprietary costs reasons (for a review see Beyer et al., 2010; Leuz and Wysocki, 2016). However, theory on product markets and disclosure suggests that firms facing competition from only a few competitors could have strong incentives to provide *more* disclosure, as it allows them to tacitly coordinate with their competitors (e.g. Fried, 1984; Bertomeu and Liang, 2015). Specifically, firms may use future product price disclosures to induce their competitors to raise their prices above competitive levels (Corona and Nan, 2013). In this chapter, I first investigate whether firms use disclosures in such a strategic way and, second, I identify an important and previously under-explored effect that may limit this behavior: antitrust oversight.

Firms face a trade-off when they consider disclosing future price increases (FPIs). There are capital market benefits for disclosing FPIs, as shareholders value and respond to FPI announcements (Lim, Tuli, and Dekimpe, 2018). However, firms may face adverse consequences in their product markets when making FPI disclosures because this may reduce their ability to price discriminate and may provide proprietary information to their competitors (Verrecchia, 1983). In concentrated industries, firms may experience an additional benefit for disclosing FPIs. FPI disclosures may facilitate price collusion and allow firms to "induce each other into a less competitive equilibrium in which both can obtain higher profits" (Corona and Nan, 2013) when offering similar products and compete on prices.

One of the main objectives of antitrust oversight is to limit firms' ability to act together, especially in ways that can lead to higher prices (Federal Trade Commission, 2015). In the U.S., antitrust oversight of collusive behavior traditionally focuses on private agreements between firms. In the last decade, however, antitrust authorities considered public statements on pricing and capacity as evidence of collusion (Steuer, Roberti, and Jones, 2011).[1] Firms may therefore have an incentive not to provide such disclosures to limit the likelihood of an intervention by antitrust oversight. As a consequence, antitrust oversight can hamper managers' ability to inform the market. Wary of these unintended consequences, antitrust authorities around the world have differing opinions on whether to regulate public FPI disclosures. The European Commission, for example, states that it generally allows public statements on pricing and capacity, as it believes that the market has disciplining mechanisms to ensure that firms refrain from making these disclosures for antitrust reasons (OECD, 2010). For example, FPI disclosures may increase the probability that new firms enter the market (Darrough and Stoughton, 1990), or that customers decide to produce the product internally. Given these different viewpoints among antitrust authorities, it is important to understand whether increased scrutiny by antitrust oversight affects the disclosure behavior of firms and whether it interferes with firms' ability to inform shareholders.

To address this trade-off, I develop a novel textual analysis measure of future price disclosures to identify whether firms in concentrated industries provide more FPI disclosures. I apply this algorithm to a large sample of U.S. conference calls over the period from 2003 to 2013, as conference call disclosures are likely to be the least

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[1]Even when these disclosures are made for capital market reasons and absent any anticompetitive intent, firms still risk additional scrutiny by antitrust authorities under the concept of 'invitation to collude'. This legal concept allows antitrust authorities to charge firms solely for an action (e.g. a disclosure) that may facilitate collusion on price or capacity.

costly compared to other forms of disclosure.[2] Roughly nine percent of the conference calls in my sample contain such disclosures, which corresponds to 14 percent of the firm-year observations. I then exploit a shock to the strength of antitrust oversight to analyze its effects on the usage of future price increase disclosures and the information environment.

In my first analysis, I investigate whether firms in concentrated industries provide more FPI disclosures than firms in less concentrated industries. Theory predicts that only firms in concentrated markets could use FPI disclosures to coordinate price increases (Corona and Nan, 2013). Consistent with this prediction, I find cross-sectional evidence that firms in industries with a higher Herfindahl-Hirschman index score provide more FPI disclosures.

The above finding does not necessarily imply that firms use FPI disclosures to collude. Firms in concentrated industries could, for example, have different disclosure incentives or more shocks to common input prices. I, therefore, examine whether firms in concentrated industries also provide more future price *decrease* disclosures. Future price increase and decrease disclosures are likely both correlated with economic forces that lead firms in competitive industries to discuss/disclose prices generally (i.e. disclosure incentives), while only future price increase disclosures can be used to collude. I do not find evidence that firms in concentrated industries also provide more future price decrease disclosures. This finding is robust to using alternative definitions of industry concentration. Furthermore, I find no evidence that firms in concentrated industries provide more disclosures on future profit margin changes or general discussions of future prices that are neither price increases or decreases. Taken together, these results suggest that firms in concentrated industries use FPI disclosures to coordinate future price increases.

To explore the effects of antitrust oversight on these FPI disclosures, I use a major change in civil antitrust litigation as a plausibly exogenous shock to antitrust oversight. In the 2007 case, *Bell Atlantic Corp. v. Twombly*, the Supreme Court increased the burden of proof for civil antitrust litigation and required litigants to meet this standard when filing the lawsuit. Traditionally, lawyers could file a lawsuit against firms based on a suspicion of collusion and then use the discovery phase to

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[2]Given the unscripted nature of the questions and answers during a conference call, firms have a stronger legal defense against antitrust allegations compared to other prepared disclosures that are heavily redacted by the firm's legal department and/or auditors. Furthermore, providing an FPI disclosure in a conference call, compared to other types of disclosure, reduces the likelihood that customers are aware of FPI, thereby reducing the likelihood that customers will decide to change supplier or produce the product in-house. This is mainly driven by the higher search costs for customers to obtain the information conveyed during a conference call compared to more readily available types of disclosure.

gather actual evidence of collusion by examining private communications, which made scrutiny of indirect evidence, such as public statements by managers, unnecessary. However, when the Supreme Court ruled that litigants had to present evidence of collusion before the discovery phase, lawyers started to search through conference calls for public statements on prices and capacity that could be used as evidence to avoid the dismissal of the lawsuit (Steuer et al., 2011). Thus, after the Supreme Court ruling, managers in concentrated industries suddenly faced an additional cost for making FPI disclosures, namely civil antitrust litigation risk.[3]

Applying a difference-in-difference design, I analyze the effects of increased antitrust oversight on the use of FPI disclosures in concentrated industries, with firms in less concentrated markets as a control group. The results show that firms in highly concentrated industries respond to an increase in civil litigation risk by providing fewer FPI disclosures. This pattern is consistent with the notion that firms account for civil antitrust litigation risk in their disclosure decisions. In a sensitivity analysis, I find similar results when I replace the continuous industry concentration measure by an indicator equal to one when a firm is in the top quartile of industry concentration. Furthermore, I find that retail firms, which traditionally receive more leeway by antitrust authorities to communicate FPI to consumers (OECD, 2010), do not decrease their FPI disclosures as much as other firms after the new precedent was set by the Supreme Court.

I conduct several falsification tests to confirm that the observed decrease is due to an increase in antitrust oversight and not driven by confounding events. First, I show that the Supreme Court ruling only affects FPI disclosures, not price decrease or general price disclosures. If a confounding event, such as the financial crisis, more negatively affects the future prices of firms in concentrated industries and, thus, the economic rationale to provide FPI disclosure, one would expect firms in concentrated industries to announce more future price decreases. When a confounding event reduces the overall incentive of firms in concentrated industries to disclose future price changes, one would also expect firms to disclose less future price decreases. However, the results are inconsistent with either alternative explanation. Second, I find that analysts of firms in concentrated industries are not asking fewer questions about FPIs following the Supreme Court ruling. When asking questions, analysts are likely aware of all economic reasons to increase prices, but they are presumably unaffected by antitrust oversight. Thus, if an unobservable change in the fundamentals of firms in concentrated industries affects my inferences, one would also expect analysts to ask fewer questions

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[3]Firms in unconcentrated industries are unaffected, as these firms are not the target of civil antitrust lawsuits.

about FPIs. However, I do not find that analysts change the number of questions about FPIs in concentrated industries in response to the Supreme Court ruling, supporting the notion that the relative decrease of FPI disclosures is not driven by confounding events, but rather, by antitrust oversight.

Furthermore, while antitrust oversight may be effective in limiting FPI disclosures and, consequently, preventing collusion, the change could also affect the ability of managers to effectively communicate with their investors. Antitrust oversight substantially increases the costs for making FPI disclosure, but may thereby also discourage firms from disclosing FPIs to inform their investors absent any anticompetitive intent. To test whether antitrust oversight affects the information environment, I examine the equity market bid-ask spreads for both concentrated and less concentrated industries after the Supreme Court ruling. I find that the information environment deteriorates in concentrated industries after the increase in antitrust oversight. My results indicate that increased antitrust oversight has the unintended consequence that stock markets are less informed.

Finally, I investigate whether the decrease in usage of FPI disclosures is isolated to civil antitrust litigation risk, or if the same inferences can be generalized to other forms of antitrust oversight. Specifically, I study a change in enforcement occurring after the Federal Trade Commission (FTC) reached a settlement in the U-Haul case in 2010. In their final order, the FTC ruled that statements made by U-Haul in their conference calls were anti-competitive and stated that, from that moment forward, they were going to pursue similar cases and those with "less egregious" conduct. This FTC case, thus, puts further pressure on managers to be cautious when providing public statements on competitively sensitive topics. The results of my investigation indicate that firms reduced their usage of price increase disclosures regardless of industry concentration. While the increase in enforcement appears to be effective in deterring firms from disclosing FPI in industries where it could be used to tacitly collude, a negative unintended consequence seems to be that firms with no anticompetitive opportunities reduce their FPI disclosures.[4] My results are in line with concerns of both congressional leaders and the American Bar Association who warned that the FTC provided too little guidance on what was allowable conduct (Wyatt, 2010). Overall, the effect of antitrust oversight on disclosure seems, thus, not limited to civil litigation risk, but also applies to antitrust regulatory enforcement.

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[4]Due to the lack of a natural control group and concurrent event affecting the information environment (e.g. the Dodd-Frank act), it is difficult to draw inferences from to market level tests and are, therefore, not included in this study.

As the FTC alleged that U-Haul was aware and specifically talked to its competitor and not to its investors, I test whether firms stopped making FPI disclosures completely or changed their way of communicating. I find that some firms changed their disclosure channel, and instead relayed their FPI disclosures via analyst conference presentations. This change of disclosure channel, presumably allows firms to defend the notion that they are talking to investors and not to competitors, but comes at the expense of retail investors that do not have easy access to this information. Overall, the results suggest that firms use FPI disclosures to coordinate price increases, but are limited in their ability to do so after increases in antitrust oversight.

This paper contributes to several strands of literature. First, I contribute to the disclosure literature by providing empirical evidence for the theoretical prediction that firms in concentrated industries can use disclosures to improve their competitive positioning. Recent work finds similar results while focusing on antitrust leniency laws (Bourveau et al., 2019) and strategic alliances (Kepler, 2019) on disclosures. However, this study provides more general evidence of the usage of FPI disclosures to coordinate prices and how antitrust oversight significantly reduced this method of tacit collusion in the last decade. This study also highlights that the competition-disclosure link varies with the type and nature of disclosure in question. While there is a link between industry concentration and future price increase disclosures, these conclusions do not apply to future price decrease disclosures. Furthermore, this is one of the first studies to document that the channel through which firms disclose information, in particular on future pricing, to the market is relevant and that enforcement can lead firms to substitute their conference call disclosures for analyst conference presentation to avoid regulatory scrutiny.

Second, this study is, to the best of my knowledge, the first to document that civil antitrust litigation risk can affect firm disclosures and, consequently, also the information environment for capital markets. Civil antitrust litigation discourages managers from making competition sensitive disclosures, even though investors find these disclosures informative. Furthermore, the litigation risk from civil antitrust lawsuits is substantially different in both theory and implications from the security regulations litigation risk often discussed in the disclosure literature. Contrary to security regulation litigation risk, civil litigation risk may expose firms to litigation risk even when the managers make *timely* and *truthful* disclosure about, for example, future prices.

Lastly, this paper adds to the literature on antitrust oversight. My results show that an increase in antitrust oversight can be effective in limiting the use of disclosures

for anticompetitive reasons, but can make capital markets less efficient. Given that antitrust authorities around the world have differing approaches regarding regulating public disclosures, this study could provide empirical evidence to antitrust authorities about the intended and unintended consequences of doing so.

2.2   Institutional background and prediction
2.2.1   Price disclosures and collusion

Firms typically weigh costs and benefits when deciding whether to disclose information. Theory predicts that an important reason for managers to withhold information from the market is the presence of proprietary costs (Verrecchia, 1983; Dye, 1985). Prior studies have frequently used industry concentration as an empirical proxy of proprietary costs, and found evidence that firms in concentrated industries provide fewer disclosures (Harris, 1998; Botosan and Stanford, 2005; Ali, Klasa, and Yeung, 2014).

This literature provides valid reasons to expect that firms in concentrated industries face high proprietary costs, but it ignores another stream of literature that theorizes that some public disclosures in such concentrated industries could be beneficial. Specifically, public disclosures on future capacity and pricing can provide firms with product market benefits (e.g. Fried, 1984; Doyle and Snyder, 1999; Corona and Nan, 2013; Suijs and Wielhouwer, 2014; Bertomeu and Liang, 2015). Firms can use disclosures to inform competitors of their future plans to either raise its prices or reduce their capacity to induce competitors to follow their lead, depending on whether firms compete in a Cournot (i.e. capacity based), or Bertrand (i.e. price based) type of competition. Disclosure can thus be beneficial, especially *because* their competitors learn about them.

Firms in concentrated industries can thus use FPI disclosures to coordinate price increases with their competitors.[5] When a firm increases its price, competitors have the choice to follow or continue to compete at current price levels. If all firms successfully commit to an increase in prices, each firm will have higher profits than it would have if it continued to compete.[6] However, if competitors do not raise their prices, the firm could face adverse consequences for being the only firm to raise its prices in the form of customer loss. For this reason, it is beneficial to use FPI disclosures rather than simply increasing prices, as it allows firms to test the waters, coordinate,

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[5]Price coordination effectiveness is decreasing in the number of firms in the industry.
[6]Under the condition that firms do not increase the price above the profit-maximizing price (i.e. monopoly price).

and possibly still withdraw the FPI before facing the adverse consequence.[7] That is, FPI disclosures are beneficial to firms, as they may induce competitors into a less competitive equilibrium where all firms can increase their profits (Corona and Nan, 2013).

Firms do incur costs when making an FPI disclosure, even if they subsequently decide to withdraw the price increase decision. First, an FPI disclosure may induce other firms to enter the market (Darrough and Stoughton, 1990), or customers may decide to produce the product in-house. Once firms have made the initial investments to either enter the market or produce a product in-house, they may not easily adjust their decision, even if the FPI disclosing firm withdraws the price increase. Second, withdrawing the FPI announcement may reduce the managers' disclosure credibility. Lastly, investors could consider the backtrack as a sign of uncertainty and riskiness and, therefore, require a higher rate of return. Albeit, FPI disclosures are credible because they are costly to provide.

Managers could mitigate some of these concerns by concealing their FPI announcement. Customers and potential entrants will only respond to the FPI announcement and potential withdrawal when they are aware of it. It is therefore likely that the firms prefer to make FPI disclosures in conference calls compared to other more easily accessible disclosure channels. [8]

However, some empirical evidence does suggest that firms use disclosures to collude, especially in the airline industry, where there is strong evidence that firms used public announcements to coordinate capacity decreases (Aryal, Ciliberto, and Leyden, 2018) and price increases (Borenstein, 1999). Firms also seem to adjust their public disclosures of, for example, product information and customer contracts after an increase of cartel enforcement regulations (Bourveau et al., 2019) and more revenue forecasts with common ownership within small product markets (Pawliczek, Skinner, and Zechman, 2019). Moreover, evidence suggests that firms reduce their public disclosures when they enter private partnerships that allow them to provide this information in private (Kepler, 2019). Previous studies, though, do not analyze the price coordination through forward-looking pricing disclosures.

Overall, theory provides a strong indication that firms in concentrated industries can use FPI disclosure to improve their competitive positioning. Combined with recent

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[7]theory shows that even cheap talk can be effective in facilitating collusion (Awaya and Krishna, 2016, 2019)

[8]However, firms cannot completely mitigate these costs, so firms that make the first FPI disclosure in an attempt to coordinate prices are facing adverse consequences, while competitors could free-ride on the price increase or temporarily benefit by undercutting the firm's price. Theory suggests that this could prevent any firm from making the first FPI disclosure (Pastine and Pastine, 2004).

empirical evidence, I predict that firms in concentrated industries provide more FPI disclosures than non-concentrated firms.

2.2.2   Antitrust oversight and price fixing

One of the most important goals of antitrust oversight is to prevent firms from abusing their market power at the expense of consumers. To achieve this goal, both antitrust authorities and customers monitor firms for wrongdoing. Antitrust authorities can launch investigations into potential market abuse and discipline firms, while many countries allow customers to file a civil lawsuit against firms to uncover evidence and seek compensation.

In the U.S., the FTC typically focuses its investigations on price fixing through private meetings and communication and imposes severe penalties for this type of misconduct. Under U.S. antitrust law, the FTC does not have to prove that there was an actual agreement to fix prices. Proposing to raise prices or taking actions that could induce competitors to raise their prices above competitive levels is a violation of law under the legal concept of 'invitation to collude'.[9] Traditionally, public disclosures on prices and capacity have not been a priority for FTC, nor considered an invitation *per se* to collude. However, the FTC shifted its position on public disclosure when it charged Vallassis in 2006 and, in particular, U-Haul in 2010 for inviting collusion through their public statements.

In addition to antitrust regulation enforcement, civil antitrust litigation is another form of antitrust oversight that deters firms from colluding. Customers can go to court and seek compensation and punitive damages from firms that have increased their prices through collusion. The amounts awarded to customers are substantial: Connor and Lande (2015) finds that firms are forced to pay, on average, 500 million per civil antitrust case. Typically, important evidence in these cases is obtained *after* the litigants go to court, namely in the discovery phase (Epstein, 2008). During this phase, litigants can demand that the accused firm provides internal documents and communication, which can then be used as evidence during the trial. Even if firms are not found guilty of collusion, both the litigation costs and the cost to produce all documents during the discovery phase are borne by the accused firm and can be substantial (Easterbrook, 1989). A survey over the period 2004-2008 found that the self-reported costs of discovery in civil cases was more than USD 1.8 million per case

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[9]Although the security regulation mandates the disclosure of material information, it is unlikely to prevent antitrust regulation from having an impact on public disclosures. Price and capacity disclosures are not by definition considered material information and FTC rules may therefore apply to these disclosures.

and required firms to produce nearly 5 million pages of documents (U.S. Chamber Institute for Legal Reform, 2010).

Given the high potential costs, firms have incentives to avoid antitrust oversight scrutiny regarding collusion through private or public disclosures. I, therefore, predict that antitrust oversight reduces the use of FPI disclosures by firms in concentrated industries.

2.2.3   Information content of FPI disclosures and antitrust oversight

Prior research shows that FPI disclosures are, on average, informative to investors (Lim et al., 2018). FPI disclosures are value relevant because they are informative about future customer demand, prices, and/or margins. In addition, FPI disclosures could reduce the uncertainty about future performance and reduce the information asymmetry between sophisticated and unsophisticated investors when sophisticated investors have access to extensive market research. A beneficial consequence of firms using FPI disclosures to coordinate prices could, therefore, be that shareholders are better informed. When an increase in antitrust oversight reduces the ability of firms to provide FPI disclosures and firms have no alternative way to effectively provide this information, it may thus result in investors being less informed.

There are also reasons to expect that FPI disclosures are not informative in concentrated industries. First, FPI disclosures may be too noisy to be informative because investors are not able to determine whether firms are using FPI disclosures to collude or due to changes in, for example, input prices or market condition. Second, FPI disclosures may be insufficiently credible to be informative (Ng, Tuna, and Verdi, 2013; Stocken, 2000; Rogers and Stocken, 2005), because firms could still withdraw the price increase when other firms are not responding to the FPI disclosure. If FPI disclosures are not informative, one would not expect to see any change in the information environment when antitrust oversight prevents all firms in concentrated industries from providing FPI disclosures. There could even be an improvement in the information environment when the consequence of antitrust oversight is that firms in concentrated industries only use FPI disclosures for competitive reasons.

Overall, it is an empirical question whether an increase in antitrust oversight affects the information environment of concentrated firms.

2.2.4   Civil antitrust litigation risk setting

This study exploits a sudden change in U.S. antitrust oversight to investigate its effects on disclosures. In *Bell Atlantic Corp. v. Twombly*, the Supreme Court changed the burden of proof to start a civil suit alleging antitrust wrongdoing.[10] Before this 2007 ruling, plaintiffs in civil antitrust cases only need to meet a low burden of proof to be allowed to search through confidential records to build their case and find concrete evidence of collusion during pre-trial discovery (Steuer et al., 2011).[11] However, the court decided that litigants had to provide more evidence of collusion when filing a lawsuit and thus before being able to access private communication. Consequently, lawyers began scrutinizing managers' public statements to support lawsuits and companies were instructed by their lawyers to be careful with any statement on competition sensitive topics (Dechert LLP, 2010). Thus, after the Supreme Court ruling, firms could (substantially) reduce their exposure to civil antitrust lawsuits by limiting the amount of discussion on topics that could be seen as anticompetitive and thus be used as supporting evidence to start civil antitrust lawsuits.

It is important to note that the change in antitrust oversight was unexpected and unlikely to be timed endogenously. The Supreme Court overturned a precedent that was set in 1957 and was not intended to affect public disclosure. Given the presumably exogenous timing of the misconduct itself and the time it took to go through the judicial system, it is also unlikely that this change was timed to correspond with changes in the information environment or disclosure behavior.

2.3   Data

2.3.1   Measuring future price increase disclosure

This study uses a novel text-based measure that captures whether managers discuss future price increases during their conference calls. Specifically, the algorithm identifies those conference calls where managers use the words *raise* or *increase* in the same sentence as the word *price*. The algorithm classifies the following examples as FPI

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[10]This change specifically affects civil antitrust cases brought primarily by customers or competitors not participating in the cartel. This change does not specifically affect cases brought by the FTC or the Department of Justice.

[11]Plaintiffs only had to provide "very spare allegations to meet the pleading burdens" (Epstein, 2008) in order to start the discovery phase. In practice, plaintiffs could successfully start a lawsuit and enter discovery by showing that the current market outcomes are corresponding to outcomes that are driven by collusion. The Supreme Court deemed this threshold to be too low and increased this threshold to "enough facts to state a claim to relief that is plausible on its face." Before the ruling, litigants in civil antitrust cases were required to provide substantial evidence of anticompetitive behavior only after pre-trial discovery.

disclosures:

"No, if anything we are looking to increase prices in the collectible side and several of our markets."

"As prices rise, going back to other things we've talked about here, as Mills increase their price and as we have to increase ours..."

"So we keep an eye on them, and I think, people continue to raise prices, and we will as well."

Several important enhancements were made to improve the quality of the algorithm. First, the algorithm is designed in such a way that it does not classify discussions of increases in input prices as FPI disclosures by excluding 15 of the most discussed materials[12]. Second, the algorithm disregards discussions about past and current pricing, as only the information on future pricing is truly proprietary and unable to be obtained from, for example, financial statements. I, therefore, exclude price increase discussions in sentences that contain regular verbs in the past tense (words ending with *-ed*) or the commonly used irregular verbs such as *was*, *had* and *were*. Third, the algorithm excludes price increases that are preceded by *no* or *not*. Fourth, I also exclude word combinations that were frequently incorrectly classified by the algorithm as a discussion on product prices. The list of words are *stock, market, share, exercise, closing, trading, offer, conversion, resulted, discounted, declining, low, contract, realized, average, home, real, under* in combination with the word price.

Using a similar approach, another algorithm used in this study detects the disclosure of future price decreases by managers. To be more precise, it searches for the words *decrease, drop, reduce or lower* in the same sentence as the word *price* while using the same refinements discussed above.

I make several important design choices in this study. First, the main measure of FPI disclosures is based on textual disclosures instead of other established disclosures, such as management forecasts. FTC's final consent orders, civil lawsuits, and academic literature specifically refer to textual statements as the primary channel through which firms communicate on prices and rarely discuss the use of disclosures, such as management forecast, in this context. Furthermore, sales forecasts are a combination of price and quantity predictions and, therefore, not as effective for communicating future pricing choices to competitors.

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[12]The following words were excluded: oil, gas, fuel, energy, electricity, steel, gold, silver, copper, metal, paper, pulp, raw materials, commodity and purchase.

Second, I opt for a bag-of-words approach instead of a machine learning approach. Given the specificity and infrequency of FPI disclosures, a regular expression-based textual analysis is better equipped to detect these disclosures than machine learning. Moreover, the bag-of-words approach makes my results replicable and allows me to apply the algorithm to other disclosure channels.

Third, the algorithm is designed to exclude the discussion of input prices, even though prior literature does not make this distinction. I make this distinction as antitrust authorities do not consider statements on input prices to be anticompetitive. Furthermore, it reduces the likelihood that my inferences are driven by changes in industry fundamentals.[13]

2.3.2 Future price increase disclosures and summary statistics

This study analyzes Factset conference call transcripts in the period 2003-2013. For these transcripts to be included in the sample, I require that the transcripts have valid GVKEY link, and the issuing firm cannot be a financial institution (SIC code 6000-6999) or operate in a regulated industry (SIC code 4900-4999). Table 2.1 displays the consequences of these sample selection choices on the sample size. In the final sample, I aggregate the transcripts to firm-year observations[14] and combine these with financial information from CRSP, Compustat, and Edgar. I include firm-year observations starting in the first year for which a firm has at least one transcript availabile in Factset[15]. I record the value zero for the different price disclosure variables when no conference call transcript was available in a particular firm-year.

When looking at FPI disclosures on a transcript level, I find that the algorithm detects managers discussing FPIs at least once during 8,684 out of 94,095 conference calls (9 percent). Furthermore, 53 percent of the firms provide at least one FPI disclosure during the sample period. When aggregating to firm-years, I find that managers provide an FPI disclosure in approximately 14 percent of firm-years, as depicted in Panel A of Table 2.2. This percentage is lower than the amount of managers that discuss future price decreases, which occurs in 22 percent of the firm-years. The univariate difference between price increase and decrease disclosures is significant, but could be considered as partly driven by the overall negative economic growth during

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[13]Changing input prices are typically affecting whole industries, and may therefore fully explain any industry level variation.

[14]I aggregate to firm-years instead of, say, firm-quarters because the number of conference calls are not equally spread over quarters.

[15]The start of the sample period coincides with the earliest moment that Factset provides extensive conference call coverage, namely in 2003, and ends three years after the FTC enforcement action of U-Haul in 2013.

my sample period and underline that it is unlikely that all FPI disclosures are used to tacitly collude.

The main measure of industry concentration used in the analyses is the Herfindahl-Hirschman index for Text-Based Network Industries, as developed by Hoberg and Phillips (2010, 2016). This measure compares the similarity of firms' product descriptions in 10-K filings to find product market peers and subsequently calculates the industry concentration based on public and private firms. This measure is more effective in analyzing product markets than SIC or GICS based measures (Jayaraman, Milbourn, Peters, and Seo, 2018). This variable has a value between 0 and 1 and is on average 0.235 in the sample. Alternative HHI measures described in Table 2.3 Panel B are calculated using sales in that particular industry subset.

Table 2.2 Panel B provides insight into the average use of FPI disclosures for every quartile of industry concentration. The descriptive evidence shows a monotonically increasing use of FPI disclosures. I do not find the same pattern for future price decrease disclosures. Taken together, this descriptive evidence seems to confirm the notion that firms in concentrated industries use FPI disclosures more frequently and perhaps strategically. To preview the effects of antitrust oversight, I examine the average future price increase and decrease announcements both before and after the Supreme Court ruling (see section 2.2.4). I find descriptive evidence that only the most concentrated firms provide fewer FPI disclosures after an increase in antitrust oversight, while I do not find the same pattern for future price decrease disclosures.

2.4   Empirical findings

2.4.1   Industry concentration and future price increase disclosures

As a first analysis, I examine whether firms in concentrated industries are using FPI disclosures more frequently than those in fragmented industries, presumably to coordinate price increases among competitors. Given the difficulties of determining the intent behind a particular disclosure, I compare the disclosure of future price increases in highly concentrated industries to the disclosure of future price decreases that do not have competitive benefits. If disclosure incentives correlate with industry concentration independent of competitive reasons, one would expect to see future price increase and decrease disclosures to vary similarly with concentration. I therefore jointly test the association between industry concentration and future price increase and decrease disclosures using the following equations:

$$Price\ increase\ indicator_{i,t} = \beta_0 + \beta_1\,TNIC\_HHI_{i,t} + \gamma X_{it} + \tau_t + \epsilon_{i,t} \quad (2.1)$$

$$Price\ decrease\ indicator_{i,t} = \beta_0 + \beta_1\,TNIC\_HHI_{i,t} + \gamma X_{it} + \tau_t + \epsilon_{i,t} \quad (2.2)$$

where i indexes firms and t indicates time, which, in these tests, is a fiscal year. τ_t are year fixed effects. $Price\ increase\ indicator_{i,t}$ is an indicator variable that switches on when managers discuss future price increases at least once during a conference call in the fiscal year. $TNIC\_HHI_{i,t}$ measures the Herfindahl-Hirschman index for Text-Based Network Industries. X_{it} is a vector of control variables that will be used throughout the analyses. *Log total words press releases* is a control that proxies for the overall level of disclosures. I also control for different firm characteristics and size variables that prior research identifies as generic drivers of disclosures, such as *Return on assets*, *Total assets* and *Market to book*. I include firm-specific stock return during the fiscal year and *industry GDP growth* as proxies for overall performance and economic outlook that are directly relevant in determining whether there are economic reasons to expect future price increases and decreases.

I present the estimation results of equation (2.1) and (2.2) in Table 2.3 panel A. The results show that firms in concentrated industries provide more FPI disclosures than firms in non-concentrated industries. I do not find a correlation between industry concentration and future price *decrease* disclosures. Taken together, these results indicate that collusion is likely an important consideration for providing FPI disclosures. To illustrate the economic significance, I find that, compared to the unconditional mean of FPI disclosures, one standard deviation increase in industry concentration is associated with 11 percent more FPI disclosures.

Next, to strengthen the inference that FPI disclosures are likely used by firms for antitrust purposes, I further examine whether two other closely related disclosures, future profit margin disclosures and future price disclosures without the discussion of an increase or decrease, are associated with industry concentration. The disclosure incentive for these disclosures are arguably similar to those for FPI disclosures, but are less likely to be used to coordinate price increases. Consistent with finding no significant association between future price decreases and industry concentration, I do not find that firms in concentrated industries provide more disclosures on their future pricing (specification 3) or future margins (specification 4).

As a robustness check, I examine the association between FPI disclosures and four alternative measures of industry concentration. In column (1) of Table 2.3 Panel B, I replace the continuous HHI measure with an indicator that equals one if the firm is in the top quartile of industry concentration to reduce concerns of measurement

error. I find that firms in the top quartile of industry concentration provide 25 percent more FPI disclosures than firms in the bottom 3 quartiles, relative to the unconditional mean of FPI disclosures. Columns (2) to (4) show that the results are also robust to industry concentration measures based on SIC2, SIC3, and SIC4 level industry classifications. I find consistent results for all three other SIC-based industry classifications, indicating that the results are not driven by an anomaly in the text based industry classification HHI.

2.4.2 Civil antitrust litigation risk and future price increase disclosures

Having established that firms in concentrated industries provide more frequent FPI disclosures compared to firms in non-concentrated industries, I turn to analyzing the effects of antitrust oversight. I start with examining the effects of increased civil antitrust litigation risk for public disclosures after the Supreme Court ruling in 2007, as described in section 2.2.4. I estimate the following difference-in-differences model:

$$
\begin{aligned}
Price\ increase\ indicator_{i,t} = \beta_0 &+ \beta_1\,TNIC\_HHI_{i,t} + \beta_2 Post_{SC_t} \\
&+ \beta_3\,TNIC\_HHI_{i,t} \times Post\dot{\,}SC + \gamma X_{it} + \alpha_i + \tau_t + \epsilon_{i,t}
\end{aligned}
$$

$$(2.3)$$

where i indexes firms and t indicates time, which, in these tests, is a fiscal year. α_i and τ_t are firm and year fixed effects, respectively. $Post_{SC_t}$ and $TNIC\_HHI_{i,t}$ are the two components of the DiD model. $TNIC\_HHI_{i,t}$ is an industry concentration measure based on text-based industry classifications and $Post_{SC_t}$ is an indicator variable that is one for fiscal year 2007 until 2009.[16] X_{it} is a vector of control variables as described in equation 2.1. This test examines how concentrated firms are changing their FPI disclosures in response to increased antitrust oversight *relative* to non-concentrated firms.[17]

Table 2.4 presents the findings for the effects of antitrust oversight on FPI disclosures. The results in columns (1) and (2) show that firms in concentrated industries

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[16]The overlap between some of the post period and the financial crisis should not be a concern for the inferences drawn from this test. I interpret only the difference-in-difference estimator, which takes into account the difference between the pre and post period.

[17]The DiD is designed to compare firms in concentrated industries with those in non-concentrated industries, instead of comparing firms that have provided FPI disclosures in the past to those that have not provided FPI disclosures. The most important reason is that one should not expect firms to provide FPI disclosures every year. Firms will potentially only engage in price coordination if the price is lower than, or equal to, the price that a monopoly would charge. Any price increase above the monopoly price would lead to lower profits. So when input prices and demand are stable, we would not expect firms to provide FPI disclosures in perpetuity.

reduce their FPI disclosures in response to the change in civil antitrust litigation risk and do so more than firms in non-concentrated industries. After the increase in antitrust oversight, one standard deviation increase in a firm's industry concentration corresponds with a 25 percent decrease in the usage of FPI disclosures relative to the unconditional post period mean frequency of FPI disclosures. This is both the case for the specification with only firm fixed effects and a $Post_{SC}$ indicator variable and for the more stringent specification that includes both firm and time fixed effects. I repeat the same analysis after replacing the FPI disclosure indicator with a continuous variable of FPI disclosures in column (3) and (4). The inference based on these specifications is similar to that of the indicator variable: the higher industry concentration, the more firms reduce their FPI disclosures after the increase in civil antitrust litigation risk. Previous findings also implicitly provide additional support for the notion that firms in concentrated industries use FPI disclosures to collude, as increases of antitrust oversight would otherwise likely not affect the usage of FPI disclosures.

Next, I substantiate the finding in Panel A that antitrust oversight reduces firms' usage of FPI disclosures by using two more treatment and control groups. First, I replace the truncated industry concentration measure $TNIC\_HHI_{i,t}$ with an indicator variable that is one if the firm is in the top quartile of industry concentration in column (1) and (2) of panel B. Using a top quartile indicator can be especially effective in narrowing the potential impact of measurement error in $TNIC\_HHI_{i,t}$ and is consistent with the theoretical prediction. Consistent with prior findings in Panel A, I find that firms from the 25 percent most concentrated industries are 41 percent less likely to disclose FPI after the Supreme Court ruling, relative to the unconditional mean occurrence of at least one FPI disclosure. As an additional robustness, I verified the parallel trends assumption for both the top quartile indicator and the TNIC HHI measure and provide yearly coefficients in Appendix B.

The prior analyses were all based on industry concentration. In the following test, I take a different approach by utilizing a known difference in antitrust oversight. Antitrust authorities and courts give firms that sell directly to consumers more leeway in providing FPI disclosures, as they acknowledge and value that FPI disclosures could help to reduce the search costs for consumers (OECD, 2010). Lowering search costs can in itself lead to non-negligible increases in consumer surplus and making

more informed decisions may lead to increased competition among suppliers.[18] The results in columns (3) and (4) show that non-retail firms indeed reduce their FPI disclosures more than retail firms in response to increased civil litigation risk.

So far, the results indicate that especially firms in concentrated industries reduce their FPI disclosures after an increase in antitrust oversight. However, if the reduction is driven by antitrust concerns, one would not expect to see any changes in other disclosures that are not considered to be anticompetitive. As a falsification test, I therefore replace the dependent variable of equation 2.3 with two other forward-looking price disclosures: future price decrease disclosures and neutral future price discussions. Columns (1)-(4) of Table 2.5 show that firms in concentrated industries do not alter their usage of other price disclosures any differently than firms in fragmented industries after the increase in antitrust oversight. The results, therefore, provide additional support for my prediction that antitrust oversight can reduce issuances of FPI disclosure in industries where firms can use these disclosures to collude.

Prior results further indicate that the relative change in the usage of FPI disclosures is not driven by a potential difference in firm fundamentals between firms in both concentrated and non-concentrated industries. If firms in concentrated industries experience a stronger decline in firm fundamentals and, therefore, initiate fewer price increases, we should expect that these firms provide more future price decrease disclosures, all else equal. The results do not indicate that this is the case. To further alleviate this concern, I report another falsification test in columns (5) and (6). Instead of relying on a specific firm or industry specific indicator to control for changing fundamentals, I examine the number of times that analysts question managers about FPIs. Analysts are informed about the firms' prospects and presumably have insight in whether firms are willing to discuss FPI disclosures in conference calls. Thus, if a confounding event more negatively affects the firm fundamentals in concentrated industries, one would expect that analysts are aware of this and accordingly enquire less frequently about FPIs. The results indicate that analysts do not change the number of questions they ask about future price increases to a different degree for firms in concentrated industries than for firms in non-concentrated industries after the Supreme Court decision. Furthermore, they also indicate that the observed changes in FPI disclosures are not elicited by changes in the questions asked by analysts.

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[18]The fact that retail firms are given more discretion by antitrust authorities does not mean that firms are fully immune, given numerous antitrust cases targeting firms that directly target consumers (e.g. U-Haul). Antitrust authorities also might not provide this extra discretion to FPI disclosures made in conference calls that are more hidden from consumers. It is an empirical question, however, whether it affects how firms perceive these factors, in particular, in light of changes in antitrust oversight.

All evidence taken together, the results indicate that antitrust oversight reduces the tendency of firms in concentrated industries to provide FPI disclosures.

2.4.3   Antitrust oversight and the information environment

In this section, I examine whether the information environment is affected by the reduction of FPI disclosures by firms in concentrated industries following the increase in antitrust oversight. By reducing firms' ability to provide FPI disclosures, antitrust oversight may be effective in reducing price coordination through disclosure, but may also limit the firms' ability to properly inform the market. However, as argued in section 2.2.3, investors may consider FPI disclosures as noisy or insufficiently credible. To see whether the information environment is affected by antitrust oversight, I investigate whether the spreads change in concentrated industries after the Supreme Court ruling. I estimate the following model:

$$
\begin{aligned}
Spread_{i,t} = \ & \beta_0 + \beta_1\, TNIC\_HHI_{i,t} + \beta_2 Post_{SC_t} \\
& + \beta_3\, TNIC\_HHI_{i,t} \times Post_{SC_t} + \gamma X_{it} + \alpha_i + \tau_t + \epsilon_{i,t}
\end{aligned} \tag{2.4}
$$

where i indexes firms and t indicates time, which, in these tests, is a fiscal year. α_i and τ_t are firm and year fixed effects, respectively. The dependent variable is $Spread_{i,t}$, which equals the average bid-ask spread in the fiscal year. $Post_{SC}$ and $TNIC\_HHI_{i,t}$ are the two components of the DiD model. $TNIC\_HHI_{i,t}$ is an industry concentration measure based on text based industry classifications and $Post_{SC}$ is an indicator variable that is one for fiscal year 2007 until 2009. X_{it} is a vector of control variables in line with those used in Amiram, Owens, and Rozenbaum (2016).

   The results in Table 2.6 show that the information environment in concentrated industries is negatively affected. Following an increase in antitrust oversight, a one standard deviation increase in industry concentration is associated with a four percent increase in the firm's stock spread, relative to the post-period average spread. This finding suggests that investors consider FPI disclosures to be informative in concentrated industries. Even though firms in these industries could use FPI disclosures for anticompetitive reasons, investors appear to consider these disclosures sufficiently credible and not too noisy. In addition, this result implies that firms are unable to convey the information in a different way. Overall, this result supports the notion that the antitrust oversight's effort to limit the usage of FPI disclosures does have the unintended consequence that it negatively affects capital markets.

2.4.4 Antitrust enforcement and future price increase disclosures

Prior inferences about the effects of antitrust oversight on FPI disclosures and the information environment were based on one exogenous shock to civil antitrust litigation risk. To investigate whether prior inferences for civil antitrust litigation risk can be generalized to other forms of antitrust oversight, I also examine whether firms' disclosure of FPIs are affected by a sudden change in antitrust enforcement.

In 2010, the FTC announced that it would start to focus on firms that make public statements on pricing that could be considered anticompetitive. The FTC, in a unanimous decision, accused U-Haul of inviting competitors to collude by discussing future pricing during a conference call. More importantly, the FTC announced in an accompanying press release that it would start prosecuting similar cases and also cases with "less egregious" conduct. The move by the FTC was widely criticized by both lawmakers and lawyers, due to the lack of clear guidance on what firms were permitted to discuss publicly.[19]

I examine the effects of this change of enforcement by comparing firms' disclosure of future price increases in the three years before the change with the three years afterward. As the increase in FTC enforcement occurred three years after the increase in civil antitrust litigation risk, it may not incrementally alter the behavior of firms in concentrated industries, as they already significantly reduced their FPI disclosures. I, therefore, perform two different tests: (1) a general pre- and post test across all firms and (2) a DiD estimation partitioning on industry concentration as performed in prior tests.

$$Price\ increase\ indicator_{i,t} = \beta_0 + \beta_1 Post_{U-Haul_t} + \gamma X_{it} + \alpha_i + \epsilon_{i,t} \quad (2.5)$$

$$Price\ increase\ indicator_{i,t} = \beta_0 + \beta_1 TNIC\_HHI_{i,t} + \beta_2 Post_{U\text{-}Haul_t}$$
$$+ \beta_3 TNIC\_HHI_{i,t} \times Post_{U\text{-}Haul_t} + \gamma X_{it} + \alpha_i + \tau_t + \epsilon_{i,t}$$
$$(2.6)$$

where i indexes firms and t indicates time, which, in these tests, is a fiscal year. α_i and τ_t are firm and year fixed effects, respectively. The dependent variable is the indicator variable $Price\ increase\ indicator_{i,t}$, which equals one if a firm provides an

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[19]E.g., Senator Orrin Hatch stated during a Senate meeting that the "unfair and deceptive" standard was too vague for companies to know whether their conduct was illegal. The American Bar Association said in their 2010 antitrust publication that "the Commission should clarify the line between an advance public announcement of a future price increase and an invitation to collude that would be actionable under Section 5."

FPI disclosure in the fiscal year. Specification 2.5 has a pre and post design, while Specification 2.6 represents a DiD model where $Post_{U-Haul}$ and $TNIC\_HHI_{i,t}$ are the two components. $TNIC\_HHI_{i,t}$ is a industry concentration measure based on text-based industry classifications and $Post_{SC}$ is an indicator variable that is one for fiscal year 2010 until 2013. X_{it} is a vector of control variables as described in equation 2.1.

Table 2.7 displays the results for the effect of the increased FTC enforcement on FPI disclosures. Column (1) shows that firms overall reduce their usage of FPI disclosures after the increase in FTC enforcement. This result is based on a within-firm estimation. While I specifically control for changes in the economic conditions and firm fundamentals, it is still possible that these factors drive some of my results. Given the time frame, however, the economic environment is more likely to work against me finding results instead of driving the result. One would expect firms to provide more FPI disclosures in times of economic growth and inflation, while that is the exact opposite of what I find.

The results in column (2) and (3) do not indicate that firms in concentrated industries provide fewer FPI disclosures. This result is consistent with the notion that the increase in FTC enforcement does not lead to any incremental reduction of FPI disclosures above and beyond the effect of the earlier increase in civil antitrust litigation risk. Given that I do find differences in column 1, this provides supporting evidence for the complaints by the American Bar Association and members of Congress that argued the FTC did not provide enough guidance as to when firms are, and when firms are not, permitted to provide FPI disclosures. Firms in both concentrated and less concentrated industries reduced their usage of FPI disclosures. Strong antitrust oversight can, therefore, even affect firms and investors in non-concentrated industries.

Given that the reduction in the usage of FPI disclosures is occurring for all firms in the U.S., there is no clear treatment and control group that can be used to conduct tests on the information environment to investors.[20] As there are many confounding events that could affect spreads when we compare time periods, an analyses of pre- and post-spread would be insufficient. Since the FTC accuses firms of using conference calls to communicate directly to competitors instead of its investors, firms could try to get the information out in a different way. I, therefore, test whether firms are providing FPI disclosures more often in conference presentations to analysts. These

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[20]A natural control group would have been firms in the EU. However, this test was not feasible, as the financial crisis affected the United States and the EU differently with a faster economic recovery in the U.S., and the relative limited availability of transcripts for European firms in Factset that have no US operations (i.e. not under FTC jurisdiction).

conferences are more shielded from the public and include mostly analysts. While statements in conference presentations could still be used in civil antitrust cases, it does provide firms plausible deniability to the FTC and argue that they are communicating with analysts. Column (4), indeed, provides evidence that firms are changing their disclosure channels and convey FPI disclosures in conference presentations. This does not mean that the increase in FTC enforcement does not affect investors at all, since conference calls are much less accessible and create information asymmetry between sophisticated investors and retail investors.

2.5 Conclusion

In this study, I examine the effects of competition and antitrust oversight on disclosures. I develop a new disclosure measure of future price increases and exploit two institutional changes in U.S. antitrust oversight, namely, an increase in civil antitrust liability for making price increase disclosures and an increase in FTC enforcement, resulting in plausibly exogenous variation in antitrust oversight over time.

I find evidence that firms in concentrated industries provide more future price increase disclosures consistent with the notion that these firms use FPI disclosures to coordinate prices. The results in my study suggest that antitrust oversight could be an effective way to limit this behavior by firms. However, it may come with the unintended consequence that it becomes more difficult for investors to be informed. Firms may change their disclosure channel in an attempt to have plausible deniability against claims of collusion, or stop providing this information. While we typically consider only security regulation and industry specific regulators to affect disclosure decisions, this study indicates that other regulations could affect disclosure decisions without considering the impact on shareholders.

Table 2.1: Sample selection and future price increase disclosures

This table presents the effects of the sample restriction criteria on the number of transcripts and firms in the sample period 2003-2013. Furthermore, this table provides insight into the number of transcripts and firms that have FPI disclosures before aggregating to firm-year observations

Panel A	
Sample by transcripts:	
Total number of conference call transcripts	125,450
Total number of conference call transcripts ex regulated industries and financial institutions	99,650
Total number of conference call transcripts with GVKEY	94,095
Total number of conference call transcripts with future price increase discussion	8,684
Sample by firms:	
Total number of firms with conference call transcripts	5,700
Total number of firms with conference call transcripts ex regulated industries and financial institutions	4,511
Total number of firms with conference call transcripts and GVKEY link	4,166
Total number of firms with conference call transcripts and future price increase discussion	2,196

Table 2.2: Summary statistics

This table presents the sample statistics and quartile distribution of FPI disclosures. Each observation is a firm fiscal year in the period 2004 until 2013. Only mean values are presented for indicator variables. All variables are defined in Appendix A. Panel B shows a per quartile split in the usage of future price *increase* and *decrease* disclosures. The pre and post period refer to the 3 year preceding and following the Supreme Court ruling as described in section 2.2.4.

Panel A								
	N	mean	sd	p10	p25	p50	p75	p90
Industry concentration variables								
TNIC HHI	22,981	0.235	0.207	0.064	0.097	0.163	0.298	0.515
2-digit SIC HHI	22,870	0.655	0.679	0.242	0.311	0.413	0.708	1.244
3-digit SIC HHI	22,870	1.609	1.620	0.445	0.563	1.013	1.953	3.501
4-digit SIC HHI	22,869	2.388	1.901	0.644	1.075	1.876	3.107	4.978
Future price disclosures								
Price increase indicator	22,981	0.136	0.343					
Log price increase	22,981	0.274	0.535	0.000	0.000	0.000	0.000	1.099
Price decrease indicator	22,981	0.218	0.413					
Price indicator	22,981	0.582	0.493					
Margin indicator	22,981	0.447	0.497					
Control variables								
Log total words press releases	22,981	7.824	0.763	6.829	7.366	7.870	8.347	8.756
Return on assets	22,981	-0.019	0.221	-0.227	-0.023	0.036	0.078	0.127
Market to Book	22,981	2.870	4.645	0.746	1.285	2.123	3.595	6.283
Stock return during fiscal year	22,981	0.007	0.524	-0.648	-0.225	0.075	0.313	0.564
Log total assets	22,981	6.598	1.813	4.327	5.306	6.515	7.781	9.009
Industry GDP	22,981	5.809	0.915	4.699	5.093	5.727	6.513	7.011
Industry GDP Growth	22,981	-0.009	0.214	-0.191	0.005	0.048	0.081	0.116
Capital market variables								
Spread	22,908	0.408	0.702	0.047	0.086	0.162	0.372	1.001
Log market cap	23,195	6.743	2.121	4.401	5.414	6.571	7.796	9.114
Trading volume	22,996	1.354	4.195	0.048	0.131	0.355	1.014	2.877
Log stock price	22,996	2.727	1.043	1.252	2.055	2.887	3.512	3.940
Standard deviation of returns	22,996	0.031	0.018	0.015	0.020	0.027	0.037	0.051
Stock turnover	22,996	0.010	0.009	0.003	0.004	0.008	0.012	0.019

Panel B

		TNIC concentration quartiles			
		1	2	3	4
Future price *increase* disclosures	**overall**	12%	12%	15%	16%
	pre	11%	13%	15%	17%
	post	11%	11%	14%	12%
Future price *decrease* disclosures	**overall**	23%	20%	22%	22%
	pre	21%	18%	19%	18%
	post	25%	21%	23%	23%

Table 2.3: Effects of industry concentration on price disclosures

This table presents the results of the OLS estimation of the association between industry concentration and disclosures of future pricing in conference calls. Panel A examines whether industry concentration is correlated with future pricing disclosures. Panel B investigates whether the results for future price increase disclosure are robust to different measures of industry concentration. *Price increase indicator* is an indicator variable that equals one if a manager discusses future price increases at least once during the fiscal year. *Price decrease indicator* is an indicator variable that equals one if a manager discusses price decreases at least once during the fiscal year. *Price indicator* is an indicator variable that equals one if a manager discusses price at least once during the fiscal year, but does not discuss either an increase or decrease. *Margin indicator* is an indicator variable that equals one if a manager discusses future margins at least once during the fiscal year. The sample period for these tests is 2004-2013 and the other variables are as defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC), with the exception of Panel B column (1) & (3) that are robust to respectively within 2 and 4 digit SIC industry correlation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A

	(1) Price increase indicator	(2) Price decrease indicator	(3) Price indicator	(4) Margin indicator
TNIC HHI	0.073***	0.023	0.005	0.034
	(3.268)	(0.881)	(0.173)	(0.498)
Log total words press releases	0.004	0.012**	-0.004	-0.018**
	(0.892)	(2.187)	(-0.592)	(-2.290)
Return on assets	0.055***	0.056**	0.106***	0.193***
	(3.960)	(2.364)	(3.535)	(3.496)
Market to Book	0.000	-0.001	0.000	0.002
	(0.336)	(-0.964)	(0.503)	(1.206)
Stock return during fiscal year	0.005	-0.053***	0.023***	-0.014
	(1.104)	(-6.907)	(3.141)	(-1.474)
Log total assets	0.025***	0.043***	-0.011*	0.021***
	(7.166)	(12.089)	(-1.673)	(3.135)
Industry GDP	0.003	-0.016*	-0.004	-0.031
	(0.456)	(-1.929)	(-0.431)	(-1.477)
Industry GDP Growth	-0.103**	-0.130***	0.101***	-0.041
	(-2.113)	(-5.115)	(2.898)	(-0.654)
Observations	22,929	22,929	22,929	22,929
Adjusted R-squared	0.033	0.055	0.006	0.022
Year Fixed Effects	Yes	Yes	Yes	Yes

Panel B

	(1)	(2)	(3)	(4)
		Price increase indicator		
Top Quartile TNIC HHI	0.034***			
	(3.517)			
2-digit SIC HHI		0.027**		
		(2.059)		
3-digit SIC HHI			0.019***	
			(4.458)	
4-digit SIC HHI				0.012***
				(4.112)
Log total words press releases	0.004	0.004	0.005	0.005
	(0.870)	(1.082)	(1.205)	(1.226)
Return on assets	0.057***	0.057***	0.048***	0.054***
	(4.090)	(2.935)	(3.477)	(4.128)
Market to Book	0.000	0.000	0.000	0.000
	(0.343)	(0.293)	(0.557)	(0.466)
Stock return during fiscal year	0.005	0.005	0.005	0.005
	(1.067)	(0.985)	(1.109)	(1.021)
Log total assets	0.024***	0.023***	0.023***	0.024***
	(7.044)	(5.890)	(6.450)	(8.022)
Industry GDP	0.003	0.006	0.002	0.003
	(0.452)	(0.571)	(0.289)	(0.509)
Industry GDP Growth	-0.103**	-0.080	-0.090*	-0.101**
	(-2.111)	(-1.385)	(-1.655)	(-1.977)
Observations	22,929	22,940	22,940	22,939
Adjusted R-squared	0.033	0.034	0.039	0.036
Firm Fixed Effects	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes

Table 2.4: Effect of Supreme Court on price increase disclosures

This table presents the results of the OLS estimation of the effects of the Supreme Court ruling in the case *Bell Atlantic Corp. v. Twombly* on future price increase disclosures. Panel A presents the main treatment effect and Panel B provides a robustness test with different treatment and control groups. The sample period is 2004-2009 where the $post_{SC}$ period is defined as the period 2007-2009. *Price increase indicator* is an indicator variable that equals one if a manager discusses future price increases at least once during the fiscal year. *Log Price increase* is a natural log of the total mentions of price increases by a manager in the fiscal year. Variables are defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A

	(1)	(2)	(3)	(4)
	Price increase indicator		Log price increase	
TNIC HHI $\times$ Post$_{SC}$	-0.107***	-0.105***	-0.091*	-0.086*
	(-3.713)	(-3.643)	(-1.922)	(-1.836)
Post$_{SC}$	0.007		-0.002	
	(0.583)		(-0.110)	
TNIC HHI	0.066**	0.064*	0.077	0.074
	(1.990)	(1.918)	(1.577)	(1.497)
Log total words press releases	-0.010*	-0.010*	-0.018**	-0.018**
	(-1.796)	(-1.795)	(-2.123)	(-2.140)
Return on assets	0.029	0.024	0.058**	0.048*
	(1.619)	(1.322)	(2.083)	(1.652)
Market to Book	0.000	0.000	0.000	0.000
	(0.182)	(0.109)	(0.345)	(0.222)
Stock return during fiscal year	-0.016**	-0.004	-0.029**	-0.002
	(-2.278)	(-0.552)	(-2.520)	(-0.156)
Log total assets	-0.002	0.003	0.013	0.022
	(-0.160)	(0.221)	(0.721)	(1.202)
Industry GDP	0.042	0.051	0.038	0.045
	(0.962)	(0.893)	(0.572)	(0.514)
Industry GDP Growth	0.148***	0.077	0.274***	0.127
	(2.607)	(1.138)	(2.931)	(1.189)
Observations	13,863	13,863	13,835	13,835
Adjusted R-squared	0.276	0.277	0.341	0.343
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	Yes

Panel B

	(1)	(2)	(3)	(4)
		Price Increase indicator		
Top quartile TNIC HHI $\times$ Post$_{SC}$	-0.055***	-0.054***		
	(-4.093)	(-4.072)		
Top Quartile TNIC HHI	0.023**	0.023**		
	(2.011)	(1.978)		
Non-retail firm $\times$ Post$_{SC}$			-0.068**	-0.077**
			(-2.196)	(-2.355)
Post$_{SC}$	-0.003		0.041	
	(-0.283)		(1.355)	
Log total words press releases	-0.010*	-0.010*	-0.010*	-0.010*
	(-1.770)	(-1.768)	(-1.844)	(-1.817)
Return on assets	0.029	0.024	0.027	0.021
	(1.631)	(1.330)	(1.475)	(1.118)
Market to Book	0.000	0.000	0.000	0.000
	(0.195)	(0.116)	(0.224)	(0.109)
Stock return during fiscal year	-0.017**	-0.004	-0.016**	-0.003
	(-2.310)	(-0.584)	(-2.236)	(-0.391)
Log total assets	-0.002	0.002	-0.001	0.004
	(-0.168)	(0.213)	(-0.091)	(0.320)
Industry GDP	0.040	0.049	0.064	0.089
	(0.917)	(0.875)	(1.412)	(1.460)
Industry GDP Growth	0.149***	0.076	0.129**	0.038
	(2.623)	(1.129)	(2.287)	(0.535)
Observations	13,863	13,863	13,874	13,874
Adjusted R-squared	0.276	0.277	0.276	0.278
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	Yes

Table 2.5: Falsification test

This table presents the results of the falsification test for the effects of an increased burden of proof by the Supreme Court on other types of future price disclosures. The sample period is 2004-2009 where the $post_{SC}$ period is defined as the period 2007-2009. *Price decrease indicator* is an indicator variable that equals one if a manager discusses future price decreases at least once during the fiscal year. *Price indicator* is an indicator variable that equals one if a manager discusses price at least once during the fiscal year, but does not discuss either an increase or decrease. *Price increase indicator analyst* is an indicator variable that equals one if analysts ask at least one question on future price increases during the fiscal year. Variables are defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Price decrease indicator		Price indicator		Price increase indicator analyst	
TNIC HHI × Post$_{SC}$	0.014	0.003	0.078	0.082	-0.015	-0.010
	(0.281)	(0.062)	(1.290)	(1.401)	(-0.436)	(-0.281)
Post$_{SC}$	-0.006		-0.006		-0.015	
	(-0.315)		(-0.298)		(-1.153)	
TNIC HHI	-0.101**	-0.101**	0.033	0.035	0.015	0.013
	(-2.174)	(-2.158)	(0.602)	(0.644)	(0.396)	(0.340)
Log total words press releases	-0.006	-0.007	0.011	0.011	0.006	0.006
	(-0.902)	(-1.086)	(1.190)	(1.187)	(1.076)	(1.144)
Return on assets	-0.026	-0.001	-0.008	-0.022	-0.015	-0.026
	(-0.902)	(-0.036)	(-0.198)	(-0.585)	(-0.670)	(-1.115)
Market to Book	0.001	0.001	-0.000	-0.001	-0.000	-0.000
	(0.680)	(1.380)	(-0.077)	(-0.415)	(-0.268)	(-0.552)
Stock return during fiscal year	-0.031***	-0.052***	0.014	0.027**	-0.010*	0.002
	(-4.011)	(-5.208)	(1.356)	(2.176)	(-1.711)	(0.350)
Log total assets	0.043**	0.037**	0.037**	0.038**	0.014	0.018*
	(2.591)	(2.201)	(2.379)	(2.387)	(1.421)	(1.882)
Industry GDP	0.135**	0.064	-0.071	-0.090	-0.015	0.021
	(2.587)	(0.912)	(-0.959)	(-1.068)	(-0.344)	(0.391)
Industry GDP Growth	-0.338***	-0.025	0.149*	-0.030	0.179***	0.036
	(-4.218)	(-0.316)	(1.748)	(-0.275)	(2.976)	(0.578)
Observations	13,863	13,863	13,863	13,863	13,835	13,835
Adjusted R-squared	0.258	0.264	0.188	0.190	0.195	0.197
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	Yes	No	Yes

Table 2.6: Information asymmetry and price increase disclosures

This table presents the results of the OLS estimation of the effects of the Supreme Court ruling in the case *Bell Atlantic Corp. v. Twombly* on information asymmetry for firms with differing amounts of industry concentration. The sample period is 2004-2009 where the $post_{SC}$ period is defined as the period 2007-2009. *Spread* is the average daily spread in percentage points during the fiscal year. *Log Price increase* is a natural log of the total mentions of price increases by a manager in the fiscal year. Variables are defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Spread	
TNIC HHI × Post$_{SC}$	0.137***	0.141***
	(2.618)	(2.719)
TNIC HHI	-0.016	-0.022
	(-0.328)	(-0.472)
Post$_{SC}$	-0.022	
	(-1.000)	
Log market cap	-0.057***	-0.048***
	(-2.826)	(-2.823)
Trading volume	-0.013***	-0.013***
	(-2.881)	(-2.824)
Standard deviation of returns	10.399***	10.144***
	(6.461)	(5.293)
Stock turnover	-11.629***	-11.439***
	(-2.694)	(-2.667)
Log stock price	-0.355***	-0.365***
	(-7.772)	(-7.183)
Observations	15,321	15,321
Adjusted R-squared	0.735	0.740
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	No	Yes

Table 2.7: Price increase disclosures after increased enforcement

This table presents the results of the OLS estimation of the effects of increased enforcement by the FTC (after its U-Haul enforcement action) on future price increase disclosures. The sample period is 2006-2013 where the *post period U-Haul* is defined as the period 2010-2013. *Price increase indicator* is an indicator variable that equals one if a manager discusses future price increases at least once during the fiscal year. *Price increase indicator conference presentation* is an indicator variable that equals one if a manager discusses future price increases at least once in a conference presentation during the fiscal year. Variables are defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Price Increase indicator			Price increase indicator conference presentation
Post period U-Haul	-0.013**	-0.005		0.128***
	(-2.065)	(-0.557)		(8.444)
TNIC HHI × Post period U-Haul		-0.033	-0.033	
		(-1.280)	(-1.268)	
TNIC HHI		0.004	0.009	
		(0.182)	(0.387)	
Log total words press releases	-0.009**	-0.009**	-0.008*	-0.015
	(-2.061)	(-2.072)	(-1.954)	(-1.610)
Return on assets	0.041***	0.041***	0.024	0.059
	(2.782)	(2.798)	(1.639)	(1.439)
Market to Book	0.001	0.001	0.001	-0.001
	(1.219)	(1.272)	(1.214)	(-0.777)
Stock return during fiscal year	-0.025***	-0.025***	-0.005	-0.025**
	(-4.980)	(-4.955)	(-0.894)	(-2.269)
Log total assets	-0.006	-0.007	0.000	0.007
	(-0.904)	(-1.033)	(0.055)	(0.475)
Industry GDP	-0.057**	-0.057**	-0.028	-0.258***
	(-2.136)	(-2.135)	(-0.863)	(-4.179)
Industry GDP Growth	0.161***	0.161***	0.051	0.166***
	(5.738)	(5.730)	(1.358)	(3.165)
Observations	18,098	18,080	18,080	9,672
Adjusted R-squared	0.227	0.227	0.230	0.108
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No

Appendix A: Variable Definitions

Variable Name	Definition	Data Source
Price increase indicator	Indicator equals one for firms with a conference call in the fiscal year where they discuss future price increases.	Self-constructed
Log price increase	Log count of number of times managers discuss future price increases in conference calls in the fiscal year.	Self-constructed
Price decrease indicator	Indicator equals one for firms with a conference call in the fiscal year where they discuss future price decreases.	Self-constructed
Price indicator	Indicator equals one for firms with a conference call in the fiscal year where they discuss future prices without a discussion of price increase or decrease.	Self-constructed
Margin indicator	Indicator equals one for firms with a conference call in the fiscal year where they discuss future margins (unsigned).	Self-constructed
TNIC HHI	HHI score for the Hoberg and Philips 3-digit Text-based Network Industry classification.	Hoberg-Phillips website
Log total words press releases	Log of the total number of words used in press releases that particular year.	Edgar
Return on assets	Net income (NI) scaled by the total assets (AT) of the company.	COMPUSTAT
Industry GDP	Log of the yearly industry specific GDP.	U.S. Bureau of Economic Analysis
Industry GDP Growth	Percentage change in year-to-year industry specific GDP.	U.S. Bureau of Economic Analysis
Market to Book	Share price (PRC) multiplied by shares outstanding (SHROUT), scaled by shareholder equity (SEQ) minus Preferred stock redemptions (PSTKRV). Alternatively, the denominator book value of equity was calculated as total assets (AT) minus total liabilities (LT) and minority interest (MIB).	CRSP & COMPUSTAT
Stock return during fiscal year	Cumulative return of the firm's stock during fiscal year	CRSP
Log total assets	Log of total assets (AT)	Compustat
Log market cap	Log of price times shares outstanding at fical year end	CRSP
Trading volume	Average trading volume in fiscal year in millions	CRSP
Standard deviation of returns	Standard deviation of daily returns in fiscal year	CRSP
Stock turnover	Average of dollar volume scaled by market value in fiscal year	CRSP
Log stock price	Log of average stock price in fiscal year	CRSP

Appendix B: Parallel Trends

This table presents the results of the OLS estimation of the effects of the Supreme Court ruling in the case *Bell Atlantic Corp. v. Twombly* on future price increase disclosures. The sample period is 2004-2009 with 2006 being the reference year. Column (1) & (2) display the coefficients for *TNIC HHI* interacted with year indicators Column (3) & (4) display the coefficients for *Top Quartile TNIC HHI* interacted with year indicators. *Price increase indicator* is an indicator variable that equals one if a manager discusses future price increases at least once during the fiscal year and the dependent variable in all specifications in this table. *Log Price increase* is a natural log of the total mentions of price increases by a manager in the fiscal year. Variables are defined in Appendix A. T-statistics (reported in parentheses) are robust to within-industry correlation (3 digit SIC). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	TNIC HHI		Top Quartile TNIC HHI	
	(1)	(2)	(3)	(4)
	Price increase indicator			
t-3	0.020	-0.000	0.009	0.002
	(0.543)	(-0.012)	(0.497)	(0.102)
t-2	0.065*	0.072*	0.016	0.016
	(1.914)	(1.885)	(0.951)	(0.861)
t-1 (reference year)	0	0	0	0
t	-0.053	-0.068*	-0.039***	-0.045***
	(-1.569)	(-1.732)	(-2.700)	(-2.725)
t+1	-0.041	-0.038	-0.026	-0.025
	(-1.315)	(-1.053)	(-1.630)	(-1.368
t+2	-0.071**	-0.080**	-0.048***	-0.055***
	(-2.300)	(-2.231)	(-3.107)	(-3.100)
Log total words press releases		-0.008		-0.009*
		(-1.620)		(-1.652)
Return on assets		0.027		0.027
		(1.579)		(1.570)
Market to Book		0.000		0.000
		(0.205)		(0.220)
Stock return during fiscal year		-0.003		-0.003
		(-0.476)		(-0.525)
Log total assets		-0.000		-0.001
		(-0.053)		(-0.068)
Industry GDP		0.047		0.047
		(1.169)		(1.163)
Industry GDP Growth		0.093*		0.089*
		(1.887)		(1.827)
Observations	15,530	13,418	15,530	13,418
Adjusted R-squared	0.274	0.280	0.274	0.280
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Chapter 3

Do Firms Strategically Announce Capacity Expansions to Deter Entry?

3.1   Introduction

Ample prior literature examines the causes and consequences of voluntary disclosure from a capital markets perspective. Recently, there's been a push to understand the role disclosures play as *strategic* devices that firms can use to improve their competitive positions (e.g., Tomy, 2017; Bloomfield, 2018; Burks et al., 2018; Bourveau et al., 2019; Glaeser and Landsman, 2019; Kepler, 2019). In that vein, we explore firms' use of voluntary disclosures as part of their entry deterrence strategies, and provide evidence that firms issue capacity expansion announcements (hereafter "CEAs"), strategically, to ensure that potential entrants are aware of ongoing capacity investments—a necessary condition for a successful capacity-based approach to entry deterrence.

Analytical work on industrial organization suggests that firms can deter entry by investing in capacity expansions (e.g., Spence, 1977; Dixit, 1980; Tirole, 1988;

Ellison and Ellison, 2011). Moreover, survey and archival evidence supports the notion that firms actually employ this strategy (e.g., Smiley, 1988; Cookson, 2017a,b). However, by themselves, strategic capacity expansions are unlikely to be broadly effective at deterring product market entry, because investments in capacity are not necessarily immediately observable to potential entrants. Such expansions can only be an effective deterrent when the investments are observable *before* the potential entrants make their own (initial) investments and decision to enter the market. If potential entrants are unaware of an incumbent's capacity expansion efforts, they are likely to be undeterred, leaving the incumbent with suboptimal excess capacity, a lot of wasted capital expenditure costs, and new rivals to contend with.

We posit that this weakness can be substantially remedied by accompanying voluntary disclosures. By voluntarily announcing their capacity expansions, incumbents can make potential entrants aware their investment plans more quickly—perhaps even before the expansion begins—thus making voluntary disclosures a vital component of firms' capacity-based entry deterrence strategies. Accordingly, we examine whether firms are more likely to use voluntary disclosures to announce their current and future capacity expansion plans when facing increased threats of entry. We find that, for a given level of investment, greater entry threats are associated with a greater likelihood that an expansion is preceded by a voluntary CEA. Moreover, CEAs appear to be effective at deterring entry; holding the underlying capacity investments fixed, CEAs are associated with a 13% reduction in subsequent entry. Absent an accompanying disclosure, we find no evidence to suggest that investments in capacity beget reduced subsequent entry. Collectively, our results suggest that voluntary disclosures—CEAs, in particular—are an integral part of firms' capacity-based entry deterrence strategies.

While CEAs can be beneficial by signaling the firm's aggressive stance to potential entrants, they can also be costly to the disclosing firm. Any such disclosure can carry significant proprietary costs, revealing the firm's private information to both incumbent and potential rivals (e.g., Wagenhofer, 1990). In particular, a CEA could backfire by signaling excellent industry prospects (e.g., strong future demand), thereby encouraging greater entry from potential entrants. Thus, we do not believe that all firms would benefit by responding to entry threats in this way. We posit that firms rationally weigh the strategic benefits against the proprietary costs, when making their disclosure decisions, and will only issue a CEA if they believe the net benefits are positive.

We begin our empirical investigation by identifying CEAs. We use a text-mining approach to construct a novel measure of voluntary disclosure reflecting firms' CEAs.

Our algorithm analyzes all US public firms' press-releases, over the period of 1995-2016, and codes them as CEAs if they include explicit forward-looking statements about capacity increases. We find that CEAs comprise about 2% of all press releases. Roughly 4.5% of firm-years includes one such disclosure, and over 20% of firms issue at least one CEA during our sample period. We validate our measure by showing that CEAs are informative about firms' future capacity expansions. After controlling for firm and industry-year fixed effects, CEAs are associated with 15% year-over-year increases in CAPEX, and 4% year-over-year increases in PP&E.

To assess the strategic intent behind these voluntary disclosures, we look to Chinese imports as a source of plausibly exogenous variation in the threat of entry. Prior work shows that changes in Chinese exports are largely driven by exogenous increases in China's productivity (e.g., Autor, Dorn, and Hanson, 2013). Accordingly, several recent studies examine the effects of competition by using Chinese exports to the other developed countries as an instrument for exports to the own-country (e.g., Autor et al., 2013; Autor, Dorn, Hanson, and Song, 2014; Autor, Dorn, Hanson, Pisano, and Shu, 2018; Hummels, Jørgensen, Munch, and Xiang, 2014; Hombert and Matray, 2018). Under similar intuition, we use Chinese exports to the developed world (excluding the US) as a source a plausibly exogenous variation in entry threats. Our empirical strategy differs from the prior work in one key aspect. Rather than using Chinese exports to non-US countries to instrument for contemporaneous Chinese exports to the US (i.e., *current* competition faced by US firms), we instead use a reduced form approach and include Chinese exports to non-US countries directly as our regressor of interest. This alteration allows us to control for contemporaneous Chinese exports to the US, and interpret the Chinese exports to non-US countries as *potential*, but not currently realized, competition from China—i.e., the level of entry threat.

We identify these threats at the industry-year level and find that firms respond to them by announcing capacity expansions. Consistent with our predictions, larger firms (whose capacity investments have more serious implications for an entrant's profitability, further discouraging entry) are more likely to respond in this fashion, while more opaque firms (whose disclosures are more likely to convey private information about industry demand, potentially *encouraging* entry) are less likely to respond in this fashion. The magnitude of the effect is substantial. Holding the firm fixed, and controlling for actual investments in capacity, a one standard deviation increase in the threat of entry is associated with a roughly 5% to 7% increase in the likelihood of a CEA. Moreover, the likelihood of a CEA is not associated with contemporaneous imports into the US, suggesting that neither domestic demand

conditions, nor incumbent competition can explain our results.[1] Jointly, we interpret these facts as evidence that CEAs are, in part, intended to serve a strategic purpose: entry deterrence. Any alternative interpretation of our results must explain why US firms' CEAs are associated with Chinese exports into other developed countries, but not Chinese exports to their own country.

Our research design is intended to mitigate the potentially confounding effects of domestic demand shocks. Another possibility is that common supply shocks, in the US and China, affect both Chinese exports to the developed world, and US firms' need for capacity. Our setting will not be as effective at combating this confound. As a sensitivity analysis, we control for US exports to the developed world, and confirm that our results continue to hold. The robustness of our results bolsters the notion that *strategic considerations* (i.e., entry deterrence), and not supply shocks, are responsible for our findings.

An alternative concern could be that US investors simply demand more information from firms, when entry threats are greater. Since CEAs are the primary disclosure variable that we are testing, CEAs may be proxying for overall changes in disclosures. That is, perhaps the observed increase in CEAs is not a strategic response to entry threats, but rather a byproduct of a secular increase in disclosure, in response to investors' demand for transparency. To assess this possibility, we examine whether aggregate levels of disclosure increase in response to entry threats. We find no evidence that overall disclosure levels increase in response to entry threats. Our evidence suggests that overall transparency remains constant or decreases in response to entry threats; the increase disclosure we document seems to be specific to CEAs. Thus, it appears unlikely that investor demand for information can explain our findings.

As a final analysis, we examine whether CEAs appear to be effective at deterring entry. We find evidence to suggest that they are. Controlling for China's current exports to the US and the Developed World, issuing a CEA is associated with about a 1.2 basis point reduction in future Chinese imports. This corresponds to about a 13% reduction, relative to the base-rate of entry. However, we caution that even if our measure of entry threat is perfectly exogenous, firms' responses to entry threats are endogenous. Therefore, one cannot interpret the association between CEAs and subsequent entry as a cleanly identified causal effect. If firms are more likely to issue a CEA when entry threats are more severe, then the estimated association will underestimate the true causal effect. Conversely, if there is heterogeneity in the

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[1]Even if Chinese exports to the developed world are incrementally informative about US demand, it is highly improbable that Chinese exports to the developed world are a better signal of US demand than exports to the US.

effectiveness of such disclosures, it is likely that the firms which choose to issue CEAs are precisely those firms for which their effectiveness is greatest. Thus, the estimated association may reflect the average deterrence effect among those firms who chose to issue CEAs, and not necessarily the average effect for the typical firm. For these reasons, we offer these analyses only as supplemental descriptive evidence regarding the effectiveness of CEAs as entry deterrence tools.

One might wonder whether our results attain because firms are truly using CEAs, *strategically*, to respond to entry threats. Perhaps firms respond to entry threats by strategically expanding capacity (as suggested by prior literature), and then make the non-strategic choice to voluntarily disclose their plans, purely for the sake of transparency. We use three different methods to address this concern. First, we control for firms' actual capacity expansion activities (as captured by increases in CAPEX and PP&E) in both the contemporaneous and subsequent fiscal year. We find that, even holding fixed the underlying capacity expansion actions, firms are more likely to issue a CEA when they face a greater threat of entry. Second, we conduct our tests on a subsample of firms that substantially expanded capacity, and document the same pattern: the firms facing greater entry threats are the ones that are more likely to issue an accompanying (or preceding) CEA. Third, when we examine subsequent entry decisions, we find that it is the CEAs, rather than the underlying capacity expansion actions, that are best able to explain reduced subsequent entry. Collectively, our results suggest that the disclosure choice itself (i.e., the decision to issue a CEA) is an economically important strategic tool that firms use in response to heightened entry threats.

Lastly, one might be concerned that CEAs are mechanically driven by regulatory disclosure requirements. To this end, we offer two important remarks. First and foremost, we emphasize that regulation does *not* compel firms to provide CEAs.[2] Second, we note that the vast majority of capacity expansions are not accompanied (nor preceded) by CEAs. Thus, it appears that the decision to issue a CEA is

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[2]Securities regulation requires the disclosure of material information and agreements such as lease contracts or financing, and as such seem relevant to firms issuing CEAs. However, prior rulings by the courts and guidance by the SEC make it reasonable to assume that CEAs are not material and thus not mechanically driving our results. CEAs seem unlikely to affect shareholders' decision making and therefore do not meet the supreme court's definition of material information, as clarified in TSC Industries, Inc. v. Northway Inc (1976). They define material information to be information that "a reasonable shareholder would consider it important in deciding how to vote". It also does not fit the SEC's definition of material as it has stipulated material agreements to be those that are not made in the "ordinary course of business" and it even explicitly mentions that the opening a new store is not material if it already has stores (see https://www.sec.gov/investor/pubs/readan8k.pdf).

economically distinct from the decision to expand in the first place; no mechanical relation appears to exist between the two.

This study makes multiple contributions. First and foremost, we contribute to the voluntary disclosure literature, by developing and validating a new measure of voluntary disclosure—the capacity expansion announcement—and providing evidence that firms use such disclosures as strategic devices, designed to discourage product market entry. The vast majority of the voluntary disclosure literature examines the causes and consequences of disclosure from a capital markets perspective.[3] The role of the product market is typically—though not always—considered only as a way to justify non-disclosure through a proprietary costs channel (e.g., Verrecchia, 1983). We depart from this perspective by providing evidence that firms issue voluntary disclosures, not only *in spite of* product market concerns, but also *because* of them.

In so doing, we contribute to the budding literature on the role of accounting/disclosure decisions in facilitating strategic product market goals (e.g., Li, 2010; Tomy, 2017; Bloomfield, 2018; Burks et al., 2018; Bourveau et al., 2019; Glaeser and Landsman, 2019). Ours is not the only study to examine accounting-based entry deterrence strategies. Prior accounting literature examines whether firms attempt to deter entry by using downwards earnings manipulations and negatively toned press releases to create the impression of poor industry prospects, in a practice known as 'profit hiding' (e.g., Tomy, 2017; Burks et al., 2018). We study a different channel through which firm disclosures might discourage entry and show that firms use voluntary disclosures to deter entry by making explicit announcements about future capacity expansions, a la the Spence-Dixit model. This behavior is especially pronounced among larger and more transparent firms, for which the net entry deterrence benefits of such disclosures are predicted to be greater. These findings complement and extend upon those of Li (2010), who documents that firms are less pessimistic in their investment forecasts when barriers to product market entry are lower (i.e., industries with low capital intensity). Our study further complements concurrent work by Glaeser and Landsman (2019), who examine firms' choices to voluntarily accelerate their patent application disclosures, finding that timelier disclosures are more successful in carving out a product market niche, by pushing potential rivals to purse less similar product offerings.

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[3]See, for example: Lang and Lundholm (1993); Botosan (1997); Core (2001); Healy and Palepu (2001); Lambert et al. (2007); Francis et al. (2008); Bischof and Daske (2013); Balakrishnan et al. (2014); Leuz and Wysocki (2016); Dyer et al. (2016); Guay et al. (2016); Schoenfeld (2017); Gow et al. (2019).

Lastly, we contribute to the industrial organization literature on strategic entry deterrence by being the first to provide evidence that firms use voluntary disclosures of their capacity expansion plans as an integral component of their capacity-based entry deterrence strategies. We find that firms respond to a plausibly exogenous increase in the threat of entry by preannouncing capacity expansions, and document that such disclosures appear to be effective at deterring entry. These findings complement existing analytical, survey and archival work in the industrial organization literature which shows the use of capacity investments to deter entry (e.g., Spence, 1977; Dixit, 1980; Smiley, 1988; Tirole, 1988; Cookson, 2017a,b). Existing work in this area relies predominately on the Spence-Dixit model of entry deterrence, which assumes that potential entrants become [exogenously] aware of capacity expansions, prior to making their own entry decisions. We provide evidence of a *mechanism* through which this revelation occurs: incumbents' voluntary disclosures of their capacity expansion plans.

3.2   Motivation/Hypothesis Development

In imperfectly competitive product markets, incumbents can sustain positive economic profits. However, these profit opportunities can encourage entry, which pushes the industry closer to perfect competition, reducing incumbents' profits. Accordingly, incumbents often seek ways to deter entry, in order to sustain their stream of rents (e.g., Salop, 1979). Decades of analytical work in the industrial organization literature shows that investments in capacity can be used to deter entry (e.g., Spence, 1977; Dixit, 1980; Tirole, 1988; Ellison and Ellison, 2011). In the context of a monopolistic incumbent, Ellison and Ellison (2011) summarize these models as follows:

"The prototypical model of strategic entry deterrence is a three-stage game... In the first stage, the incumbent firm 1 chooses an investment level A... *Before the second stage, the potential entrant (firm 2) observes the incumbent's choice of A.* Firm 2 then chooses whether to enter the market, which requires paying a sunk cost of entry. In the third stage, either the incumbent is a monopolist or the incumbent and entrant compete as duopolists" (Ellison and Ellison, 2011, emphasis added).

Most important, for our purposes, is the italicized portion—that the potential entrant becomes aware of the incumbent's actions before making an entry decision (and incurring the associated entry costs). This assumption is ubiquitous throughout the extant industrial organization literature on capacity-based entry deterrence. Our motivating question is simple: how does this revelation occur? While some expansions may be immediately obvious to potential entrants, *prima facie*, this is unlikely to be

the case, generally. Thus, in order to deter entrants, firms have a strategic incentive to ensure that potential entrants are aware of their capacity expansion plans. Voluntary CEAs are a natural avenue for firms to achieve this goal.

These voluntary disclosures can both accelerate and improve the dissemination of a firm's investment plans. By making potential entrants aware of a capacity expansion plan, firms can deter entry, even if the CEA has no real effect on the investment itself. A non-disclosed capacity expansion may not evidence itself in an annual report (or other outlets) for an extended period of time, at which point the potential entrant may already have incurred the sunk costs of entry. Once the potential entrant incurs these costs, entry becomes almost inevitable. Thus, entry deterrence strategies depend crucially on preventing potential entrants from incurring these initial costs. Publicly pre-announcing an expansion plan (i.e., issuing a CEA) helps in this regard by making potential entrants aware of an investment more quickly and directly—perhaps even before the investment occurs—thereby mitigating the possibility that a potential entrant will decide to enter.

It is also conceivable that a CEA could function as a commitment device, alleviating the constraints of sequential rationality, and/or serving as a costly signal (à la Spence, 1973). Such a commitment could push an incumbent firm to make larger investments than would otherwise be rationalizable, and/or help firms stick to a capacity expansion plan, further pushing potential entrants to stay out of the marketplace. If deviating from an announced plan is costly,[4] a firm can garner a "first-mover advantage" by preannouncing an investment decision (see Stackelberg, 1934; Schelling, 1960; Corona and Nan, 2013). By gaining such an advantage, an incumbent could prevent entry or soften competition among incumbent rivals.

In motivating our study, we consider the role of CEAs as conveyors of information, and not as commitment devices. However, we remain agnostic as to the true strategic purpose of the CEA as our analysis is unable to distinguish between these non-mutually exclusive possibilities. We do not view this as a concern for our findings, as the purpose of our study is to document whether firms use CEAs strategically in response to entry threats. The exact specifics of their strategic function (i.e., signaling versus commitment, etc.) lies beyond the intended scope of our study. Future work

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[4]There are several costs to firms and managers when not completing previously announced expansions. First, firms could face legal liability for providing incorrect statements to shareholders. Second, managers lose some of their credibility which consequently reduces the efficacy of any future disclosures. Third, both the firm and manager may face reputation harm for not being able to meet prior commitments.

could determine the extent to which CEAs have any 'real effects' on the disclosing firms' competitive actions.[5]

We examine whether firms respond to entry threats by voluntarily disclosing plans to increase capacity through a CEA. Our analysis is predicated on the notion that such disclosures are credible signals regarding future expansion plans, and that when a firm discloses a capacity expansion, the firm follows through. Accordingly, our first prediction is:

P1: Capacity expansion disclosures are associated with increases in capacity.

An affirmative test of this prediction would jointly serve to validate our measure of CEAs and provide support for the notion that such disclosures are credible. We caveat that an affirmative test of P1 would not prove that deviations from an announced expansion plan are costly, but simply show that such disclosures are informative about future investment actions. Most importantly, an affirmative test of P1 would not speak to the *strategic* intents (or lack thereof) behind such disclosures. We look to Chinese imports as a setting in which we can identify firms' strategic intents by exploiting plausibly exogenous variation in the threat of entry. Our second prediction is:

P2: Firms respond to entry threats by announcing capacity expansions.

Not all firms are equally likely to respond in this fashion. For example, small firms would not be as able to materially alter an entrant's industry prospects and would therefore be less likely to attempt a capacity-based approach entry deterrence. Alternatively, CEAs could 'backfire' by signaling strong future demand, which could entice potential entrants—the opposite of the desired effect. This possibility is likely more salient for opaque firms, which may have more private/proprietary information about future industry prospects. Accordingly, we predict:

P2a: P2 is stronger for larger firms.
P2b: P2 is weaker for more opaque firms.

Lastly, we examine the relation between CEAs and subsequent entry, predicting:

P3: Capacity expansion disclosures are associated with reduced subsequent entry.

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[5]'Real effects' of disclosure refer to the causal effect of a disclosure on the disclosing party's behavior. In the context of our study, 'real effects' would be the causal effect of issuing a CEA on the disclosing firms' investment decisions. Absent a change in regulatory requirements, such an effect would be difficult to estimate given the endogenous nature of the choice to disclose investment plans.

For our last prediction, we caveat that the choice to issue a CEA is endogenous. We cannot observe the counterfactual level of entry, under a different disclosure policy, so we are unable to cleanly identify the causal effect of a CEA on entry. For example, if firms issue CEAs in response to omitted entry threats, our analysis will understate the true causal effect of the disclosure on subsequent entry. Conversely, if [as predicted] only the firms which stand to benefit the most decide to issue CEAs, then the observed effect would likely be an overestimate of the average causal effect of a CEA on entry.

3.3   Measure of capacity expansion announcements

This study utilizes a novel, text-based measure that reflects whether firms have announced that they will increase their production in the future. We use an algorithm based on regular expressions (Regex) to search for such announcements in press releases. Specifically, we classify a press release as containing a capacity expansion announcement if it contains the words increase or expand in combination with the words production or capacity and/or the words *open* or *build* in combination with the words *factory*, *facility*, *store* or *production* (including its plural forms) in a sentence. Examples of announcements identified by our algorithm include:

"During the third quarter, GrafTech purchased building and land in Northeast Ohio for $3 million, which will be used to expand Engineered Solutions' manufacturing capacity for our advanced consumer electronics."

"The multi-million-dollar shredder project, when completed, will expand processing capacity, offer specialty grades of scrap and improve end-product quality"

"We plan to expand the capacity of our GTN pipeline by at least 500 million cubic feet per day by the end of 2004. We expect the first phase of this expansion, 200 million cubic feet per day, to be completed by the end of 2002."

We provide more examples of such disclosures in our Appendix A.

We make several refinements to our algorithm described above to improve its quality. First, we ensure that the algorithm only classifies current and forward-looking announcements of production increases and not past increases. We therefore exclude capacity expansion announcements in sentences that contain regular verbs in the past tense (words ending with *-ed*) or the commonly used irregular verbs such as *was*, *had*

and *were*. The verbs in the announcements can also not be preceded by the words continue to. Second, we are careful not to include a CEA that is precluded by the negation term *not*. Third, we only identify CEAs made in the main text as opposed to the safe harbor statements where these word combinations may occur to limit legal liability. Fourth, we specifically exclude a few word combinations that were found in the press releases, but do not directly indicate an increase in production. The following word combinations are specifically excluded: *credit facility, loan production, production efficiency, increase in earnings, increase(d) price, production cost, capacity cost, builder, building* and *build on.*

To ensure that our measure picks up only voluntary disclosures, our algorithm searches for CEAs in press releases[6] that are released separately or in combination with an earnings announcement. Our algorithm has found 4,209 CEAs in 203,591 analyzed press releases (2.1%) as depicted in Table 2.1 Panel A. We further note that both the identified press releases and the CEAs are well spread over all industries and firms as depicted in panel B. The only exception is the Petroleum and Gas industry (21.6% of the number of CEAs), which is likely due to investors' demanding more frequent updates on capacity changes as these changes occur more frequently and directly affect firm profits. On average, firms' press releases contain CEAs between 0.5% and 8.9% of the time. Moreover, we do not find systematic differences in the types of press releases that include CEAs and those that do not. In our final sample, firm-years with no CEAs are coded as a zero, while firm-years with one or more CEAs are coded as a one.

We have opted to use a 'bag-of-words' approach to obtain CEAs, as opposed to machine learning, as it offers several advantages. First, our procedure is fully transparent and replicable with regards to what it classifies as a CEA and what does not. Second, our algorithm can also be seamlessly applied to other types of disclosures, such as annual reports and conference calls. Third, there is no reduction in our sample size as it does not require a training dataset. A limitation of this approach is that the functioning of the algorithm depends on the inclusions and exclusions of word combinations that we programmed. We note that the inputs for our algorithm are based on studying thousands of randomly selected press releases. All alterations, however, are subject to the tradeoff between type 1 and type 2 errors and we have chosen to prioritize limiting type 1 errors. That is, our approach is intended minimize the possibility that press releases get incorrectly coded as CEAs. It seems unlikely however that the design choices for our measure are in any way driving the results

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[6]Press releases are defined as any exhibit added to an 8-k filing that has press or news release in the title or an exhibit that starts with a city name, date or "for immediate release."

as the firm fixed effect resolve any differential sensitivity of firms. Any measurement error arising from the CEA classification algorithm will likely manifest as attenuation bias, biasing *against* finding results.

With respect to our research question, CEAs offer several specific advantages when compared to other types of voluntary disclosures (e.g., management earnings forecasts). First, CEAs could function as strategic commitment devices, whereby disclosing firms are obliged to complete their expansions or else be subject to costs such as the loss of reputational capital, or even shareholder lawsuits in extreme cases. By exposing themselves to such costs, firms can alleviate the constraints of sequential rationality, making credible otherwise untenable investment strategies. Second, CEAs can provide a direct and clear signal to the product market before the expansion is completed (or even begun). CEAs are therefore timely signals which allow potential entrants to obtain this information quickly, and without incurring high search costs; when using disclosures to deter entry, it is vital that the disclosed information reaches the potential entrants *before* they make their own (initial) investments and decision to enter the market. Another benefit of CEAs compared to other types of voluntary disclosures (e.g., issuance of additional management forecast and/or disclosing customer contracts) is that it does not require firms to alter their long-term disclosure policies. CEAs therefore do not create a commitment toward increased levels of disclosure in the future as the issuance of an (additional) management forecast for example would (see Graham, Harvey, and Rajgopal, 2005).

However, CEAs also carry potential costs. For example, if a CEA is [relatively] binding, preannouncing an investment could become inefficient if circumstances change such that the investment is no longer profitable, ex post. Furthermore, such disclosures may reveal private information about favorable industry prospects that the firm would prefer to keep secret (e.g., Wagenhofer, 1990; Darrough and Stoughton, 1990; Darrough, 1993; Graham et al., 2005).

3.4   Empirical setting

Finding the correct setting is essential for a study on entry deterrence as it is not possible to observe threats of entry within industries directly. We use the international trade setting to provide plausibly exogenous variation in entry threats. Specifically, we use the changes in exports from China to the Fama & French 48 industries in eight

developed countries[7] (not including the U.S.) as our industry-year measure of entry threats faced by US firms. We deflate the changes in exports by the sum of firms' sales in a Fama & French 48 industry to make the changes in exports comparable.

We focus on Chinese exports because prior literature shows that the increase in exports from China were driven primarily by exogenous increases in China's productivity (Autor et al., 2013). To further bolster the claim of plausible exogeneity, we use Chinese exports *to other developed countries* (not the US) to capture entry threats facing US firms. Chinese exports to other developed countries are strongly predictive future of Chinese exports to the US, even after controlling for contemporaneous exports to the US. Accordingly, we expect that an uptick in Chinese exports to other developed countries would be threatening to economically similar domestic firms, who would worry about imminent potential exports to the US.

Our setting and measure allows us to circumvent the problems associated using regulatory changes (for example: import tariffs) as instruments. Such regulatory changes are not exogenously determined, and are often driven in large part by demand-side factors. In contrast, our measure of entry threat is not dependent on the composition and importance of the domestic incumbents nor by supply/demand in the United States.

Our identification strategy is very similar to those of Autor et al. (2013, 2014); Autor, Dorn, Katz, Patterson, and Van Reenen (2017); Hombert and Matray (2018). We differ in that these prior studies use Chinese exports to developed countries (excluding the US) as an instrument for contemporaneous Chinese exports to the US (i.e., *current* competition from China). In contrast, we use Chinese exports to developed countries (excluding the US) as a measure of *potential future* competition from China. By controlling for contemporaneous Chinese exports to the US, we can rule out the possibility that CEAs are issued in response to *current* competition from China, but instead in response to *potential*, but not yet realized competition (i.e., the level of entry threat).

3.5   Sample and variable definition

Our main sample includes all U.S. firm years in the period 1995-2016. We exclude firm-year observations when there has not been a press releases issued in the current year

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[7]We use the Chinese exports to Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland as our proxy for threats of entry, which is consistent with the countries used by Autor et al. (2013).

or the past to reduce the likelihood of type 2 errors affecting our results.[8] We further narrow our sample by only including firms that can potentially face competition of Chinese goods. We therefore exclude firms operating in pure service industries[9], utilities and banks. An overview of the effects of these choices on our sample size can be found in Table 3.1 (Panel A).

We combine our CEA dataset with Compustat for firm and industry fundamentals and with CRSP for our measures of opacity/private information. Our measure of entry threats, THREAT, relies on Chinese export data that is obtained from the UN Comtrade database. This database contains the total value of all exports between countries on a 6-digit product level. We therefore use the classification by Schott (2008) to convert exports to 4 digit SIC industries. In the case that one product code corresponds to multiple SIC codes, we allocate the exports per product code proportionately based on the U.S. imports two year earlier .[10] As described more in section 3.4, our measure of threats of entry is based on the changes in exports from China to the Fama & French 48 industries in eight developed countries. We therefore aggregate all Chinese exports to Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland by SIC code and then calculate the year-to-year difference. As this year-to-year difference is likely strongly related to the size of the industry, we deflate it by the total sales of all firms in the corresponding industry. This makes our estimates comparable across industries, but does have the disadvantage that it leads to relatively small data points because the year-to-year differences are deflated by very large numbers.

In some tests we also explicitly control for changes in the imports from China to the U.S. with the variable PRESENCE. This variable is constructed in a similar way as threats of entry, but instead uses the year-to-year changes in exports from China to the U.S. instead of exports to other developed countries. Similarly, in some of our analyses we examine ex post entry, using the variable ENTRY. This variable is similar in construction to PRESENCE, but reflects one-year-ahead changes in exports from China to the U.S. In some of our robustness tests, we also control specifically for the

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[8]Our algorithm is not always able to detect press releases correctly due to for example firms not uploading their press releases to EDGAR or doing so in a format that is unreadable by our scraper. If our scraper has not identified a press release for a firm before, we cannot be certain whether firms made a CEA or not. We therefore exclude these firm years as we may mistakenly assign a 0 for our CEA measure when managers did in fact make a CEA.

[9]We exclude Fama and French industry codes: 31 (Utilities), 33 (Personal Services), 42 (Retail), 43 (Restaurant, hotel), 44 (Banking), 45 (Insurance), 46 (Real Estate). In additional robustness checks we include all 48 industries and find that our results are not contingent on this design choice.

[10]Taking the proportion of U.S. import two years earlier reduces the likelihood that a U.S. based demand shocks drive our results in the export setting while at the same time ensuring the classification is relevant.

exports from the U.S. to the developed countries with the variable US_EXPORTS. Like THREAT, this variable is defined as the year-to-year difference in exports to eight developed countries from respectively the United States. A definition of all variables can be found in Appendix B. Descriptive statistics for all variables used in the analysis can be found in Table 3.2.

3.6   Empirical results

3.6.1   Relation Between CEAs and Capacity Expansions

The first step of our empirical analysis is to document, descriptively, whether CEAs associate with actual capacity expansions. The purpose of these analyses is two-fold: (1) provide confirmatory evidence to bolster the construct validity of our CEA measure, by showing that CEAs can explain actual capacity expansions; and (2) provide evidence to suggest that CEAs are credible. If CEAs were unassociated with firms' real capacity expansions, it would suggest that such disclosures are cheap talk, which would likely diminish their usefulness in deterring entry. We test for the association between CEAs and capacity expansions using variants on the following regressions specification:

$$\Delta CAPACITY_{i,t} = \beta_1 CEA_{i,t} + \mu_i + \theta_{j,t} + \tau_t + \epsilon_{i,j,t} \tag{3.1}$$

Our two primary measures for the dependent variable: year-over-year percentage increase in CAPEX, and year-over-year percentage increase in PP&E. We further include three additional measures: year-over-year percentage increases in Sales, COGS, and Inventories. We use these last three as supplemental measures, rather than primary measures, because they are equilibrium outcomes over which the firm has less direct control; investments in PP&E and CAPEX are explicit choices the firm makes. For each outcome variable, we present two specifications, which differ only with respect to fixed effect structure. The two different structures are: (1) firms fixed effects; and (2) firm and industry-year fixed effects, jointly. We present results in Table 3.3.

We find that CEAs are strongly associated with real capacity expansion behavior. Holding the firm and industry-year fixed, a CEA is associated with a 15% (4%) increase in CAPEX (PP&E), and a 3% (3%) [6%] increase in sales (COGS) [inventories]. These effects are economically significant, but not so large as to be implausible. We note that these results, in and of themselves, do *not* indicate that firms are choosing to make CEAs *strategically*, nor that the CEAs have any causal effect on capacity expansions.

Rather, these results validate our measure of CEAs, and provide evidence to suggest that such disclosures are credible signals of firms' capacity expansion plans.

We find comparable (untabulated) results when we Winsorize our dependent variables at the 1% level instead of the 5% level currently used, control for growth trends by including lagged values of the dependent variables, and when we loosen restrictions on the sample to include announcements made in non-press release 8K filings.

3.6.2   CEAs and Entry Threats

In the next set of tests, we examine whether firms make these announcements, in part, as a strategic response to entry threats. In order to assess the causal effect of entry threats on CEAs we look to Chinese exports as a source of plausibly exogenous variation in the threat of entry. Prior literature finds that increases in Chinese exports are largely driven by supply-side productivity increases. While trade to the US is endogenous to US demand, we use Chinese exports to the rest of the developed world as a proxy for the threat of entry into the US (see Autor et al., 2013).

We test for the effect of entry threats on CEAs using variants on the following regression specification:

$$
\begin{aligned}
CEA_{i,t} = &\beta_1\,THREAT_{j,t} + \beta_2\,PRESENCE_{j,t} + \beta_3\Delta CAPACITY_{i,t} \\
&+ \beta_4\Delta CAPACITY_{i,t+1} + \mu_i + \tau_t + \epsilon_{i,j,t}
\end{aligned} \tag{3.2}
$$

The coefficient of interest is the coefficient on THREAT, which reflects the relation between Chinese imports to the *rest of the developed world* on domestic firms' CEAs. In specifications two and five, we include PRESENCE as a control variable, to hold fixed the current intensity of Chinese imports to a firm's industry. In specifications three through five, we also include controls for actual changes in capacity, in the current and subsequent year, to show that the importance of a CEA is not subsumed by a firm's real actions. In all specifications, we include firm and year fixed effects. Results from these regressions can be found in Table 3.4.

We find that Chinese exports to the rest of the developed world are significantly associated with CEAs. The statistical and economic significance of the effect remains stable if we control for actual changes in capacity, as captured by contemporaneous and one-year-ahead changes in PP&E and CAPEX. This suggests that the *disclosure* of the capacity expansion is an important component of a firm's entry deterrence strategy. That is, firms do not appear to respond to entry threats by expanding

capacity, and then non-strategically disclose the decision. Rather, for a given capacity expansion, a firm is much more likely to issue a CEA if they face a credible entry threat.

These patterns continue to hold if we further control for the current level of imports into an industry (PRESENCE). When we control for PRESENCE, the magnitude of the coefficient on THREAT grows larger by about 16%. However, the statistical significance falls somewhat, likely driven by the high degree of correlation between THREAT and PRESENCE variables (correlation > 0.8). When we control for both PRESENCE and the underlying investment behavior, the coefficient on CEA falls just below the 10% significance threshold (t-stat equals 1.453). However, we note that the sample size is significantly reduced in this specification, and reiterate that the coefficient is actually larger than without the control for PRESENCE (0.94 versus 0.81). Moreover, we do not expect all firms engage in this type of strategic behavior; our results become more robust in the next set of tests, which exploit cross-sectional variation in the viability of this deterrence strategy.

Notably, we find no evidence that CEAs are associated with current imports to the US (PRESENCE)—we only find that CEAs are associated with imports to the rest of the developed world. This makes it even more unlikely that our results can be driven by demand shocks in the US; if demand shocks caused both the CEAs and the increases in Chinese exports, then we would expect to see a strong association between PRESENCE and CEAs. We interpret our results as evidence that firms use CEAs, in part, as a strategic entry deterrence tool. Any alternative explanation for our results must explain why US firms' CEAs are associated with Chinese exports to non-US developed countries, but are *not* associated with Chinese exports to the US.

In the last two specifications, we limit our sample to include only those firm years with large increases in capacity (i.e. firm-year observations with at least 5% increases in PPE and/or 10% increases in CAPEX compared to the previous year). This approach allows us to focus on the disclosure behavior of firms *conditional* on have made large investments and reduces the impact of potential cheap talk. It also further strengthens the power of the firm fixed effects as it now averages out firms' normal disclosure behavior during large investment periods. These tests also provide further evidence that CEAs are not mandatory and therefore mechanical, as there is a lot of variation in the dependent variable, even within the subsample of expanding firms.

In line with our previous results, we find that CEAs are correlated with threats of entry, even when we condition the sample on firms that are making substantial

investments. The coefficients on THREAT are in fact higher than our previous estimates (1.50 compared to 0.94 in our most stringent specification).

3.6.3   Threat Response Variation in the Cross-Section

In the preceding analyses, presented in Table 3.4, we find evidence that, on average, firms respond to the threat of Chinese entry in their product markets by issuing CEAs. However, we do not expect all firms would be equally likely to respond in this fashion. For example, small firms' capacity expansions would not have a significant enough effect on an industry to deter entry. Therefore, such firms would be unlikely to respond to the threat of entry by releasing a CEA. Conversely, for more opaque firms with substantial private information, a CEA could backfire by signaling excellent industry prospects, which could *encourage* entry. Therefore, such firms would be less likely to make such a disclosure. Accordingly, we examine whether cross-sectional variation in these characteristics can explain variation in responses to entry threats in a predictable manner.

For our first cross-sectional split, we look at how entry threat responses vary with firm size. Larger firms are better able to deter entry through their capacity investments, and would therefore be much more likely to use strategic disclosures to ensure potential entrants are aware of their ongoing investments. Smaller firms typically do not control enough of the market for an capacity-based entry deterrence strategy to be viable, and we would therefore not expect to observe such firms responding to entry threats with CEAs. Accordingly, we examine whether larger firms (as captured by average total assets, and revenues) are differentially likely to respond to entry threats by making a CEA, testing for the interactive effect of THREAT and SIZE using variants on the following regression specification:

$$CEA_{i,t} = \beta_1\,THREAT_{j,t} \times SIZE_{i,t} + \beta_2\,THREAT_{j,t} + \beta_3 SIZE_{i,t} + \beta_4 PRESENCE_{j,t}$$
$$+ \beta_5 \Delta CAPACITY_{i,t} + \beta_6 \Delta CAPACITY_{i,t+1} + \mu_i + \tau_t + \epsilon_{i,j,t}$$

$$(3.3)$$

Our regression specification differs slightly from the specification used in our previous analyses, since we are interested specifically in the interactive effect of SIZE with THREAT and therefore requires additional controls for confounding effects. Expansions initiated by larger firms could be of different relative size, thereby also triggering CEAs at different levels of investments. For example, a large firm might issue a CEA for an abnormal 1 or 2% increase in capacity, while such an increase may be barely noteworthy for a smaller firm in our sample. Furthermore, firms may make

(potentially differently sized) investments when faced with more threats of entry that may trigger CEAs at higher rates. We therefore also include the interaction of both THREAT and SIZE with actual changes in capacity in our regression specification.

Results from these regressions can be found in Table 3.5. In Panel A (Panel B), we proxy for SIZE using GAAP revenues (average total assets). Consistent with our predictions, we find significant variation in how firms of different sizes respond to entry threats. The relation between entry threats and CEAs is much stronger for larger firms. As before, this result is robust to including controls for actual increases in capacity, as well as a control for current Chinese imports. In untabulated analyses, we examine whether firms of different sizes respond differentially to current Chinese imports (PRESENCE), and find no evidence that they do. The fact that these patterns are specific to Chinese exports to non-US countries, and absent for Chinese exports to the US, bolsters the notion that we are capturing firms' strategic attempts at entry deterrence. Economic forces other than entry deterrence would mostly likely make firms more responsive to current conditions, at home, rather than current conditions elsewhere—in stark contrast to our findings.

One potential drawback of using a CEA to deter entry is that it could reveal private information about future demand. If an incumbent firm has a lot of private information about the future evolution of demand, then a CEA could easily (and perhaps correctly) be interpreted as a strong positive signal about future industry prospects. Such a signal would reduce the net benefits of the CEA, and in some cases could even make a CEA entry-*encouraging*. Thus, firms with ample private information about industry prospects would likely be much more judicious about using CEAs as part of an entry deterrence strategy. Such firms would be more likely to engage in profit hiding, or pessimistic disclosures to deter entry (e.g., Li, 2010; Tomy, 2017; Burks et al., 2018). Accordingly, we examine whether firms' private information moderates the relation between entry threats and CEAs. We test for the interactive effect of THREAT and INFO using variants on the following regression specification:

$$CEA_{i,t} = \beta_1 \, THREAT_{j,t} \times INFO_i + \beta_2 \, THREAT_{j,t} + \beta_3 PRESENCE_{j,t} \\ + \beta_4 \Delta CAPACITY_{i,t} + \beta_5 \Delta CAPACITY_{i,t+1} + \mu_i + \tau_t + \epsilon_{i,j,t}$$ (3.4)

It is difficult to identify exactly how much private information a firm has about future industry prospects. In order to parse this variation, we rely on information asymmetry variables typically used in the accounting and finance literatures: bid-ask spreads and trading volume (both in shares and dollars). With this in mind, we note two caveats: these measures reflect information asymmetry *among investors*,

and do not necessarily correspond to how much private information firm insiders have; and (2) these measures do not allow us to pinpoint the source of information asymmetry, and it need not necessarily stem from private information regarding future industry prospects. Therefore, our cross-sectional splitting variables used in Table 3.6, include measurement error. As long as this measurement error is uncorrelated with omitted drivers of entry threat reaction, it will bias away from finding any significant results, due to attenuation. Importantly, our measures of private information are at the firm-level (not firm-year), so the main effects of any measurement error is mechanically suppressed by the fixed effect structure. Our analyses identify only the *interactive* effects of entry threats and private information. Thus, in order to leads to spurious inferences, measurement error in our proxies for private information must interact with entry threats in such a manner as to explain CEAs. It is not obvious how such a confound might arise.

3.6.4   Robustness: Controlling for US exports

Our identification strategy is designed to shield our analyses from the potential confounding effects of US-side demand. The idea is the following: Chinese exports to the US (the variable PRESENCE) subsumes the information content that Chinese exports to the rest of the developed world (the variance THREAT) contains about US demand. At the very least, we must only assume that Chinese exports to the US are a better measure of US demand than are Chinese exports to the rest of the developed world. We find that Chinese exports to the US are not associated with US firms' CEAs, but Chinese exports to the rest of the developed world are associated with CEAs. Therefore, it is unlikely that our results can be driven by US demand.

However, our identification strategy is not as effective at guarding against the potential confounding effects of supply shocks. Suppose China and the US both experienced positive supply shocks, making it cheaper for them to produce. China and the US would both respond to the shock, by exporting more to the developed world, and building up capacity. In the context of our analysis, this could lead capacity expansions to be correlated with Chinese exports to the developed world, for entirely nonstrategic reasons.

First and foremost, we note that our study is about capacity expansion *announcements*, and not capacity expansion *investments*. We control for the underlying capacity expansion investments, and find that THREAT is incrementally informative about the choice to *disclose* the expansion. While the supply shock story provides a non-strategic explanation for a correlation between THREAT and capacity expansion investments,

it would not explain why firms would be differentially likely to publicly disclose their expansions, when facing greater entry threats. That said, we attempt to further mitigate this concern by incorporating US exports as a control variable, to better capture supply-side confounds. US_EXPORTS is constructed in a similar way as our main dependent variable THREAT, but then uses the year-to-year difference in exports from the US to developed countries in each Fama & French industry deflated by total sales in that industry.

We find that our inferences are not affected by this alteration. Even after controlling for US_EXPORTS, firms are more likely to make CEAs when facing greater entry threats. Moreover, this result is more prevalent among larger firms, and less prevalent among firms with more private information about industry prospects. We present these results in Table 3.7.

3.6.5   Robustness: Overall disclosure quantity

Thus far, we have not specifically looked at the overall disclosure environment of a firm and relied on firm fixed effects to investigate whether firms provide more CEAs when faced with threats of entry. However, concurrent changes in the overall disclosure environment could potentially affect our results. For example, if entry threats represent a source of uncertainty about which investors are uneasy, firms may respond to these entry threats by becoming more transparent overall, simply to allay investors' fears. As CEAs are a dimension of firm transparency, such behavior might give rise to our findings, but this would not be a matter of strategic entry deterrence, but rather a response to capital market pressure.

To assess this possible alternative explanation for our results, we conduct two additional sets of tests. First, we replicate our analyses with a control for overall disclosure quantity (Table 3.8 Panel A). Second, we re-run our analyses with overall disclosure quantity as the outcome variable (Table 3.8 Panel B). We find that controlling for overall disclosure does not adversely affect our results; higher levels of entry threats are associated with a greater likelihood of a CEA. Moreover, we find no evidence that overall disclosure increases in response to entry threats.

We tabulate results using the total number of words in firms' press releases as a firm-year level of disclosure quantity. Our inferences remain unchanged if we use the number of 8-k's, or the number of earnings forecasts, instead.

3.6.6   Supplemental analysis: Effectiveness of Capacity Expansion Announcements at Deterring Entry

As the final step of our analysis, we examine how successful firms' CEAs appear to be at deterring entry. We test for the association between CEAs and subsequent entry using variants on the following regressions specification:

$$\begin{aligned} ENTRY_{i,t} = {} & \beta_1\, CEA_{i,t} + \beta_2\, THREAT_{j,t} + \beta_3\, PRESENCE_{j,t} \\ & + \beta_4 \Delta CAPACITY_{i,t} + \beta_5 \Delta CAPACITY_{i,t+1} + \mu_i + \tau_t + \epsilon_{i,j,t} \end{aligned} \quad (3.5)$$

Results from these specifications can be found in Table 3.9. The coefficient of interest is on CEA, and reflects the extent to which CEAs are associated with a subsequent change in Chinese imports.

We find that issuing a CEA is associated with about a 1.2 basis point reduction in subsequent entry, which is about 13% of the average annual base-rate of entry. This association arises even when controlling for actual capacity expansion actions, indicating that the disclosure itself seems to be important in deterring entry. Furthermore, we find a significant positive coefficient on THREAT, which corroborates the notion that Chinese trade to the rest of the developed world constitutes a heightened threat of entry into the US.

We caveat that CEAs are endogenous firm choices. Thus, the regressions described by specification (3.5) do not necessarily reflect the causal effect of CEAs on subsequent entry. As noted by Burks et al. (2018), exogenous variation in entry threats is insufficient to identify the causal effect of an entry deterrence strategy. To identify the causal effect of an entry deterrence strategy on subsequent entry requires exogenous variation in the strategy itself. Such variation cannot be obtained in our setting. Given the endogeneity of the choice to issue a CEA, our estimates are likely to be underestimates of their true effectiveness, as we discuss below.

If firms disproportionately issue CEAs in response to the most severe entry threats, then our analysis is likely to understate their true effectiveness. Such a confound would bias the estimated coefficient downward, creating the false impression that CEAs are less effective than they truly are at deterring entry. Moreover, if CEAs are effective as entry deterrents, then they yield spillover benefits to other firms in the same industry—those who free-ride on their peers' successful entry deterrence strategies. Such a spillover will result in further understatement of the causal effect of a CEA (Armstrong and Kepler, 2018).

In addition to the above concern, we further caveat that our analysis identifies the effectiveness of CEAs *for those firms who choose to use them*. If, as predicted, there is

heterogeneity in the effectiveness of a CEA (e.g., due to different firm sizes or different amounts of private information), then our analysis will fail to reflect the average effect of a CEA on subsequent entry. That is, even if we accurately identify how effective the observed CEAs were (i.e., their unbiased causal effect on entry), it would not be appropriate to infer that other firms—those that chose not to issue CEAs—would have enjoyed the same level of entry deterrence success, had they chosen to issue them. It is perhaps precisely because their CEAs would not have been as effective that they chose not to issue them in the first place. This caveat does not imply that our estimates are overstated (as mentioned before, they are likely *understated* due to omitted sources of entry threats), but rather suggests that one should interpret them cautiously as the estimated effectiveness for the firms who chose to use them.

3.7   Conclusion

We construct and validate a novel measure of firms' voluntary disclosures, that captures explicit forward-looking statements about capacity expansion plans. Our measure associates with substantial real changes in firm investment (e.g., CAPEX and PP&E), suggesting that it does indeed reflect firms' actual capacity expansion plans.

Using plausibly exogenous variation in entry threats, we further provide evidence that firms *strategically* preannounce capacity expansions in order to deter entry into their product markets. Consistent with our predictions, larger firms are more likely to respond in this fashion, while more opaque firms—that that plausibly have more private information—are less likely to respond in the fashion. Finally, CEAs appear to be effective at deterring entry.

More broadly, our study provides novel evidence that firms make accounting choices *strategically*, with the aim of conferring competitive advantages to themselves in their product markets. CEAs could provide strategic value by functioning as strategic commitment devices, and/or by accelerating and improving the dissemination of relevant information to potential entrants. Our analysis does not allow us to distinguish between these two channels. Further research is needed to assess the exact mechanism through which CEAs discourage through which CEAs discourage entry.

We cannot definitively rule out the possibility that [for some firms] CEAs are cheap talk. While we view CEAs as fairly credible, our inferences would remain valid even if CEAs were cheap talk, so long as firms are using them, strategically, in response to

entry threats. We welcome future work to identify the extent to which firms follow through on (or back out of) their disclosed expansion plans.

Table 3.1: Sample selection and number of identified press releases and CEAs by industry

Panel A shows the impact of our sample selection on our final sample and provides insight into the issuances of press releases and CEAs on both 8k-filing and firm level. Panel B outlines the number of press releases and CEAs per industry.

Panel A	
By filings: Total number of 8-K	657,228
Total number of press releases	290,560
Total number of 8-K with GVKEY link	513,940
Total number of 8-K with GVKEY link excl. banking, utilities and non-importing industries	384,293
Total number of press releases excl. banking, utilities and non-importing industries	215,754
Total number of CEAs (excl. banking, utilities and non-importing industries)	4,388
By firms:	
Total number of firms with GVKEY link	15,292
Total number of firms with GVKEY link excl banking, utilities and non-importing industries	11,204
Total number of firms with press releases excl. banking, utilities and non-importing industries	9,357
Total number of firms with at least one CEA (excl. banking, utilities and non-importing industries)	1,809

Panel B

Fama & French Industry	Nr of CEAs	% of total CEAs	Nr of press releases	% of press releases with CEA
Agriculture	38	0.9%	766	5.0%
Aircraft	26	0.6%	986	2.6%
Almost Nothing	62	1.5%	1542	4.0%
Apparel	97	2.3%	2296	4.2%
Automobiles and Trucks	151	3.6%	3591	4.2%
Beer & Liquor	14	0.3%	574	2.4%
Business Services	201	4.8%	28593	0.7%
Business Supplies	79	1.9%	2357	3.4%
Candy & Soda	6	0.1%	645	0.9%
Chemicals	219	5.2%	5210	4.2%
Coal	74	1.8%	941	7.9%
Communication	84	2.0%	8547	1.0%
Computers	36	0.9%	7507	0.5%
Construction	32	0.8%	2803	1.1%
Construction Materials	134	3.2%	3285	4.1%
Consumer Goods	77	1.8%	2701	2.9%
Defense	5	0.1%	559	0.9%
Electrical Equipment	79	1.9%	3676	2.1%
Electronic Equipment	232	5.5%	13094	1.8%
Entertainment	65	1.5%	3708	1.8%
Fabricated Products	16	0.4%	438	3.7%
Food Products	98	2.3%	3359	2.9%
Healthcare	55	1.3%	4915	1.1%
Machinery	174	4.1%	6,326	2.8%
Measuring and Control Equip.	56	1.3%	4,052	1.4%
Medical Equipment	83	2.0%	8,084	1.0%
Non-Metallic and Industrial Metal Mining	118	2.8%	1,328	8.9%
Petroleum and Natural Gas	910	21.6%	13,629	6.7%
Pharmaceutical Products	178	4.2%	22,888	0.8%
Precious Metals	82	1.9%	1,197	6.9%
Printing and Publishing	22	0.5%	1,659	1.3%
Recreation	17	0.4%	1,421	1.2%
Rubber and Plastic Products	40	1.0%	1,519	2.6%
Shipbuilding, Railroad Equip.	23	0.5%	490	4.7%
Shipping Containers	11	0.3%	604	1.8%
Steel Works Etc	176	4.2%	3,250	5.4%
Textiles	36	0.9%	618	5.8%
Tobacco Products	3	0.1%	310	1.0%
Trading	125	3.0%	18,872	0.7%
Transportation	140	3.3%	7,317	1.9%
Wholesale	135	3.2%	7,934	1.7%
Total	4,209	100%	203,591	
Average				2.1%

Table 3.2: Descriptive statistics

Table 3.2 shows the distribution of the variables used in the analyses. Variables are as defined in Appendix B.

Variable	N	Mean	σ	p25	Median	p75
CEA_{fy}	73,163	0.045	0.208	0.000	0.000	0.000
CEA_{cy}	73,529	0.045	0.208	0.000	0.000	0.000
COGS_increase_t	68,965	0.143	0.414	-0.060	0.071	0.245
SALES_increase_t	68,994	0.144	0.399	-0.057	0.071	0.244
PPE_increase_t	66,486	0.142	0.315	0.002	0.066	0.189
CAPEX_increase_t	65,617	0.346	1.147	-0.344	0.040	0.567
$\text{INVENTORY\_increase}_t$	48,069	0.111	0.477	-0.129	0.040	0.237
THREAT	73,737	0.001	0.003	0.000	0.000	0.001
PRESENCE	73,737	0.001	0.004	0.000	0.000	0.001
ENTRY	73,301	0.001	0.004	0.000	0.000	0.001
US_EXPORTS	73,737	0.000	0.002	0.000	0.000	0.000
log(AVGAT)	72,412	5.303	2.664	3.627	5.481	7.156
log(REVT)	70,036	5.019	2.741	3.460	5.312	6.879
$\text{PPE\_increase}_{t+1}$	59,028	0.123	0.290	0.000	0.061	0.169
$\text{CAPEX\_increase}_{t+1}$	58,265	0.314	1.113	-0.351	0.028	0.526
log(SPREAD)	53,488	-4.591	1.212	-5.320	-4.562	-3.724
log(TRADEVOL)	53,593	12.249	1.713	11.137	12.324	13.365
log(TOTAL WORDS)	59183	7.375	1.052	6.688	7.520	8.148

Table 3.3: Relation between CEAs and firm investments/scale

This table presents the results of a regression of several firm investment and scale variables on CEAs made during the fiscal year. The sample consists of all Compustat firm years in non-service industries that have issued at least one press release in the past in the period 1995-2016. CEA_{fy} is an indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in that particular fiscal year. The definition of all other variables can be found in Appendix B. A firm year is included in the sample only when it has non-missing values for all variables used in this table. All regressions are clustered by Gvkey. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

VARIABLES	(1) CAPEX_increase$_t$	(2)	(3) PPE_increase$_t$	(4)	(5) Sales_increase$_t$	(6)	(7) COGS_increase$_t$	(8)	(9) Inventory_increase$_t$	(10)
CEA_fy	0.189***	0.151***	0.042***	0.043***	0.049***	0.033***	0.050***	0.035***	0.074***	0.059***
	(7.475)	(5.966)	(6.545)	(6.963)	(6.100)	(4.432)	(5.750)	(4.247)	(6.552)	(5.346)
Observations	45,013	45,002	45,013	45,002	45,013	45,002	45,013	45,002	45,013	45,002
Adjusted R-squared	0.009	0.046	0.183	0.236	0.130	0.223	0.103	0.179	0.061	0.106
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table 3.4: CEAs and threats of entry

This table presents the results of a OLS regression of CEAs on a proxy for threats of entry. The sample consists of all Compustat firm years in non-service industries that have issued at least one press release in the past in the period 1995-2016. CEA_{cy} is an indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in that particular calendar year. The definition of all other variables can be found in Appendix B. All regressions are clustered by Gvkey and Industry-Year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
				CEA_{cy}		
THREAT	0.864**	1.015*	0.800**	0.939	0.905**	1.501*
	(2.512)	(1.739)	(2.042)	(1.441)	(2.184)	(1.779)
PPE_increase$_t$			0.022***	0.022***		0.023***
			(5.278)	(5.282)		(4.626)
CAPEX_increase$_t$			0.003***	0.003***		0.003***
			(2.703)	(2.701)		(2.646)
PPE_increase$_{t+1}$			0.013***	0.013***		0.014**
			(3.102)	(3.104)		(2.416)
CAPEX_increase$_{t+1}$			-0.002**	-0.002**		-0.004***
			(-2.064)	(-2.065)		(-2.943)
PRESENCE		-0.185		-0.174		-0.602
		(-0.377)		(-0.329)		(-0.868)
Observations	72,027	72,027	53,949	53,949	51,655	37,616
Adjusted R-squared	0.155	0.155	0.165	0.165	0.166	0.175
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	Expansion	Expansion

Table 3.5: Moderating effect of size

This table presents the results of a regression of CEAs on firm size. The sample consists of all Compustat firm years in non-service industries that have issued at least one press release in the past in the period 1995-2016. CEA_{cy} is an indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in that particular calendar year. We use two different measures of firm size. In Panel A (Panel B), we measure firm size using the natural logarithm of annual revenues (average total assets). The definition of all other variables can be found in Appendix B. All regressions are clustered by Gvkey and Industry-Year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			CEA_{cy}			
log(REVT) × THREAT	0.380***	0.379***	0.412**	0.411**	0.429***	0.512**
	(2.845)	(2.857)	(2.416)	(2.428)	(2.672)	(2.405)
THREAT	-1.088*	-1.022	-1.580*	-1.510	-1.274*	-1.768
	(-1.715)	(-1.360)	(-1.792)	(-1.600)	(-1.663)	(-1.501)
log(REVT)	0.007***	0.007***	0.006***	0.006***	0.007***	0.006***
	(6.048)	(6.050)	(3.939)	(3.939)	(4.908)	(2.870)
PPE_increase$_t$			0.013*	0.013*		0.019**
			(1.712)	(1.714)		(2.162)
CAPEX_increase$_t$			-0.003	-0.003		-0.002
			(-1.602)	(-1.602)		(-0.845)
PPE_increase$_{t+1}$			0.008	0.008		0.009
			(1.134)	(1.134)		(0.964)
CAPEX_increase$_{t+1}$			-0.001	-0.001		-0.004
			(-0.574)	(-0.575)		(-1.321)
PRESENCE		-0.074		-0.080		-0.542
		(-0.154)		(-0.153)		(-0.780)
Observations	68,397	68,397	52,804	52,804	48,789	36,856
Adjusted R-squared	0.157	0.157	0.169	0.169	0.168	0.177
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Interacted controls	No	No	Yes	Yes	No	Yes
Sample	Full	Full	Full	Full	Expansion	Expansion

Panel B

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
				CEA_{cy}		
log(AVGAT) × THREAT	0.372***	0.369***	0.388**	0.387**	0.408**	0.473**
	(2.609)	(2.614)	(2.152)	(2.165)	(2.324)	(2.122)
THREAT	-1.076	-0.922	-1.513	-1.456	-1.177	-1.554
	(-1.557)	(-1.171)	(-1.597)	(-1.463)	(-1.383)	(-1.270)
log(AVGAT)	0.007***	0.007***	0.009***	0.009***	0.007***	0.007***
	(6.269)	(6.272)	(4.241)	(4.241)	(4.964)	(2.724)
PPE_increase$_t$			0.002	0.002		0.007
			(0.231)	(0.232)		(0.733)
CAPEX_increase$_t$			-	-		-0.005**
			0.006***	0.006***		
			(-3.155)	(-3.155)		(-2.277)
PPE_increase$_{t+1}$			-0.004	-0.004		-0.003
			(-0.429)	(-0.428)		(-0.226)
CAPEX_increase$_{t+1}$			-0.001	-0.001		-0.004
			(-0.713)	(-0.713)		(-1.161)
PRESENCE		-0.171		-0.065		-0.539
		(-0.359)		(-0.125)		(-0.776)
Observations	70,670	70,670	53,949	53,949	50,307	37,616
Adjusted R-squared	0.156	0.156	0.167	0.167	0.168	0.176
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Interacted controls	No	No	Yes	Yes	No	Yes
Sample	Full	Full	Full	Full	Expansion	Expansion

Table 3.6: Moderating effect of Information Asymmetry

This table presents the results of a regression of CEAs made during the calendar year on information asymmetry variables. The sample consists of all Compustat firm years in non-service industries that have issued at least one press release in the past in the period 1995-2016. CEA_{cy} is an indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in that particular calendar year. We use two different measures of information asymmetry. In Panel A (Panel B), we measure information asymmetry using the natural logarithm of the average bid-ask spread (the average daily trading volume). The definition of all other variables can be found in Appendix B. All regressions are clustered by Gvkey and Industry-Year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Panel A						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			CEA_{cy}			
log(SPREAD) × THREAT	-0.629**	-0.628**	-0.600*	-0.601*	-0.723**	-0.721*
	(-2.167)	(-2.165)	(-1.896)	(-1.896)	(-1.964)	(-1.771)
THREAT	-1.947	-1.791	-1.957	-1.915	-2.481	-2.335
	(-1.537)	(-1.338)	(-1.397)	(-1.317)	(-1.513)	(-1.246)
PPE_increase$_t$			0.022	0.022		0.024
			(1.325)	(1.326)		(1.265)
CAPEX_increase$_t$			-0.003	-0.003		0.001
			(-0.682)	(-0.682)		(0.240)
PPE_increase$_{t+1}$			-0.013	-0.013		-0.027
			(-0.654)	(-0.654)		(-1.101)
CAPEX_increase$_{t+1}$			0.006	0.006		0.007
			(1.502)	(1.501)		(1.121)
PRESENCE		-0.191		-0.054		-0.261
		(-0.360)		(-0.098)		(-0.362)
Observations	52,537	52,537	41,766	41,766	38,181	29,591
Adjusted R-squared	0.159	0.159	0.167	0.167	0.171	0.179
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Interacted controls	No	No	Yes	Yes	No	Yes
Sample	Full	Full	Full	Full	Expansion	Expansion

Panel B

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
			CEA_{cy}			
log(TRADEVOL)× THREAT	0.388**	0.392**	0.372*	0.373*	0.525**	0.572**
	(1.997)	(2.014)	(1.796)	(1.797)	(2.001)	(2.014)
THREAT	-3.856*	-3.714	-3.790	-3.745	-5.623*	-6.027*
	(-1.652)	(-1.569)	(-1.508)	(-1.480)	(-1.785)	(-1.758)
PPE_increase$_t$			0.021	0.021		0.047
			(0.597)	(0.599)		(1.102)
CAPEX_increase$_t$			-0.014	-0.014		-
						0.020**
			(-1.550)	(-1.549)		(-1.979)
PPE_increase$_{t+1}$			-0.024	-0.024		-0.041
			(-0.561)	(-0.561)		(-0.762)
CAPEX_increase$_{t+1}$			0.001	0.001		0.001
			(0.207)	(0.204)		(0.049)
PRESENCE		-0.237		-0.084		-0.329
		(-0.449)		(-0.153)		(-0.456)
Observations	52,642	52,642	41,839	41,839	38,260	29,650
Adjusted R-squared	0.159	0.159	0.167	0.167	0.171	0.179
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Interacted controls	No	No	Yes	Yes	No	Yes
Sample	Full	Full	Full	Full	Expansion	Expansion

Table 3.7: Robustness to common economic shocks

This table presents the results of a regression of CEAs on a proxy for threats of entry (Column 1-5) and Entry on CEA's (Column 6). All columns correspond to the first specification of each table (3.4-3.6) with the addition of US exports as a control variable. Please refer for an elaborate description to the corresponding table. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

VARIABLES	(1)	(2)	(3)	(4)	(5)
			CEA_{cy}		
THREAT	0.629*	-1.411**	-1.363**	-2.360*	-4.438*
	(1.721)	(-2.182)	(-1.971)	(-1.859)	(-1.900)
US_exports	0.783*	0.933**	0.865**	1.133**	1.183**
	(1.947)	(2.312)	(2.114)	(2.217)	(2.310)
log(REVT) × THREAT		0.388***			
		(2.914)			
log(REVT)		0.007***			
		(6.062)			
log(AVGAT) × THREAT			0.377***		
			(2.651)		
log(AVGAT)			0.007***		
			(6.301)		
log(SPREAD) × THREAT				-0.646**	
				(-2.232)	
log(TRADEVOL) × THREAT					0.407**
					(2.087)
Observations	72,027	68,397	70,670	52,537	52,642
Adjusted R-squared	0.155	0.157	0.156	0.159	0.159
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table 3.8: Robustness to overall firm disclosures and falsification test

This table presents the results of a regression of CEAs on a proxy for threats of entry. All columns correspond to the first specification of each table (3.4-3.6). Panel A controls explicitly for the log of total number of words in press releases during the calendar firm year that is a proxy for overall disclosures. Panel B depicts a falsification tests where CEAs are replaced by the log of total number of words in press releases during the calendar firm year that is a proxy for overall disclosures. Please refer for an elaborate description to the corresponding table. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

VARIABLES	(1)	(2)	(3)	(4)	(5)
			CEA_{cy}		
log(TOTAL WORDS)	0.036***	0.036***	0.036***	0.038***	0.038***
	(17.609)	(17.341)	(17.257)	(16.532)	(16.536)
THREAT	1.059***	-0.874	-1.034	-1.259	-3.369
	(2.664)	(-0.994)	(-1.045)	(-0.840)	(-1.315)
log(REVT) × THREAT		0.368**			
		(2.137)			
log(REVT)		0.004***			
		(2.630)			
log(AVGAT) × THREAT			0.385**		
			(2.052)		
log(AVGAT)			0.003		
			(1.583)		
log(SPREAD) × THREAT				-0.492	
				(-1.469)	
log(TRADEVOL) × THREAT					0.355*
					(1.699)
Observations	57,309	55,141	56,386	45,077	45,143
Adjusted R-squared	0.172	0.174	0.172	0.178	0.178
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

	(1)	(2)	(3)	(4)	(5)
VARIABLES			log(TOTAL WORDS)		
THREAT	0.090	-5.443	-6.096*	-6.753	-8.583
	(0.057)	(-1.626)	(-1.724)	(-1.468)	(-0.985)
log(REVT)		0.098***			
		(13.255)			
log(REVT) × THREAT		0.805			
		(1.411)			
log(AVGAT) × THREAT			0.880		
			(1.389)		
log(AVGAT)			0.161***		
			(18.833)		
log(SPREAD) × THREAT				-1.578*	
				(-1.666)	
log(TRADEVOL) × THREAT					0.736
					(0.996)
Observations	57,309	55,141	56,386	45,077	45,143
Adjusted R-squared	0.492	0.497	0.503	0.473	0.473
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table 3.9: Relation between CEA and subsequent entry

This table presents the results of a regression of entry on CEAs, threat of entry and firm investments. The sample consists of all Compustat firm years in non-service industries that have issued at least one press release in the past in the period 1995-2016. CEA_{cy} is an indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in that particular calendar year. Entry is defined as Chinese exports to the US in the next year minus Chinese exports to the US in the current year divided by total sales of firms in Compustat Universe operating within the Fama and French Industry. The definition of all other variables can be found in Appendix B. All regressions are clustered by Gvkey and Industry-Year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

VARIABLES	(1)	(2)	(3) Entry	(4)	(5)
CEA_{cy}	-0.000*	-0.000*	-0.000**	-0.000**	-0.000**
	(-1.947)	(-1.864)	(-1.981)	(-2.159)	(-2.126)
THREAT	0.238**	0.024	0.232**	0.212**	0.017
	(2.226)	(0.132)	(2.196)	(2.070)	(0.095)
PPE_increase$_t$			0.000	0.000	0.000
			(1.252)	(1.318)	(1.188)
CAPEX_increase$_t$			-0.000	-0.000	-0.000
			(-0.383)	(-0.286)	(-0.035)
PPE_increase$_{t+1}$				-0.000	-0.000
				(-0.338)	(-0.674)
CAPEX_increase$_{t+1}$				-0.000	-0.000
				(-0.664)	(-0.488)
PRESENCE		0.263			0.243
		(1.514)			(1.395)
Observations	71,596	71,596	62,474	53,573	53,573
Adjusted R-squared	0.486	0.505	0.486	0.498	0.515
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Appendix A: Examples of capacity expansion announcements

Stage Stores - 10 October 2002
"With respect to store-based activity, the Company reconfirmed that it plans to open seven additional stores on October 24th. Five of the stores will be opened in Texas, while one each will be opened in Louisiana and Arkansas."

Helen of Troy Ltd. - 14 July 2005
"We are currently building a 1,200,000 square foot distribution center in Southaven, Mississippi, that will expand our eastern United States capacity to accommodate the distribution needs of OXO International."

Texas Industries – 29 March 2007
"We continue to make progress toward our goal of expanding TXI's annual cement capacity from today's 5.0 million tons to 7.5 million tons – all in three projects in a little over three years."

IJNT Net Inc. – 19 January 2000
"Mary Blake, IJNT's CEO, indicated that the Company will announce within the month the site of another major switching facility that IJNT will build with Nortel as well as expansion of the Company's wireless broadband facilities in [. . .] "

Graftech International Ltd. – 25 Oct 2012
"During the third quarter, GrafTech purchased building and land in Northeast Ohio for $3 million, which will be used to expand Engineered Solutions' manufacturing capacity for our advanced consumer electronics."

Clarcor - 19 March 2004
"We will make investments this year to expand our production facilities at several of our filtration companies. We will also expand technical and research facilities in our Industrial/Environmental filtration segment at our environmental air filtration and process liquid filtration companies. We plan this year to invest in areas which we believe will grow strongly in the future, such as process liquid filter applications, and also to complete various restructuring programs to drive improved operating

profitability."

Alcoa - 08 July 2005

"The company also began work to expand its global aerospace heat-treated sheet and plate production by approximately 50 percent over the next 18 months in response to orders from its aerospace customers, such as the nearly $2 billion high-performance sheet and plate supply agreement it signed with Airbus."

Appendix B: Variable Definitions

Variable Name	Definition	Level
CEA_fy CEA_cy	Indicator variable that is 1 when a firm issues at least one press release that contains a capital expansion announcement in the fiscal(fy)/calendar (cy) year.	Firm-year
$PPE\_increase_t$	Percentage increase in the firm's PP&E from the previous to the current fiscal year, winsorized on 5% level.	Firm-year
$PPE\_increase_{t+1}$	Percentage increase in the firm's PP&E from the current to the next fiscal year, winsorized on 5% level.	Firm-year
$CAPEX\_increase_t$	Percentage increase in the firm's capital expenditures from the previous to the current fiscal year, winsorized on 5% level.	Firm-year
$CAPEX\_increase_{t+1}$	Percentage increase in the firm's capital expenditures from the current to the next fiscal year, winsorized on 5% level.	Firm-year
COGS_increase	Percentage increase in the firm's cost of goods sold from the previous to the current fiscal year, winsorized on 5% level.	Firm-year
SALES_increase	Percentage increase in the firm's sales from the previous to the current fiscal year, winsorized on 5% level.	Firm-year
INVENTORY_increase	Percentage increase in the firm's inventories from the previous to the current fiscal year, winsorized on 5% level.	Firm-year
log(REVT)	Log of the GAAP revenues	Firm-year
log(AVGAT)	Log of the average total assets.	Firm-year
Log(SPREAD)	Log of the average spread of a firm's main stock for the whole period.	Firm
log(TRADEVOL)	Log of the average trading volume of a firm's main stock for the whole period.	Firm
log($TRADEVOL)	Log of the average dollar trading volume of a firm's main stock for the whole period.	Firm

International trade setting

THREAT	Chinese exports to eight developed countries in the current year minus Chinese exports to eight developed countries last year divided by total sales of firms in Compustat Universe operating within the Fama and French Industry.	Industry-Year
PRESENCE	Chinese exports to the US in the current year minus Chinese exports to the US last year divided by total sales of firms in Compustat Universe operating within the Fama and French Industry.	Industry-Year
ENTRY	Chinese exports to the US in the next year minus Chinese exports to the US in the current year divided by total sales of firms in Compustat Universe operating within the Fama and French Industry.	Industry-Year
US_Exports	U.S. exports to eight developed countries in the current year minus U.S. exports to eight developed countries last year divided by total sales of firms in Compustat Universe operating within the Fama and French Industry.	Industry-Year

Chapter 4

Financial Intermediation through Financial Disintermediation: Evidence from the ECB Corporate Sector Purchase Programme

4.1 Introduction

Small- and medium-sized enterprises (SMEs) are the backbone of an economy. They account for a vast majority of firms and contribute heavily to output and employ-

ment.[1]However, SMEs have comparatively limited access to credit, are highly dependent on bank financing, and are therefore disproportionately more exposed to credit market fluctuations (e.g., Berger and Udell, 1995, 1998; Petersen and Rajan, 1994a; Rice and Strahan, 2010). SMEs' dependence on bank lending impedes their potential for growth and remains an important concern that academics and regulators have studied for decades (e.g., Beck, Demirgüç-Kunt, and Maksimovic, 2005; DeYoung, Gron, Torna, and Winton, 2015; Carbó-Valverde, Rodríguez-Fernández, and Udell, 2016). Moreover, SME credit access contracted substantially during the financial crisis and did not fully recover in the aftermath (Ferrando et al., 2017; Bord et al., 2018; Cortés et al., 2018). Accordingly, policymakers and regulators search for policy tools that can increase the supply of bank credit to SMEs.

Typically, these tools aim to increase banks' willingness to lend to small businesses by making this type of lending more attractive to banks. For instance, regulatory authorities might engage in risk-sharing or provide direct credit guarantees to stimulate bank lending (Beck et al., 2010). Likewise, policymakers may choose to extend long-term financing to lenders under the explicit conditions that banks pass on these funds to designated borrowers, such as consumers and small businesses (as reported in the ECB Economic Bulletin, no 7, 2015). Despite the appeal of these measures, they do not monitor banks or provide them with direct economic incentives to engage in new lending (Duchin and Sosyura, 2014; Stern and Feldman, 2014). Furthermore, these actions do not seem to create the desired boost to the supply of bank credit to SMEs.

In this paper, we examine whether regulator-led financial disintermediation in non-SME credit enhances financial intermediation in the SME sector. Banks could extend more credit to small businesses if large corporate loans become less attractive and the opportunity cost of lending to SMEs decreases. To investigate how financial disintermediation in large corporate credit markets affects the availability and cost of bank financing to SMEs, we exploit a major monetary policy intervention by the European Central Bank (ECB). In particular, we study the ECB's latest outright asset purchase program, the Corporate Sector Purchase Programme (CSPP), launched in June 2016. Under this program, member central banks purchase non-

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[1]Ninety nine percent of U.S. and European companies are SMEs. SMEs are also responsible for two-thirds of employment and an even larger fraction of new jobs. https://web.archive.org/web/20180212010528/https://ec.europa.eu/growth/smes˙en.

financial investment-grade corporate bonds in the primary and secondary markets, thus circumventing the banking channel of financial intermediation.[2]

Why would financial disintermediation for a group of borrowers facilitate financial intermediation for another group of borrowers? The ECB argues that the CSPP would increase the supply and liquidity of credit in the economy, reducing the cost of debt for eligible firms and allowing them to rely (more) on bond financing.[3] As a result, banks with affected corporate borrowers would experience a reduction in demand for their loans from the corporate sector and smaller yields. The reduced demand by corporate customers could increase banks' willingness to lend to SMEs. As SMEs are typically a part of banks' commercial lending portfolio, they provide a natural substitute to large corporate loans. On the other hand, banks might not respond to a decline in large corporate lending by enhancing the credit supply to SMEs. Instead, they might resort to distributing dividends, investing in non-loan assets, or steering toward other types of loans, such as mortgages (Chakraborty, Goldstein, and MacKinlay, 2018). Thus, it is unclear a priori whether regulator-led financial disintermediation is an effective tool that can induce banks to increase lending to SMEs.

The CSPP setting offers several unique advantages for testing our research question. First, unlike other unconventional asset purchase programs and refinancing operations pursued by the ECB or other central banks, the CSPP is a novel example of direct centralbank lending to nonfinancial corporations.[4] Second, with total bond purchases of more than €178 billion as of 2019, the program is an economically significant intervention in the commercial credit markets, potentially affecting the population of firms rather than a few specific borrowers. Third, we can examine a variety of data essential to study rigorously credit supply and spillover effects of financial disintermediation. For example, in contrast to other monetary policy interventions, in which identities of targeted banks and firms often remain undisclosed, the Eurosystem makes information on bonds purchased under the CSPP public.

As a starting point, we investigate whether affected banks respond to the program by reallocating funding towards SMEs. We utilize the European Banking Association's (EBA) Transparency Exercise disclosures, whose granular details on banks' asset composition allow us to distinguish SME lending from large corporate lending.

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[2]The official decision can be found at http://www.ecb.europa.eu/ecb/legal/pdf/celex_32016d0016_en_txt.pdf. For details, see the Committee on Economic and Monetary Affairs Dialogue with Mario Draghi (November 28, 2016, and November 20, 2017). Also, see Mr. Draghi's letter to the European Parliament dated November 2017: https://www.ecb.europa.eu/pub/pdf/other/ecb.mepletter171108˙S&D˙Members.en.pdf

[3]See, for example, https://www.ecb.europa.eu/pub/pdf/other/ecb.mepletter170626_several_meps.en.pdf.

[4]We discuss the ECB's and other central banks' unconventional monetary policy programs in Section 4.2.2

Consistent with the Eurosystem's direct lending to corporations, which enhances financial intermediation in the SME sector, we find that, relative to a control group, banks directly affected by financial disintermediation—i.e., banks whose borrowers benefited from the CSPP purchases in the primary market—increase their SME exposures by 12 percent relative to the sample standard deviation.

Next, we investigate why banks might have refrained from lending to SMEs before the CSPP. We observe that our main results are stronger for banks that did not have enough loanable funds (i.e., liquidity-constrained banks) in the pre-CSPP period. This inference is consistent with the interpretation that an exogenous decline in large corporate borrowing frees up the balance sheet of constrained banks and induces them to switch to SME lending (Kashyap and Stein, 2000; Cortés et al., 2018; Schwert, 2018; Jiménez, Mian, Peydró, and Saurina, 2019). It could also be the case that the enhanced post-CSPP lending to SMEs is not a positive NPV project for banks. This concern is consistent with Heider, Saidi, and Schepens (2018), who find that the negative policy rates recently observed in the EU resulted in banks offering loans to overly risky borrowers. We find, however, that the subsequent level of nonperforming SME loans of affected banks is no different from that of unaffected banks. Our findings are consistent with the hypothesis that various frictions in the pre-CSPP period prevented banks from lending to SMEs.[5]

The bank-level analysis provides evidence of a link between affected banks and SME borrowing. However, realized lending or borrowing numbers lead to incomplete inferences. A positive association between ECB bond purchases and ineligible firms' access to financing could be driven by local economic trends or demand shocks rather than the CSPP. We overcome this simultaneity problem by examining the EC/ECB Survey of Access to Financing by Enterprises (SAFE, or the Survey, hereafter).

The Survey has been conducted semi-annually since 2009 and includes a series of questions to small businesses to understand their financing conditions and expectations, as well as operational decisions.[6] It also has anonymized firm identifiers but does not include links between banks and borrowers. Due to this limitation and because the effects of the CSPP are likely to extend beyond relationship lending, as the basis of our analysis, we posit that the benefits of financial disintermediation accrue to

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[5]The EBA transparency tests apply to the largest banks operating in Europe, allowing us to construct a sample of relatively comparable banks. We also use bank fixed effects to control for other unobservable bank characteristics (Jiménez et al., 2019). Our results are also robust to controlling for the choice of the regulatory capital model for banks using model-based internal ratings approach (IRB) or the standard approach (SA) with fixed rates (Behn, Haselmann, and Wachtel, 2016). We find that our results continue to hold when we explicitly account for the differences in the choice of the regulatory capital model.

[6]For more details, see https://www.ecb.europa.eu/stats/ecb_surveys/safe/html/index.en.html.

small businesses in a given industry-region.[7] Our analysis of a sample of more than 11,000 SME-half-year observations provides inferences consistent with the CSPP's positive effects spilling over to small businesses: SMEs in affected country-industry grids receive more bank credit, *conditional on* applying for a loan. Economically, the likelihood of SMEs obtaining full financing conditional on applying for a loan increases by about 3 percent, which represents 8 percent of the sample standard deviation.

Another crucial problem that remains even after we focus on a loan application dataset is the channel through which financial disintermediation operates. The aim of our study goes beyond a policy evaluation; therefore, we need to establish that the enhanced credit access by SMEs is driven by banks' increased willingness to lend to SMEs, rather than omitted demand-side factors (Khwaja and Mian, 2008). These confounding effects could be two-fold. First, if CSPP purchases merely target booming country-industry grids, the policy does not have a causal effect on SMEs and might be susceptible to a selection bias. Alternatively, the CSPP might indeed have a causal effect on SMEs, but this effect operates through the strengthening of the corporate sector. The following example clarifies this explanation. Suppose the CSPP purchases have been large in the German automobile manufacturing industry, and suppose we find a relative increase in the access to credit by German SMEs in the automotive sector. According to this narrative, financing to SMEs increases not because banks are more willing to lend but simply because large German auto manufacturers' access to additional cheaper capital increases the investment and growth opportunities of German automotive sector SMEs, which do business their large counterparts. These two issues remain a concern since in these cases, banks increase SME lending not because they are more *willing* to lend but because the average applicant SME has better fundamentals.

To address these issues, we adopt a two-pronged approach. First, we examine SMEs' perceptions on non-bank financing, including trade credit and leasing. If SMEs affected by the corporate-sector financial disintermediation also enjoy improvements in fundamentals and get more bank credit as a result, then these SMEs should be confident about all types of external financing.[8] Our tests, however, show this not to be the case. Second, we also isolate the effect of financial disintermediation incremental to bond issuance. If our inferences are an artifact of regional or sectoral dynamics rather

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[7]This definition is consistent with local features of SMEs and is supported by prior work, which documents that companies in the same industries and regions compete for funds (e.g., Cetorelli and Strahan, 2006).

[8]These tests examine SMEs' perceptions, not actual financing. This is an important distinction, since realized financing decisions may be insufficient evidence as they are affected by the pecking order of external financing.

than the ECB's actions, CSPP purchases should have no impact on SME credit access once we account for the amount of corporate bond issuance in that country-industry grid. After all, bond issuances in a particular region or sector should be a better indicator of improved economic conditions than the ECB's primary market purchases. We find, however, that purchases under the CSPP continue to explain a significant amount of variation in SME credit access after controlling for bond issuance.[9] This result also allows us to rule out that the increased access of SMEs to financing is only due to decreasing bond yields, and consequently increased reliance and issuance of bonds.[10] Finally, in additional analyses, we also provide direct evidence that our inferences are not driven by other policies implemented by the ECB (e.g., TLTRO) or banks' choice of the regulatory capital model.

We supplement our inferences on credit supply by using a novel dataset that contains bank–borrower links and borrower financials from the Bureau van Dijk's Amadeus database. We find that SMEs, whose relationship banks are affected by the CSPP, get on average €77,750 more credit, compared to SMEs in the same country and industry and over the same period. This crucial analysis does not only eliminate lingering concerns about confounding economic trends and developments that might vary within-country and across-industries, but it also exploits a direct link between banks and SMEs that we are unable to use in the EBA and Survey tests.

Utilizing a large sample of SME loan-level data, we also document that SMEs in the industry-regions that are more exposed to disintermediation are also more likely to report a reduction in interest costs. SMEs are also less likely to refuse a loan because the offered interest rate is too high. Overall, our results indicate that financial disintermediation in the corporate sector leads to a reduction in the cost of borrowing for SMEs, which is in line with an exogenous decline in the cost of credit in the corporate bond markets inducing banks to reduce their interest margins.

Finally, we shift our focus to real effects. Prior literature documents that when banks deny credit to borrowers, this often leads to negative consequences for firms and ultimately for the overall economy (Bernanke, 1983; Berg, 2018). Our setting allows us to measure whether SMEs use increased access to financing to fund their real activities. We find that affected SMEs are more likely to use newly obtained

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[9]Our additional placebo tests focusing on the period prior to the CSPP's implementation also confirm our conclusions that it is CSPP purchases rather than other trends in the data that drive our results.

[10]Our focus on the primary market bond purchases within a country-industry-gird captures the supply effect. We also conduct several robustness tests to take into account the shocks to supply and demand for financing (in the spirit of Khwaja and Mian, 2008; Behn et al., 2016). For a subset of SMEs that appear multiple times in the Survey data we include firm fixed effects and find similar results.

bank financing to increase capital investment and hire more employees, rather than to finance working capital or refinance their existing debt. These inferences are consistent with central bank lending in the large corporate sector boosting the real activities of SMEs.[11]

Even though our inferences suggest that financial disintermediation increases banks' willingness to lend to SMEs and affects the latter positively, we draw these conclusions based on the time right after the policy shock. However, are these changes due to banks' short-term portfolio reallocations or a longer-term structural change in bank lending relationships with SMEs? To shed some light on this question, we examine whether the effect of financial disintermediation changes over time. We find that in the longer term, financial disintermediation has an insignificant effect on banks' SME exposures and a substantially smaller influence on the likelihood of SMEs' credit access. Likewise, the impact of financial disintermediation on employment and investment disappears over time. Overall, these findings suggest that while the effects we document are positive, they are somewhat short-lived and do not have a structural impact on SME access to credit and ability to invest into real activities in the long-run.

Our paper contributes to several strands of the literature. The inferences we present shed new light on the financing and operating activities of small businesses. SMEs are a crucial engine of economic growth and are heavily bank-dependent, yet they face considerable obstacles to access financing (World Bank, 2013).[12] Our conclusions extend the literature on SMEs by suggesting that regulator-led financial disintermediation in the corporate sector could be an effective short-term solution for SMEs' credit access problems by inducing banks to increase lending to small businesses, which remains (or becomes) an attractive investment option for banks. Furthermore, we document the formation of new borrowing relationships for SMEs and the impact that credit access has on their real activities (Petersen and Rajan, 1994b; Berger and Udell, 1995; Amore, Schneider, and Žaldokas, 2013).

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[11]We also investigate the real effects of financial disintermediation on banks. In keeping with enhanced SME lending entailing greater lender-borrower interaction and more monitoring, we find a relative increase in affected banks' number of branches and employees.

[12]Prior studies offer a variety of solutions to the SME financing problem: public credit guarantees (Beck et al., 2010), lenders' information sharing (Pagano and Jappelli, 1993), stronger property rights (Beck, Demirgüç-Kunt, and Maksimovic, 2008), more effective collateral regulations (Campello and Larrain, 2016; Calomiris, Larrain, Liberti, and Sturgess, 2017), and more efficient technologies, which facilitate transactional lending (de la Torre, Martínez Pería, and Schmukler, 2010). Berger and Udell (2006) identify asset-based lending, factoring, small business credit scoring, and trade credit as potential remedies to bridge the SME financing gap.

In the specific context of the CSPP, Grosse-Rueschkamp, Steffen, and Streitz (2019) and Arce, Gimeno, and Mayordomo (2017) show empirically that ECB purchases of eligible bonds lead to affected corporate borrowers substituting bank lending with bond issuances. The authors argue that banks then increase lending to private firms and report some compelling evidence relating to evaluating the CSPP as a policy intervention. The difference between our work and these papers is that we identify the bank-lending channel by investigating loan applications, by holding credit demand constant, and by removing confounding factors that could have improved the fundamentals and investment opportunities of applicant firms. Our empirical research design and data allow us to go this extra mile and shed light on the underlying mechanism. This endeavor is a critical contribution in this setting because both supply and demand factors could explain the changes in SME financing, yet the implications of these channels would be completely different.[13] Indeed, possibly due to tighter identification, our conclusions are different from the papers studying the same policy shock. For example, despite the promising short-term trends, our inferences cast doubt on the structural effects of financial disintermediation. Also, unlike the other work on the CSPP, we find no effect for capital-constrained banks. This result is intuitive because CSPP-ineligible borrowers, like SMEs, would require a much larger risk weight allocation than investment-grade corporates, rendering an increase in lending to SMEs particularly difficult for capital-constrained banks. In this sense, our findings are in line with Cortés et al. (2018), who find that capital-constrained banks reduce their lending to SMEs, and with Schwert (2018), who shows that bank-dependent borrowers are more likely to borrow from well-capitalized banks.

We also show that this increase in lending to SMEs comes directly from the ECB's primary market bond purchases and not from the general rise in liquidity in the secondary bond market or from increased bond issuances overall. This important difference is another novel feature of our research. The aforementioned papers investigate primary and secondary market purchases together, which allows a comprehensive evaluation of the CSPP as a policy intervention. In contrast, our investigation of financial disintermediation focuses exclusively on primary market purchases because secondary market purchases do not constitute an example of financial disintermediation. In fact, the CSPP's secondary market purchases are quite similar to the ECB's then-existing asset purchase programs (i.e., PSPP, ABSPP, and CBPP3).

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[13]Similar to Ferrando et al. (2017); Ferrando, Popov, and Udell (2018), our identification approach allows us to measure supply effects directly from the firm-level Survey dataset designed for this purpose and to be representative for the overall Eurozone economy. This type of data has been used in other studies, which analyze the credit crunch in Europe during the financial crisis (Beck et al., 2008; Popov and Udell, 2012; Ferrando et al., 2017).

Finally, from a broad macroeconomic perspective, a number of studies have investigated the efficacy of unconventional monetary policy tools.[14] Related to this line of work, we document that financial disintermediation in the corporate sector enhances financial intermediation in the SME sector by leading banks to rebalance their portfolios. We also observe that SMEs experience higher availability of bank funding at a cheaper cost, which they utilize to invest in real activities. However, we also show that the positive effects we observe are short-lived, which questions the structural efficacy of financial disintermediation. Collectively, we contribute to the emerging literature on the impact of unconventional monetary policy interventions. More generally, our findings on the impact of financial disintermediation extend the theoretical and empirical work that studies substitution between bank and bond financing (e.g., Aiyar, Calomiris, and Wieladek, 2014a,b; Bolton and Freixas, 2000; Crouzet, 2018; de Fiore and Uhlig, 2011, 2015; Diamond, 1991; Santos and Winton, 2008).

4.2   Institutional Background

4.2.1   Description of the CSPP

Since the financial crisis of 2007–2009, the Governing Council of the European Central Bank (ECB) has taken extraordinary steps to stabilize prices and stimulate the economy and job creation in the Eurozone. By January 2016, the ECB had lowered the nominal interest rates to negative values and started to purchase marketable debt instruments from banks. With very few other tools left at its disposal, on March 10, 2016, the ECB announced a novel program to increase inflation aimed directly

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[14]These papers mostly focus on quantitative easing (QE) in the US or asset purchases in Europe. Acharya, Eisert, Eufinger, and Hirsch (2019) find that the ECB's Outright Monetary Transactions helped to stabilize European periphery countries but did not have any meaningful positive real effects likely because of banks' zombie lending and borrowers' cash hoarding. In this regard, the conclusions of Acharya et al. (2019) echo the importance of our tests on SMEs' relationship formation and investment undertakings. Studying the impact of QE in the US, Rodnyansky and Darmouni (2017) find that large-scale asset purchases by the Federal Reserve have a positive effect on banks most exposed to the QE programs (i.e., banks with significant holdings of mortgage-backed securities). These banks in turn also significantly increase their lending.

at the corporate sector (ECB, 2016a).[15] Launched on June 8, 2016, the Corporate Sector Purchase Programme (CSPP) allows designated central banks in the Eurozone to purchase corporate bonds in the primary and secondary markets. Corporate debt instruments are eligible for the CSPP if they satisfy the following criteria: 1) denominated in euros; 2) have investment-grade credit rating (as determined by Standard & Poor's, Moody's, Fitch Ratings, or DBMS); 3) have remaining maturity longer than six months, but shorter than 31 years at the time of purchase; 4) satisfy eligible collateral requirements under the Eurosystem collateral framework for credit operations; 5) issued by a company incorporated in the euro area, but may have a parent company outside of the Eurozone; and 6) issued by a non-bank corporation, whereby both the issuer and its parents are not subject to banking supervision.[16]

While the ECB supervises the bond purchase program, the actual purchases are carried out by Belgian, Finnish, French, German, Italian and Spanish central banks. These six national central banks are responsible for purchases based on the geographic location of the borrower. The amount of purchases from the overall allocated volume is based on these banks' contribution to the Eurosystem. Central banks are instructed not to differentiate among securities whose parent operates in the Eurozone, nor favor local securities over foreign securities that are under the responsibility of a particular central bank.

The ECB refrains from announcing in advance the total volume it intends to purchase in a given week to allow for flexibility in providing additional liquidity to bond markets if needed. In addition, national central banks are only allowed to buy up to 70 percent of the outstanding amount, and on the issuer-group level, there is an unspecified cap to ensure "a diversified allocation of purchases across issuers" (ECB, 2016b). The six national central banks do not provide a direct overview of the securities purchased on both the primary and secondary market, but instead, offer

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[15]We choose the CSPP as our setting because of its economic significance and because it provides us with the necessary data to examine our research question. We note, however, that the CSPP is not the only corporate bond purchase program in history. In fact, the Bank of England (BOE) and the Bank of Japan (BOJ) have conducted similar operations. These activities, however, were limited to the secondary market purchases and were significantly smaller in magnitude (BOE's cumulative purchases amount to some 11 billion euros, while BOJ's monthly purchases are less than 1 billion euros). As for other programs pursued by central banks to date, the CSPP is fundamentally different from the ECB's SMP and the Federal Reserve's large-scale asset purchases, which targeted financial entities only.

[16]See https://www.ecb.europa.eu/mopo/implement/omt/html/cspp-qa.en.html for details on CSPP eligibility criteria.

weekly updates on these securities. In Section 4.3 below, we discuss how we infer the volume of corporate bond purchases under the CSPP.[17]

4.2.2   The CSPP and the ECB's Other Monetary Policy Interventions

To maintain price stability in the post-financial and sovereign crises Eurozone, the ECB has followed several strategies, including open market operations, standing facilities, minimum reserve requirements, and asset purchase programs. The CSPP is an example of an asset purchase program—a category that also includes the ECB's purchase of covered bonds, asset-backed securities, and public sector securities.[18] These other asset purchase programs provide a significant amount of liquidity to Eurozone banks. However, their initiation does not overlap with that of the CSPP; these policies have been in place for at least a few years.[19] For covered bonds, asset-backed securities, and public sector securities purchase programs to confound our inferences, their initiation or changes in amounts purchased should coincide with the introduction of the CSPP. As Online Appendix Figure OAI shows, this is not the case.[20] Likewise, standing facilities and minimum reserve requirements do not directly target private sector credit and seldom vary in the cross-section of banks.

As for the open market operations, the ECB pursues two types of such actions, which differ from one another in terms of procedure, clauses, frequency, and objectives. "Main refinancing operations" have short maturity (typically a week) and aim for regular liquidity provision. Administered by national central banks, these activities provide the majority of refinancing to Eurozone banks. The second type of refinancing efforts pursued by the ECB is "longer-term refinancing operations." Whereas regular long-term refinancing operations have a maturity of about one to three months,

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[17]European Union countries also have specific credit guarantee schemes to address SMEs credit gaps. Since these schemes were in existence before the CSPP intervention, we use the CSPP as a setting to identify an incremental effect of financial disintermediation in the presence of existing credit guarantee schemes (see Chatzouz, Gereben, Lang, and Torfs, 2017, for more information on credit guarantees in Europe).

[18]Other outright asset purchases by the ECB are the currently active Public Sector Purchase Programme (PSPP), Asset-Backed Securities Purchase Programme (ABSPP), and the third Covered Bond Purchase Programme (CBPP3), as well as the terminated Securities Market Programme (SMP) and the first two Covered Bond Purchase Programmes (CBPP and CBPP2). By refinancing operations, we mean the ECB's non-traditional repo activities, including the Long-Term Refinancing Operations (LTRO) and the targeted Longer-Term Refinancing Operations (TLTRO I and II).

[19]The ABSPP started in November 2014, the CBPP3 commenced in October 2014, and the PSPP began in March 2015. These dates are included in the pre-treatment period of our tests; therefore, the effects of these programs are already included in our baseline.

[20]For details, see https://www.ecb.europa.eu/mopo/implement/omt/html/index.en.html.

targeted longer-term refinancing operations, or TLTROs, could have maturities of up to 48 months.

Unlike in its regular refinancing operations with liquidity objectives, through TLTROs, the ECB aims to enhance credit to the private sector by providing banks with stable funding at affordable prices (i.e., negative rates). The first TLTRO series was conducted in 2014, while TLTRO II was announced in March 2016, launched in June 2016, and performed quarterly over the following 12 months. Under TLTRO II, which allowed banks to borrow an amount of up to 30% of their outstanding eligible loans, the ECB allotted about 235 billion euros to 474 banks.[21]

TLTROs are related to the CSPP for two main reasons. First, the TLTRO period (the second wave) overlaps with the CSPP period. Second, the ECB's targeted lending to the Eurozone banks requires recipient banks to use these funds only for lending to non-financial corporations and households, including SMEs. Similar to the CSPP, the effects of TLTROs vary highly in the cross-section of banks, with certain banks obtaining funding under these programs. We note that target refinancing could interact with or confound the effects of the CSPP. Accordingly, in Section 4.4 and the Online Appendix, we discuss the implications of TLTROs (and, to a lesser extent, of other monetary policy interventions) on our inferences.

4.3   Data

4.3.1   Measuring CSPP Exposures

We use two main independent variables in our study. *Disintermediation* is the total amount of corporate bonds that the ECB has purchased in the primary market in a particular country-industry and *Affected Bank* is an indicator variable taking the value of one for banks with clients whose corporate bonds have been purchased on the primary market by the ECB.[22] The ECB does not release the specific trade-level data for CSPP but instead provides an overview of new securities purchased on a bi-weekly basis as well as the total purchase amounts for securities acquired in both the primary and secondary markets. This allows us to estimate which securities were purchased on the primary market and the ECB investment amount per security under the assumption that the ECB buys the same percentage of all bonds it purchased on the primary market in a given month. More precisely, the ECB investment in a particular

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[21]For more details, please see:
https://www.ecb.europa.eu/pub/pdf/other/ebbox201704_03.en.pdf?4c92fdae71b53a5515155bd2678e8157.

[22]In the robustness tests, we use a continuous measure based on the value of corporate bonds purchased relative to the affected banks overall corporate loan portfolio.

bond is defined as the bond tranche value divided by the total tranche values of all securities purchased by the ECB in the primary market during a given month and multiplied by the total monthly amount of all ECB purchases in the primary market (see Section 4.2 for more details on the CSPP's primary and secondary market purchases.).

We extract all weekly updates on security lending from the participating national central banks' websites starting from May 9, 2017. For prior periods, we obtain our data directly from the six national banks. All weekly overviews begin in the third week of July 2016, which corresponds to the initial coverage of the CSPP by the media. As the CSPP started its operations on June 8, 2016, we cannot accurately determine whether bonds were purchased in the primary or secondary markets between June 8 and the first public reporting of bond purchases in July and hence exclude from our analyses the 13 bonds issued during this period. We compare the date that securities appear for the first time on the securities lending overview with their issue date to classify the bond as a primary or secondary market purchase. We categorize a central bank purchase as a primary market purchase if the security's ISIN appears on the central bank's security lending overview within eight days of the issue date. This approach allows us to account for bonds that are not settled on the issue date.

We aggregate all primary market purchases by the ECB at the country-industry-time level and deflate them by the total of all bonds outstanding in that specific country-industry grid as of June 1, 2016 (i.e., before the start of the CSPP) to compute our main variable of interest (*Disintermediation*). The total bonds outstanding data is from Dealogic and used to make our dependent variable comparable across countries and industries. Our second dependent variable, *Affected Bank,* is an indicator that takes the value of one when a bank has at least one client with corporate bonds purchased by the ECB in the primary market. We obtain the bank-client relationships from Dealogic and Amadeus.

We make several important design choices in our study. First, we focus specifically on the ECB's primary market purchases as the capital market effects of CSPP purchases in the primary and secondary markets differ. In particular, for this study, purchases in the primary market represent directly affected eligible borrowers' ability to raise new financing in the public debt market. Secondary market purchases, on the other hand, have a more indirect effect through increased bond market liquidity and therefore decreased marginal cost of new debt issuance in the public debt market.

Second, we perform our test of affected banks on the bank level instead of the bank-parent level. The main advantage is that this allows us to better control for

country-specific trends that affect SME lending through country fixed effects. We have conducted our tests also on the bank-parent-level and find similar results.

4.3.2 Sample Construction

We use several data sources in our study. We start our analyses by examining the effect of the CSPP on large EU bank exposures to corporate and SME borrowers using the exposures data from the EBA regulatory disclosures related to the 2016 and 2017 Transparency Exercises. The transparency exercises and stress tests cover the largest banks in the European Union and include bank-level information from 2015 to 2017. We then combine this data with the Dealogic syndicated loan dataset and CSPP purchases to identify which banks lent money to companies that received a direct investment by the ECB when they issued corporate bonds.[23]

To study the impact of financial disintermediation on SMEs more directly, we rely on the Survey on the Access to Finance of Enterprises (SAFE). The critical role of the Survey for our study is that it allows us to hold constant any confounding demand effects and focus directly on the supply of bank credit to SMEs.[24] It is crucial to highlight that the Survey is the source used by the ECB itself for the evaluation of the impact of its monetary policy interventions on small businesses.[25] The Survey also allows us to study the effects of the CSPP on a very timely basis—before the real actions are observable in financial statements—and for a group of firms that are typically not well covered by financial databases.[26] The Survey is a collection of sensitive information about small businesses, which, for privacy reasons, masks company identities and is not very granular about industry information (providing

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[23]The Dealogic dataset contains 75,183 outstanding loans and credit facilities in the EU with a maturity later than June 1, 2016, and issued before June 1, 2016. Dealogic also provides full firm-parent and bank-parent hierarchy structures with identifiers that are fully compatible across the DCM bond and Loan datasets. This allows us to link firms that issue bonds purchased by the ECB via their loans and bank subsidiaries to the banks included in the EBA regulatory disclosures. We exclude a handful of banks that appear in the EBA regulatory disclosures but not in Dealogic DCM or Loans datasets. Most of these banks are subsidiaries of foreign banks from outside of the EU. We believe this is a conservative assumption allowing us to focus on banks that we can identify with a high degree of certainty as being directly affected by the CSPP.

[24]To sidestep this problem, one can resort to cross-sectional analyses by linking eligible borrowers to non-eligible borrowers in various ways, including sharing the same bank, industry, or geographical region. Since these links would also be affected by changes in credit demand, any such inferences would remain confounded and misleading.

[25]See, for example, https://www.bis.org/review/r160623b.pdf.

[26]See Balakrishnan and Ertan (2018) and Ferrando et al. (2017) for a description of the Survey data and its geographical coverage.

industry information on the aggregate level of six sectors). For this reason, we exploit the industry variation in CSPP purchases at a somewhat coarse level.[27]

We gather information on firms' bank relationships through two different data sources. First, we use the Bureau van Dijk Amadeus Bankers dataset that contains all bank relationships between banks and private and public firms. This dataset includes rich information on countries and industries; however, Amadeus does not report this data historically. In our tests, therefore, we compare a snapshot of this dataset containing bank relationships as of 2014 and 2015 to a snapshot of banking relationships as of 2016 and 2017. We also manually match banks from the EBA data to Amadeus bank relationship data to identify directly affected SME borrowers. We use borrower-level financial information from the Bureau van Dijk's Amadeus Financials Dataset.[28]

We supplement our analysis using the detailed data provided by the ECB Loan-Level Disclosure (LLD) Initiative (Ertan, Loumioti, and Wittenberg-Moerman, 2017). The contract-level data comes from the ECB member banks' disclosures of loan-level details of the SME-loan-backed securities they offer as collateral to borrow from the ECB standing facilities programs. These disclosures include information on the performance and structure of individual loans and have been reported in a standardized format every quarter since 2013. We focus on loans issued during the CSPP period and identify a sample of 327,452 individual SME loan contracts, which we use to analyze the impact of the program on loan characteristics, such as interest rates. We also use SNL Financial data for bank-specific characteristics.

4.4   Empirical Methodology and Results

In this section, we introduce our empirical research design and discuss how we use the combined data to study the spillover and real effects of financial disintermediation for

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[27]According to the ECB, in the most recent waves of the survey, the typical response rate was 12–14 percent, with country-specific rates 7–19 percent for wave 17. The ECB also conducts validity checks to ensure accurate responses to questions. In particular, validity and consistency checks are run both by the survey company and by the ECB. Some additional quality checks are performed, for example, on the variable on interest rates (Q8b), which is also checked against official interest rates statistics. Finally, the ordering of questions in the survey is respected by the interviewers and questions always appear in the same order.

[28]For a more frequently updated but smaller sample, we use (syndicated) loan data from the Dealogic Loans database. This option allows us to create a panel dataset on a quarterly basis and to determine whether firms (1) obtain a new loan, (2) establish a new bank relationship or (3) have a new bank as a contributor on a new syndicated loan. A new relationship in this context is defined as a bank with whom the company has not had an active borrower relationship in the prior six months.

large borrowers on bank lending to SMEs using banks' and industry exposure to the
CSPP as our setting.

4.4.1   Banks' Exposure to SMEs

We first examine whether large European banks change their exposure to corporate
and SME borrowers following the introduction of the CSPP. To do so, we estimate
the following standard differences-in-differences (DiD) model at the bank-half-year
level:

$$SME\ Exposure_{bt} = \beta_1 Affected\ Bank_b \times Post\ CSPP_t + \gamma X_{bt} + \alpha_b + \delta_t + \epsilon_{bt} \quad (4.1)$$

where b indexes banks and t indicates time, which, in these tests, is a half-year,
as per the frequency of the EBA data α_b and δ_t are bank and time fixed effects,
respectively. *Affected Bank* and *Post CSPP* are the two components of the DiD model.
Affected Bank is an indicator that equals one if the bank in question has at least one
large corporate borrower with bonds purchased by the Eurosystem under the CSPP.[29]
(We use a continuous version of this variable as a robustness check.) *Post CSPP*
switches on for the two half-year data points after the implementation of the CSPP:
2016H2 and 2017H1. This data is from the results of the 2017 Transparency Exercise.
(The two data points from the pre-CSPP period are 2015H2 and 2016H1 and are from
the results of the 2016 Transparency Exercise.) *SME Exposure* is bank b's exposure
to SMEs relative to its total exposures at time t. X_{bt} is a vector of control variables,
consisting of banks' exposures to the corporate sector (*CORP Exposure*) measured
as bank b's relative exposure to the corporate borrowers (excluding SMEs) at time
t. We obtain SME and corporate sector exposures from the regulatory disclosures
because information on European banks' asset composition, and especially details of
commercial lending, is not available in other publicly available databases. Given our
reliance on the EBA's regulatory disclosures, which encompass the biggest banks in
Europe, both affected and non-affected banks are all relatively large entities and have
a statistically indistinguishable size of SME portfolios relative to their total assets.

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[29]We have also conducted our tests with an alternative definition for affected banks, where the
affected bank indicator is one for banks that had at least one lender-borrower relationship end in a
half-year in which that particular borrower received CSPP financing from the Eurosystem. These
results are similar to those in Table 4.2. There are two reasons to rely on the current definition.
First, Dealogic does not provide much detail on discontinued loans, which forces us to rely simply on
maturity dates or estimate which deal has replaced which other deal. Second, we believe that a bank
is also affected when a borrower that receives CSPP financing reduces its borrowing or would not
borrow money it would have absent CSPP financing.

Table 4.1 presents the summary statistics for the sample used in our empirical analyses. As shown in Panel A, almost half of the observations are from the post-CSPP period, while half of the sample banks are affected by the CSPP (*Affected Banks*). SME lending constitutes 8.2 percent of banks' total exposures. By comparison, bank exposures to large corporate borrowers are bigger, with an average of about 13 percent of total exposures.

We present the estimation results of equation (4.1) in Table 4.2, which shows that the sample banks affected by financial disintermediation (i.e., banks whose borrowers have benefited from direct purchases of their bonds by the ECB) increase their exposures to SMEs relative to unaffected banks.[30] Columns (1) through (6) of Table 4.2 show the results of three specifications for SME exposures, all of which exhibit significant positive increases. We find that on average, exposures to SMEs at affected banks increase by a relative 2.28 percent if we take into account time fixed effects, 2.03 percent if we also use country fixed effects, and 0.95 percent if we also use bank-specific fixed effects.[31] These results are economically meaningful as well, respectively representing 28 percent, 25 percent and 12 percent of the sample standard deviation of SME exposures.

Next, we control for banks' exposures to the corporate sector. Columns (4) to (6) of Table 4.2, Panel A show that banks with corporate exposures are also more likely to have exposures to the SME sector. In specifications (5) and (6), we also control for the pre-treatment trend by introducing an interaction variable of *Affected Bank* × *Pre CSPP*, which takes the value of one for banks exposed to corporate borrowers affected by the CSPP in the period before 2016H2. We find no evidence of a differential trend before the introduction of the CSPP. In specification (6), we use *Affected Bank* as a continuous variable, which equals the natural logarithm of banks percentage exposure to corporate borrowers whose bonds are purchased under the CSPP. This estimation model also supports our main inferences. Overall, our first set of findings suggests that the large EU banks with borrowers who have benefited from financial disintermediation appear to increase their exposures to SMEs following the introduction of the CSPP.

Thus far, we find that banks increased their lending to SMEs following the commencement of financial disintermediation in the corporate sector. However, this might not be sufficient to address the concerns regarding why banks were not lending

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[30]In our models, we take into account the within-bank correlation by including clustering of standard errors by banks.

[31]Each model includes individual indicators for Affected Bank and Post CSPP as well. The coefficients on these terms are not identified in the presence of bank and time fixed effects, respectively.

to SMEs before the introduction of the CSPP. If lending to SMEs is profitable and represents a positive NPV opportunity for a given level of risk and funding costs, we would have expected banks to lend to SMEs prior to the CSPP intervention in the absence of economic frictions. We therefore investigate whether banks that might have been relatively liquidity- or capital-constrained before financial disintermediation in the corporate sector were less likely to lend to the SME sector prior to the CSPP intervention. We measure liquidity as a ratio of liquid assets relative to deposits in 2015 (i.e., before the start of our pre-period). Similarly, capital corresponds to the regulatory Tier 1 capital ratio at the end of 2015. We multiply both of these ratios by minus one for ease of interpretation.

Column (1) of Panel B in Table 4.2 shows that, among affected banks, those with greater liquidity constraints become significantly more likely to lend to SME borrowers following the introduction of the CSPP. Column (2) shows that relatively capital-constrained banks do not incrementally increase their lending to SMEs following the introduction of CSPP. This is not surprising as SME loans tend to be relatively riskier and hence might result in a higher capital charge for banks, therefore, suggesting that capital-constrained banks are less likely to increase lending to SMEs even in the presence of financial disintermediation.[32] Our findings on capital-constrained banks are also consistent with Cortés et al. (2018), who document that capital-constrained banks reduce their lending to SMEs relative to unconstrained banks.

To validate our inferences, we conduct two sets of robustness analyses, the results of which are included in the Online Appendix. First, we test the sensitivity of our choice of the dependent variable (i.e., SME exposure as a proportion of total exposures). To minimize the concern that a decline in corporate exposures may mechanically trigger a relative increase in SME exposures as a fraction of total exposures, we define this dependent variable in raw values (billions of euros). As can be seen from Online Appendix Table OAI, our conclusions continue to hold.

The second robustness test we undertake relates to the concurrent monetary policy interventions conducted by the ECB. As discussed in Section 4.2.1, TLTRO II appears to be the most relevant program because it overlaps with the CSPP, affects the cross-section of banks differently, and requires banks to lend to non-financial entities, including SMEs. We tackle this problem by controlling for *TLTRO* (an indicator variable that switches on only if Bloomberg records include a TLTRO borrowing by

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[32]Another potential friction that might create constraints on banks' lending choices is government intervention through liquidity support, recapitalization and full nationalization prior to and during our sample period (see, for example, Kleymenova, Rose, and Wieladek, 2016; Rose and Wieladek, 2014). We find that banks that received government support were less likely to increase lending to SMEs than banks that received no government support.

the bank). The estimates presented in column (1) of Online Appendix Table OAII show that our main inferences do not change, in that *Affected Bank × Post CSPP* has a positive and significant coefficient after controlling for *TLTRO*. (We also note that TLTRO banks increase SME lending, consistent with the objectives of the program.) In columns (2) and (3), we present our findings from a subsample that is limited to banks that borrow (column 3) or do not borrow (column 2) under TLTRO. In both cases, we continue to find that exposure to the CSPP and resulting financial disintermediation in the corporate sector enhances bank lending to SMEs. Finally, TLTRO programs provide access to long-term financing for banks at a fixed rate based on existing funding available through the Eurosystem. While the amount that banks could borrow is indeed derived from their lending portfolio to the non-financial sector, the fact that the CSPP was introduced alongside TLTRO suggests that the former is complementary to the existing programs (including TLTRO). Hence, financial disintermediation via the CSPP represents a separate macroeconomic policy tool aimed at strengthening the pass-through of asset purchases to ease corporate financing conditions and credit growth ultimately creating financial disintermediation in the corporate sector and providing positive spillover effects to credit access for SMEs.

In addition to the robustness analyses above, we also perform tests on banks' non-SME exposure levels. This investigation helps us provide a fuller picture of the affected banks' portfolio decisions and shed light on the substitution effect we propose. Here, we estimate the regression in equation (4.1) with corporate assets and all other non-corporate, non-SME exposures on the left-hand side. The results shown in Online Appendix Table OAIII provide economically meaningful insights. Column (1) includes our main result on SMEs shown in Tableekt˙dynamic. Consistent with our prediction that financial disintermediation reduces commercial bank lending to corporations, we observe a decrease in affected banks' corporate exposures post-CSPP (column (2)). We do not find a significant fluctuation in banks' other exposures post-CSPP (column (3)).

The EBA's regulatory exercises apply to the largest banks operating in Europe, leading to a sample comprising relatively similar banks. Unobserved bank effects, therefore, do not pose a significant threat to our inferences, especially once we account for bank fixed effects (Jimenez et al., 2014). However, we perform two additional tests to ensure that time-varying bank confounds do not drive the effect that we attribute to financial disintermediation in line with Behn et al. (2016). First, we identify banks that rely on internal ratings-based (IRB) approach and then interact this indicator variable with each half year. Second, we repeat the same exercise for bank size, as we

create a dummy for large banks (i.e., entities with above-median total euro exposures every half-year). These two additional sets of regressors help us verify whether our conclusions are an artifact of banks' size or risk-modeling practices. Untabulated tests alleviate this concern. Specifically, we find coefficients of 0.94 and 1.39 for our DiD estimator (both statistically significant at the 5 percent level of significance) when we control for IRB-time and bank-size-time fixed effects, respectively.

4.4.2   SME Credit Access

Next, we examine the effects of the CSPP corporate bond purchases on SMEs' ability to access bank financing. Specifically, we estimate the following cross-sectional model:

$$SME\ Credit\ Access_{it+1} = \beta_1 Disintermediation_{cjt} + \gamma X_{it} + \nu_{cj} + \lambda_{ct} + \sigma_{jt} + \epsilon_{it+1}$$
(4.2)

where i indexes firms, t indicates half-year survey data frequency (survey waves), c corresponds to a country and j to an industry. ν_{cj} are country-industry fixed effects, λ_{ct} are country-time fixed effects and σ_{jt} are industry-time fixed effects. X_{it} is a vector of control variables. *SME Credit Access* corresponds to SMEs' responses to the Survey question 7b_a, which asks companies about the outcome of their application for bank loans in the prior six months. One key contribution of our paper is that we keep the demand for credit constant, which we do by concentrating exclusively on companies that apply for a bank loan. In particular, SME borrowing could be cyclical, which may coincide with the implementation of unconventional monetary policy. Examining the survey and focusing on the subset of SME borrowers seeking credit help us address this concern.

We define *SME Credit Access* as an indicator variable that takes the value of one if applicants receive the full amount of the loan they apply for and zero if they receive less than the full amount or if their application is unsuccessful. Since not all firms apply for credit, we observe this variable for 11,180 observations. Panel B of Table 4.1 presents the summary statistics of the Survey variables we use in our analyses. *SME Access to Bank Credit* has a mean (standard deviation) of 0.802 (0.398) and median of 1, suggesting that the average SME firm in our sample obtains a full amount of the loan it seeks.

Our primary variable of interest, *Disintermediation*, is the intensity of the CSPP impact in a given country and industry. More specifically, *Disintermediation* is measured as the aggregate corporate bond purchases by the ECB in the primary market within a country-SAFE industry during the corresponding wave period and

deflated by the total value of all bonds outstanding in the country-SAFE industry. This variable is zero for the survey observations before June 2016 and the country-industry grids without eligible corporate bond purchases in the post-June 2016 Survey waves. For ease of interpretation, we express this amount in percentage points. Panel B of Table 4.1 shows that the average share of CSPP purchases for industries and countries represented in the Survey is 0.09 percent of total bonds outstanding at the time of the Survey (including zeros).

An important innovation of this paper is that our disintermediation variable captures the actual purchases of corporate bonds by the Eurosystem and allows us to exploit the variation in purchases over time, across targeted industries and in the magnitude of purchases relative to the country-industry size. Our fixed effects structure also allows us to estimate the effect of CSPP purchases on SME financing much more precisely than if we were to do this on a country level, as we can control for the changing economic fundamentals in that particular country or industry. Furthermore, given that the ECB has the goal to provide the same relative amount of quantitative easing per country, a country-level analysis would therefore be less desirable as it relies solely on mistiming of this principle and would lack clear controls for changing economic fundamentals.

We also control for SME characteristics based on the demographic information available in the Survey. In particular, we control for *SME size*, measured as one if annual sales are up to €2 million, two if annual sales are between €2 and 10 million, three if sales are between €10 and 50 million, and four if sales are over €50 million. Table 4.1, Panel B shows that the average size of SMEs in our sample is 2.18, corresponding to firms with annual sales of between €2 and 10 million. *SME age* measures the age of the company and varies between one (one to two years) and four (older than 10 years). The average SME firm in our sample ranges in age from five to older than 10 years. We also control for the change in credit quality over the prior six months (*SME credit quality*) and change in profitability (*SME profitability growth*). These ordinal variables range in values from one (credit quality deteriorated) to three (credit quality improved) and one (profitability decreased) to three (profitability increased). Panel B of Table 4.1 indicates that, on average, SMEs' credit quality and profitability growth remained the same. We also control for overall bond issuances in a given country-industry grid (*Bond issuance*). Panel B of Table 4.1 shows that on average, 2.58 percent of bonds were issued during our sample period relative to all bonds outstanding in the same country-industry and time.

We estimate SMEs' credit access model in the post-CSPP period after June 2016, as well as during the full period from 2015 to 2017. While the post-CSPP period analysis focuses on the cross-sectional variation, the full period is effectively a difference-in-differences (DiD) specification, in which we compare SMEs' access to financing before and after the introduction of the CSPP. In this estimation, the *Disintermediation*, which is a continuous variable, captures the heterogeneous intensity of the CSPP impact.

In additional tests, we also examine SMEs' views on external financing. We do so to address the lingering concern that even if we hold the demand for credit constant, some of our inferences might be driven by increasing credit quality or fundamentals of SMEs. This could be a problem, especially if the Eurosystem targets booming industries.[33] Even though there is no reason to believe that CSPP purchases are statistical artifacts of country-industry performance, we deal with this potential problem by comparing the variation in *Bank Loan Availability* to that in *Trade Credit Availability* and *Lease Financing Availability*. If the CSPP has a genuine impact, we should observe that SMEs perceive an increase in the supply of bank funding but not trade credit. As before, we observe *Bank Loan Availability* and *Trade Credit Availability* at the SME-half-year level and define them as indicator variables. Some 26 percent (19 and 21 percent) of the respondents state that they believe the availability of bank financing (trade and lease credit) has improved (Panel B of Table 4.1).

Table 4.3 presents our findings for credit availability for SMEs following the introduction of the CSPP. Models (1) through (3) of Panel A show availability of bank credit in the full period (i.e., DiD setting) controlling for time-varying SME attributes, including firm size, age, employees, credit quality, and profitability growth. Model (1) does not include fixed effects while model (2) incorporates wave, industry, and country fixed effects to take into account unobserved heterogeneity at the time, industry, and country dimensions. Model (3) uses the multi-dimensional fixed effects structure of industry and survey waves (i.e., time), country and wave, as well as industry and country. We find positive and statistically significant results across all specifications. Economically, a 1 percentage point increase in CSPP intensity increases SMEs' access to full bank financing between 2.4 percent (model 2) and 4.1 percent (model 1). Columns (4) through (6) in the same table show the results of CSPP intensity on SME bank credit in the period following the introduction

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[33]We focus our analysis on the direct impact of the CSPP by identifying the ECB's purchases of corporate bonds in the primary market. We find that more than 80 percent of eligible bonds issued in the primary market are purchased under the program. In addition to our empirical identification strategy, this fact also gives us some comfort that the ECB is not cherry-picking bonds in a particular sector or region of the European Union.

of financial disintermediation using the same specifications as the previous three models. Consistent with the earlier results, we find that in the post-CSPP period, a 1 percentage point increase in *Disintermediation* results in an increase in SMEs' access to bank credit by 2.1 to 3.0 percent.[34]

We substantiate our finding that the CSPP's primary market purchases have contributed to an increase in SME lending by performing several robustness checks in Panel B of Table 4.3. First, we utilize the survey population weights, which allow us to scale our findings to the overall populations of European SMEs and take into account any potential oversampling of smaller SMEs by the Survey (Ferrando et al., 2017). We find that using survey weights in the full DiD setting (column 1) as well as in the post-CSPP period only (column 2) results in similar findings as before, namely SMEs access to credit increases in their exposure to CSPP. This is not surprising as the Survey is created using randomized sampling and even if oversampling were to occur it is likely to be at the country level, which is subsumed by our use of country-level fixed effects (Ferrando et al., 2017). Next, we also introduce SME-fixed effects for a subsample of SMEs that appear multiple times in the survey (columns 3 and 4). Controlling for unobserved SME heterogeneity, we continue to find similar results that exposure to CSPP increases their ability to obtain bank financing.[35]

4.4.3   Ruling out Alternative Hypotheses

Thus far, we find that SME access to bank financing increases with their exposure to the CSPP. To rule out alternative explanations that overall improvement in macroeconomic conditions and not CSPP might drive our findings, we conduct a number of tests. Another lingering concern is that our results might be driven by the positive effects that financial disintermediation might have had on bond issuances through increased liquidity (instead of the CSPP's primary market purchases).[36] As new bond issuances are a necessary condition for the ECB to purchase bonds in the primary market, these variables could be correlated and therefore drive our main results. In Table 4.4, Panel A, models (1) and (2), we replace CSPP purchases by new bond issuances within a particular country-industry-time period. We find that bond issuances do not explain the increase in SME lending, as the coefficients are neither statistically nor economically different from zero, while CSPP purchases

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[34]We also examine SMEs' applications to bank credit lines. The results from these tests are statistically and economically comparable to those on term loans (not tabulated).

[35]The observations count in the table includes singletons. In total, we have 8,030 distinct SMEs for a sample of 11,180 firm-years.

[36]See, for example, the findings of Abidi and Miquel-Flores (2018) on the impact of the CSPP on yields and bond issuances of the eligible large corporates.

have a significantly positive effect on SME lending.[37] As bond issuances are highly correlated with CSPP purchases (with a pairwise correlation coefficient of 0.64), we also orthogonalize our variables. However, orthogonalizing our variables does not alter our prior conclusions, as we find similar results.

We also conduct several placebo tests to confirm the robustness of our results and to alleviate further any concerns that the increase in SME lending may be driven by a correlation between the CSPP's primary market purchases and issuance of investment-grade bonds. If instead of the CSPP's primary market purchases, the correlation between the CSPP and the issuances of investment-grade bonds is driving our results, we should see similar results in the period that directly precedes our pre-treatment period.[38] We therefore use the eligibility criteria for CSPP corporate bond purchases to estimate the choice and the value of the bonds that the ECB likely would have purchased in the placebo period. We first determine which bonds would be eligible to be bought by the ECB in both our regular sample period and the placebo period and then calculate the percentage of all eligible bond issuances that were purchased by the ECB on the primary market every half year. We then multiply these semi-annual country-industry percentages with the corresponding value of eligible bonds in the placebo period to generate a placebo amount of CSPP purchases.

We find no economically or statistically significant results of our placebo tests presented in column (3) of Table 4.4, Panel A. This strengthens our interpretation that the CSPP's primary market purchases indeed represent an important driver of SMEs' access to bank credit and are unlikely to be driven by factors related to CSPP (eligible) bond issuances. In two additional (untabulated) placebo tests, we use the actual CSPP purchase amounts of our regular sample period in the period that directly precedes our pre-treatment period and a rescaled version where we rescale the purchase amounts in that period to match the relative impact of purchases in our regular sample period. Each of the placebo tests leads to similar results, in which the placebo CSPP purchases are not statistically significantly related to SME financing.

Next, we examine whether improvements in the overall economic conditions in region-sector grids affected by financial disintermediation, not bond purchases themselves, may drive banks' willingness to lend to SMEs. Holding credit demand

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[37]Additionally, by using the Survey data, we investigate the likelihood of SMEs making a loan application. We find no increase in SME' decision to apply in country-industry grids with intense bond purchases under the CSPP. This inference provides further support that the economic conditions in treatment grids are not significantly better than that in non-treatment grids.

[38]The pre-period with no CSPP purchases runs from January 2012 until June 2013, and our placebo post-period with CSPP purchases runs from July 2013 until December 2014. For all placebo tests, we use the actual SME lending and control variables as observed in this period.

constant and including country-time and industry-time fixed effects alleviate this
concern to a significant extent. Nevertheless, because these potentially confounding
effects may not be ruled out entirely even on a dataset focusing on loan applications
or eliminated by our fixed effects structure, we investigate SMEs' expectations and
views on different types of external financing opportunities after the introduction of
the CSPP. In Panel B of Table 4.4, we present our findings on SMEs' *expectations*
of the availability of credit by capturing their perceptions about the likelihood of
getting financing in the future. Model (1) shows the results for SMEs' perceptions
of whether the availability of bank financing changes, while model (2) presents the
results of SMEs' perceptions of whether their access to trade credit changes and
model (3) shows the results for SMEs' perceptions about lease finance availability. We
find that following financial disintermediation in the large corporate sector, SMEs'
perceive that bank loan availability increases by 2.4 percent. However, the coefficients
for *Disintermediation* for trade credit availability and lease financing availability
are indistinguishable from zero. We therefore interpret these results as providing
additional evidence that CSPP-induced availability of bank credit, and not the overall
market conditions, drives our findings of increased access to financing for SMEs.

4.4.4   Lending Relationships and SME Borrowing

Our results thus far suggest an increase in banks' SME lending and SMEs' bank
borrowing; however, despite their identification benefits and granularity, the tests
above explore banks and borrowers in isolation. In this section, we discuss the results
of our investigation that relies on the bank–borrower links. Specifically, we examine
the borrowings of SMEs as a function of the CSPP exposure of their relationship
banks.

   To shed more light on the intensive margin, we examine the amount of lending by
affected banks to SMEs in the industry-regions with non-zero CSPP interventions
using Amadeus Financials dataset. Given that Amadeus Bankers dataset provides
us with a snapshot of data before and after the introduction of CSPP, we present
our findings in first differences using the intensity of the CSPP exposure as our
heterogeneous treatment variable from the following model:

$$Log(Debt)_{it} = \beta_2 Post\ CSPP_t + \beta_3 Affected\ SME_i \times Post\ CSPP_t + \gamma X_{it} + \alpha_i + \eta_{cjt} + \epsilon_{it}$$
$$(4.3)$$

where *Log(Debt)* is the natural logarithm of the total debt of an SME firm i in
Amadeus Financials from 2013 to 2018. As before, *Post CSPP* is an indicator variable

that equals one for years 2016 onward and zero otherwise. *Affected SME* is based on the bank–borrower links obtained from Amadeus Bankers Dataset (2017 vintage). Since we have information on affected banks, as per our discussion in Section 4.4.2, we can compare SMEs to one another in terms of their relative exposure to the CSPP. Specifically, *Affected SME* is a dummy variable that switches on if an SME's relationship bank has corporate borrowers that received funding under the CSPP. By doing so, we can draw a relatively direct link between disintermediation and SME borrowers themselves.

Another key advantage of these tests is that, because the main variation comes from SMEs' relationship banks, we are able to compare two SMEs in the same country-industry grid in the same period. We do so by adding η_{cjt}, which stands for country-industry-year fixed effects. This additional layer of control is critical to rule out lingering concerns about local shocks and events that affect industries in a country heterogeneously. α_i denotes SME fixed effects to remove potentially confounding effects of time-invariant SME characteristics. Finally, X_{it} is a vector of control variables, including total assets (natural logarithm), profitability (return-on-assets ratio), and the number of employees (natural logarithm).

Panel C of Table 4.1 shows the summary statistics for the Amadeus sample. Each observation is an SME-year. We note that the median SME debt is 3.2 million euros ($= e^{14.976}$). The median firm has total assets of 21.7 million euros, 113 employees, and a return-on-assets ratio of about 4 percent. The inherent skewness in *firm size* and *firm employment* is removed in the logged form.

Our estimates in Table 4.5 indicate an economically and statistically significant effect on debt issuance by affected SMEs from their relationship banks. Economically, SMEs with a relationship that was affected by the CSPP increase their outstanding debt by 3.6 percent (columns (1) and (2)). In monetary terms, given the sample median of SME debt, this increase corresponds to almost €77,750. We note that the inferences in column (3) and (4) echo these findings by partitioning the treatment. In this treatment-intensity setting, we find that more affected SMEs obtain incrementally larger amounts of bank credit. Finally, as shown in columns (2) and (4), there is no differential trend between affected and unaffected SMEs before the treatment. Overall, these findings suggest that SMEs, which borrow from banks affected by the CSPP, raise more debt relative to other SMEs in the same country and industry over the same period.

Having established that financial disintermediation in the corporate sector increases SMEs' access to financing, in additional tests in the Online Appendix, we investigate

whether financial disintermediation has increased the availability of credit in a broader sense. In particular, if banks observe increased availability of funding due to their borrowers relying more on bond financing as a result of the CSPP, we would expect banks to be willing to provide access to financing for new borrowers overall. This implies that banks would be more likely to offer credit to new customers and form new banking relationships. Even though SME lending falls under the same category as corporate lending for most banks, it is not clear whether SMEs would be the first point of substitution for banks affected by the decreased demand for loans from corporate borrowers affected by the CSPP. In particular, prior research argues that banks may increase lending to their existing commercial borrowers (Acharya et al., 2017) or switch to other types of lending such as mortgages or consumer loan (Chakraborty et al., 2019). To shed light on this empirical question, we investigate the formation of new relationships for all potential borrowers using the Amadeus Bankers and Dealogic Deals data (extensive margin).[39]

We first use the Amadeus Bankers dataset to study the relationship formation using the information on private firms, most of which fall into the category of SMEs as defined by the Survey. Given that Amadeus provides us only with a snapshot of the data, we investigate the following model:

$$New \ Relationship \ Formation_i = \beta_1 Disintermediation_{cj} + \gamma X_i + \epsilon_i \qquad (4.4)$$

where i indexes firms, c corresponds to a country and j to an industry. X_{it} is a vector of control variables. *New Relationship Formation* takes the value of one if a firm shows a new relationship with a lender after the introduction of the CSPP with which it does not have a relationship before the program.

Panel A of Online Appendix Table OAIV shows our estimation results. We find that, following the introduction of financial disintermediation in the corporate credit market, affected banks form new banking relationships. These firms, on average, appear to be smaller in size, less profitable and younger. In particular, we find that a 1 percentage point increase in *Disintermediation* increases the likelihood of establishing a new lending relationship by about 1 percent, or 20 percent of the sample mean of a new relationship.[40] This inference is significant, statistically and economically.

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[39]Amadeus collects information on SME banking relationships from a limited number of countries. We are able to perform these tests for borrowers based in Austria, Germany, the Netherlands, Portugal, and Spain, because New Relationship is non-missing and non-degenerate only for these sample countries.

[40]We also limit our sample to companies with fewer than 250 employees and include additional controls for changes in size, employment, profitability, and leverage. Our conclusions continue to hold in this specification (untabulated).

We also analyze new relationship formation using the Dealogic Loans Database, which contains bank lending information, including syndicated and private bank loans. We define several proxies for a new relationship. *Log of a Number of New Relationship Formations* is the natural logarithm of the number of banks with whom a company started a borrower-lender relationship in the current quarter and did not have an active bank relationship in the prior six months. *New Relationship Formation* is an indicator variable that takes the value of one if a company started a new borrower-lender relationship in the current quarter with a bank with which it did not have a banking relationship in the prior six months. *New Loan Originations* is an indicator variable, which takes the value of one if a company obtains a new loan in a given quarter and zero otherwise. *New Relationship Formation (main bank)* is an indicator variable that switches on only if a company signed a new loan deal in the current quarter with the main bank, defined as a bank with an important role, and with whom it did not have an active main bank relationship in the prior six months. *Disintermediation* is defined as the aggregate CSPP purchases by the ECB in the primary market within a country-five-digit NAICS-industry code in a given quarter.

In the Online Appendix, Table OA5, Panel B shows that the number of new relationships (columns 1 and 2) and new loan originations (columns 3 and 4) increase significantly following the introduction of the CSPP. In particular, the larger the exposure to disintermediation in a given country-industry grid, the more likely a firm to establish a new borrowing relationship with a bank. In particular, a 1 percentage point increase in *Disintermediation* results in a 2.7 percent increase in the number of new relationships and a 1.6 percent increase in the likelihood of forming at least one new banking relationship.

Similarly, a 1 percentage point increase in CSPP intensity also results in a 1.4 percent increase in new loan originations and a 1.5 percent increase in forming a relationship with a new main (or lead arranger) bank.[41] Overall, our findings using Amadeus Bankers and Dealogic data suggest that banks exposed to the CSPP increase their lending to SMEs and form new lending relationships with new borrowers, including SMEs (extensive margin).

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[41]We find that smaller companies are more likely to create new lending relationships in the post-CSPP period as coefficients on their overall total loans outstanding (as captured by Dealogic loans data) are negative. We use the natural logarithm of total loans outstanding for a borrower in the prior period as a proxy for size as we do not directly observe borrower size or other borrower characteristics in Dealogic.

4.4.5 Loan Characteristics

Prior literature has identified one potential concern with banks responding to a macroeconomic stimulus by increasing loans to poorly performing relationship borrowers, the so-called "zombie lending" (e.g., Acharya et al., 2019; Bruche and Llobet, 2014). Using a detailed loan-level data from the ECB's LLD Initiative, we investigate whether banks affected by the CSPP, instead of offering new credit to new borrowers, continue to lend to their existing borrowers at preferential rates. Consistent with Acharya et al. (2019), we define zombie or forbearance loans as loans to existing customers which exhibit high loss given default (above the sample median) and low interest rates (below the sample median). We therefore estimate the following model:

$$Loan\ Characteristic_k = \beta_2 Disintermediation_{rjt} + \gamma X_k + \nu_{rj} + \lambda_{rt} + \sigma_{jt} + \alpha_i + \pi_p + \epsilon_k$$
(4.5)

where each observation is an individual loan, indexed by k, i indexes firms, t indicates half-year, r corresponds to a region, j to an industry, i to a borrower and p to ABS pools. ν_{rj} are region-industry fixed effects, λ_{rt} are region-time fixed effects, σ_{jt} are industry-time fixed effects, α_i are borrower fixed effects and π_p are ABS pool fixed effects. γX_k is a vector of loan control variables consisting of amount and maturity. The granularity of this data allows us to adopt an even more restrictive fixed effects structure. In addition to the multidimensional region-time, industry-region, and industry-time fixed effects, we can include indicators for borrowers and asset-backed security (ABS) pools, which are a subset of bank fixed effects. Here, the region dimension is a finer classification than the country dimension, defined as the European Commission's nomenclature of territorial units for statistics.

The two dependent variables in these tests are *Interest Rate* and *Zombie Lending*. *Interest Rate* is the cost of credit charged, with a sample average of 2.156 percent (Table 4.1, Panel D). *Zombie Lending* is an indicator variable that switches on if the spread charged on the contract is low (i.e., below the sample median) while the bank's loss given default estimate on the same loan is high (i.e., above the sample median). This empirical definition captures the spirit of the "extend and pretend" type of lending behavior, which is also referred to as zombie, forbearance, or evergreen lending, in keeping with Acharya et al. (2019) and Bruche and Llobet (2014). On average, we classify about 20 percent of the sample contracts as *Zombie Lending*.

In Panel A of Table 4.6, we present the results of the CSPP's impact on loan pricing. As we do not observe the underlying borrower characteristics, we use a tight fixed effects structure to take into account any unobserved heterogeneity across

industry-time, region-time, industry-region, ABS pool, and borrowers. We find that banks decrease interest rates on new loans following the introduction of corporate-sector financial disintermediation. In particular, a 1 percentage point increase in the magnitude of CSPP exposure leads to a 0.019 to 0.096 percentage point decrease in interest rates on new loans for smaller borrowers. These coefficient estimates are economically meaningful relative to the sample standard deviation of interest charged, which is 0.44 percentage points. This inference is in line with the ECB's stated objectives: financial disintermediation in the corporate sector lowers financing costs for SMEs by providing extra funds to the commercial credit markets and by enhancing liquidity.

In Panel B of Table 4.6, we find that the likelihood of zombie lending decreases following the introduction of the CSPP. In particular, a 1 percentage point increase in *Disintermediation* results in an 8.7 to 16.9 percent decrease in instances of zombie lending. This inference also corroborates our earlier findings on the formation of new bank-SME relationships.

In addition to the LLD results, we also check whether SMEs report lower interest rates in their Survey responses following the introduction of the CSPP. Using the same specification as in equation (4.2) above, we define *SME Interest Rate Decreased* as an indicator variable, which takes the value of one if SMEs respond that their interest rates decreased in the prior six months (question Q2_d of the Survey). As Table 4.1, Panel B shows, 29 percent of our sample saw a decrease in interest rates over the whole period. In Table 4.6, Panel C, we find positive and statistically significant results. In particular, the likelihood of getting a lower interest rate increases in the magnitude of the industry-country exposure to the CSPP by 3.9 percent in the full period. It also increases by 2.6 percent if we consider only the post-CSPP implementation period specification, albeit our results are statistically weaker.

The Survey also allows us to capture whether SMEs refused to take credit because the offered interest rate was too high. In columns (3) and (4) of Table 4.6, Panel C we show that the likelihood of SMEs refusing credit because the offered interest rate is too high decreases by 0.4 to 0.6 percent, which is economically and statistically significant. Overall, our findings for loan characteristics suggest that banks extend new credit on better terms to new borrowers, including SMEs, and reduce rolling over credit on preferential terms to poor credit quality borrowers.

4.4.6   Real Effects

Having established that financial disintermediation in the corporate credit market enhances SMEs' access to financing, new banking relationships, and improved credit terms, we turn to our last set of results on spillover effects. In particular, we are interested in whether increased access to financing leads SMEs to fund real activities such as increased investment and hiring. In these tests, we rely on the Survey data and perform a cross-sectional analysis using post-CSPP data and a quasi-DiD using the full sample data. These approaches are in the spirit of the models for credit access defined in equation (4.2). Similar to our first set of analyses, we also control for SME size, age, employment, profitability, and credit quality and include industry-time, country-time, and industry-country fixed effects.

Our main dependent variables that capture the real effects of CSPP purchases are based on answers to the variants of the Survey question (Q6a) about the purpose for which the financing is obtained. In particular, *Purpose: capital investment*, is an indicator that switches on only if the purpose of financing is fixed assets. *Purpose: employment*, is an indicator that switches on only if the purpose of financing is hiring. *Purpose: working capital,* is an indicator that switches on only if the purpose of financing is working capital. Finally, *Purpose: refinancing,* is an indicator that switches on only if the purpose of obtaining financing is to refinance.

These responses are not mutually exclusive, as the borrower can pick multiple loan purposes. Nor are they commonly exhaustive because the respondents can choose "other" or "do not know" as alternative options. As can be seen in Panel B of Table 4.1, investment and employment reasons are given 61.4 percent and 8.3 percent of the time, whereas 40.6 percent and 16.9 percent of loan applications are for working capital and refinancing purposes, respectively.

Table 4.7 presents our findings. As with the SMEs' credit access results in Table 4.3, models (1) through (4) investigate the impact of corporate-sector financial disintermediation using the full sample, while models (5) through (8) focus on the post-CSPP period. Similar to our tests for credit access, we control for SME size, age, credit quality and profitability across all specifications. We also consider potentially unobserved heterogeneity by including a tight fixed-effects structure by industry-time, country-time, and country-industry.

In both specifications, we observe that SMEs use increased access to financing to fund their real activities such as capital investments and increasing employment. This is an important takeaway, as our paper provides a direct link between the CSPP and real activities by looking exclusively at SMEs that apply for bank credit. Economically,

a 1 percentage point increase in exposure to the CSPP results in a 4.1–4.3 percent increase in affected SMEs' likelihood to invest funds in capital projects and a 2.3–2.7 percent increase in hiring new employees. SMEs, however, are 1.7 to 2.6 percent less likely to use the new funds to finance their working capital and 2.4 to 2.8 percent less likely to use the funds to refinance their existing loans. Overall, our findings indicate that increased access to financing leads to positive real activities for SMEs, suggesting positive real effects of corporate-sector financial disintermediation on the SME sector.

In the final stage of our analysis, we focus on the real effects of financial disintermediation on banks' operations. The shock that induced banks to steer toward lending to the SME sector may have also triggered a change in their lending technology and operational features. To shed light on this issue, we look at the number of bank branches and employees by using SNL Financial data. Since we observe this data on an annual frequency, we conduct our tests on a sample spanning 2014–2017. The numbers presented in the natural logarithm form in Panel E of Table 4.1 suggest that the median bank in this sample has 660 branches and 7,772 employees. Table 4.8, Panel A shows our main findings. The estimates on *Affected Bank × Post CSPP* vary between 5.0 and 8.3 percent and are statistically significant. This suggests that the switch to SME lending necessitates banks to increase their access to small businesses, which need physical interactions to obtain a loan.

Disintermediation may result in banks' lending to riskier borrowers, chasing higher yields and, as a result, decreasing the quality of their loan portfolios. Using EBA data, we test whether non-performing loans (NPLs) for banks' SME portfolios have significantly increased following disintermediation. Using a similar model as in equation (4.1) and changing our dependent variable to the percentage of SME NPLs relative to banks' overall SME loan portfolio (*SME NPLs % of SME Loans*), we find that the quality of SME loan portfolios has not changed significantly for affected banks after financial disintermediation in the corporate sector. Table 4.8, Panel B shows that following the introduction of the CSPP, the coefficients on loan portfolio quality for affected banks are statistically insignificant and negative. Finally, we test whether banks' default risk changes as a result of their increased exposures to the CSPP and SME lending. As a timely and market-based metric of credit risk, we examine banks' credit default swaps (CDS). The estimates in columns (3) and (4) in Panel B of Table 4.8 suggest no significant changes in banks' default risk using CDS spreads following financial disintermediation.

Overall, our findings are consistent with financial disintermediation in the corporate sector having positive real effects on SMEs through increased access to bank financing,

as SMEs use new funds to invest into real activities such investments and hiring. We also find that banks invest in opening more branches and hiring more employees. Furthermore, we do not find evidence that the quality of the overall SME loan portfolios or bank default risk deteriorates for affected banks.

4.4.7   Dynamic Effects of the CSPP

Thus far, our findings have focused on the relatively short-term effects of disintermediation on credit access for SMEs. In this section, we extend our analyses to investigate whether the changes we document are due to banks' short-term portfolio reallocations or a longer-term structural change in bank lending relationships with SMEs. As the CSPP is still a relatively new program, we focus our analyses on banks' exposures tests for which we can get semi-annual data until December 2017 and the Survey data with waves covering the period until June 2018. Table 4.9 presents our findings for an extended sample for SME exposures by breaking down the interaction effect into three components of the first, second and third half-years after the introduction of the CSPP using the semi-annual EBA transparency exercise data. As Panel A of Table 4.9 shows, we find that the positive effect on SME exposures for banks affected by disintermediation is the strongest in the second period after the introduction of CSPP and dissipates by the third period. These results are statistically and economically significant and suggest that the increase in lending to SMEs might be a medium rather than a long-term effect. In untabulated results, we also find that the quality of affected banks' loan portfolios (based on our NPL measures) does not change significantly.

Panel B of Table 4.9 presents the Survey data results and similarly splits out the interaction effect into two survey waves (first year and second year). Column (1) shows the results for our main specification for SME access to bank credit. We find that SME access to bank financing increases in economically and statistically significant ways for both Survey waves. However, the magnitude of the effect is smaller in the second wave, which is consistent with our findings for the SME exposures results in Panel A. We also find that the real effects are predominantly coming from the first wave of the Survey. Finally, we also observe that the increase in SME borrowing from banks affected by disintermediation is significant only in the short term (Panel C of Table 4.9). Overall, these findings suggest that while the effects we document are positive, they might be somewhat short-lived and not have a structural impact on SMEs access to credit and ability to invest into real activities in the long run.

4.5   Conclusion

What is the role of banking regulation in credit creation to small businesses, which are essential for the economy yet widely regarded as underserved by banks? Could the shrinking of one credit market for banks (e.g., large corporate bank debt market) prompt banks to increase lending to small businesses? If so, what are the channels through which financial disintermediation in one sector facilitates financial intermediation in another? To answer these important economic questions, we examine the European Central Bank's Corporate Sector Purchase Programme, as a plausibly exogenous increase in financial disintermediation in the corporate sector.

We overcome a variety of empirical challenges by utilizing regulatory disclosures, SME credit access surveys, relationship banking information, and individual SME loan contracts. Overall, we provide evidence that SMEs affected by financial disintermediation enjoy a relative increase in the amount of bank credit, especially from lenders that faced liquidity constraints. We also find that affected SMEs are more likely to forge new borrowing relationships, use the additional funds for investment and hiring purposes, and pay lower interest costs, consistent with positive real effects of corporate-sector financial disintermediation. However, these positive effects disappear in the long term, casting doubt on the efficacy of financial disintermediation.

Overall, our conclusions contribute to the broader literature on SME financing, as well as the ongoing debate about the economy-wide effects of financial disintermediation. We use a specific example of an unconventional monetary policy intervention to study the impact the large corporate borrowers' sector has on the financial intermediation for SMEs. However, it is important to note that not all such interventions have desirable or long-lasting outcomes. Moreover, our paper does not explore potentially adverse or unintended consequences of regulator-led financial disintermediation. Future research could shed light on these important questions and help extend the literature on the potential regulatory policy tools that aim at enhancing SME financing.

Table 4.1: Descriptive statistics

This table presents the sample statistics. Panel A presents summary statistics for the EBA sample in which each observation is a bank-half-year. Panel B lists the summary statistics for the ECB Credit Access Survey for SMEs (SAFE), in which each observation is a firm-half-year. Panel C presents the summary statistics for the Dealogic sample, in which each observation is a firm-quarter. Panel D includes the summary statistics for the Amadeus sample, in which each observation is a firm. Panel E shows the summary statistics for the LLD sample, in which each observation is a loan contract. Panel F includes the summary statistics for the SNL sample, in which each observation is a bank-year. Only mean values are presented for indicator variables. All variables are defined in Appendix A.

Panel A. EBA

	Mean	stdev	p10	p50	p90	N
Affected Bank × Post CSPP	0.256					386
Affected Bank	0.500					386
Post CSPP	0.492					386
Affected Bank × Pre CSPP	0.122					386
SME Exposure (%)	8.220	7.982	0.0	7.135	18.488	386
CORP Exposure (%)	12.882	12.237	1.739	9.758	27.560	386
Balance Sheet Illiquidity	2.486	1.115	1.0	2.0	4.0	368
Capital Constraints	2.522	1.127	1.0	3.0	4.0	364
SME NPLs (% of SME Loans)	5.137	7.835	0.017	2.258	13.556	386

Panel B. ECB Credit Access Survey

	Mean	stdev	p10	p50	p90	N
Disintermediation (%)	0.085	0.316	0.0	0.0	0.180	11,180
SME Access to Bank Credit	0.802	0.398	0.0	1.0	1.0	11,180
SME Interest Rate Decreased	0.288					10,927
Borrower Refused Because Interest Rate Was High	0.012					12,587
Purpose: capital investment	0.614					11,180
Purpose: employment	0.083					11,180
Purpose: working capital	0.406					11,180
Purpose: refinancing	0.169					11,180
SME size	2.184	1.059	1.0	2.0	4.0	11,180
SME age	3.840	0.478	3.0	4.0	4.0	11,180
SME credit quality	2.264	0.624	2.0	2.0	3.0	11,180
SME profitability growth	2.054	0.815	1.0	2.0	3.0	11,180
Bond issuance	2.583	6.653	0.0	0.0	6.978	11,180
Bank Loan Availability	0.263					5,606
Trade Credit Availability	0.190					5,606
Lease Financing Availability	0.211					3,333

Panel C. Amadeus

	Mean	stdev	p10	p50	p90	N
Affected SME × Post CSPP	0.160					193,494
Affected SME	0.333					193,494
Post CSPP	0.499					193,494
Affected SME × Pre CSPP	0.087					193,494
Log(debt)	14.585	2.663	11.248	14.976	17.372	193,494
Log(total assets)	17.033	1.347	15.505	16.892	18.816	193,494
Return on assets (%)	5.273	9.010	-2.955	4.052	15.911	193,494
Log(number of employees)	4.651	1.473	2.890	4.727	6.449	193,494

Panel D. ECB Loan-level Data

	Mean	stdev	p10	p50	p90	N
Disintermediation (%)	0.131	0.082	0.096	0.121	0.270	327,452
Interest Rate (%)	2.156	0.444	2.170	2.190	2.240	327,452
Zombie Lending (%)	20.295					327,452

Panel E. SNL Data on Bank Branches and Employees

	Mean	stdev	p10	p50	p90	N
Affected Bank × Post CSPP	0.269					271
Affected Bank	0.520					271
Post CSPP	0.502					271
Affected Bank × Pre CSPP	0.133					271
Pre CSPP	0.258					271
Log(Number of Branches)	6.049	1.812	3.912	6.492	7.826	271
Log(Number of Employees)	9.178	1.224	7.632	8.958	10.759	271
Log(Total Assets)	18.268	1.294	16.705	18.098	20.181	271

Table 4.2: Effects of the CSPP on banks' exposures to the SME sector

This table presents the results of the OLS estimation of the impact of the CSPP on systemically important European Banks using the EBA Transparency Exercise data, which reports banks' SME and corporate exposures. Panel A presents the main treatment effect and Panel B presents the cross-sectional variation in treatment effects. *SME Exposure (CORP Exposure)* corresponds to a given bank's SME (Corporate) loan assets relative to total exposures. *Affected Bank* is an indicator that switches on only if the bank has at least one large corporate relationship borrower (as per Dealogic), whose bonds are purchased under the CSPP. As a robustness check, *Affected Bank* is a continuous variable in column (6) of Panel A and defined as the total primary market purchases by the ECB of that the bank's clients divided by the value of this bank's loans outstanding. *Post CSPP* is an indicator variable that switches on for 2016H2 and 2017H1. *Pre CSPP* is an indicator variable that switches on for 2015H2 and 2016H1. *I* corresponds to a quartile rank which takes the value of one if a bank is in the top quartile of liquidity (capital) and four if the bank is in a lower quartile, based on the proportion of liquid assets (Tier 1 regulatory capital) before the introduction of CSPP in June 2016. As denoted in the table, *T*-statistics (reported in parentheses) are robust to within-bank correlation and heteroscedasticity. *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

Panel A. The Main Treatment Effect

	(1)	(2)	(3)	(4)	(5)	(6)
	SME Exposure	SME Exposure	SME Exposure	SME Exposure	SME Exposure	SME Exposure
Affected Bank	-2.257	-2.630				
	(-1.41)	(-1.50)				
Affected Bank × *Post CSPP*	2.279***	2.027***	0.947**	1.088**	1.442**	0.119*
	(2.84)	(3.10)	(2.20)	(2.51)	(2.24)	(1.79)
CORP Exposure				0.135	0.136	0.131
				(1.48)	(1.49)	(1.43)
Affected Bank × *Pre CSPP*					0.707	0.294
					(1.29)	(0.68)
Observations	386	386	386	384	384	384
Adjusted R-squared	-0.001	0.381	0.947	0.949	0.949	0.948
Definition of *Affected Bank*	Indicator	Indicator	Indicator	Indicator	Indicator	Continuous
Time FE	Y	Y	Y	Y	Y	Y
Country FE	N	Y	N	N	N	N
Bank FE	N	N	Y	Y	Y	Y

Panel B. Cross-sectional Variation in Treatment Effect

	(1)	(2)
	I = Liquidity Constraints	I = Capital Constraints
	SME Exposure	*SME Exposure*
Affected Bank × Post CSPP × I	0.396*	-0.094
	(1.84)	(-0.05)
Affected Bank × Post CSPP	1.459***	0.804
	(2.65)	(1.13)
Post CSPP × I	-0.040	1.445
	(-0.44)	(0.82)
CORP Exposure	0.191**	0.132*
	(2.60)	(1.72)
Observations	336	340
Adjusted R-squared	0.950	0.958
Time FE	Y	Y
Bank FE	Y	Y

Table 4.3: Spillover effects on credit access for SMEs

This table presents the results of OLS regressions of SME credit access on the intensity of the CSPP. The unit of observation is at the firm and Survey-wave level. *SME Access to Bank Credit* captures SMEs' ability to raise financing through loan applications (Panel A). This variable varies between 1 (full financing received upon application) and 0 (less than the full amount received). *Disintermediation* is a continuous variable measured as the aggregate corporate bond purchases by the ECB in the primary market within a country-SAFE industry during the corresponding wave period and deflated by the total value of all bonds outstanding in the country-SAFE industry. This variable is zero for the survey observations before June 2016 and for SAFE industries, which do not observe eligible corporate bond purchases in the post-June 2016 waves. Models (1) to (3) include the full sample of observations in a quasi-DiD setting. Models (4) to (6) include a post-treatment period only. Panel B presents the results of regressions, which uses survey weights (columns (1) and (2)) and SME fixed effects (columns (3) and (4)). *T*-statistics (reported in parentheses) are robust to within-country correlation and heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: SMEs' Loan Applications

	(1)	(2)	(3)	(4)	(5)	(6)
	Full period			Post-CSPP only		
	SME Access to Bank Credit			SME Access to Bank Credit		
Disintermediation	0.041***	0.024**	0.032**	0.028**	0.021*	0.030*
	(2.72)	(2.09)	(2.46)	(2.19)	(1.78)	(1.98)
SME size	0.042***	0.036***	0.037***	0.044***	0.035***	0.035***
	(9.89)	(8.39)	(8.34)	(7.24)	(6.72)	(6.35)
SME age	0.042***	0.045***	0.044***	0.050***	0.054***	0.052***
	(4.59)	(5.04)	(4.88)	(3.19)	(3.51)	(3.32)
SME credit quality	0.061***	0.064***	0.065***	0.054***	0.060***	0.060***
	(7.48)	(8.27)	(8.15)	(4.72)	(5.52)	(5.46)
SME profitability growth	0.036***	0.034***	0.033***	0.030***	0.028***	0.028***
	(6.08)	(6.96)	(6.40)	(3.48)	(5.00)	(4.58)
Observations	11,180	11,180	11,180	5,632	5,632	5,632
Adjusted R-squared	0.040	0.086	0.091	0.038	0.091	0.093
Wave FE	N	Y	N	N	Y	N
Industry FE	N	Y	N	N	Y	N
Country FE	N	Y	N	N	Y	N
Industry-wave FE	N	N	Y	N	N	Y
Country-wave FE	N	N	Y	N	N	Y
Industry-country FE	N	N	Y	N	N	Y

Panel B: Robustness

	(1)	(2)	(3)	(4)
	Full period	Post-CSPP only	Full period	Post-CSPP only
	Specification: Use survey weights		Specification: Include SME fixed effects	
Disintermediation	0.035**	0.031*	0.048**	0.060*
	(2.39)	(1.89)	(2.11)	(1.78)
SME size	0.038***	0.045***	-0.033	0.014
	(6.70)	(4.99)	(-0.98)	(0.31)
SME age	0.033***	0.032	-0.007	-0.013
	(2.82)	(1.58)	(-0.18)	(-0.06)
SME credit quality	0.063***	0.059***	0.022	0.017
	(6.81)	(4.53)	(1.56)	(0.51)
SME profitability growth	0.040***	0.040***	0.007	0.007
	(5.71)	(4.13)	(0.60)	(0.29)
Observations	11,180	5,632	11,180	5,632
Adjusted R-squared	0.102	0.106	0.511	0.580
Industry-wave FE	Y	Y	Y	Y
Country-wave FE	Y	Y	Y	Y
Industry-country FE	Y	Y	Y	Y

Table 4.4: Financial disintermediation vs. changing economic fundamentals

This table presents the results of OLS regressions of SME credit access on the intensity of the CSPP. The unit of observation is at the firm and Survey-wave level. *SME Access to Bank Credit* captures SMEs' ability to raise financing through loan applications (Panel A). This variable varies between 1 (full financing received upon application) and 0 (less than the full amount received). *Disintermediation* is a continuous variable measured as the aggregate corporate bond purchases by the ECB in the primary market within a country-SAFE industry during the corresponding wave period and deflated by the total value of all bonds outstanding in the country-SAFE industry. In Panel A, *Bond issuance* is total primary market issuances within a country-SAFE industry during the corresponding wave time period deflated by the total value of all bonds outstanding in the country-SAFE industry (columns (1) and (2)). Column (3) presents the results of the placebo test for the main findings presented in Table 4.3. In Panel B *Bank Loan Availability* is an indicator variable, which takes the value of one if SMEs perceive an increase in funding through the availability of new loans. *Trade Credit Availability* (*Lease Financing Availability*) is an indicator variable, which takes the value of one if SMEs perceive an increase in the availability of trade credit (lease financing). Variables are defined in Appendix A. *T*-statistics (reported in parentheses) are robust to within-country correlation and heteroscedasticity. *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

Panel A. Main Results after Controlling for Economic Activities and Placebo Specifications

	(1)	(2)	(3)
	Controlling for Bond Issuance		Placebo Test
	SME Access to Bank Credit	SME Access to Bank Credit	SME Access to Bank Credit
Disintermediation		0.027*	-0.007
		(1.88)	(-1.66)
SME size	0.037***	0.037***	0.038***
	(8.29)	(8.32)	(5.76)
SME age	0.044***	0.044***	0.048***
	(4.89)	(4.88)	(5.57)
SME credit quality	0.065***	0.065***	0.098***
	(8.15)	(8.15)	(11.98)
SME profitability growth	0.033***	0.033***	0.030***
	(6.43)	(6.42)	(5.77)
Bond issuance	0.001**	0.001	
	(2.13)	(1.16)	
Observations	11,180	11,180	10,465
Adjusted R-squared	0.091	0.091	0.143
Industry-wave FE	Y	Y	Y
Country-wave FE	Y	Y	Y
Industry-country FE	Y	Y	Y

Panel B. SMEs' Perceptions on Availability of Funds

	(1)	(2)	(3)
	Bank Loan Availability	Trade Credit Availability	Lease Financing Availability
Disintermediation	0.024*	0.006	0.005
	(1.70)	(0.51)	(0.22)
SME size	0.027***	0.008	0.017*
	(4.29)	(1.17)	(1.82)
SME age	-0.031**	-0.036***	-0.029*
	(-2.30)	(-2.86)	(-1.71)
SME credit quality	0.100***	0.082***	0.076***
	(11.06)	(9.04)	(6.28)
SME profitability growth	0.063***	0.033***	0.047***
	(7.24)	(4.59)	(4.78)
Observations	5,606	5,606	3,333
Adjusted R-squared	0.073	0.052	0.030
Industry-wave FE	Y	Y	Y
Country-wave FE	Y	Y	Y
Industry-country FE	Y	Y	Y

Table 4.5: Evidence from bank-borrower links

This table presents the results of the OLS estimation of the variation in SMEs' borrowing with their relationship banks around the CSPP. *Log(Debt)* is the natural logarithm of the total debt (Amadeus item totdebt) of the SME. *Affected SME* is an indicator variable that switches on only if the SME has at least one relationship bank whose corporate borrowers received ECB funding. In columns (3) and (4), *Affected SME* is used as median ranks in the spirit of a treatment intensity estimation. *Post CSPP* switches on for years 2016 and after. *Pre CSPP* switches on for the year of 2015. Controls include SME total assets (log), return on assets (%), and the number of employees (log). Country-Industry-Year FE are the three-dimensional fixed effects denoting an SME's country, two-digit NAICS industry, and year of financials. *T*-statistics (reported in parentheses) are robust to within-firm correlation and heteroscedasticity. *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Log(Debt)	*Log(Debt)*	*Log(Debt)*	*Log(Debt)*
Affected SME × *Post CSPP*	0.036**	0.035**	0.023**	0.025**
	(2.54)	(2.02)	(2.24)	(1.99)
Affected SME × *Pre CSPP*		-0.001		0.004
		(-0.10)		(0.35)
Observations	193,494	193,494	193,494	193,494
Adjusted R-squared	0.837	0.837	0.837	0.837
Definition of *Affected Bank*	Indicator	Indicator	Continuous	Continuous
Controls	Y	Y	Y	Y
SME FE and Country-Industry-Year FE	Y	Y	Y	Y

Table 4.6: Effects of the CSPP on loan characteristics

This table presents the results of the OLS estimation of the impact of the CSPP on loan characteristics using LLD data for Panels A and B and Survey data for Panel C. *Interest Rate* corresponds to the interest rates charged on the loans (in percentage points). *Zombie Lending* is an indicator variable that switches on only if the loan's *Interest Rate* is below the sample median, and *Loss Given Default* estimate is above the sample median. Consistent with the structure of the ECB loan-level data, *Industry* is defined as one-digit Nomenclature of Economic Activities (NACE) codes and the *Region* as Nomenclature of Territorial Units for Statistics (NUTS), which vary within countries. Loan controls include *Log (Amount),* the natural logarithm of the total amount of loans offered and *Loan Maturity,* the number of months until the loan matures. Panel C presents Survey responses to the question of whether interest rates on new loans changed or SMEs refused credit. *SME Interest Rate Decreased* takes the value of one if SMEs respond that their interest rates on new loans decreased in the past six months and zero otherwise. *Borrower Refused Because Interest Cost Was High* takes the value of one if the respondent SME states that it refused the offer of bank credit because the offered rate was too high. Models (1) and (3) include the full sample of observations in a DiD setting. Models (2) and (4) include a post-treatment period only. Variables are defined in Appendix A. *T*-statistics (reported in parentheses) are robust to within-ABS-Deal correlation and heteroscedasticity (Panels A and B) and within-country correlation and heteroscedasticity (Panel C). *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

Panel A. SMEs' Cost of Debt

	(1)	(2)	(3)	(4)
	Interest Rate	*Interest Rate*	*Interest Rate*	*Interest Rate*
Disintermediation	-0.096**	-0.094**	-0.035***	-0.019**
	(-2.65)	(-2.77)	(-5.21)	(-2.66)
Observations	327,452	327,452	327,452	327,452
Adjusted R-squared	0.556	0.566	0.888	0.889
Industry-time FE	Y	Y	Y	Y
Region-time FE	Y	Y	Y	Y
Industry-region FE	Y	Y	Y	Y
ABS Pool FE	N	Y	Y	Y
Borrower FE	N	N	Y	Y
Loan Controls	N	N	N	Y

Panel B. Recipients of Funds

	(1)	(2)	(3)	(4)
	Zombie	*Zombie*	*Zombie*	*Zombie*
	Lending	*Lending*	*Lending*	*Lending*
Disintermediation	-0.115*	-0.102	-0.087***	-0.169***
	(-2.03)	(-1.60)	(-6.73)	(-5.73)
Observations	327,452	327,452	327,452	327,452
Adjusted R-squared	0.293	0.300	0.396	0.396
Industry-time FE	Y	Y	Y	Y
Region-time FE	Y	Y	Y	Y
Industry-region FE	Y	Y	Y	Y
ABS Pool FE	N	Y	Y	Y
Borrower FE	N	N	Y	Y
Loan Controls	N	N	N	Y

Panel C: SMEs' Interest Rates

	(1)	(2)	(3)	(4)
	Full period	Post-CSPP only	Full period	Post-CSPP only
	SME Interest Rate Decreased		Borrower Refused Because Interest Cost Was High	
Disintermediation	0.039**	0.026*	-0.006**	-0.004**
	(2.63)	(1.87)	(-2.58)	(-2.06)
SME size	0.080***	0.062***	-0.004***	-0.004**
	(10.17)	(5.43)	(-4.71)	(-2.68)
SME age	0.042***	0.041***	-0.001	0.002
	(4.34)	(3.13)	(-0.61)	(0.90)
SME credit quality	0.086***	0.101***	0.000	0.002
	(7.16)	(5.39)	(0.17)	(0.75)
SME profitability growth	0.041***	0.027***	-0.000	-0.000
	(7.27)	(4.66)	(-0.32)	(-0.34)
Observations	9,558	4,881	12,587	6,233
Adjusted R-squared	0.115	0.079	0.006	0.003
Industry-wave FE	Y	Y	Y	Y
Country-wave FE	Y	Y	Y	Y
Industry-country FE	Y	Y	Y	Y

Table 4.7: Real effects of the CSPP on European SMEs

This table presents the results of the OLS estimation of the impact of the CSPP on SMEs using the Survey data and the responses that correspond to the purpose of obtaining new financing. *Purpose: Capital Investment* is an indicator that switches on only if the purpose of financing is a capital investment. *Purpose: Employment* is an indicator that switches on only if the purpose of financing is hiring. *Purpose: Working capital* is an indicator that switches on only if the purpose of financing is working capital. *Purpose: Refinancing* is an indicator that switches on only if the purpose of financing is refinancing. Models (1) to (4) include the full sample of observations in a DiD setting. Models (5) to (8) include a post-treatment period only. Variables are defined in Appendix A. *T*-statistics (reported in parentheses) are robust to within-country correlation and heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full period				Post-CSPP only			
	Purpose: Capital Investment	Purpose: Employment	Purpose: Working Capital	Purpose: Refinancing	Purpose: Capital Investment	Purpose: Employment	Purpose: Working Capital	Purpose: Refinancing
Disintermediation	0.041***	0.027***	-0.017	-0.028***	0.043***	0.023**	-0.026**	-0.024***
	(3.00)	(2.97)	(-1.30)	(-3.70)	(4.51)	(2.32)	(-2.26)	(-3.61)
SME size	0.033***	-0.014***	0.037***	-0.008	0.030***	-0.012***	0.045***	0.000
	(5.29)	(-4.85)	(5.89)	(-1.49)	(3.44)	(-2.83)	(5.24)	(0.00)
SME age	0.014	-0.008	-0.028***	-0.002	0.015	-0.010	-0.028	-0.010
	(1.54)	(-1.31)	(-2.69)	(-0.30)	(0.99)	(-1.34)	(-1.53)	(-0.91)
SME credit quality	0.041***	0.001	-0.023***	-0.024***	0.036***	0.002	-0.016	-0.012
	(5.25)	(0.28)	(-3.32)	(-3.47)	(3.54)	(0.34)	(-1.50)	(-1.41)
SME profitability growth	0.038***	0.002	-0.010*	-0.023***	0.033***	0.006	-0.013*	-0.031***
	(6.84)	(0.47)	(-1.67)	(-5.28)	(4.19)	(1.16)	(-1.73)	(-5.25)
Observations	11,180	11,180	11,180	11,180	5,632	5,632	5,632	5,632
Adjusted R-squared	0.105	0.019	0.132	0.093	0.099	0.025	0.134	0.103
Industry-wave FE	Y	Y	Y	Y	Y	Y	Y	Y
Country-wave FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry-country FE	Y	Y	Y	Y	Y	Y	Y	Y

Table 4.8: Real effects of the CSPP on European Banks

This table presents the results of the OLS estimation of the impact of the CSPP on systemically important European Banks. *Affected Bank* is an indicator that switches on only if the bank has at least one large corporate relationship borrower (as per Dealogic), whose bonds are purchased under the CSPP. In Panel A, per the frequency of data provided by SNL, each observation is a bank-year, and the sample period is 2014–2017. *Post CSPP* is an indicator variable that switches on for years 2016 and 2017. *Pre CSPP* is an indicator variable that switches on for 2015. *Number of Branches (Number of Employees)* corresponds to a given bank's average annual number of branches (full-time employees). In Panel B, per the frequency of EBA Transparency Exercise, each observation is a bank-year. *SME NPLs (% of SME Loans)* corresponds to a given bank's non-performing loans in the SME sector relative to their total SME lending. *CDS Spread* is the half-yearly average of the premium charged on a bank's five-year CDS contract, presented in percentage points. *Post CSPP* is an indicator variable that switches on for 2016H2 and 2017H1, and *Pre CSPP* is an indicator variable that switches on for 2016H1. T-statistics are robust to within-bank correlation and heteroscedasticity. *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

Panel A. Banks' Operations and Business Model

	(1)	(2)	(3)	(4)
	Log(Number of Branches)	*Log(Number of Branches)*	*Log(Number of Employees)*	*Log(Number of Employees)*
Affected Bank × *Post CSPP*	0.050**	0.060*	0.067***	0.083***
	(2.03)	(1.75)	(2.95)	(2.77)
Affected Bank × *Pre CSPP*		0.019		0.031
		(0.73)		(1.54)
Observations	271	271	271	271
Within R-squared	0.031	0.038	0.026	0.028
Cluster	Bank	Bank	Bank	Bank
Bank and Time FE	Yes	Yes	Yes	Yes

Panel B. Loan Portfolio and Credit Quality

	SME NPLs (% of SME Loans)	*SME NPLs (% of SME Loans)*	*CDS Spreads (in pct. points)*	*CDS Spreads (in pct. points)*
Affected Bank × *Post CSPP*	-0.224	-0.330	0.501	1.475
	(-0.15)	(-0.21)	(0.34)	(0.88)
Affected Bank × *Pre CSPP*		-0.212		0.396
		(-0.58)		(0.55)
Observations	341	341	198	198
Adjusted R-squared	0.953	0.953	0.808	0.875
Controls	Y	Y	Y	Y
Bank and Time FE	Y	Y	Y	Y

Table 4.9: Dynamic effects of the CSPP on European Banks

This table presents the results of the OLS estimation of the impact of the CSPP on systemically important European Banks (Panel A) and on SMEs using the Survey data and the responses that correspond to the purpose of obtaining new financing (Panel B) over time. *Affected Bank* is an indicator that switches on only if the bank has at least one large corporate relationship borrower (as per Dealogic), whose bonds are purchased under the CSPP. In Panel A, per the frequency of data provided by SNL, each observation is a bank-year, and the sample period is 2014–2017. *Post CSPP* (first half-year) is an indicator variable that switches on for December 2016, *Post CSPP (second half-year)* switches on for June 2017 and *Post CSPP (third half-year)* switches on for December 2017. In Panel B, *Disintermediation* is a continuous variable measured as the aggregate corporate bond purchases by the ECB in the primary market within a country-SAFE industry during the corresponding wave period and deflated by the total value of all bonds outstanding in the country-SAFE industry. *Post CSPP (first year)* is an indicator variable that switches on for 2016H2 -and 2017H1 and *Post CSPP (second year)* switches on for 2017H2-2018H1. Panel C presents the results of the OLS estimation of the variation in SMEs' borrowing with their relationship banks around the CSPP. *Log(Debt)* is the natural logarithm of the total debt (Amadeus item *totdebt*) of the SME. *Affected SME* is an indicator variable that switches on only if the SME has at least one relationship bank whose corporate borrowers received ECB funding. In columns (3) and (4), *Affected SME* is used as median ranks in the spirit of a treatment intensity estimation. This variable used *Post CSPP (short term)* switches on for years 2016 and 2017. *Post CSPP (long term)* switches on for years 2018 and after. *Pre CSPP* switches on for the year of 2015. Controls include SME total assets (log), return on assets (%), and the number of employees (log). Other variables are defined in Appendix A. T-statistics are robust to within-bank (Panel A), within-country (Panel B), and within-SME (Panel C) correlation and heteroscedasticity. *** , ** , and * denote statistical significance at the 1% , 5% , and 10% levels, respectively.

Panel A. SME Exposures Dynamics

	(1)	(2)	(3)	(4)
	SME	*SME*	*SME*	*SME*
	Exposure	*Exposure*	*Exposure*	*Exposure*
Affected Bank	-2.200	-2.727		
	(-1.36)	(-1.53)		
Affected Bank × Post CSPP (first half-year)	2.118***	1.834***	0.777**	0.830**
	(2.73)	(3.03)	(2.26)	(2.38)
Affected Bank × Post CSPP (second half-year)	2.326**	2.123***	1.059*	1.117*
	(2.49)	(2.64)	(1.73)	(1.84)
Affected Bank × Post CSPP (third half-year)	1.659	1.440	0.700	0.668
	(1.49)	(1.58)	(0.93)	(0.88)
CORP Exposure				0.067
				(1.13)
Observations	484	484	484	484
Adjusted R-squared	-0.007	0.390	0.938	0.938
Time FE	Y	Y	Y	Y
Country FE	N	Y	N	N
Bank FE	N	N	Y	Y

Panel B: Credit Access and Real Effects Dynamics

	(1)	(2)	(3)	(4)	(5)
	SME Access to Bank Credit	*Purpose: Capital Investment*	*Purpose: Employment*	*Purpose: Working Capital*	*Purpose: Refinancing*
Disintermediation × Post CSPP (first year)	0.057***	0.070***	0.033***	-0.041*	-0.032***
	(3.59)	(3.73)	(2.61)	(-1.91)	(-3.24)
Disintermediation × Post CSPP (second year)	0.025**	-0.005	0.003	-0.006	0.002
	(2.45)	(-0.59)	(0.34)	(-0.38)	(0.16)
SME size	0.040***	0.017***	-0.007***	0.020***	-0.004
	(10.19)	(5.82)	(-4.86)	(6.28)	(-1.51)
SME age	0.049***	0.005	-0.005**	-0.007*	-0.003
	(6.16)	(1.54)	(-2.47)	(-1.78)	(-1.34)
SME credit quality	0.064***	0.022***	-0.001	-0.010***	-0.008***
	(8.75)	(6.37)	(-0.55)	(-3.19)	(-3.21)
SME profitability growth	0.030***	0.019***	0.000	-0.010***	-0.012***
	(7.33)	(6.50)	(0.22)	(-3.41)	(-5.30)
Observations	14,794	31,775	31,775	31,775	31,775
Adjusted R-squared	0.097	0.481	0.060	0.340	0.175
Industry-wave FE	Y	Y	Y	Y	Y
Country-wave FE	Y	Y	Y	Y	Y
Industry-country FE	Y	Y	Y	Y	Y

Panel C: Bank-Borrower Links and SME Borrowing Dynamics

	(1)	(2)	(3)	(4)
	Log(Debt)	*Log(Debt)*	*Log(Debt)*	*Log(Debt)*
Affected SME × Post CSPP (short term)	0.037***	0.036**	0.024**	0.026**
	(2.63)	(2.09)	(2.33)	(2.06)
Affected SME × Post CSPP (long term)	-0.019	-0.020	-0.027	-0.025
	(-0.23)	(-0.24)	(-0.41)	(-0.38)
Affected SME × Pre CSPP		-0.001		0.004
		(-0.09)		(0.35)
Observations	193,494	193,494	193,494	193,494
Adjusted R-squared	0.837	0.837	0.837	0.837
Definition of *Affected Bank*	Indicator	Indicator	Continuous	Continuous
Controls	Y	Y	Y	Y
SME FE and Country-Industry-Year FE	Y	Y	Y	Y

Appendix A: Variable Definitions

Sample: EBA Disclosures

Variable Name	Definition	Data Source
Affected Bank	Indicator equals one for banks with at least one relationship borrower whose bonds were purchased by ECB	ECB and Dealogic
Affected Bank (continuous)	Total primary market purchases by the ECB of that the bank's clients divided by the value of loans outstanding of that bank	ECB and Dealogic
Post CSPP	Indicator equals one for half-years on or after 2016H2	EBA Transparency Exercise results
SME Exposure	SME lending as a fraction of total exposures of the bank	EBA Transparency Exercise results (Retail - of which: SME and Standardised Total)
CORP Exposure	Non-SME corporate lending as a fraction of total exposures of the bank	EBA Transparency Exercise results (Corporates and Standardised Total)
Liquidity Constraints	The ratio of liquid assets to deposits as of 2015 (before the introduction of the CSPP), multiplied by -1 for ease of interpretation.	SNL Financial (256797)
Capital Constraints	The ratio of total regulatory capital to risk-weighted assets as of 2015 (before the introduction of the CSPP), multiplied by -1 for ease of interpretation.	SNL Financial (131990)

Sample: ECB Survey on the Access to Finance of Enterprises (SAFE)

Variable Name	Definition	Data Source
Disintermediation	Total purchases by the ECB within a country-SAFE industry during the corresponding wave deflated by the total value of all bonds outstanding in the country-SAFE industry. ECB purchases by security are defined as the tranche value divided by the total tranche value of all securities purchased by the ECB on the primary market during the month and multiplied by the monthly total value of ECB purchases on the primary market	ECB and Dealogic

Variable	Description	Source
Credit Access (bank loan)	Indicator that switches on only if the respondent SME receives the full amount of the loan applied for.	ECB SAFE (Original question: Q7b.a)
Bank Loan Availability	Indicator that switches on only if the respondent SME believes that the availability of bank loans is increasing.	ECB SAFE (Original question: Q23_b)
Trade Credit Availability	Indicator that switches on only if the respondent SME believes that the availability of trade credit is increasing.	ECB SAFE (Original question: Q23_d)
Lease Financing Availability	Indicator that switches on only if the respondent SME believes that the availability of trade credit is increasing.	ECB SAFE (Original question: Q23_i)
Purpose: Capital Investment	Indicator that switches on only if the purpose of financing is fixed investment	ECB SAFE (Original question: Q6a_1)
Purpose: Employment	Indicator that switches on only if the purpose of financing is hiring	ECB SAFE (Original question: Q6a_3)
Purpose: Working Capital	Indicator that switches on only if the purpose of financing is working capital	ECB SAFE (Original question: Q6a_2)
Purpose: Refinancing	Indicator that switches on only if the purpose of financing is refinancing	ECB SAFE (Original question: Q6a_5)
SME Interest Rate Decreased	Indicator that switches on only if the respondent SME states that interest expense has decreased	ECB SAFE (Original question: Q6a_5)
Borrower Refused Because Interest Cost Was High	Indicator that switches on only if the respondent SME states that its loan application was accepted, but it decided not to take the loan because the cost was too high.	ECB SAFE (Original question: Q2.d)
Bonds issuance	Total primary market issuances within a country-SAFE industry during the corresponding wave period deflated by the total value of all bonds outstanding in the country-SAFE industry.	Dealogic
SME size	1 if annual sales up to €2 million, 2 if between €2 and 10 million, 3 if between €10 and 50 million, and 4 if over €50 million.	ECB SAFE (Original question: d4)
SME age	1 if up to two years, 2 if between two and five years, 3 if between five and ten years, 4 if over 10 years	ECB SAFE (Original question: d5_rec)
SME credit quality	1 if credit quality deteriorated over the past six months, 2 if credit quality remained the same, 3 if credit quality improved	ECB SAFE (Original question: Q11_e)
SME profitability growth	1 if profits decreased over the past six months, 2 if profits remained the same, 3 if profits increased	ECB SAFE (Original question: Q2.e)

Sample: Amadeus

Variable Name	Definition	Data Source
Affected SME	Indicator equals one for SMEs with at least one relationship bank that is coded as *Affected Bank* (i.e., a bank with corporate borrowers whose bonds were purchased by the ECB under the CSPP).	ECB, Dealogic, and Amadeus Bankers
Post CSPP	Indicator equals one for firm-years ending in 2016 or after	ECB
Log(debt)	The natural logarithm of SME debt (short-term and long-term debt)	Amadeus (*loan* and *ltdb*)
Firm size	Natural logarithm of total assets	Amadeus (*toas*)
Firm employment	Number of employees	Amadeus (*mpl*)
Firm profitability	Pre-tax income as a percentage of total assets	Amadeus (*plbt* and *toas*)

Sample: ECB Loan-level Data

Variable Name	Definition	Data Source
Disintermediation	Total purchases by the ECB within a country-one-digit NACE industry during the quarter, deflated by the total value of all bonds outstanding in the country-one-digit NACE industry. The ECB's purchases by security are the tranche value divided by the total tranche value of all securities purchased by the ECB in the primary market in a month and multiplied by the monthly total value of the ECB's purchases on the primary market	ECB and Dealogic
Interest Rate	Percentage spread	ECB Loan-level Data (as80)
Zombie Lending	Indicator that switches on only if the sample interest rate is below the median and loss given default estimate is above the median.	ECB Loan-level Data (as80 and as37)
Log (Amount)	Original loan amount	ECB Loan-level Data (as54)
Loan Maturity	Tenor of the loan, calculated as the difference between the stated maturity date and origination date (in months)	ECB Loan-level Data (as51 and as50)

Sample: SNL Bank Data

Variable Name	Definition	Data Source
Affected Bank	Indicator equals one for banks with at least one relationship borrower whose bonds were purchased by ECB	ECB and Dealogic
Post CSPP	Indicator equals one for the years 2016 and 2017	SNL
Pre CSPP	Indicator equals one for the year 2015	SNL
Log(Number of Branches)	Number of branches of the bank	SNL Financials (134882)
Log(Number of Employees)	Annual average number of full-time employees of the bank	SNL Financials (134875)
Log(Total Assets)	Bank total assets	SNL Financials (131929)

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Summary

This dissertation examines how firms and public institutions could use disclosures and policies in an unconventional way to affect the decision making of other firms. In particular, I investigate whether firms use public disclosure to investors in order to improve their competitive positioning and how ECB corporate bond purchases bolster bank lending to SME's.

In chapter 2, I investigate how firms use disclosures to their investors as a method to tacitly collude with their competitors. I show that antitrust oversight can be effective in limiting this behavior. Specifically, using textual analysis to create a novel measure, I find that firms in concentrated industries provide more future price increase disclosures than firms in dispersed industries, presumably to coordinate price increases with their competitors. Exploiting two separate shocks in U.S. antitrust oversight, I find that an increase in antitrust monitoring reduces use of future price increase disclosures. Significantly, I find that this decline in the use of future price increase reduces the information available to all investors, as reflected in increased bid-ask spreads. Thus, increased antitrust oversight appears to be effective in reducing the ability of firms to collude via future price increase disclosures, but it comes with the unintended consequence that stock markets are less informed.

In another study on the strategic use of disclosure to influence product market competition (chapter 3), we examine whether firms preannounce capacity expansions to deter entry into their product markets. Using plausibly exogenous variation in entry threats and textual analysis to observe capacity expansion announcements, we show that firms respond to heightened entry threats by announcing capacity expansions. Consistent with our predictions, larger firms are more likely to respond in this fashion, while firms with more private information about industry prospects are less likely to respond in this fashion. Capacity expansion announcements appear to be effective at deterring entry.

In chapter 4, we examine the spillover effects of financial disintermediation on the supply of credit to small and medium enterprises (SMEs). We find that direct central-bank lending to large firms induces banks to increase lending to SMEs by 8-12 percent and that this effect is stronger for liquidity-constrained banks. SMEs with affected relationship banks increase borrowing by approximately €77,750. We verify that these inferences are not due to changing economic fundamentals or selection in central-bank financing. Despite documenting positive effects, we also find that they disappear in the long term, casting some doubt on the structural efficacy of financial disintermediation. This study thus highlights how central banks could use financing to large corporations as an unconventional tool to induce banks to increase their lending to SMEs without directly targeting them.

Nederlandse Samenvatting (Dutch summary)

In dit proefschrift onderzoek ik hoe bedrijven en publieke organen op een ongewone manier gebruik maken van marktcommunicatie en beleid, om zo de belissingen te beinvloeden van bedrijven. In het bijzonder onderzoek ik of bedrijven gebruik kunnen maken van publieke toelichtingen om hun marktpositie te verbeteren en of het opkopen van bedrijfsobligaties door de Europese Centrale Bank (ECB) kan helpen om de de financiering voor het midden- en kleinbedrijf (MKB) te stimuleren.

In hoofdstuk 2 analyseer ik of bedrijven gebruik kunnen maken van publieke toelichten om zo impliciet prijsafspraken te maken. Ik toon aan dat mededingings regelgeving dit top op zeker hoogte kunnnen voorkomen. Met behulp van tekstanalyse vind ik dat bedrijven in geconcentreerde markten vaker bekendmaken dat zij hun prijzen gaan verhogen dan bedrijven in niet-geconcentreerde markten, waarschijnlijk met als doel prijsafspraken te maken. Gebruikmakend van een verandering in Amerikaanse mededingingsregelgeving, vind ik dat dit kan leiden tot een verlaging van het aantal bedrijven dat prijsverhogingen bekend maakt. Daarentegen vind ik wel dat aandeelhouders minder goed geinformeerd zijn doordat bedrijven minder informatie geven over toekomstige prijsverhogingen. Mededingingsregelgeving lijkt dus effectief te zijn in de strijd tegen prijsafspraken door middel van publieke bekendmakingen van prijsverhogingen, maar heeft ook negatieve bijwerkingen voor aandeelhouders.

In een tweede onderzoek (hoofdstuk 3) naar het strategisch gebruik van publieke toelichtingen om de marktpositionering van bedrijven te verbeteren, bekijken wij of bedrijven aankondigingen van een bedrijfsuitbreiding gebruiken om te voorkomen dat nieuwe bedrijven hun markt betreden. Met behulp van exogene variatie in de dreiging van nieuwe toetreders tot een markt en tekstanalyse, vinden tonen wij aan dat bedrijven vaker een uitbereiding van hun bedrijf aankondigen als er een

grotere kans is dat een nieuw bedrijf hun markt toetreedt. Overeenkomstig met
onze voorspelling, vinden wij dat grotere bedrijven vaker op deze manier reageren,
terwijl bedrijven waarover minder informatie beschikbaar is dit minder vaak doen. De
vooraankondiging van een bedrijfsuitbereiding lijkt effectief te zijn in het voorkomen
dat bedrijven inderdaad toetreden tot een markt.

In hoofdstuk 4 onderzoeken wij of geldstromen zonder de tussenkomst van banken
indirect ook kunnen leiden to meer financiering voor het MKB. Wij vinden dat het
opkopen van bedrijfsobligaties door centrale banken ertoe leidt dat banken 8-12
procent meer leningen verstrekken aan het MKB. Dit komt met name doordat klanten
van banken met een liquiditeitsprobleem minder leningen van deze banken nodig
hebben en daardoor meer geld beschikbaar hebben om dit uit te lenen aan het MKB.
MKBs kunnen ongeveer €77,750 meer lenen als zij een bank hebben die op deze manier
indirect beinvloed zijn door het opkoopprogramma. We verifiëren dat deze bevindingen
niet worden veroorzaakt door een verandering in het economisch klimaat of door
voorkennis van centrale banken. Toch blijkt dat de langetermijneffecten beperkt zijn.
Er bestaat daarom twijfel of het direct financieren van bedrijven daadwerkelijk een
effectieve manier is om het MKB te stimuleren. Samenvattend, dit onderzoek laat
zien hoe het opkopen van bedrijfsobligaties door centrale banken een onconventionele
manier kan zijn om banken te stimuleren meer uit te lenen aan het MKB zonder
direct geld te geven aan banken.

About the Author

Marcel Tuijn (1988) received his bachelor's degree in International Business Administration and his master degree both from the Rotterdam School of Management, Erasmus University. After graduation, he started his PhD under the supervision of Erik Peek and Erik Roelofsen. For two years, he was a visiting research professional and student at the University of Chicago Booth School of Business and was then invited to visit the University of Notre Dame for two more years.

Author's Portfolio

Working Papers

- "Shall we talk price increases? The fine line between disclosure and antitrust oversight"

- "Financial intermediation through financial disintermediation: evidence from the ECB corporate sector purchase program"

    - with Aytekin Ertan (London Business School) and Anya Kleymenova (University of Chicago)

- "Do firms strategically announce capacity expansions to deter entry?"

    - Matt Bloomfield (University of Pennsylvania)

Work in Progress

- "All in one place: the effects of centralizing access to financial information"

    - with Gurpal Sran and Lauren Vollon (both University of Chicago)

- "Transient institutional ownership and managers' strategic disclosure decisions"

    - with Erik Peek (Erasmus University Rotterdam)

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    - with Anya Kleymenova (University of Chicago)

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- Empirical Asset Pricing (Multiple professors)

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