




Defunding controversial industries: Can targeted credit rationing choke firms? [☆]

Kunal Sachdeva ^a, André F. Silva ^b, Pablo Slutzky ^c,^{*} Billy Y. Xu ^d

^a University of Michigan, Stephen M. Ross School of Business, 701 Tappan Ave, Ann Arbor, MI 48109, USA

^b Federal Reserve Board, 20th St. & Constitution Ave NW, Washington, DC 20551, USA

^c University of Maryland, R.H. Smith School of Business, 4427 Van Munching Hall, College Park, MD 20742, USA

^d University of Rochester, Simon Business School, 300 Wilson Blvd, Rochester, NY 14620, USA

ARTICLE INFO

JEL classification:

G21
G28
G30
G32
G38

Keywords:

Targeted credit rationing
Firm–bank relationships
Controversial industries

ABSTRACT

This paper examines the effects of targeted credit rationing by banks on firms likely to generate negative externalities. We exploit an initiative of the U.S. Department of Justice, labeled Operation Choke Point, which compelled banks to limit relationships with firms in controversial industries. Using supervisory loan-level data, we show that, as intended, targeted banks reduced lending and terminated relationships with affected firms. However, most of these firms fully substituted credit through nontargeted banks under similar terms. Overall, we find no significant shifts in the performance and investment of affected firms, suggesting that targeted credit rationing is widely ineffective in promoting change.

1. Introduction

Stakeholders are increasingly seeking to hold companies accountable for their negative societal externalities. A commonly used method has been the active divestment of equity, where investors sell off the stocks of firms perceived as socially irresponsible to raise firms' cost of capital and pressure them to address the externalities. While this strategy has become popular, recent research has shown that it has limited effectiveness. In particular, equity divestments are mostly restricted to listed firms and may result in the loss of investor influence

within a company (Broccardo et al., 2022; Edmans et al., 2024). Equity divestments can also stifle innovation (Cohen et al., 2024) and targeted firms may respond by offshoring or divesting parts of their controversial businesses (Ben-David et al., 2021; Duchin et al., 2025). Finally, the limited capital behind these efforts restricts their impact (Berk and van Binsbergen, 2025) and can even be counterproductive (Hartzmark and Shue, 2023).

In response to these limitations, stakeholders have turned to alternative strategies, including targeting access to bank credit for firms

[☆] We thank the editor, Philipp Schnabl, an anonymous referee, Salem Abo-Zaid (discussant), Thorsten Beck, Aymeric Bellon, Andrew Bird (discussant), Alex Butler, Charles Calomiris, Tony Cookson, Sergio Correia, Alan Crane, Kevin Crotty, Ralph De Haas, Joseph Kalmenovitz, Michael Faulkender, Mariassunta Giannetti (discussant), Stefano Giglio, Todd Gormley, Daniel Green (discussant), Ron Kaniel, Elena Loutskina (discussant), Max Maksimovic, Camelia Minoiu, Thomas Mosk (discussant), Karsten Muller, S. Lakshmi Naaraayanan, José-Luis Peydró (discussant), Alexander Popov, Robert Prilmeier (discussant), Bernd Schwaab (discussant), Janis Skrastins (discussant), Jessie Wang, Mitch Warachka (discussant), James Weston, Allan Wright (discussant), and Edison Yu (discussant) for their extremely helpful comments. We are also grateful to conference and seminar participants at the AFA Annual Meeting, SFS Cavalcade, EFA Annual Meeting, FIRS Conference, MoFIR Workshop on Banking, University of Kentucky Finance Conference, Lubrafin Meeting, University of Sydney, National University of Singapore, International Research Conference on Empirical Approaches to Anti-Money Laundering and Financial Crime, Loyola Financial Ethics Conference, Midwest Finance Association Annual Meeting, OCC Symposium on Emerging Risks in the Banking System, IBEFA Summer Meeting, Boca-ECGI Corporate Finance and Governance Conference, European University Institute, Baylor University, University of Michigan, Federal Reserve Board, University of Maryland, and Rice University. The views expressed in this paper solely reflect those of the authors and should not be interpreted as reflecting the views of the Board of Governors of the Federal Reserve System or other members of its staff.

believed to be socially controversial. This approach, known as targeted credit rationing, has gained appeal in recent years due to its ability to impact both publicly traded and privately held firms, capitalizing on the persistent nature of firm–bank relationships. Unlike equity divestments, such policies have the potential to influence a broader set of firms by directly affecting their access to credit. However, given the potential countermeasures that affected firms may take, little is known about the overall effectiveness of targeted credit rationing. In this paper, we show that while the intended effects of targeted credit rationing on lending relationships are realized, this practice does not impact most firms' overall credit availability, performance, or investment, indicating its potential limitations as a tool for promoting change.

The main challenge of studying targeted credit rationing is that a bank's decision to extend credit depends not only on a firm's past, current, and expected financial performance, but also on the bank's nonfinancial preferences, such as ideological considerations, which may vary over time. Our paper addresses this issue by studying Operation Choke Point (OCP), a major initiative led by the U.S. Department of Justice (DOJ) which compelled a subset of banks to limit relationships with firms in certain industries that operated legally but were deemed socially controversial—including ammunition and firearms, tobacco, dating and escort services, pornography, and online gambling. This setting provides a near-ideal quasi-random experiment to study the causal effects of targeted credit rationing on firms' operations. First, the operation was a large and credible shock to banks, as the DOJ threatened significant sanctions for noncompliant banks. Second, according to court documents, the targeting of banks was not based on lending volumes to firms in targeted industries, a claim we confirm in an analysis that additionally finds no relation between the targeting of banks and several bank and firm characteristics, mitigating concerns surrounding a potential selection bias. Third, given that the operation impacted a subset of banks at different times, we can identify its effects by exploiting its staggered implementation in a dynamic difference-in-differences setting. Finally, given that OCP was not a blanket ban on lending to the industries in question, it allows us to examine whether the actions of a single bank or a subset of banks can influence the operations of firms in particular industries.

To provide an in-depth analysis of the credit supply and real effects of targeted credit rationing, we employ confidential, quarterly loan-level data for the U.S. from the Federal Reserve's Y-14Q corporate loan schedule. This matched firm–bank supervisory credit register dataset covers corporate loans with commitment amounts over \$1 million for banks with at least \$50 billion in total assets, along with firm-level characteristics for both publicly-listed and private corporations.

Our study is organized into three parts. First, we show that banks targeted by OCP reduced their committed credit to firms in affected industries. This credit contraction was concentrated among small and medium-sized enterprises (SMEs), which experienced a 10 percent reduction in committed credit. We find no changes in the share of drawn credit, implying a lower volume of credit utilized by SMEs. This effect is similar across SMEs with different levels of profitability, liquidity, and leverage prior to the shock. In contrast, we find that the level of committed credit and the share of drawn credit did not change for large firms — also irrespective of their profitability, liquidity, or leverage — suggesting that firm size was a key driver of banks' decisions to cut lending.

These baseline results are robust to a battery of tests. First, we add firm–time fixed effects and thus focus on firms with relationships with multiple banks to better control for credit demand and the nonrandom matching between firms and banks (Khawaja and Mian, 2008). Second, we balance the panel data in a Poisson specification to examine the intensive and extensive margins, combined, while accounting for the issues involving an outcome variable that is positive but can often equal zero (Cohn et al., 2022; Correia et al., 2020; Chen and Roth, 2024). Third, we use a stacked regression specification to address the potential biases in staggered difference-in-differences settings (Gormley

and Matsa, 2011, 2016; Baker et al., 2022b). We also conduct several falsification tests, such as considering unaffected industries, unaffected cash-intensive industries, and random treatment dates. Overall, we find consistent evidence that targeted banks reduced lending to firms in industries affected by the operation.

We also examine how the initiative affected the termination of existing firm–bank relationships and the creation of new banking relationships. We show that affected firms experience an increase in the frequency at which their relationships with targeted banks are terminated, reinforcing our evidence that banks responded to regulatory pressure. In contrast, the existing relationships with nontargeted banks were preserved. When considering relationship creation, we find that affected firms responded by initiating new relationships with nontargeted banks and, consistent with the intent of the policy, that these firms did not initiate new relationships with already targeted banks.

Finally, we focus on affected firms' aggregate access to credit, performance, and investment, comprehensively assessing the initiative's overall impact. We find that there were no statistically significant changes in the aggregate level of credit following the initiative for the average affected firm. Nonetheless, when focusing on heterogeneous treatment effects, we show that large profitable firms managed to increase their level of committed credit, suggesting that they were hedging against future potential terminations. In contrast, highly levered SMEs experienced a modest reduction in committed credit. These results indicate that targeted credit rationing driven by OCP did not reduce overall credit for most firms, except for a small subset, making the policy broadly ineffective. Similarly, we find no impact of the operation on firms' leverage, profitability, or investment. Together, our results indicate that targeted credit rationing is largely ineffective at imposing costs on firms in controversial industries.¹

This paper contributes to several branches of literature. First, it contributes to the broad and flourishing literature on responsible investing, which focuses on affecting change through divestment, negative selection, and investors' attempts to discipline firms by raising their cost of capital.² While these approaches are gaining popularity, several studies have identified shortcomings. For instance, Oehmke and Opp (2025) show that divestment only works if responsible investors are affected by externalities and coordinate, while (Broccardo et al., 2022) find that divestment reduces the ability to voice preferences. Another restricting factor is that these actions are mostly limited to publicly listed firms, leaving private firms largely unscathed. Instead, we examine the effectiveness of an alternative approach at the center of policymakers' current discussions.

Second, our paper contributes to the literature on targeted credit rationing by focusing on an exogenous shock to study the causal effects of an *externally driven* targeted credit rationing program. This quasi-random intervention allows us to answer the following question: can a single bank or a subset of banks effectively influence the operations of firms in particular industries such as those that potentially generate negative externalities? Our results show that OCP had an impact on bank lending to firms in affected industries at the intensive margin, but these firms substituted their relationships between targeted and

¹ While research in different contexts has documented the pivotal role of bank–firm relationships in influencing a firm's access to credit, particularly for smaller firms (e.g., Beck et al., 2005, 2018), we find that the substitution was relatively seamless during a benign noncrisis period. This result holds for the majority of firms with loan commitments above \$1 million, with the exception of highly levered SMEs, offering insights into the role of relationships when banks actively terminate accounts with firms in specific industries.

² Empirical evidence includes Teoh et al. (1999) and Becht et al. (2025). Theoretical studies of impact investing include Hart and Zingales (2017), Chowdhry et al. (2019), and Berk and van Binsbergen (2025).

nontargeted banks, offsetting the initiative's effect.³ Overall, we find consistent evidence of no impact on the operations of firms.⁴

Our paper also complements a distinct but related literature centered on *internally driven* credit rationing, where banks themselves may change their credit policies due to, for instance, environmental concerns. This literature has provided mixed evidence. On the one hand, Green and Vallee (2025) find that banks with more stringent environmental policies divest from the coal sector and that firms cannot fully substitute credit, experiencing significant operational consequences. Relatedly, Kacperczyk and Peydró (2024) show that bank participation in the Science-Based Targets Initiative reduces borrowers' ability to secure funds but does not significantly affect environmental outcomes. On the other hand, Haushalter et al. (2023) find that banks occasionally fail to adhere to their own policies on exiting mountaintop mining, leading to inconclusive effects on targeted companies. Similarly, Giannetti et al. (2024) and Sastry et al. (2024) show that banks often do not divest from high-emission firms following environmental disclosures or net zero commitments.⁵ In contrast, we study the effects of credit rationing in response to an exogenous, externally driven shock that forces a subset of banks to terminate or limit relationships with specific firms, thereby mitigating concerns about selection. Moreover, the firms affected by OCP do not operate in high-polluting industries, which limits their ability to adjust their businesses to address investor concerns. Our results suggest a substitution of credit for borrowing firms following this exogenous shock. Together, these papers expand the understanding of the effectiveness of credit rationing.

Finally, this paper contributes to the broader literature examining the role of financial markets and intermediaries in driving environmental and social change. While research in this area has grown, much of it has focused on climate finance, examining how financial markets incorporate changes in preferences for climate change risks (Bolton and Kacperczyk, 2021) and beliefs (Baldauf et al., 2020; Stroebel and Wurgler, 2021), and how this is likely reflected in asset prices.⁶ In the context of credit markets, the literature has primarily concentrated on green bonds (Baker et al., 2022a; Flammer, 2021). Instead, our paper examines targeted credit rationing by exploring the implications of negative selection in bank-intermediated credit markets driven by ideological beliefs.⁷

³ Unlike Aiyar et al. (2014) and Kim et al. (2018), who examine macroprudential policies' effects on banks and spillovers, our study focuses on direct impacts on firm operations. We also differ from Agca et al. (2023), who exploit changes to AML enforcement to study the effect of a shift in the banking sector composition on lending to small firms and households.

⁴ In a counterfactual scenario in which all regulated banks were simultaneously targeted, it is unclear whether we would have a different overall result, given the increasing importance of nonbank lending in recent years (e.g., Gopal and Schnabl, 2022; Chernenko et al., 2022) and potential substitute lending by foreign banks.

⁵ Bellon (2024) delves into the connection between lender liability and debtor behavior in environmental compliance, while (Laeven and Popov, 2023) explore how bank lending to foreign companies shifts with the introduction of carbon taxes. Alternative approaches are taken by Ivanov et al. (2024), who investigate the effect of carbon pricing policy on bank credit to greenhouse gas emitting firms, while (Miguel et al., 2024) study the impact of climate risk-related capital requirements on Brazilian banks.

⁶ Theoretical papers include Pedersen et al. (2021) and Pástor et al. (2021). Empirical evidence includes Pástor et al. (2022), Engle et al. (2020), and Bolton and Kacperczyk (2023).

⁷ Research addressing socially controversial issues and the impact of financial intermediaries has examined different aspects. For instance, one line of research studies state government bans that exclude pro-social financial institutions from underwriting municipal bonds (Garrett and Ivanov, 2025). Separately, Berger and Seegert (2024) study how limitations on access to cash management services from financial intermediaries affect the profitability of firms in the marijuana industry. However, unlike these and other studies, our research centers on the effects of credit rationing directed at legal but socially

2. Institutional background: Operation Choke Point

2.1. Timeline

OCP was a highly debated initiative led by the DOJ, in collaboration with the Federal Deposit Insurance Corporation (FDIC), aimed at discouraging banks from providing financial services to firms in legal industries that were deemed socially controversial.⁸ In this section, we outline the timeline of OCP, discuss the affected firms and banks, and provide key institutional details, as summarized in Table A.1 of the Online Appendix.

In November 2012, attorneys within the DOJ's Civil Division proposed an internal initiative called *Operation Choke Point*, recognizing that the DOJ could potentially influence bank behavior through the threat of subpoenas and regulatory actions.⁹ For instance, an internal memo between DOJ employees, dated November 5, 2012, noted that "banks [were] sensitive to the risk of civil and/or criminal liability and regulatory actions".¹⁰ The DOJ and the FDIC seemingly employed various methods, including regulatory threats and legal intimidation, to try to influence bank behavior (Figure A.1 of the Online Appendix).

Early in 2013, the DOJ began issuing subpoenas to banks and payment processors, effectively compelling banks to restrict certain merchants' access to finance. These subpoenas were guided by a list of high-risk merchants, a list originally included in the FDIC's publication of a Supervisory Insights article, "Managing Risks in Third-Party Payment Processor Relationships". This article warned of heightened risks for financial institutions engaged in services with certain industries, but was originally intended to serve as informal and educational guidance and had no direct policy implications.¹¹ Indeed, the FDIC's high-risk list was created in 2011, well before OCP was operationalized in 2013 and its creation was entirely unrelated to OCP itself.

While OCP was not an official law or regulation, banks perceived it as a credible threat, with the FDIC's role and involvement in OCP raising concerns of "regulatory threats, undue pressure, coercion, and intimidation" (Figure A.1 of the Online Appendix). Under pressure, targeted banks began terminating services and reducing lending to firms in certain industries in early 2013 (Calomiris, 2017). Documentary and testimonial evidence produced during government investigations and lawsuits suggests that the initiative was seemingly unrelated to the underlying credit risk of the targeted industries (for example, see Figures A.1 and A.2 of the Online Appendix).

The program first came to public attention through an article in *The Wall Street Journal* in August 2013 and, subsequently, members of Congress submitted a letter to the FDIC chairman and the U.S. attorney general expressing their concerns about the pressure the DOJ was exerting to terminate lawful lending relationships. In December 2014, the U.S. House Committee on Oversight and Government Reform issued

controversial industries. While not posing systemic financial risks, these firms often face social stigma unrelated to traditional risk factors considered in pricing models.

⁸ For example, see Letter from the Office of the Assistant Attorney General to Honorable Bob Goodlatte, Chairman of U.S. House Committee on the Judiciary (August 16, 2017); and Office of Inspector General, The FDIC's Role in Operation Choke Point and Supervisory Approach to Institutions that Conducted Business with Merchants Associated with High-Risk Activities (Office of Audits and Evaluations Report No. AUD-15-008, September 2015).

⁹ For example, see "Memorandum: Operation Choke Point", sent from Joel M. Sweet, assistant U.S. attorney, to Stuart F. Delery, acting assistant attorney general, U.S. Department of Justice Civil Division, dated November 5, 2012 (HOCR-3PPP000017-21).

¹⁰ Operation Choke Point: Hearing before the Subcommittee on Oversight and Investigations, dated July 15, 2014.

¹¹ Committee on Oversight and Government Reform Staff Report, dated December 8, 2014, "Federal Deposit Insurance Corporation's Involvement in 'Operation Choke Point'."

a report titled *Operation Choke Point* (Calomiris, 2017). Growing public disapproval and government hearings led to the operation's termination in August 2017.

2.2. Objective and scope

The intent behind OCP has been debated in lawsuits and Congressional hearings.¹² While the official reason for OCP is linked to regulatory concerns regarding increased risk of fraud among certain merchants, documentary evidence suggests that OCP may have been motivated by reasons “entirely outside of FDIC’s mandate” (Calomiris (2017) and Figure A.1 of the Online Appendix). For instance, a 2014 House Committee Staff Report noted the following:

*In a particularly egregious example, a senior official in the Division of Depositor and Consumer Protection insisted that FDIC Chairman Martin Gruenberg’s letters to Congress and talking points always mention pornography when discussing payday lenders and other industries, in an effort to convey a “good picture regarding the unsavory nature of the businesses at issue”.*¹³

The firms affected by OCP were those on a list of high-risk merchants, which included industries such as ammunition and firearms, tobacco, dating and escort services, pornography, and online gambling—see Table A.2 of the Online Appendix for a complete list. The article listing merchant types warned of heightened reputational risks for financial institutions engaging with certain industries due to those industries’ potentially “questionable or fraudulent” nature.

Notably, OCP appeared to affect a random subset of banks, with no discernible evidence that the initiative had a systematic method for targeting financial institutions. Anecdotal, the indiscriminate nature of the targeting is highlighted in a report by the Office of Inspector General, which found “no evidence that the FDIC used the high-risk list to target financial institutions”.¹⁴ This lack of clear criteria introduced a degree of arbitrariness into the initiative’s implementation, which this paper exploits in a staggered difference-in-differences setup to establish causality.

The scope of the initiative had broad implications. Indeed, although payday lenders were one of the main targets of OCP (Zywicki, 2015; Stevenson, 2022), the effects of the initiative on other high risk merchants were also a subject of concern for Congress.¹⁵ Indeed, an

expert report indicates that firms such as firearms and ammunition dealers were also affected by the initiative (Figure A.3 of the Online Appendix).¹⁶ For instance, as discussed by Calomiris (2017):

*The experience of one entry on the list — firearms and ammunitions merchants — effectively traces the downstream influence of the high-risk merchants list. MOUs between supervised banks and FDIC Regional Offices, as well as bank policies submitted pursuant to FDIC Consent Orders, variously “prohibit” payment processing for firearms merchants, characterize loans to firearms dealers as “undesirable”, and generally subject firearms and ammunitions merchants to significantly higher due diligence standards.*¹⁷

Overall, while OCP was a controversial initiative, it provides a near-ideal setting to study the effects of targeted credit rationing. In the next section, we discuss the data and target selection model.

3. Data and target selection

3.1. Federal reserve Y-14Q data

Our main data source is confidential quarterly loan-level data for the U.S. obtained from the corporate loan schedule H.1 of the Federal Reserve’s Y-14Q.¹⁸ These data have been collected to support the Dodd–Frank Act’s stress tests and assess bank capital adequacy for large banks. The credit register provides information on credit exposures with commitment amounts exceeding \$1 million for banks with at least \$50 billion in total assets.¹⁹ These loans account for around 75% of all commercial and industrial lending volume during the period we analyze.²⁰ In addition to the amount of committed credit for each firm–bank pair, the dataset contains information on drawn amounts on credit lines, amounts past due, interest rate spreads, and maturities. It also provides information on each bank’s internal assessment of the default probability of a given firm, among other details. Finally, the dataset includes firm-level information such as total assets, net income, cash holdings, total debt, and capital expenditures.

We supplement this data with financial information at the bank holding company level from publicly available FR Y-9C reports, including consolidated quarterly balance sheets, income statements, and detailed supporting schedules. Given that the Federal Reserve started collecting the Y-14Q data in the second quarter of 2012, we employ quarterly data spanning the period of the second quarter of 2012 to the second quarter of 2016.

Pivotal to our study, we also determine which banks were part of Operation Choke Point and their targeting date. We accomplish this by manually reviewing publicly available government and legal documents. We present a timeline listing the targeted banks and the corresponding dates in Fig. 1.

¹² See, for example, Second Declaration of Dennis Shaul in the matter of Community Financial Services Association of America, Ltd., et al. v. Federal Deposit Insurance Corporation, et al. (Civil Action No. 14-953-GK); Committee on Oversight and Government Reform Staff Report, dated December 8, 2014, “Federal Deposit Insurance Corporation’s Involvement in ‘Operation Choke Point’”; Hearing Before the Subcommittee on Oversight and Investigations of the U.S. House Committee of Financial Services, dated July 15, 2014, “The Department of Justice’s ‘Operation Choke Point’”; and Hearing Before the Subcommittee on Oversight and Investigations of the U.S. House Committee of Financial Services, dated March 24, 2015, “The Federal Deposit Insurance Corporation’s Role in Operation Choke Point”.

¹³ Committee on Oversight and Government Reform Staff Report, dated December 8, 2014, “Federal Deposit Insurance Corporation’s Involvement in ‘Operation Choke Point’”.

¹⁴ Office of Inspector General Report, dated September 2015, “The FDIC’s Role in Operation Choke Point and Supervisory Approach to Institutions that Conducted Business with Merchants Associated with High-Risk Activities”.

¹⁵ For example, see U.S. House Committee on Oversight and Government Reform, Federal Deposit Insurance Corporation’s Involvement in Operation Choke Point (Staff Report 113th Congress, December 2014). Baugh (2016) exploited this initiative to analyze the effect of limiting credit to online payday lenders on households’ consumption and borrowing patterns, using data from an aggregator of financial transactions. In addition to the different research questions explored in this paper, we focus exclusively on nonfinancial firms since, as observed in our data, there are relatively few payday lenders borrowing from regulated banks with at least \$50 billion in total assets.

¹⁶ See, for example, Committee on Oversight and Government Reform Staff Report, dated December 8, 2014, “Federal Deposit Insurance Corporation’s Involvement in ‘Operation Choke Point’” and Hearing Before the Subcommittee on Oversight and Investigations of the U.S. House Committee of Financial Services, dated March 24, 2015, “The Federal Deposit Insurance Corporation’s Role in Operation Choke Point”.

¹⁷ See Expert Report of Charles Calomiris, dated January 11, 2017, in the matter of Community Financial Services Association of America, Ltd., et al. v. Federal Deposit Insurance Corporation, et al. (Civil Action No. 14-953-GK).

¹⁸ Recent studies using the Federal Reserve’s Y-14Q data include Brown et al. (2021), Chodorow-Reich et al. (2021), and Crosignani et al. (2023).

¹⁹ It is important to note that firms cannot avoid regulatory oversight by borrowing amounts below the \$1 million threshold or from banks with total assets below \$50 billion.

²⁰ While we do not have information on this share for the subset of industries affected by OCP, using data on the universe of firms of similar size from the Quarterly Census of Employment and Wages (QCEW), we find that the Y-14Q data covers, on average, 73.6% of the number of firms within these industries.

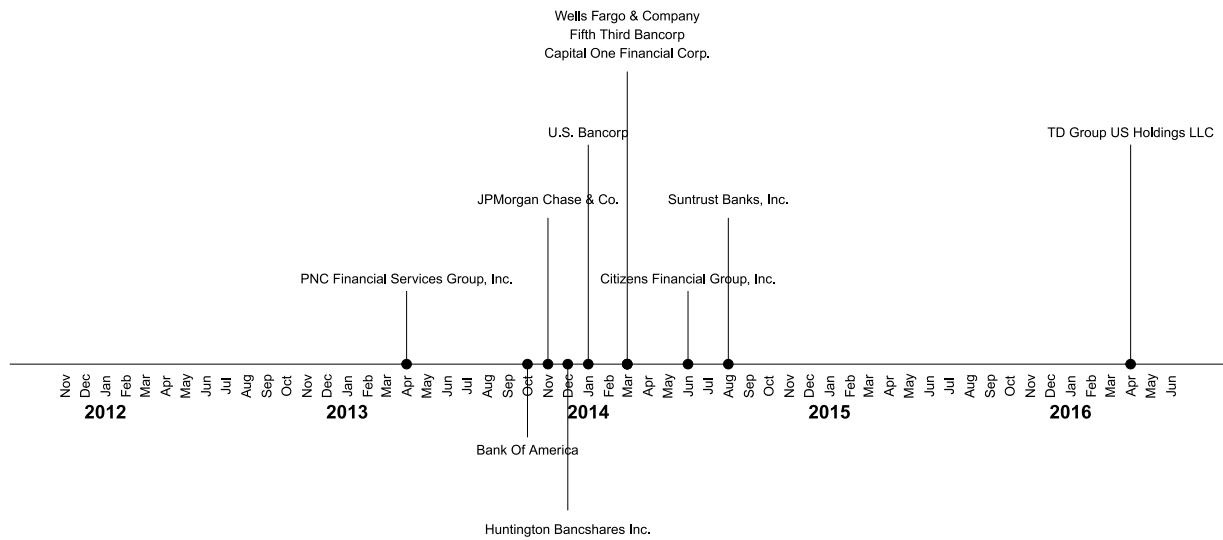


Fig. 1. Timeline of targeting. This figure plots the timeline of the targeting of bank holding companies by the Department of Justice (DOJ) used in our paper.

Table 1

Summary statistics.

Data source: Federal Reserve Y-14Q.

	N (1)	Mean (2)	Median (3)	SD (4)
<i>Panel A: Firm-Bank-Quarter</i>				
Committed Credit	51,105	29.82	5.981	90.07
Share of Drawn Credit	51,105	0.545	0.604	0.393
<i>Panel B: Firm-Quarter</i>				
Committed Credit	41,891	36.38	3.867	182.5
Share of Drawn Credit	41,891	0.637	0.767	0.386
SME	41,891	0.845	1.000	0.362
Large Firm	41,891	0.155	0.000	0.362
Relationship Creation with Any Bank	41,891	0.106	0.000	0.308
Relationship Termination with Any Bank	41,891	0.048	0.000	0.214
Relationship Creation with Targeted Bank	41,891	0.069	0.000	0.254
Relationship Termination with Targeted Bank	41,891	0.024	0.000	0.154
Relationship Creation with Nontargeted Bank	41,891	0.039	0.000	0.194
Relationship Termination with Nontargeted Bank	41,891	0.025	0.000	0.155
Firm Debt to Assets	41,891	0.298	0.260	0.248
Firm Profitability	41,891	0.095	0.063	0.156
Firm Investment	41,891	0.022	0.000	0.046

This table reports summary statistics for the key variables in the firm-bank-quarter (Panel A) and firm-quarter (Panel B) samples relating to lending by the 30 largest U.S. banks to U.S. firms in industries affected by OCP. The table shows the number of observations (N), mean, median, and standard deviation (SD). The sample period is 2012:Q2 to 2016:Q2. Variable definitions are presented in Table A.3 of the Online Appendix.

We report key summary statistics for our main dataset focused on lending by U.S. banks to firms in industries affected by OCP in Table 1, with the variable definitions presented in Table A.3 of the Online Appendix. In Panel A, we provide details on the firm-bank-quarter sample, including information on committed credit and the share of drawn credit. For our analysis of the effect of the initiative on bank-firm relationships and firm-level outcomes, we aggregate the information at the firm-quarter level. Panel B summarizes this data, including firms' financial information and summary statistics on the initiation and termination of bank relationships.

3.2. Targeted banks

To analyze the effect of credit rationing on targeted industries, a key issue is to understand the criteria employed by regulators to target banks, as this consideration can potentially introduce bias in our estimations. To this end, we analyze data on Operation Choke

Point's targeting from expert witness testimonies and other supporting documents. As described above, we find administrative and regulatory documents suggesting that the selection process was not driven by particular bank characteristics.

To test this claim, we collect information on targeted banks from documentary evidence produced during lawsuits, regulatory reports, internal communications, and hearings. In particular, the empirical design in this paper is primarily based on expert witness testimony from Calomiris (2017), which identifies targeted banks and some of the first known dates in which those banks allegedly began terminating credit to firms in high-risk industries' while under the influence of Operation Choke Point.²¹ Although the exact list of banks targeted by

²¹ The affected firms engaged in legal disputes with banks in Calomiris (2017) were payday lenders, who were among the most vocal in addressing banking practices that they perceived as unfair.

Table 2

Ex-Ante characteristics, targeted vs. nontargeted banks.

Data sources: Federal Reserve Y-14Q and Y-9C.

	Targeted Banks (1)	Nontargeted Banks (2)	Difference t-stat (3)
Bank Size	19.58	19.04	0.231
Bank Capital	0.085	0.086	0.984
Bank Liquidity	0.278	0.345	0.289
Bank Profitability	0.007	0.004	0.115
Bank Share of Lending to Targeted Industries	0.071	0.062	0.416
Bank Share of Relationships in Targeted Industries	0.061	0.060	0.875
Profitability of Firms in a Bank's Portfolio	0.045	0.043	0.682
Liquidity of Firms in a Bank's Portfolio	0.106	0.075	0.330
Leverage of Firms in a Bank's Portfolio	0.311	0.311	0.996
PD of Firms in Bank's Portfolio	0.016	0.015	0.706
PD of Firms in Affected Industries in Bank's Portfolio	0.020	0.018	0.678

This table compares pre-period characteristics of targeted banks ($n=11$) vs. nontargeted banks ($n=19$) i.e., as of 2012:Q2. Variable definitions are presented in Table A.3 of the Online Appendix. ***, **, * denote significance at the 1%, 5%, and 10% level.

the DOJ is redacted,²² we find corroborating evidence regarding the involvement of these lenders in other documents.²³

We formally examine Operation Choke Point's selection criteria by comparing pre-period characteristics of targeted and nontargeted banks. Specifically, in Table 2 we compare banks' financial measures including lenders' size, capital, liquidity, and profitability as of 2012:Q2—that is, prior to the targeting of the first bank. We further consider whether there are differences between targeted and nontargeted banks' shares of lending to targeted industries, both in terms of volume and number of relationships with firms in those industries. Finally, we compare the average profitability as well as the average liquidity, leverage, and assessed probability of default of the firms in the banks' portfolios. Overall, we find that targeted and nontargeted banks do not differ significantly in any commonly observable characteristic.

3.3. Affected firms

We identify firms that were targeted by the DOJ using as a baseline the list of targeted sub-industries identified by the FDIC and listed in the expert witness report (Calomiris, 2017). Using this list, we manually search for the NAICS codes corresponding to the targeted industries on the NAICS Association website. For each industry, we conduct keyword searches, summarized in Table A.4 of the Online Appendix, to obtain the associated six-digit industry NAICS codes. Given the potential illegality of certain targeted industries, we exclude firms in industries such as cable box de-scramblers, credit card schemes, debt consolidation scams, get rich products, government grants, home-based charities, life-time guarantees and memberships, money transfer networks, Ponzi schemes, racist materials, and travel clubs. In addition, we remove financial firms which, in the context of our paper, implies excluding the relatively few payday lenders borrowing from regulated banks with at least \$50 billion in total assets. Our final dataset contains 5,670 affected firms, 595 of which are publicly listed.

²² See U.S. Department of Justice Civil Division Communication, titled "Payment Processor Investigation – Request for Issuance of Subpoenas to Payment Processors and Banks used to Process Fraudulent Payments", from Michael S. Blume, Consumer Protection Branch, to Stuart F. Delery, principal deputy assistant attorney general, dated February 8, 2013 (HOCR-3PPP000029-34).

²³ See, for example, "Statement of Financial Service Centers of America To the U.S. House of Representatives Committee on Financial Services Regarding The Impact of Recent Regulator Supervisory and Enforcement Actions on Consumer Financial Services", dated April 8, 2014.

4. Bank-level analysis

We start our analysis by examining whether Operation Choke Point affected lending to firms in affected industries by targeted banks, relative to lending by nontargeted banks.

4.1. Empirical specification

Our baseline specification is a staggered difference-in-differences model, exploiting the fact that firms that operated in the same industry and location borrowed from banks that were targeted at different points in time or were never targeted. Specifically, we estimate:

$$Y_{f,i,b,t} = \beta_1 \text{ChokePoint}_{b,t} + X_{b,t}\gamma + \delta_b + \delta_f + \delta_{t,size,industry,state} + \varepsilon_{f,i,b,t}, \quad (1)$$

where $Y_{f,i,b,t}$ is one of our outcomes of interest (e.g., total committed credit, share of drawn credit, interest rate spread) for firm f , operating in industry (six-digit NAICS code) i , borrowing from bank b in quarter t . Our baseline specification includes bank, δ_b , and firm fixed effects, δ_f , to control for time-invariant heterogeneity of both banks and firms. We include time–firm size–industry–state fixed effects, $\delta_{t,size,industry,state}$, with size attributed by quartiles to control for time-varying trends that affect firms of similar size operating in the same six-digit NAICS code and state. $\text{ChokePoint}_{b,t}$ is an indicator variable at the bank level and is set to one following the targeting of the bank by Operation Choke Point. The vector γ includes time-varying bank controls, such as size, capital, liquidity, and profitability. In the baseline analysis, we focus on the sample of loans to firms in industries affected by OCP.²⁴ As such, the primary coefficient of interest, β_1 , captures the within bank–firm changes following the targeting of the bank by Operation Choke Point. Standard errors are double clustered at the bank and state levels.²⁵

²⁴ As a robustness test, we also consider a triple-interaction specification using cash-intensive industries and firms from the same three-digit NAICS code as a control group.

²⁵ The state in which firms are located impacts the regulation of their activities in some cases. For instance, in 2011, the Department of Justice changed the way the federal government interpreted the Wire Act of 1961, which criminalized and prohibited the operation of certain betting or wagering businesses, such as online gambling. Over the following years, six states legalized online casino games, one of the targeted industries in our analysis.

Table 3
Impact of operation choke point on bank lending.
Data sources: Federal Reserve Y-14Q and Y-9C.

	Log(Committed credit)			Share of drawn credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Choke Point _{<i>it</i>}	−0.034*** (0.012)	−0.046*** (0.015)		−0.001 (0.004)	0.001 (0.006)	
Choke Point _{<i>it</i>} × SME _{<i>f</i>}			−0.095*** (0.026)			0.001 (0.009)
Choke Point _{<i>it</i>} × Large Firm _{<i>f</i>}			0.020 (0.040)			0.001 (0.012)
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Time FE	Y	–	–	Y	–	–
Time × Firm Size Quartiles × Industry × State FE	N	Y	Y	N	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Observations	51,105	51,105	51,105	51,105	51,105	51,105

This table reports the impact of Operation Choke Point on bank lending. The regression uses firm–bank–quarter-level data to compare lending between targeted and nontargeted banks, within the same industries affected by OCP. The dependent variables are the log of committed credit in columns (1)–(3) and the share of drawn credit in columns (4)–(6). *Choke Point* is an indicator variable at the bank level and is set to one following the targeting of the bank by OCP. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. Bank controls include bank size, capital, liquidity, and profitability. Standard errors are double clustered at the bank and state levels. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

4.2. Effect of Operation Choke Point on credit supply

We present the results of the estimation of Eq. (1) in Table 3. The coefficient in column (1) is negative and statistically significant at the 1% level, suggesting that banks targeted by the DOJ reduced their level of committed credit to firms in “high-risk” industries, relative to nontargeted banks, by approximately 3.4%. In column (2), we use our preferred specification including a tighter set of fixed effects (time–firm size quartiles–six-digit NAICS industry code–state, all interacted), and the effect remains economically large (4.6%) and statistically significant at the 1% level.

Given the literature documenting the heterogeneous effect of financing across firms based on their size, we consider how lending practices change across large firms and SMEs—as in Chodorow-Reich et al. (2021), for instance, firms are classified as SMEs if their assets are less than \$250 million. As reported in column (3) of Table 3, we find that the reduction in lending is concentrated among these smaller firms, with a decline of 9.5% in committed credit. In contrast, we find no significant effect for large firms. We next consider the share of drawn credit in columns (4) to (6), defined as the volume of utilized credit divided by the level of committed credit. We find no significant effect, suggesting that the level of credit drawn by firms changes proportionately to changes in committed credit.

We then analyze in Fig. 2 the effects of OCP on committed credit over time by plotting the dynamic coefficients relative to the quarter before the targeting. First, and crucial for our identification strategy, the results support the parallel trends assumption—that is, the point estimates before the program are close to zero and are statistically insignificant, indicating that there is no differential pre-trend in lending to firms in affected industries by targeted versus nontargeted banks. Second, there is a gradual and significant decrease in committed credit after OCP by targeted relative to nontargeted banks to affected firms.

Next, we explore the drivers of the decline in committed credit. As reported in Table A.5 of the Online Appendix, we find evidence that targeted banks reduce not only committed credit lines but also the rollover of term loans for affected firms. Lastly, we study whether the terms of the credit to affected firms are impacted, following the empirical specification described in Eq. (1). The results in Table A.6 of the Online Appendix suggest no effect on interest rate spreads (columns 1 and 2). In contrast, the results in columns (3) and (4) show that SMEs experience a decline in maturity of approximately 2.2 months or 4.5% of the mean maturity of 46 months. We also find that affected firms are more likely to post collateral, an effect driven by the terms imposed on small and medium-sized firms (columns 5 and 6).

4.3. Additional tests of Operation Choke Point on credit supply

The previous results suggest that the operation reduced lending by targeted banks to firms in affected industries. In this subsection, we conduct a series of additional tests to mitigate several concerns related to our tests and interpretation. First, we explore whether our results are biased based on our empirical specification. Second, we examine whether our results are biased due to the issues identified by the literature related to staggered differences in differences designs. Third, we consider whether spurious correlations may drive our results. Fourth, we assess whether events other than Operation Choke Point may drive our results. Finally, we assess whether loans with volumes close to the reporting threshold affect our results.

4.3.1. Empirical specification

We first rule out the possibility that our empirical specification drives our estimates. In column (1) of Panel A of Table 4, we present the estimates of a fixed effects Poisson specification on the level of committed credit and find similar economics and statistical results as those in Table 3. Next, we balance our sample by adding zeros to bank–firm–year observations with no reported loans to examine the intensive and extensive margins, combined, and estimate an OLS model.²⁶ We report the estimates in column (2) of Panel A, where we find similar results to our baseline. In column (3), we present the estimates resulting from the balanced panel but using a Poisson specification to account for the issues involving an outcome variable that is positive but can often equal zero (Cohn et al., 2022; Correia et al., 2020; Chen and Roth, 2024). We again find consistent evidence that there was a contraction in credit supply in response to Operation Choke Point. Finally, we use these three alternative estimation approaches for the share of drawn credit (columns 4 through 6) and estimate no statistical change around the initiation of Operation Choke Point, again matching our baseline estimates in Table 3.

4.3.2. Firm–bank matching

Another potential concern is that firms that borrow from targeted banks differ from those that borrow from other banks (e.g., have different demand for credit), even if these two types of firms operate in the same six-digit NAICS industry, have similar size, and are headquartered

²⁶ In our baseline intensive margin specification, following a relationship termination, the bank–firm pair would exit the sample.

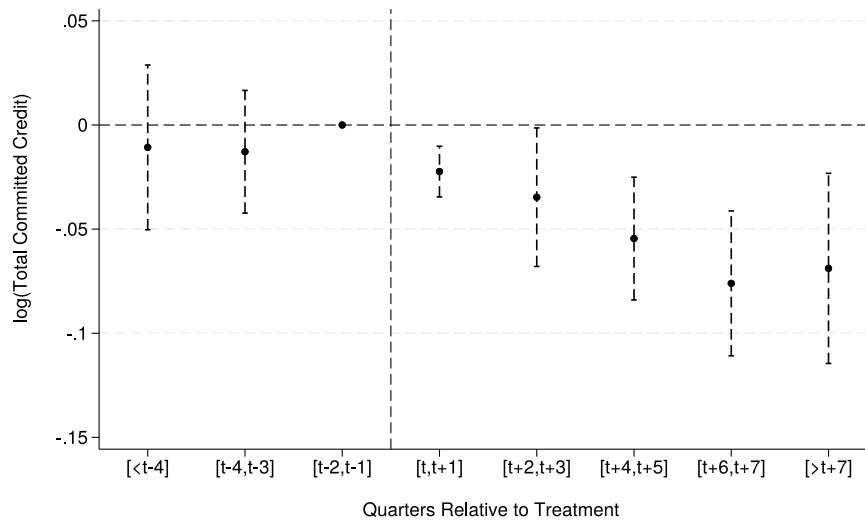


Fig. 2. Impact of operation Choke point on bank lending over time. This figure plots coefficients from a difference-in-differences specification comparing lending between targeted and nontargeted banks within the same industries affected by OCP. The dependent variable is the natural logarithm of committed credit at the bank–firm–quarter level. The horizontal axis is in event time relative to the quarter before targeting by Operation Choke Point. The estimated coefficients and their corresponding 90% confidence intervals correspond to the difference in the committed credit lending between targeted and nontargeted banks, within the same industries affected by OCP.

Data source: Federal Reserve Y-14Q and Y-9C.

Table 4

Impact of operation choke point on bank lending—additional tests.

Data sources: Federal Reserve Y-14Q and Y-9C.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Balanced Panel</i>	Committed credit (\$bn)			Share of drawn credit		
	Poisson, Unbalanced Panel	OLS, Balanced Panel	Poisson, Balanced Panel	Poisson, Unbalanced Panel	OLS, Balanced Panel	Poisson, Balanced Panel
Choke Point _{<i>b,t</i>}	−0.067** (0.033)	−3.640** (1.373)	−0.158*** (0.049)	0.000 (0.014)	−0.017 (0.014)	−0.020 (0.047)
Observations	51,105	87,313	87,313	51,105	87,313	87,313
<i>Panel B: Robustness Tests</i>	Log(Committed credit)			Share of drawn credit		
	Firm × Time FE (Khawaja–Mian)	Stacked Regression	Randomization Test	Firm × Time FE (Khawaja–Mian)	Stacked Regression	Randomization Test
Choke Point _{<i>b,t</i>}	−0.073** (0.029)	−0.061*** (0.021)	0.000 (0.018)	0.003 (0.014)	0.005 (0.006)	0.000 (0.006)
Observations	12,884	241,617	51,105	12,884	241,617	51,105

This table reports the impact of Operation Choke Point on bank lending. The dependent variables are the committed credit (in USD billions in Panel A and in logs in Panel B) and the share of drawn credit. *Choke Point* is an indicator variable at the bank level and is set to one following the targeting of the bank by OCP. Bank controls include bank size, capital, liquidity, and profitability. Standard errors are double clustered at the bank and state levels. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

in the same state. To mitigate this concern, in column (1) of Panel B (Table 4), we present the results of a specification that includes firm–time fixed effects to better control for credit demand and the nonrandom matching between firms and banks (Khawaja and Mian, 2008). Exploiting variation within firms that borrow from multiple banks, we find that the coefficient is still negative, large, and statistically significant. This finding suggests that, for the same firm, targeted banks reduce lending more than nontargeted ones. As in our baseline, in column (4) of Panel B, we find no change in the share of drawn credit when using a similar approach.

4.3.3. Biases in differences-in-differences designs

We explore whether our results are affected by the biases identified by the recent literature on staggered differences-in-differences designs—see Roth et al. (2023) for a detailed review. We repeat our

analysis using the stacked regression estimator methodology developed by Gormley and Matsa (2011, 2016) and described by Baker et al. (2022b) and present the results in columns (2) and (5), where we find estimates for the log of committed credit and share of drawn credit, respectively, that are largely consistent with those obtained using the two-way fixed effects methodology.

4.3.4. Minimum reporting threshold

The credit register provides information on credit exposures exceeding \$1 million for banks with more than \$50 billion in assets. Thus, one potential concern is that data truncation affects our results. In our setting, if a bank reduces the committed amount from \$1.01 million to \$0.99 million, it would be identified as an account termination and bias our results.

To alleviate the concern that this feature of the data is biasing our results, we run an additional test excluding loans close to the reporting threshold. The idea behind this test is that, for a loan of \$2 million to be dropped from our sample, it would need to be reduced by at least 50%. We sequentially exclude loans below the 25th percentile (\$2 million), the median (\$6 million), \$10 million, and \$20 million and re-estimate our baseline specification. We report the results in Table A.7 of the Online Appendix, where we find evidence suggesting that our results are not driven by loans close to the reporting threshold. In particular, we find that, regardless of the threshold used, the level of credit commitment decreases for small firms in affected industries that borrow from targeted banks, relative to nontargeted banks, while we find no effect on large firms. These results resemble those in the baseline test, including all loans. Further, in columns (5) to (8), we still do not observe a change in the share of drawn credit, again similar to our baseline. Importantly, in contrast to the setting of [Ivanov et al. \(2023\)](#) where two-lender deals were excluded from supervision, banks cannot avoid supervision by lending below the \$1 million threshold.²⁷

An additional potential concern is that affected firms can initiate relationships with or increase borrowing from nonreporting banks, that is, those with assets below the \$50 billion threshold. This would limit our ability to measure the full impact of the initiative on affected firms. We address this potential issue in Section 6.2, where we show, using balance-sheet information, that affected firms, on average, do not experience a significant change in total debt. This suggests that nonreporting banks and other types of lenders do not play a significant role in the dynamics studied.

4.3.5. Banks and industry trends

A potential remaining concern is that Operation Choke Point targeted banks that could have already been cutting lending to specific industries for reasons unrelated to the initiative. For instance, banks may have been de-risking following the effect of the Global Financial Crisis. We address this concern in several ways. First, we compare the riskiness of firms in industries affected by OCP with that of comparable cash-intensive firms in industries that were not impacted by OCP and with that of other similar firms in the same three-digit NAICS industries. We find that the assessed probability of default of firms in industries affected by OCP (2.1%) was similar to that of the other two groups (1.9% and 2%, respectively) and that the small difference was statistically insignificant.

Second, we expand our analysis and estimate the impact of the initiative on affected firms using a triple-differences estimator, including loans made by banks to firms in other industries. For robustness, we provide the results using two different sets of unaffected firms as controls. In the first group, we include as controls firms in comparable, cash-intensive industries, as identified by the NAICS association in conjunction with industry experts, that were not affected by OCP.²⁸ In the second group, we include all the firms in the same three-digit NAICS industries as those affected by OCP. We present the results in Table A.8 of the Online Appendix.

Using either set of control firms, we find results that suggest that our findings are not driven by contemporaneous events. When studying *Committed Credit* in columns (1) to (4), we find that the baseline coefficient for banks targeted by the initiative, *Choke Point*, is small and statistically insignificant. In contrast, the estimate for firms affected by the initiative, as measured by the interaction term *Choke Point* \times *OCP Industries* is negative and statistically significant, suggesting that targeted banks only reduced lending to affected industries. Moreover,

we find that the effect is concentrated among small firms (columns 2 and 4). When we explore the impact on the share of drawn credit in columns (5) to (8), we find results similar to the ones in our baseline analysis. That is, there was no change in the share of drawn credit, suggesting that the level of utilized credit reduced proportional to the reduction in committed credit. Overall, these sets of tests further suggest that targeted banks were limiting the supply of credit to firms in affected industries.

4.3.6. Other robustness

To test whether a specific industry drives our results, we iteratively drop each industry from our sample and re-estimate our baseline specification. We present the estimates in Table A.9 of the Online Appendix, where we show the effect on *committed credit* in Panel A and the effect on *share of drawn credit* in Panel B. In each column, we exclude from the sample firms in one industry (e.g., in Column (1) we exclude firms in the ammunition/firearms sales industry). Overall, we find that the effect of the policy remains similar in magnitude and statistically significant across columns, mitigating concerns that one industry drives the results. In the last column, we exclude firms in the three industries with significantly more firms (tobacco sales and paraphernalia, pharmaceutical sales, and ammunition/firearms Sales) and find that the results are not driven by firms in those industries.

Further, we address the concern that the results are biased by specific drivers of the timing of each bank's targeting date. We conduct a placebo test to mitigate this concern by randomizing treatment dates for affected banks. We run our baseline test 1,000 times, randomizing the targeting dates, and present the mean coefficient in Panel B column (3) in [Table 4](#), where we find no significant effect. Finally, we re-run this test for the share of drawn credit in column (6) and estimate no statistical relation. Overall, these tests provide further evidence that targeted banks were limiting the supply of credit to firms in affected industries in response to the initiative.

4.4. Heterogeneity across firm characteristics

We examine whether specific types of firms were more affected by Operation Choke Point. Specifically, we explore whether banks rationed credit differently for firms in different financial situations. To test this, we expand our empirical specification by interacting our main explanatory variables with a series of firm-level ratios that proxy for financial strength. In particular, we include profitability, liquidity, and leverage measures, splitting the sample based on the pre-period values.

Testing the relationship between financial strength and committed credit, columns (1) to (3) of [Table 5](#) show consistent evidence that SMEs were affected, irrespective of their profitability, liquidity, or leverage. These results underline our evidence that targeted credit rationing primarily sorted on firm size and not financial strength. We also find no differences across firms with different financial strengths when focusing on the share of drawn credit—as seen in columns (4) to (6).²⁹

5. Bank-firm relationships

A core aspect of Operation Choke Point was to cut off banking relationships for affected firms. Having established that targeted banks

²⁷ This serves only to identify loans to be included in the Y-14 data, which is used in the assessment of capital and stress testing, but bank examiners have access to information on all loans, regardless of size.

²⁸ These include gas stations, convenience stores, liquor stores, and parking lots, among others.

²⁹ The Y-14 data includes information on firms' assessed probability of default, but this information is included for a subset of firms and is measured by each bank without providing information on how it is measured. Given these limitations, we use this measure as an additional cross-sectional characteristic and present the results in Table A.10 of the Online Appendix, where we find that a firm's probability of default does not seem to affect the degree to which it is affected by the initiative and that it is size and not the probability of default that affects this, mitigating concerns of de-risking driving our results.

Table 5
Impact of operation choke point by firm characteristics.
Data source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed credit)			Share of drawn credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Choke Point _{it} × SME _f × Low Profitability _f	−0.102*** (0.024)			0.003 (0.009)		
Choke Point _{it} × SME _f × High Profitability _f	−0.083** (0.034)			−0.002 (0.011)		
Choke Point _{it} × Large Firm _f × Low Profitability _f	−0.020 (0.051)			0.012 (0.015)		
Choke Point _{it} × Large Firm _f × High Profitability _f	0.110 (0.072)			−0.026 (0.020)		
Choke Point _{it} × SME _f × Low Liquidity _f		−0.097*** (0.032)			−0.003 (0.009)	
Choke Point _{it} × SME _f × High Liquidity _f		−0.086*** (0.028)			0.013 (0.013)	
Choke Point _{it} × Large Firm _f × Low Liquidity _f		0.045 (0.074)			0.014 (0.020)	
Choke Point _{it} × Large Firm _f × High Liquidity _f		−0.005 (0.090)			−0.013 (0.020)	
Choke Point _{it} × SME _f × Low Leverage _f			−0.075** (0.030)			0.008 (0.011)
Choke Point _{it} × SME _f × High Leverage _f			−0.120*** (0.031)			−0.007 (0.009)
Choke Point _{it} × Large Firm _f × Low Leverage _f			−0.038 (0.063)			−0.012 (0.021)
Choke Point _{it} × Large Firm _f × High Leverage _f			0.073 (0.051)			0.012 (0.014)
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Time × Firm Size Quartiles × Industry × State FE	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Observations	51,105	51,105	51,105	51,105	51,105	51,105

This table reports the impact of Operation Choke Point on lending by firm characteristics. The regression uses firm–bank–quarter level data to compare lending between treated and non-treated banks, within the same industries affected by OCP. The regressions use the following dependent variables to estimate the effects on lending: columns (1)–(3) use the natural logarithm of committed credit; and columns (4)–(6) use the share of drawn credit. *Choke Point* is an indicator variable at the bank level and is set to one following the targeting of the bank by OCP. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. High and low measures of firm profitability, liquidity, and leverage are split relative to pre-period mean values. Standard errors are double clustered at the bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

reduced their supply of credit to firms in affected industries at the intensive margin, we turn our attention to bank–firm relationships. In this section, we first discuss our empirical strategy to explore this issue and then discuss the results of the termination and creation of bank relationships.

5.1. Empirical specification

To study bank–firm relationships, we aggregate data at the firm–quarter level to examine whether Operation Choke Point had an overall impact on firms in affected industries. As before, our baseline specification is a staggered difference-in-differences model, where we exploit the fact that firms that operate in the same industry and location borrowed from banks that were targeted at different points or were never targeted. Specifically, we estimate:

$$Y_{f,i,t} = \beta_1 \text{FirmExposureToChokePoint}_{f,t} + \delta_b + \delta_f + \delta_{t,size,industry,state} + \varepsilon_{f,i,t}, \quad (2)$$

where $Y_{f,i,t}$ is our outcome of interest at the firm–quarter level, studying firm f , operating in the industry i , at the calendar–quarter t time. Our baseline specification includes main bank fixed effects, δ_b , and firm fixed effects, δ_f , to control for time-invariant heterogeneity of banks and firms. Main bank is defined as the bank with the most lending to a firm in a quarter, which might vary over time. We include time–firm size quartiles–six-digit NAICS industry–state fixed effects, $\delta_{t,size,industry,state}$, to control for time-varying trends that affect firms of similar size operating in the same industry and state. $\text{FirmExposureToChokePoint}_{f,t}$ is the interaction of two variables, *Firm*

Exposure to Treated Bank and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. Importantly, the specification in Eq. (2) studies shocks at the firm level, whereas the specification described in Eq. (1) allows us to study effects at the bank–firm level. Our primary coefficient of interest, β_1 , captures the within-firm changes following the targeting of a bank from which the firm borrows. Standard errors are double clustered at the main bank and state levels.

5.2. Termination and creation of relationships

We start by examining whether firms linked to targeted banks experience account terminations and present the results in Table 6. We estimate a variation of Eq. (2) with the outcome variable defined as a dummy equal to one if, for a given time period, a firm had a relationship terminated in the following quarter with any bank (columns 1–2), with a targeted bank (columns 3–4), or with a nontargeted bank (columns 5–6) and zero otherwise. The coefficient reported in column (1) is small and statistically indistinguishable from zero, suggesting that affected firms did not experience a significant change in their number of account terminations following Operation Choke Point. This effect is similar across firm sizes, as shown in column (2). However, when we examine heterogeneous effects across bank types (targeted versus nontargeted), we find results that are consistent with our findings on the effectiveness of the initiative. Specifically, we show that affected firms did experience an increase in the frequency of the termination of

Table 6

Termination of banking relationships.

Data source: Federal Reserve Y-14Q and Y-9C.

	Relationship termination					
	with Any Bank		with Targeted Banks		with Nontargeted Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure to Choke Point _{<i>f,t</i>}	0.003 (0.008)		0.045*** (0.005)		−0.042*** (0.007)	
Firm Exposure to Choke Point _{<i>f,t</i>} × SME _{<i>f</i>}		0.005 (0.008)		0.044*** (0.005)		−0.039*** (0.007)
Firm Exposure to Choke Point _{<i>f,t</i>} × Large Firm _{<i>f</i>}		−0.015 (0.020)		0.056*** (0.017)		−0.073*** (0.010)
Time × Industry × Firm Size Quartiles × State FE	Y	Y	Y	Y	Y	Y
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,891	41,891	41,891	41,891	41,891	41,891

This table reports the impact of Operation Choke Point on the termination of bank relationships. Columns (1)–(2) study relationships with any bank, columns (3)–(4) focus on relationships with targeted banks, and columns (5)–(6) focus on relationships with nontargeted banks. *Firm Exposure to Choke Point* is the interaction of two variables, *Firm Exposure to Treated Bank* and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

their accounts (column 3). This effect is significant, with an increase in the frequency of account terminations of 4.5 percentage points, corresponding to an increase of 94% over the baseline level of 4.8 percent. This effect is driven by a significant increase in account terminations for small and medium-sized firms and by a similar effect on large firms (column 4). In addition, we find that affected firms experience a reduction in the frequency with which their accounts with nontargeted banks are terminated (column 5), suggesting that these firms try to mitigate the impact of Operation Choke Point on credit availability. Specifically, this frequency declines by 4.2 percentage points, a decline equivalent to 87.5% of the baseline level. This effect is large and significant for both SMEs and large firms. This finding is consistent with the evidence presented in the expert witness report of Calomiris (2017) and with the idea that affected firms strive to preserve their relationships with nontargeted banks.

In Table 7, we explore the effect of the initiative on the development of new relationships. The outcome variable is now defined as a dummy equal to one if, in a given time period, a firm created a relationship with any bank (columns 1–2), with a targeted bank (columns 3–4), or with a nontargeted bank (columns 5–6) and zero otherwise. We find that, on average, affected firms increase the rate at which they initiate new relations with banks by 3.4 percentage points or approximately 32% of the baseline level (column 1). We find that this effect is driven by new accounts opened by SMEs (column 2). As with account terminations, we then explore heterogeneous effects across bank types. We find that, following the targeting of their banks, affected firms initiate fewer relationships with banks singled out by Operation Choke Point (column 3). This effect is large across firm sizes and statistically significant for SMEs (column 4). In addition, we find that these firms significantly increase the rate of initiation of new relationships with nontargeted banks, evidenced by the large and significant coefficients in columns (5) and (6), which translate into an average increase of 74% over the baseline level.

We provide the results of this analysis further split across different types of SMEs and large firms in Tables A.11 and A.12 of the Online Appendix. Overall, we find that targeted banks terminate relationships with all types of firms, regardless of their size and ratios of profitability, liquidity, or leverage. Similarly, firms of all types preserve relationships with nontargeted banks. When we study the creation of new relationships with targeted banks, we find a large and similar decline across all firm types, although the effect is statistically significant only for SMEs. This effect does not depend on these firms' profitability, liquidity, or leverage ratios. Last, we find that all types of firms increase the rate at

which they initiate relationships with nontargeted banks, regardless of their type. This suggests that nontargeted banks still find it profitable to lend to firms terminated by targeted banks, given that they have not faced regulatory pressure.

Overall, the evidence presented in this subsection suggests that banks targeted by Operation Choke Point effectively terminated accounts with firms in affected industries. In response, these firms initiated new relationships or preserved relationships with nontargeted banks to mitigate or offset the operation's effect. However, given the results presented so far, the net effect on firms' access to credit remains unclear. On one hand, a long-term banking relationship can help borrowers, as it may lessen information asymmetries through the generation of private information, enabling the bank to offer improved loan conditions (Petersen and Rajan, 1994), especially within ESG decisions (Houston and Shan, 2022). On the other hand, banks could potentially leverage this exclusive information to exploit firms and provide less favorable terms (Rajan, 1992). In the next section, we explore this issue.

6. Firm-level borrowing

We next explore the overall effect of Operation Choke Point on firms. We start by studying the initiative's effect on firm-level access to credit and then analyze its impact on firm performance and operations.

6.1. Net effect on committed and utilized credit

We first examine the effect on committed credit and share of drawn credit for affected firms. As before, we use aggregate data at the firm–quarter level.

The results are reported in Table 8. We find that affected firms experience no change in aggregate committed credit, with statistically insignificant coefficients across specifications in columns (1) through (3)—that is, for not only the average firm but also for large firms and SMEs. When we analyze the share of drawn credit in columns (4) through (6), we also find that all the coefficients are statistically indistinguishable from zero, pointing to no overall effect on total committed or drawn credit. Overall, these findings indicate that affected firms were generally able to offset the reduction in committed credit by targeted banks by obtaining more committed credit from nontargeted banks.

When we study the impact of firms with different characteristics, we find evidence of mostly homogeneous effects across firm types.

Table 7
Creation of banking relationships.
Data source: Federal Reserve Y-14Q and Y-9C.

	Relationship creation					
	with Any Bank		with Targeted Banks		with Nontargeted Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure to Choke Point _{<i>f,t</i>}	0.034*** (0.012)		−0.042** (0.016)		0.079*** (0.023)	
Firm Exposure to Choke Point _{<i>f,t</i>} × SME _{<i>f</i>}		0.036*** (0.012)		−0.043** (0.017)		0.080*** (0.023)
Firm Exposure to Choke Point _{<i>f,t</i>} × Large Firm _{<i>f</i>}		0.015 (0.038)		−0.038 (0.032)		0.063** (0.023)
Time × Industry × Firm Size Quartiles × State FE	Y	Y	Y	Y	Y	Y
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,891	41,891	41,891	41,891	41,891	41,891

This table reports the impact of Operation Choke Point on the creation of bank relationships. Columns (1)–(2) study relationships with any bank, columns (3)–(4) focus on relationships with targeted banks, while columns (5)–(6) focus on relationships with nontargeted banks. *Firm Exposure to Choke Point* is the interaction of two variables, *Firm Exposure to Treated Bank* and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 8
Change in firm level borrowing.
Data source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed credit)			Share of drawn credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure to Choke Point _{<i>f,t</i>}	−0.029 (0.017)	0.005 (0.013)		0.009 (0.008)	0.003 (0.009)	
Firm Exposure to Choke Point _{<i>f,t</i>} × SME _{<i>f</i>}			−0.003 (0.011)			0.003 (0.009)
Firm Exposure to Choke Point _{<i>f,t</i>} × Large Firm _{<i>f</i>}			0.084 (0.051)			0.003 (0.015)
Time × Industry × Firm Size Quartiles × State FE	N	Y	Y	N	Y	Y
Time FE	Y	N	N	Y	N	N
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,891	41,891	41,891	41,891	41,891	41,891

This table reports the impact of Operation Choke Point on firm level borrowing. The regressions use firm-quarter level data to compare changes in total credit between treated firms of targeted and nontargeted banks. The dependent variable of columns (1)–(3) is the natural logarithm of committed credit, while columns (4)–(6) use the share of drawn credit. *Firm Exposure to Choke Point* is the interaction of two variables, *Firm Exposure to Treated Bank* and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Specifically, we find that the majority of coefficients in Table 9 for both aggregate committed credit and the share of drawn credit are not statistically significant, with two important exceptions. First, large profitable firms managed to increase their level of committed credit, suggesting that they hedged against future potential terminations (column 1). In contrast, highly levered SMEs experienced a modest reduction in committed credit (column 3). These results indicate that targeted credit rationing driven by Operation Choke Point did not reduce overall credit for most firms, except for a small subset, making it broadly ineffective.

Finally, we examine potential changes in firm loan terms and find that firms did not seem to experience aggregate changes in interest rate spreads, as evidenced in columns (1) and (2) of Table A.13 of the Online Appendix. However, they did experience a shortening in the maturity of the loans (column 3). This effect is concentrated among SMEs (column 4). We also find an increase in the likelihood of these firms pledging collateral but again only among SMEs (columns 5 and 6).

6.2. Financial performance of affected firms

Given the impact of Operation Choke Point on banking relationships, we additionally examine its impact on the financial and operational performance of affected firms. We follow the specification in Eq. (2) and analyze firm-level measures, such as leverage, profitability, and investment.

The estimates presented in Table 10 suggest no observable change, on average, for firms exposed to targeted banks. The coefficients in columns (1) and (2) suggest that there was no effect on leverage, as defined by total debt over assets. This finding is consistent across firm sizes and mitigates concerns related to the truncation of our data, given that this data includes lending by all banks. These firms also do not experience changes in profitability, as measured by return on assets (columns 3 and 4). Similarly, we find no effect on investment, as measured by capital expenditures scaled by assets (columns 5 and 6).

We find similar results across firm types. In particular, the coefficients in Table A.14 of the Online Appendix suggest that the initiative

Table 9

Change in firm level borrowing by firm characteristics.

Data source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed credit)			Share of drawn credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure to Choke Point _{f,t} × SME _f × Low Profitability _f	−0.009 (0.018)			0.006 (0.005)		
Firm Exposure to Choke Point _{f,t} × SME _f × High Profitability _f	0.006 (0.012)			−0.001 (0.017)		
Firm Exposure to Choke Point _{f,t} × Large Firm _f × Low Profitability _f	0.047 (0.067)			0.026 (0.029)		
Firm Exposure to Choke Point _{f,t} × Large Firm _f × High Profitability _f	0.225** (0.083)			−0.086 (0.066)		
Firm Exposure to Choke Point _{f,t} × SME _f × Low Liquidity _f		−0.004 (0.009)			0.006 (0.009)	
Firm Exposure to Choke Point _{f,t} × SME _f × High Liquidity _f		−0.002 (0.028)			−0.005 (0.015)	
Firm Exposure to Choke Point _{f,t} × Large Firm _f × Low Liquidity _f		0.041 (0.050)			−0.007 (0.012)	
Firm Exposure to Choke Point _{f,t} × Large Firm _f × High Liquidity _f		0.173 (0.136)			0.023 (0.045)	
Firm Exposure to Choke Point _{f,t} × SME _f × Low Leverage _f			0.017 (0.016)			0.004 (0.010)
Firm Exposure to Choke Point _{f,t} × SME _f × High Leverage _f			−0.031** (0.014)			0.003 (0.014)
Firm Exposure to Choke Point _{f,t} × Large Firm _f × Low Leverage _f			0.077 (0.059)			−0.015 (0.025)
Firm Exposure to Choke Point _{f,t} × Large Firm _f × High Leverage _f			0.098 (0.069)			0.028 (0.023)
Time × Industry × Firm Size Quartiles × State FE	Y	Y	Y	Y	Y	Y
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,891	41,891	41,891	41,891	41,891	41,891

This table reports the impact of Operation Choke Point on firm level borrowing by firm characteristics. The regressions use firm-quarter level data to compare changes in total credit between treated firms of targeted and nontargeted banks. The dependent variable of columns (1)–(3) is the natural logarithm of committed credit, while columns (4)–(6) use the share of drawn credit. *Firm Exposure to Choke Point* is the interaction of two variables, *Firm Exposure to Treated Bank* and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. High and low measures of firm profitability, liquidity, and leverage are split relative to pre-period mean values. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 10

Change in firm performance.

Data source: Federal Reserve Y-14Q and Y-9C.

	Total Debt/Assets		ROA		Capex/Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure to Choke Point _{f,t}	0.005 (0.007)		0.002 (0.004)		−0.002 (0.002)	
Firm Exposure to Choke Point _{f,t} × SME _f		0.007 (0.006)		0.002 (0.004)		−0.002 (0.002)
Firm Exposure to Choke Point _{f,t} × Large Firm _f		−0.010 (0.022)		0.004 (0.008)		−0.000 (0.004)
Time × Industry × Firm Size Quartiles × State FE	Y	Y	Y	Y	Y	Y
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,891	41,891	41,891	41,891	41,891	41,891

This table reports the impact of Operation Choke Point on firm performance. The dependent variables are total debt to assets (columns 1–2), return on assets (column 3–4), and total capital expenditures to assets (column 5–6). *Firm Exposure to Choke Point* is the interaction of two variables, *Firm Exposure to Treated Bank* and *Post*. *Firm Exposure to Treated Bank* is a firm's exposure to the shock, measured as the pre-period share of its total credit provided by banks that were subsequently targeted. *Post* is an indicator variable at the firm level and is set to one following the targeting of any bank that lends to the firm. *Large Firm* is an indicator variable at the firm level and is set to one if the firm's assets is greater than or equal to \$250 million in 2012:Q2. *SME* is an indicator variable at the firm level and is set to one if the firm's assets is less than \$250 million in 2012:Q2. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroskedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

was ineffective at impacting firms of different characteristics. We find that the overall effect on leverage, profitability, and investment was economically small and statistically insignificant, regardless of firm size, profitability, liquidity, and leverage ratios. Last, we test whether firms adjust trade credit or banks change their assessment of the affected firms' default probability but still find no effect, as shown in Table A.17 of the Online Appendix.

Overall, our results show that targeted credit rationing had a neutral effect on firms' performance. Although the operation had an initial effect on lending by targeted banks, affected firms responded by borrowing from other banks. The terms of these new loans did not differ significantly from the original terms that those firms had with the targeted banks. More generally, we find that these firms did not experience an impact in terms of total committed or drawn credit, leverage, profitability, or investment, suggesting that credit rationing had an

insignificant effect on firms in targeted industries. Our estimates underscore the challenges of using targeted credit rationing to influence corporate behavior. While this approach can initially restrict access to credit for affected firms, the observed credit substitution limits its overall effectiveness. This suggests that stakeholders may need to explore alternative or complementary strategies to hold firms accountable, especially larger ones.

6.3. Additional tests

Given the structure of the Y-14Q data, a natural concern with our analysis is that there might be an attrition bias that could affect our results. Indeed, our findings are conditional on firms continuing to receive credit from reporting banks, which could present a selection problem if some firms were more likely to be rationed to the point that their performance is unobservable *ex post*. Given the importance of this possibility, we investigate it in multiple ways.

First, we plot the evolution of the number of firms in industries affected by OCP in our sample over time. As counterfactual, we also plot the evolution of (i) the number of firms in cash-intensive industries not affected by OCP and (ii) the number of firms in industries not affected by OCP but within the same three-digit NAICS codes of those affected by OCP. As shown in Figure A.4 of the Online Appendix, the trends in the underlying data are similar, indicating comparable attrition rates and no considerable difference between these types of firms.

Second, we take a step further by estimating a Cox proportional hazard model to measure the survival rates of firms in industries affected by OCP relative to firms in unaffected industries. This model is chosen for its flexibility in providing a baseline hazard rate without relying on parametric assumptions. It handles right censoring in the data, using a maximum likelihood estimator to account for firms that have not yet dropped out of the sample. As reported in Table A.15 of the Online Appendix, we reject the null hypothesis that firms in industries affected by OCP drop out of the sample at a different rate than unaffected firms in the two counterfactual groups. Indeed, both estimates are economically small and statistically insignificant, further mitigating concerns of possible selection bias.

Third, given the possibility that riskier firms might be rationed first or more stringently and thus be at a higher risk of dropping out from the sample, we show in Table A.16 of the Online Appendix this is also unlikely in our setting since firms in industries affected by OCP do not have a higher *ex ante* default probability than firms in unaffected industries. Specifically, the mean assessed default probability for firms in industries affected by OCP is 2.1% as of the second quarter of 2012 (1.9% when considering only affected firms at targeted banks), while for firms in cash-intensive industries unaffected by OCP is 1.9% and for other firms in the same three-digit NAICS code is 2% in the same period.³⁰ None of these differences is statistically significant.³¹

Finally, we also examine whether firms' exposure to the shock has an effect on attrition rates within our baseline sample of firms in industries affected by OCP, with the outcome variable set to one at time t if a given firm f no longer appears in the data in the following quarter ($t+1$) and zero otherwise. As shown in column (5) of Table A.17 of the Online Appendix, the main coefficient of interest is small and

statistically insignificant. In column (6), we observe the same pattern when disaggregating the effect across SMEs and large firms. Overall, our evidence suggests that the results are unaffected by attrition from the sample.³²

6.4. Policy implications

Our findings have significant implications for understanding targeted credit rationing practices, especially in the context of financial intermediaries' commitments to environmental and social goals e.g., through the Net Zero Banking Alliance, a UN-convened bank-led group. For banks aiming to limit lending to so-called brown firms — those whose activities contribute to higher carbon emissions — this paper highlights the potential substitution effects that may arise. Our paper also provides valuable insights into the contentious debate surrounding the implementation of Operation Choke Point 2.0, with previous proposals suggesting that financial intermediaries could curtail lending to firms in cryptocurrency and digital assets industries (Forbes, 2024).

More broadly, this paper sheds light on one of several tools available to regulators and governments aiming to drive societal change. Historical approaches have included blanket bans on banks operating within specific jurisdictions (Garrett and Ivanov, 2025) or restricting firms' access to essential cash management systems (Berger and Seegert, 2024). Our paper contributes to the broader environmental and social finance literature by documenting the efficacy of such programs and highlighting potential unintended consequences. In doing so, we provide a comprehensive analysis that situates targeted credit rationing within the larger framework of tools to achieve societal goals.

7. Conclusion

Over the last decade, stakeholders have increasingly searched for ways to affect the operations of firms that generate negative externalities. These actions can be undertaken by shareholders, who can exercise voting rights or divest, or by banks, which can ration credit. Thus, assessing the effectiveness of targeted credit rationing in disrupting the operations of firms in controversial industries is crucial. Nonetheless, the empirical evidence on this issue is scarce.

We exploit a regulatory initiative that provides exogenous variation in credit rationing to firms in specific industries. Using supervisory loan-level data, we document that credit rationing does affect banking relationships, with targeted banks reducing lending and terminating relationships with firms in affected industries. However, these firms initiate new relationships with nontargeted banks and manage to obtain loans with similar terms to the ones they had. Indeed, we show that these firms do not experience measurable changes in performance, highlighting that target credit rationing by a subset of banks can be ineffective. Our findings have significant implications for current debates on whether credit rationing to specific industries is effective in promoting change.

³⁰ Default probabilities when considering only affected firms at targeted banks (1.9%) and that in Table 2 (default probability=2.0%) are slightly different since the former is the mean across firms, while the latter is a mean of means with differences in portfolio sizes across banks.

³¹ As shown before in Table 2, we also do not find that the *ex ante* default probability of firms in industries affected by OCP borrowing from targeted banks is statistically different from that of firms borrowing from nontargeted banks. In addition, the results reported before in Table A.10 of the Online Appendix indicate that SMEs are impacted at the intensive margin to the same degree, regardless of differences in their *ex ante* default probability.

³² A separate possible concern in our study is that, despite using comprehensive FR-Y14 data, we do not observe lending by smaller banks and nonbanks. While this is possible, substituting credit from traditional banks for other sources would bias against our finding that targeted credit rationing is ineffective. Second, some smaller banks not in the FR-Y14 data were also subject to OCP, so they were not immune to the shock. Moreover, in addition to finding that firms substitute credit within the traditional banking sector in our dataset, we find that firms' leverage ratios remain unchanged, suggesting that smaller banks and nonbanks are not key actors in providing credit to targeted firms.

CRedit authorship contribution statement

Kunal Sachdeva: Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Conceptualization. **André F. Silva:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Data curation. **Pablo Slutzky:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Conceptualization. **Billy Y. Xu:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Conceptualization.

Declaration of Generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used ChatGPT to polish the writing of certain paragraphs. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

References

- Agca, S., Slutzky, P., Zeume, S., 2023. The weight of compliance: Anti-money laundering enforcement, bank composition, and lending. Working Paper.
- Aiyar, S., Calomiris, C.W., Wieladek, T., 2014. Does macro-prudential regulatory leak? Evidence from a UK policy experiment? *J. Money, Credit, Bank.* 46, 181–214.
- Baker, M., Bergstresser, D., Serafeim, G., Wurgler, J., 2022a. The pricing and ownership of US green bonds. *Annu. Rev. Financ. Econ.* 14 (1), 415–437.
- Baker, A.C., Larcker, D.F., Wang, C.C., 2022b. How much should we trust staggered difference-in-differences estimates? *J. Financ. Econ.* 144 (2), 370–395.
- Baldauf, M., Garlappi, L., Yannelis, C., 2020. Does climate change affect real estate prices? Only if you believe in it. *Rev. Financ. Stud.* 33 (3), 1256–1295.
- Baugh, B., 2016. Payday borrowing and household outcomes: Evidence from a natural experiment. Working Paper.
- Becht, M., Franks, J.R., Wagner, H.F., 2025. The benefits of access: Evidence from private meetings with portfolio firms. *J. Finance* (forthcoming).
- Beck, T., Degryse, H., De Haas, R., Van Horen, N., 2018. When arm's length is too far: Relationship banking over the credit cycle. *J. Financ. Econ.* 127 (1), 174–196.
- Beck, T., Demirgüç-Kunt, A., Maksimovic, V., 2005. Financial and legal constraints to growth: does firm size matter? *J. Financ.* 60 (1), 137–177.
- Bellon, A., 2024. Fresh start or fresh water: The impact of environmental lender liability. Working Paper.
- Ben-David, I., Jang, Y., Kleimeier, S., Viehs, M., 2021. Exporting pollution: where do multinational firms emit CO₂? *Econ. Policy* 36 (107), 377–437.
- Berger, E.A., Seeger, N., 2024. Half banked: The economic impact of cash management in the Marijuana industry. *J. Financ.* 79 (4), 2759–2796.
- Berk, J.B., van Binsbergen, J.H., 2025. The impact of impact investing. *J. Financ. Econ.* 164, 103972.
- Bolton, P., Kacperczyk, M., 2021. Do investors care about carbon risk? *J. Financ. Econ.* 142 (2), 517–549.
- Bolton, P., Kacperczyk, M., 2023. Global pricing of carbon-transition risk. *J. Financ.* 78 (6), 3677–3754.
- Broccardo, E., Hart, O., Zingales, L., 2022. Exit versus voice. *J. Political Econ.* 130 (12), 3101–3145.
- Brown, J.R., Gustafson, M.T., Ivanov, I.T., 2021. Weathering cash flow shocks. *J. Financ.* 76 (4), 1731–1772.
- Calomiris, C., 2017. Expert report of Charles Calomiris in community financial services association of America, Ltd., et al. v. Federal deposit insurance corporation, et al. (Civil action no. 14-953-GK). pp. 1–49.
- Chen, J., Roth, J., 2024. Logs with zeros? Some problems and solutions. *Q. J. Econ.* 139 (2), 891–936.
- Chernenko, S., Erel, I., Prilmeier, R., 2022. Why do firms borrow directly from nonbanks? *Rev. Financ. Stud.* 35 (11), 4902–4947.
- Chodorow-Reich, G., Darmouni, O., Luck, S., Plosser, M., 2021. Bank liquidity provision across the firm size distribution. *J. Financ. Econ.* 144 (3), 908–932.
- Chowdhry, B., Davies, S.W., Waters, B., 2019. Investing for impact. *Rev. Financ. Stud.* 32 (3), 864–904.
- Cohen, L., Gurun, U.G., Nguyen, Q.H., 2024. The ESG-innovation disconnect: Evidence from green patenting. Working Paper.
- Cohn, J., Liu, Z., Wardlaw, M., 2022. Count (and count-like) data in finance. *J. Financ. Econ.* 146 (2), 529–551.
- Correia, S., Guimarães, P., Zylkin, T., 2020. Fast Poisson estimation with high-dimensional fixed effects. *Stata J.* 20 (1), 95–115.
- Crosignani, M., Macchiavelli, M., Silva, A.F., 2023. Pirates without borders: The propagation of cyberattacks through firms' supply chains. *J. Financ. Econ.* 147 (2), 432–448.
- Duchin, R., Gao, J., Xu, Q., 2025. Sustainability or greenwashing: Evidence from the asset market for industrial pollution. *J. Financ.* 80 (2), 699–754.
- Edmans, A., Levit, D., Schneemeier, J., 2024. Socially responsible divestment. Working Paper.
- Engle, R.F., Giglio, S., Kelly, B., Lee, H., Stroebe, J., 2020. Hedging climate change news. *Rev. Financ. Stud.* 33 (3), 1184–1216.
- Flammer, C., 2021. Corporate green bonds. *J. Financ. Econ.* 142 (2), 499–516.
- Forbes, 2024. The 5 most important promises Trump made at Bitcoin 2024. July 27, 2024.
- Garrett, D., Ivanov, I., 2025. Gas, guns, and governments: Financial costs of anti-ESG policies. *J. Finance* (forthcoming).
- Giannetti, M., Jasova, M., Loumriot, M., Mendicino, C., 2024. “Glossy green” banks: The disconnect between environmental disclosures and lending activities. Working Paper.
- Gopal, M., Schnabl, P., 2022. The rise of finance companies and fintech lenders in small business lending. *Rev. Financ. Stud.* 35 (11), 4859–4901.
- Gormley, T.A., Matsa, D.A., 2011. Growing out of trouble? Corporate responses to liability risk. *Rev. Financ. Stud.* 24 (8), 2781–2821.
- Gormley, T.A., Matsa, D.A., 2016. Playing it safe? Managerial preferences, risk, and agency conflicts. *J. Financ. Econ.* 122 (3), 431–455.
- Green, D., Vallee, B., 2025. Measurement and effects of bank exit policies. *J. Financial Econ.* (forthcoming).
- Hart, O., Zingales, L., 2017. Companies should maximize shareholder welfare not market value. *J. Law* 2, 247–274.
- Hartzmark, S.M., Shue, K., 2023. Counterproductive sustainable investing: The impact elasticity of brown and green firms. Working Paper.
- Hausalter, D., Henry, J.J., Iliev, P., 2023. Can banks save mountains? *Rev. Corp. Financ. Stud.* 12 (4), 761–791.
- Houston, J.F., Shan, H., 2022. Corporate ESG profiles and banking relationships. *Rev. Financ. Stud.* 35 (7), 3373–3417.
- Ivanov, I.T., Kruttil, M.S., Watugala, S.W., 2024. Banking on carbon: Corporate lending and cap-and-trade policy. *Rev. Financ. Stud.* 37 (5), 1640–1684.
- Ivanov, I.T., Ranish, B., Wang, J., 2023. Banks' strategic responses to supervisory coverage: Evidence from a natural experiment. *J. Money, Credit, Bank.* 55 (2–3), 503–530.
- Kacperczyk, M.T., Peydró, J.-L., 2024. Carbon emissions and the bank-lending channel. Working Paper.
- Khawaja, A.I., Mian, A., 2008. Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *Am. Econ. Rev.* 98 (4), 1413–1442.
- Kim, S., Plosser, M.C., Santos, J.A., 2018. Macroprudential policy and the revolving door of risk: Lessons from leveraged lending guidance. *J. Financ. Intermediation* 34, 17–31.
- Laeven, L., Popov, A., 2023. Carbon taxes and the geography of fossil lending. *J. Int. Econ.* 144, 103797.
- Miguel, F., Pedraza, A., Ruiz-Ortega, C., 2024. Climate-change regulations: Bank lending and real effects. *J. Financ. Stab.* 70, 101212.
- Oehmke, M., Opp, M.M., 2025. A theory of socially responsible investment. *Rev. Econ. Stud.* 92 (2), 1193–1225.
- Pástor, L., Stambaugh, R.F., Taylor, L.A., 2021. Sustainable investing in equilibrium. *J. Financ. Econ.* 142 (2), 550–571.
- Pástor, L., Stambaugh, R.F., Taylor, L.A., 2022. Dissecting green returns. *J. Financ. Econ.* 146 (2), 403–424.
- Pedersen, L.H., Fitzgibbons, S., Pomorski, L., 2021. Responsible investing: The ESG-efficient frontier. *J. Financ. Econ.* 142 (2), 572–597.
- Petersen, M.A., Rajan, R.G., 1994. The benefits of lending relationships: Evidence from small business data. *J. Financ.* 49 (1), 3–37.
- Rajan, R.G., 1992. Insiders and outsiders: The choice between informed and arm's-length debt. *J. Financ.* 47 (4), 1367–1400.
- Roth, J., Sant'Anna, P.H., Bilinski, A., Poe, J., 2023. What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *J. Econometrics* 235 (2), 2218–2244.

- Sastry, P., Verner, E., Marqués Ibáñez, D., 2024. Business as usual: bank climate commitments, lending, and engagement. Working Paper.
- Stevenson, D.D., 2022. Operation choke point: Myths and reality. *Adm. Law Rev.* 75.
- Stroebel, J., Wurgler, J., 2021. What do you think about climate finance? *J. Financ. Econ.* 142 (2), 487–498.
- Teoh, S.H., Welch, I., Wazzan, C.P., 1999. The effect of socially activist investment policies on the financial markets: Evidence from the South African boycott. *J. Bus.* 72 (1), 35–89.
- Zywicki, T., 2015. Rent-seeking, crony capitalism, and the crony constitution. *Supreme Court. Econ. Rev.* 23 (1), 77–103.