Defunding Controversial Industries: Can Targeted Credit Rationing Choke Firms?[†]

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Abstract

This study investigates the effects of targeted credit rationing by banks on firms that are likely to generate negative externalities. We use data from Operation Choke Point, a regulatory initiative in the United States that aimed to limit bank relationships with firms in high-risk industries for fraud and money laundering. Our analysis of supervisory loan-level data reveals that targeted banks reduce lending and terminate relationships with affected firms. However, these firms fully substitute credit availability by obtaining loans from non-targeted banks under similar terms, resulting in no changes in total debt, investment, or profitability. Our findings suggest that targeted credit rationing is ineffective in promoting change.

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1 Introduction

Stakeholders are increasingly seeking ways to hold companies accountable for their negative externalities on society. One common method has been the active divestment of equity, where investors sell off stocks in firms perceived as socially irresponsible in order to raise their cost of capital and apply pressure to encourage firms to address these externalities. While this strategy has gained widespread popularity in recent decades, research has shown its effectiveness is limited. In particular, equity divestments are mostly restricted to listed firms and can result in the loss of investor influence within a company (Broccardo, Hart, and Zingales, 2022; Edmans, Levit, and Schneemeier, 2022), firms may respond by offshoring or spinning out parts of their "undesirable" business (Ben-David et al., 2021; Dai et al., 2021), and it potentially stifles innovation (Cohen, Gurun, and Nguyen, 2020). Additionally, the limited amount of capital behind these efforts restricts their impact (Berk and van Binsbergen, 2021).

In response to the limitations of equity divestment campaigns, stakeholders have turned to alternative strategies, such as targeting a firm's access to credit. This approach, known as *targeted credit rationing*, has gained appeal due to its ability to impact both public and private firms. Further, unlike equity divestment where capital can be easily replaced, credit and bank relationships are typically "sticky" and can be difficult to replace due to the existence of asymmetric information. Banks often hold a monopoly on the private information generated by their relationships, which is not easily transferable or replaceable (Rajan, 1992; Sharpe, 1990). These features suggest that targeted credit rationing has the potential to be a more effective mechanism to influence corporate behavior due to its broad coverage and the likely economic cost it imposes on firms.

Despite its potential, little is known about its effectiveness. Documenting the causal impact of targeted credit rationing on firms' operations is difficult for several reasons. First, a bank's decision to extend credit depends not only on its preferences but also on its expectations about a firm's future cash flows. These factors are often time-varying

and hard to observe, making it challenging to infer the cause of changes in a firm's performance following credit rationing. Second, comprehensive loan-level data for a large number of firms of various sizes, including both publicly traded and privately held ones, is generally scarce. As a result, the effectiveness of targeted credit rationing remains an open question.

We overcome these challenges in two ways to causally identify the effectiveness of targeted credit rationing. First, we explore the effects of Operation Choke Point (OCP), a regulatory initiative led by the U.S. Department of Justice (DOJ). OCP provides a nearly ideal quasi-experimental setting to study this issue because it affected a seemingly random subset of banks that were compelled to cut lending to firms in certain industries deemed to have a high risk of money laundering and fraud. These industries included trade of ammunition, firearms, tobacco, dating and escort services, pornography, and online gambling. The DOJ threatened with significant sanctions to banks that failed to comply with the operation, which was a large and credible shock to the targeted banks. Additionally, the operation appeared to affect an arbitrary subset of banks. The DOJ did not use any clear targeting criteria or consider banks' lending volume to firms in targeted industries or other bank characteristics. This minimizes concerns about potential selection bias. Finally, the operation was implemented at different points in time, allowing us to identify its effects through a staggered difference-in-differences analysis.

Our second key element is the use of confidential credit register data for the United States, which provides detailed information on banks' loans and firm performance and allows us to overcome the data limitation problem. We use confidential quarterly loan-level data from the corporate loan schedule H.1 of the Federal Reserve's Y-14Q. The data set provides information on the universe of corporate loans with commitment amounts exceeding $1 million for banks with at least $50 billion in total assets, together with firm-level characteristics. We combine this data with bank-holding company-level financial information from publicly available quarterly reports (FR Y-9C).

We formally test and show that Operation Choke Point affected targeted banks lending behavior. At the intensive margin, we find that targeted banks reduced their committed credit to firms in affected industries. We find that the credit contraction was concentrated mainly around small and medium-sized enterprises (SME), which experienced a 10 percent reduction in committed credit. In contrast, we find no significant changes when considering the level of credit utilization by affected firms, suggesting that the contraction in the availability of credit was not binding. However, we also observe that banks imposed stricter loan terms, but again only on SMEs. Specifically, we find that the average maturity decreased by two months for these firms, a 5 percent decline relative to the mean loan term. The average level of collateral requirement was also affected, with an increase of approximately 19 percent for SMEs. Interestingly, we find no significant changes in the interest rate spread. At the extensive margin, we find that targeted banks terminated relationships with firms in affected industries. Overall, these results show that targeted banks responded to Operation Choke Point by restricting access to credit for firms in affected industries, especially smaller ones.

Next, we evaluate the overall effect of the initiative on affected firms' access to credit. While our first set of results shows that lending by targeted banks decreased, we also find that affected firms maintain for longer their existing relationships with non-targeted banks and initiated new relationships with other non-targeted banks, potentially mitigating or offsetting the intended effect. To examine this further, we first analyze whether these firms experienced changes in the aggregate level and terms of credit across all their banking relationships. Interestingly, we find that affected firms obtained higher total committed credit following the initiative, suggesting that they hedged against future terminations. We find that, on average, credit utilization did not change after the operation. Affected firms also did not experience significant changes in interest rate spreads or collateral requirements. However, we show a slight reduction in loan maturity, although this effect is only observable for SMEs.

Finally, we examine the effectiveness of Operation Choke Point in imposing economic costs on firms. Analyzing firms' financial statements, we find no change in leverage, reliance on trade credit, profitability, or investment. Further, affected firms did not exhibit increased volumes of non-performing loans and their assessed probability of default did not change. Overall, our results suggest that targeted credit rationing does not significantly impact affected firms.

This paper contributes to several branches of the literature. First, it contributes to the flourishing literature on responsible investing, which focuses on affecting change through divestment and negative selection and where investors try to discipline firms by raising their cost of capital.[1] While these actions are gaining popularity, several studies have identified potential shortcomings. For instance, Oehmke and Opp (2020) suggests that divestment only works if responsible investors are affected by externalities and coordinate, while Broccardo, Hart, and Zingales (2022) suggests that divestment reduces the ability to voice preferences. Another key issue is that these actions can only be applied to publicly listed firms, leaving private firms beyond the reach of many stakeholders. We contribute to this literature by examining the effectiveness of the alternative approach of banks rationing credit to firms.

Second, our work contributes to the nascent literature on bank defunding by examining the causal effect of targeted credit rationing. Our paper complements contemporaneous work by Kacperczyk and Peydró (2022) and Green and Vallee (2022), where banks choose to divest from certain "brown" industries, a decision that could mask increased awareness of transition risks (Bolton and Kacperczyk, 2022).[2] These studies focus on firms in high-polluting industries that can likely adjust through divestment and off-

[1] Empirical evidence includes Atta-Darkua and Dimson (2020); Becht, Franks, and Wagner (2019); Teoh, Welch, and Wazzan (1999). Theoretical studies of impact investing include those by Berk and van Binsbergen (2021); Broccardo, Hart, and Zingales (2022); Chowdhry, Davies, and Waters (2018); Green and Roth (2021); Hart and Zingales (2017); Heinkel, Kraus, and Zechner (2001); Morgan and Tumlinson (2019); Oehmke and Opp (2020).

[2] Other papers, such as Delis et al. (2019), Kleimeier and Viehs (2021), and Ivanov et al. (2022), take an alternative approach and study whether banks price credit risk associated with emissions and fossil fuel reserves.

shoring.[3] In these studies, the authors find limited credit substitution, suggesting that targeted credit rationing can redress climate risks. In contrast, we study the effect of credit rationing in response to an exogenous shock that forces a subset of banks to terminate or limit relationships, which mitigates concerns of selection. Further, our firms do not operate in high-polluting industries, which limits their ability to adjust their businesses to address the concerns of investors. Our results suggest a complete substitution of credit for borrowing firms following this exogenous shock. Together, these papers expand the understanding of the ability of credit rationing to effect change.

Our paper also contributes to the large body of literature studying the value of banking relationships. Theory suggests different effects on lending. On the one hand, borrowers can benefit from a long-term relationship with a bank, given that the generation of private information throughout the relationship might reduce information asymmetries and allow the bank to offer better conditions in terms of interest rates, loan amount, and collateral (Boot and Thakor, 1994; Petersen and Rajan, 1994). On the other hand, given that banks own this private information and it is not transferable, banks could exploit firms and offer worse conditions (Rajan, 1992; Sharpe, 1990).[4]

Our results contrast with those in much of the empirical literature, which point toward a positive effect. A potential explanation for this difference is that our setting is free from two key limitations of papers in this literature. First, a number of studies focus on the link between the length (or intensity) and terms of a banking relationship. These are likely to be jointly determined, affecting the interpretation of the results.[5] Second, many studies exploit plausible exogenous relationship terminations that respond to the closure of bank branches. A potential problem is that these closures might affect not

[3]See recent papers by Ben-David, Jang, Kleimeier, and Viehs (2021); Dai, Duan, Liang, and Ng (2021); Duchin, Gao, and Xu (2022).

[4]A more recent set of papers focuses on the value of relationship lending during the global financial crisis and the COVID-19 pandemic (e.g., Banerjee et al., 2021; Beck et al., 2018; Berger et al., 2017, 2022; Bolton et al., 2016; Deyoung et al., 2015; Jiménez et al., 2022; Sette and Gobbi, 2015).

[5]For instance, a bank might decide to offer better terms to "good" firms, reducing the incentives for a firm to search for alternatives. Given that the econometrician does not observe the quality of the firm, she would find that firms with longer relationships obtain better terms.

only the firms under study but also the local economic and social conditions (Garmaise and Moskowitz, 2006). In our setting, bank terminations respond to a regulatory initiative that targets a small subset of firms and is likely has a very limited impact on local conditions.

2 Institutional Background: Operation Choke Point

We examine the effectiveness of targeted credit rationing by studying the effects of Operation Choke Point (OCP), a DOJ-led initiative. Under this operation, DOJ compelled some banks to cut lending to firms in industries that operated legally but were deemed to pose a high risk for money laundering and fraud, including ammunition, firearms, tobacco, dating and escort services, pornography, and online gambling. (See Table IA1 for a complete list of targeted industries.) Following the pressure from regulators and facing the threat of civil and criminal liability, the first targeted banks started reducing their lending to firms in these industries in early 2013.

This quasi-natural experiment provides us with a nearly ideal setting to study whether targeted credit rationing works as a policy tool. First, OCP was a large and credible shock to banks, as the DOJ, in concert with banking regulators, demanded that banks cut lending to targeted industries or face significant sanctions. Second, the operation affected an arguably arbitrary subset of banks, given that the targeting was not driven by banks' lending volume to firms in these industries or other bank characteristics.[6] Finally, OCP targeted banks at different points in time, allowing us to identify the effect of the operation by exploiting its staggered implementation.

Given the legal scrutiny around OCP, court records provide us with crucial institutional details for our study. Prior to the initiation of OCP, during the summer of 2011,

[6]We discuss these results explicitly in Section 2.2. Anecdotally, the indiscriminate nature of the choice of targeted banks can also be seen in a report by the Office of Inspector General, which reveals *"no evidence that the FDIC used the high-risk list to target financial institutions."*

the FDIC issued a Supervisory Insight Article providing banks guidance on managing reputational risk. The article explicitly warned banks of heightened risks associated with doing business with specific categories of merchants. Months later, the FDIC issued another guidance discouraging banks from engaging with merchants that might have been associated with higher incidences of consumer fraud or potential illegality (Calomiris, 2017). To further formalize the plan, attorneys within the DOJ's Civil Division proposed an internal initiative in November 2012 called *Operation Choke Point*, which recognized that the DOJ could influence bank lending using the threat of subpoenas and regulators' actions. Showing this is an internal memo between DOJ employees, dated November 5, 2012, that remarked that *"banks [were] sensitive to the risk of civil and/or criminal liability and regulatory actions."*[7] Consequently, the DOJ began issuing subpoenas to banks and payment processors in 2013, with guidance from the FDIC that included a list of "high risk" merchants. The DOJ issued 60 administrative subpoenas from February 2013 through August 2013, effectively compelling banks to restrict these merchants' access to finance.

Documentary evidence also suggests that the DOJ's initiative to impact banking relationships was done in cooperation with other regulators. As detailed in a 2015 report by the Office of Inspector General (OIG), employees at the DOJ communicated with staff from the FDIC, the Board of Governors of the Federal Reserve System (FRB), the Office of the Comptroller of the Currency (OCC), and the Consumer Financial Protection Bureau (CFPB) with regard to the implementation of Operation Choke Point.[8] Even though no law or regulation officially limited a financial institution from lending to the targeted—but in most cases legal—businesses, OCP was a concerted effort to credibly threaten banks. According to the 2015 OIG Report, bank executives felt that *"references to specific merchant types in the summer 2011 Supervisory Insights Journal article and in super-*

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

[8]Office of Inspector General Report: The FDIC's Role in Operation Choke Point and Supervisory Approach to Institutions that Conducted Business with Merchants Associated with High-Risk Activities, dated September 2015.

visory guidance created a perception among some ... that the FDIC discouraged institutions from conducting business with those merchants."

Overall the operation resulted in a wave of terminations of banking relationships with merchants enumerated on the FDIC's "high risk" list from 2013 to 2016—see a timeline of OCP's most significant events in Table IA2. Despite the initiative's apparent success, it received considerable criticism. The program was first publicized through an article in The Wall Street Journal on August 8, 2013. Following subsequent public dissent, members of Congress submitted a letter to the FDIC chairman and the US attorney general expressing their concerns regarding the pressure the DOJ was exerting to terminate lawful lending relationships, and, in December 2014, the US Household Committee on Oversight and Government Reform issued a report titled, 'Operation Choke Point' (Calomiris, 2017). Increasingly negative public sentiment and government hearings resulted in the operation's termination on August 17, 2017.

3 Data and Target Selection

In this section, we describe the data set we use in our analysis and detail the analysis we perform to identify potential drivers of OCP's targeting criteria that might bias our study or the interpretation of the results.

3.1 Summary of Data Sets

Our main source of data is confidential quarterly loan-level data obtained from the corporate loan schedule H.1 of the Federal Reserve's Y-14Q. The credit register provides information on the universe of corporate loans and leases with commitment amounts exceeding $1 million for banks with at least $50 billion in total assets.[9] The reporting financial institutions consist of 30 of the largest bank holding companies in the United

[9]Recent studies using the Federal Reserve's Y-14Q data include Brown et al. (2021), Chodorow-Reich et al. (2021), Favara et al. (2021), and Crosignani et al. (2022).

States. Besides 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, among other details. It also includes limited firm-level data collected by the banks, such as measures of net income and profitability.

We supplement this rich set of data with financial information at the bank holding company-level from publicly available FR Y-9C reports, commonly known as "call reports." Basic financial information from the FR Y-9C includes consolidated quarterly balance sheets, income statements, and detailed supporting schedules.[10] The Federal Reserve started collecting the Y-14Q data during the second quarter of 2012. Thus 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. To this end, we manually reviewed publicly available government documents and spoke to former regulators with knowledge of the operation. We summarize the banks and dates targeted in Figure 1, where we note that OCP spanned the period from the first quarter of 2013 to the third quarter of 2016.

In Table 1, we present key summary statistics for our merged sample. Panel A provides details on the sample of loan-level data at the firm-bank-quarter observations. We include information on total committed and utilized credit, credit terms (spread, maturity, collateral requirements), and the lending bank's information (capital, profitability, liquidity, and size). We aggregate the data across banks at the firm-quarter level to understand the overall effect of the initiative on firms. Panel B summarizes this information, including firms' financial information and information on bank relationship creations and terminations.

[10] https://www.federalreserve.gov/apps/reportforms

3.2 Targeted Banks

To analyze the effect of rationing credit on targeted industries, a key issue is to understand the criteria used to target banks to identify potential sources of bias. To this end, we analyze data on OCP's targeting from expert witnesses' testimony, which allows us to construct a list of targeted banks, dates, and affected industries. The information obtained indicates that targeting by OCP was at the bank level and that no individual firms were targeted. As a result, our empirical analysis in this section is conducted at the bank level.

Important for our analysis and interpretation of the results, OCP did not seem to have a clear targeting criterion. Specifically, supporting governmental documents suggest that targeting did not respond to particular characteristics of banks. For instance, a report by the Office of Inspector General suggests that the targeting of banks was unrelated to their activities related to high-risk list. As stated in that report, *"We found no evidence that the FDIC used the high-risk list to target financial institutions."*

We formally examine OCP's selection criteria by estimating the relationship between the characteristics of bank holding companies, the likelihood of being targeted by OCP, and the timing of targeting using a Cox proportional hazard model. The key identification assumption is that the timing of the targeting is unrelated to bank characteristics that could explain a reduction in lending to affected industries. Such a relationship would cause a spurious correlation between OCP and credit rationing or relationship terminations that would affect the interpretation of our results. We check this identifying assumption by examining the correlation between the time elapsed before a bank is targeted and a wide range of bank-level variables capturing initial characteristics that might affect a bank's lending portfolio.

We consider how the targeting of banks relates to the financial characteristics of the bank holding company. We use financial measures prior to the targeting of the first bank to address concerns related to anticipation. We consider the bank's size, tier 1 capital,

liquidity, and profitability ratios. We further consider the bank's share of lending to targeted industries, both in terms of volume and number of relationships with firms in those industries. Finally, we consider the average short-term, long-term, and total debt of the firms in a bank's portfolio, and the average profitability of those firms.

The results in Table 2 suggest that targeting of bank holding companies was unrelated to their size, performance measures, or their share of lending to high-risk industries, as identified by the FDIC. The estimated coefficients show that the financial characteristics of the bank holding companies are unrelated to the selection by the DOJ for OCP. These results are consistent with our discussions with former regulators and our review of legal documents, which indicate little relationship between targeting and financial characteristics. Notably, prior loans to targeted industries do not load on the likelihood of being targeted, alleviating concerns about the external validity of our later results.

3.3 Targeted 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 IA3, to obtain the associated six-digit industry NAICS codes. When required, we supplement the NAICS code search process. Given the illegality of certain targeted industries, we exclude firms in those industries, given that those categorizations are likely data errors. Excluded industries are cable box descramblers, 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 follow the literature and exclude companies in the financial sector. In total, we identify 5,670 firms, 595 of which are publicly listed.

4 Bank-Level Analysis

In this section, we first analyze whether OCP effectively affected lending to targeted industries. Then we explore what firms in those industries experienced in terms of access to credit and performance.

4.1 Empirical Specification

We start by analyzing whether OCP affected lending to firms in affected industries by targeted banks, relative to non-targeted banks. Our baseline specification is a dynamic difference-in-differences model. We exploit 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:

$$Y_{f,i,b,t} = \beta_1 I\left(Post_{b,t}\right) I\left(ChokePoint_b\right) + X_{b,t}\gamma + \delta_b + \delta_f + \delta_{t,size,industry} + \delta_{t,state} + \varepsilon_{f,i,b,t}, \tag{1}$$

where $Y_{f,i,b,t}$ is one of our outcomes of interest (committed credit, utilized credit, interest rate, etc.) for firm f, operating in industry i, borrowing from bank b, at the calendar-quarter t time. Our baseline specification includes bank (δ_b) and firm fixed effects $\left(\delta_f\right)$ to control for time-invariant heterogeneity of banks and firms. We include time–firm size–industry fixed effects $\left(\delta_{t,size,industry}\right)$, with size attributed by quartiles, to control for time-varying trends that affect firms of similar size operating in the same industry. We also include time–state fixed effects $(\delta_{t,state})$ to address time-varying heterogeneity at the state level, such as the differential regulation of activities by affected firms.[11] $I\left(Post_{b,t}\right)$ is an indicator variable at the bank level set to one following the tar-

[11] 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 business, such as online gambling. Over the following years, six states legalized online casino games, one of the targeted industries in our analysis.

geting of the bank by OCP. $I\left(ChokePoint_b\right)$ is a dummy variable at the bank level set to one for banks that were targeted by OCP. γ is a vector of bank-time level controls, including bank size, capital, liquidity, and profitability. The primary coefficient of interest, β_1, captures the within bank-firm changes following the targeting of the bank by OCP. Note all standard errors are double clustered at the bank and state level.

4.2 Effect of OCP on Credit Supply

We present the results of the estimation in Table 3. The main coefficient in column (1) is negative and statistically significant at the 5% level, suggesting that banks targeted by the DOJ reduced their level of committed credit to firms in "high-risk" industries, relative to control banks, by approximately 3.7%. In column (2), we include a tighter set of fixed effects (time–firm size–industry–state fixed effects), and the coefficient for the estimation remains large (4.9%) 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 changes across firms of different sizes. We follow Chodorow-Reich et al. (2021) and classify firms as SMEs if their total assets are less than $250 million. As presented in column (3) of Table 3, we find that the reduction in lending is concentrated among these firms, with a decline of 9.5% in committed credit. In contrast, we find no significant effect for large firms.

We next consider the volume of credit utilized by affected firms in columns (4) to (6). We find no significant effect, suggesting that the level of credit used by firms does not seem to change (columns (4) and (5)). In other words, banks reduce the level of committed credit, but this restriction does not bind, given that firms were not drawing the entire amount available. We find a consistent effect across firms of different sizes (column (6)).

We then analyze the dynamic effects of OCP on committed credit by plotting the dynamic coefficients, relative to the quarter before the targeting by the DOJ. The coeffi-

cients in Figure 2 present two key pieces of evidence. First, they show that there is no differential pre-trend in lending activity to firms in affected industries by targeted and non-targeted banks. Second, following the targeting, there is a gradual and significant decrease in committed credit between treated and control banks to firms within the same affected industry.

Last we study whether the terms of the credit to affected firms are impacted, following the empirical specification described in Equation 1. The results in Table 4 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 post higher collateral, an effect driven by the terms imposed on small and medium-sized firms (columns (5) and (6)).

4.3 Robustness Tests

Overall the previous results suggest that the operation reduced lending by targeted banks to affected industries. In this subsection, we run a series of robustness tests to mitigate several concerns related to our tests and interpretation. First, we address whether our results are biased by firm heterogeneity, which affects the matching between firms and banks. Second, we consider whether our results are affected by loans with volumes close to the reporting threshold. Third, we assess whether our results do not respond to OCP but to other mechanisms.

4.3.1 Firm-bank matching

One potential concern is that firms that borrow from targeted banks differ from those that borrow from other banks. This could lead to an erroneous interpretation of the results, given that these firms might experience heterogeneous shocks. To mitigate this concern, in column (1) of Table 5, we present the results of a specification that includes

firm-time fixed effects, thus controlling for potential firm-level shocks not considered in our main specification. Exploiting variation within firms that borrow from multiple banks, we find that the coefficient is still negative, large, and statistically significant at the 1% level. This finding suggests that, for the same firm, targeted banks reduce lending more than non-targeted banks.

4.3.2 Minimum Reporting Threshold

The credit register provides information on all corporate loans and leases with commitment amounts exceeding $1 million for banks with more than $50 billion in assets. Thus one potential concern is that this truncation of the data affects our results. For instance, if a bank reduces the committed amount from $1.01 million to $0.99 million, in our setting this minor reduction would be identified as a significant reduction in committed credit, flagged as an account termination, and bias our results.

To alleviate this concern, we run an additional test excluding loans close to the reporting threshold. Specifically, we exclude loans below $5 million and re-estimate our baseline specification. We report the results in column (2) of Table 5, where we find evidence suggesting that our results are not driven by loans close to the reporting threshold. In particular, we find that the level of credit commitment decreases for firms in high-risk industries that borrow from targeted banks, relative to non-targeted banks. These results resemble those in the baseline test including all loans.

An additional concern is that affected firms can initiate relationships or increase borrowing from non-reporting banks, those with assets below the $50 billion threshold. We address this potential issue in Section 5.4, where we show that affected firms' do not experience a significant change in short or long-term debt.

4.3.3 Banks and Industry Trends

A potential remaining concern is that OCP targeted banks that were already cutting lending for other reasons unrelated to OCP. To mitigate this concern, we run a falsification test, where we analyze whether targeted banks cut lending to other industries. First, we run our test on non-affected industries in general, that is, excluding those in our main analysis. We present the results in column (3), where we find that the coefficient is statistically indistinguishable from zero. We then run our test on a subset of industries potentially at high risk of being used to launder money –as identified by the NAICS association in conjunction with industry experts– but that were not targeted by OCP.[12] We present the results in column (4), where we find that the coefficient is positive and statistically indistinguishable from zero.

4.3.4 Timing of Targeting

Another concern is that the results are biased by specific drivers of the timing of each bank's targeting date. To mitigate this concern, we run a placebo test 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 column (5), where we find no significant effect.

5 Firm-Level Analysis

In this section, we explore the impact that OCP had on firms that borrowed from targeted banks. We first study the creation and termination of firm-bank relationships with targeted and non-targeted banks. Next we analyze whether these firms experienced a decline in total committed credit across targeted and non-targeted banks. Last we ana-

[12] Industries included are gasoline stations, convenience stores, liquor stores, parking lots, among others.

lyze the effect of OCP on firms' performance.

5.1 Empirical Specification

In this section, we conduct our analysis at the firm-level to examine whether OCP had an overall impact on firms in affected industries, thus using data aggregated at the firm-quarter level. Our baseline specification is a dynamic 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 in time or were never targeted. Specifically, we estimate:

$$Y_{f,i,t} = \beta_1 I\left(Post_{f,t}\right) I\left(ChokePoint_f\right) + \delta_b + \delta_f + \delta_{t,i} + \varepsilon_{f,i,t}, \tag{2}$$

where $Y_{f,i,t}$ is our outcome of interest at the firm-quarter level, studying firm f, operating in industry i, at the calendar-quarter t time. Our baseline specification includes main bank fixed effect (δ_b) and firm fixed effects $\left(\delta_f\right)$ 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–industry fixed effects $\left(\delta_{t,industry}\right)$ to control for time-varying trends that affect firms in the same industry. In a more restrictive specification, we also consider time-industry-firm size-state fixed effects, $\left(\delta_{t,size,industry,state}\right)$, which controls for common shocks to firms of similar sizes (split into quartiles) that operate within the same industry and state.

$I\left(Post_{f,t}\right)$ variable is a dummy variable at the firm level set to one following the targeting of any bank that lends to the firm. $I\left(ChokePoint_f\right)$ is a dummy variable at the firm level set to one for firms that borrow from banks targeted by OCP. Importantly, the specification in Equation 2 studies shocks at the firm level, whereas Equation 1 studies the shock at the bank level. Our primary coefficient of interest, β_1, captures the within-firm changes following the targeting of a bank from which the firm borrows. Note all

standard errors are double clustered at the main bank and state levels.

5.2 Bank-Firm Relationships

We start by examining whether firms linked to targeted banks experience account terminations and initiate new relationships with non-targeted banks. We define an account termination as a reduction in committed credit from any amount down to zero and an initiation of a new relationship as a loan issued by a bank with which there was no prior relationship.

We estimate a variation of Equation 2 with account terminations and initiations as the outcome variable and present the results in Table 6. In terms of terminations, the coefficient in column (1) is very small and statistically indistinguishable from zero, suggesting that affected firms do not experience a significant change in the number of account terminations following OCP. This effect is similar across firm sizes, as shown in column (2). However, when we examine heterogeneous effects across bank types (targeted versus non-targeted), we find results that are consistent with our finding on the effectiveness of the initiative. Specifically, we find that affected firms experience an increase in the frequency at which their accounts with targeted banks are terminated (column 3). In particular, we find that the frequency of account terminations increases by 4.3 percentage points or an increase of 90% over the baseline level of 4.8 percent. This effect is driven by a significant increase in account termination for small and medium firms and a similar effect on large firms (column 4). To mitigate the impact of OCP on credit availability, affected firms seem to reduce the frequency of account terminations with non-targeted banks (column 5) by a percentage equivalent to 77% 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 (Calomiris, 2017), and with the idea that affected firms try to preserve their relationships with non-targeted banks.

We next explore the effect of the initiative on the initiation of new relationships. We

present the results in Table 7, where we find that, on average, affected firms increase the rate at which they initiate new relations with banks by 1.5% or approximately 13% of the baseline level (column 1). Note that this effect is driven by new accounts opened by SMEs firms (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 OCP (column 3). This effect is large and significant across firm sizes (column 4). In contrast, we find that these firms significantly increase the rate of creation of new relationships with non-targeted banks, as evidenced by the large and significant coefficients in columns (5) and (6).

Overall the evidence presented in this subsection suggests that banks targeted by OCP effectively terminate accounts with firms in affected industries. However, these firms initiate new or maintain for longer relationships with non-targeted banks to offset the effect of terminations.

5.3 Net Effect on Committed and Utilized Credit

We first examine the effect on total committed and utilized credit for affected firms. Given that a firm might borrow from different banks and that these banks might be targeted or non-targeted, we construct a firm-level measure of exposure to treated banks that is set to one following the targeting of any of the banks the firm borrows from. To test the effect of OCP on aggregate lending to firms that borrow from targeted banks, we collapse the data at the firm-quarter level, aggregating committed credit, utilized credit, and collateral pledged across targeted and non-targeted banks. For spread and maturity, we calculate a weighted average of the terms using the volume of committed credit as the weight.

We present the results of this test in Table 8. We find that affected firms experience an increase in committed credit (columns 1 and 2) of approximately 5.3%. This effect is bigger for large firms, which experience an increase of almost 17% (column 3). Small

and medium-sized firms experience a smaller but still significant increase of 2.4%. In contrast, the coefficients in columns (4) to (6) are statistically indistinguishable from zero, suggesting that these firms do not draw down larger amounts of credit. Overall these findings suggest that affected firms offset the reduction in committed credit by targeted banks by obtaining more committed credit from non-targeted banks. However, the initiative does not seem to impact actual lending.

These firms do not seem to experience changes in interest rate spreads, as evidenced in Table 9, columns (1) and (2). However, they experience a shortening in the maturity of the loans of 2.3 months or about 5% of the mean maturity (column 3). This effect is significant only for small and medium-sized firms, as evidenced in column (4). We find that the collateral pledged by these firms does not change (columns 5 and 6).

5.4 Financial Performance of Targeted Firms

Operation Choke Point was intended to ration capital to targeted industries in an effort to adversely affect their performance. To understand whether the policy worked, we examine two types of measures. First, financial measures from the statements provided by firms to banks. Second, banks' assessments of the probability of default of these firms.

We follow the specification in Equation 2 and analyze firm-level measures, such as leverage, usage of trade credit, profitability, investment, and delinquency. We present the results in Table 10. The results in Panel A suggest that there was no effect on leverage, as defined by total debt over assets (column 1). 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 do not experience changes in the level of trade credit (columns 3 and 4). Similarly, we find no effect on profitability, as measured by return on assets (columns 5 and 6).

In Panel B, we present the results on investment, as measured by capital expenditures

scaled by assets (columns 1 and 2), and on delinquency, as measured by the level of non-performing loans (columns 3 and 4). We find no effect on any of these performance measures, a finding that is homogeneous across firm sizes. Last, we test whether banks change their assessment of the probability of default of affected firms. We find no effect across firm sizes (columns 5 and 6).

The overall results suggest that targeted credit rationing had a neutral effect on firms' performance. While the initiative had an impact on lending by targeted banks, affected firms responded by securing lending from other banks. The borrowing terms of these new loans did not differ significantly from the original terms that those firms had with the targeted banks. Moreover, we find that affected firms manage to obtain more committed credit, although their effective utilization of credit did not change. More generally, we find that these firms did not experience an impact in terms of total credit, investment, or profitability, suggesting that credit rationing had an insignificant effect on firms in targeted industries.

6 Conclusion

Over the last decade, stakeholders have increasingly searched for mechanisms 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 these firms is crucial. Despite this, the empirical evidence on this issue is scarce, potentially due to empirical and data availability challenges.

We exploit a regulatory initiative that provides exogenous variation in credit rationing to firms in industries at high risk for fraud and money laundering. Using supervisory loan-level data, we document that credit rationing does affect banking relationships. Targeted banks reduce lending and terminate relationships with firms in those

industries. However, these firms initiate new relationships with non-targeted banks and manage to obtain loans with similar terms to the ones they had. Using financial statements data, we show that these firms do not experience measurable changes in performance.

Overall our findings suggest that credit rationing does not affect the operations of firms that generate negative externalities. In our setting, where terminations are exogenous to both banks' and firms' performance and likely do not affect other local conditions, we find that banking relationships do not seem to be valuable, rendering the initiative ineffective. Our findings have significant implications for current debates on whether credit rationing to specific industries helps bring about change.

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FIGURE 1: TIMELINE OF TARGETING

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

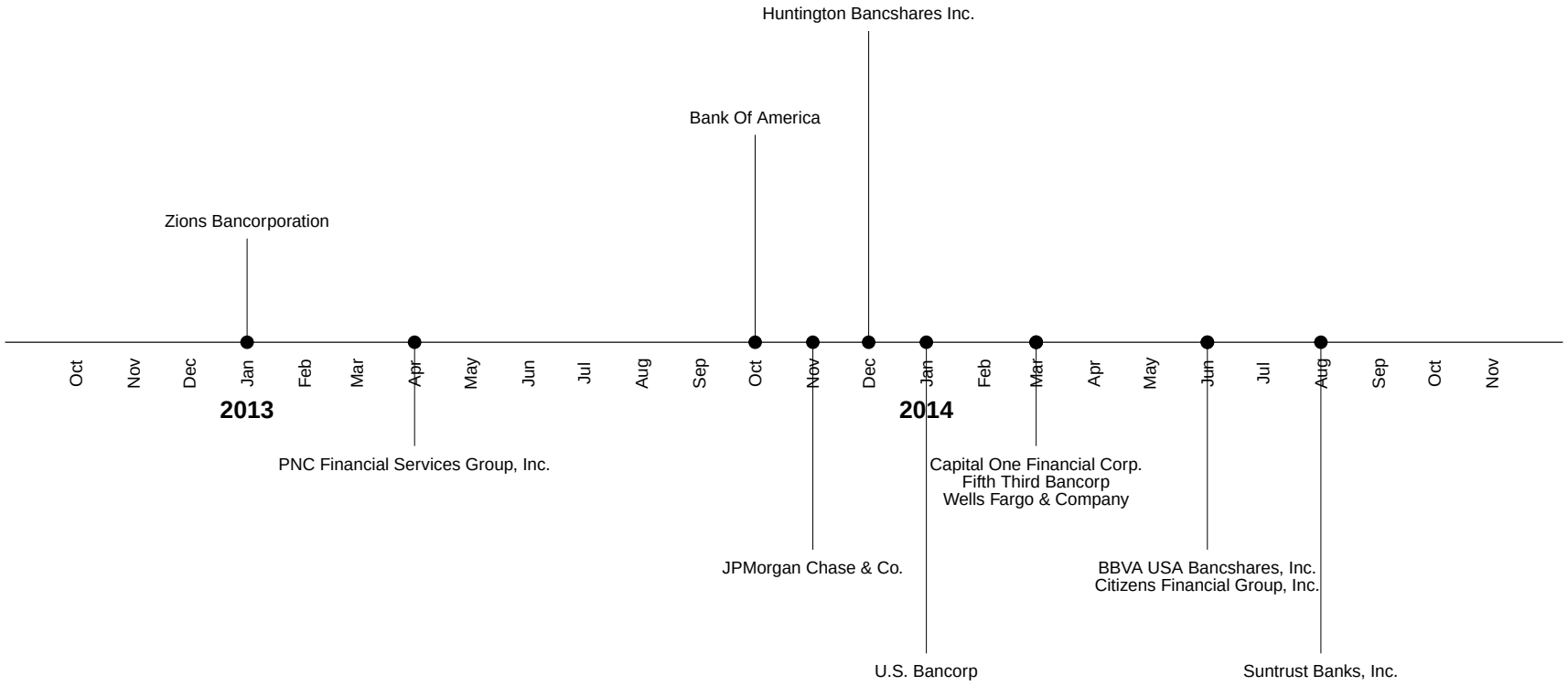

FIGURE 2: **COMMITTED CAPITAL AROUND OPERATION CHOKE POINT**

This figure plots coefficients from a difference-in-differences specification, where the dependent variable is the natural logarithm of total 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 95% confidence intervals correspond to the difference in the total committed credit lending between treated and control banks, within the same treated industry. Data Source: Federal Reserve Y-9C.

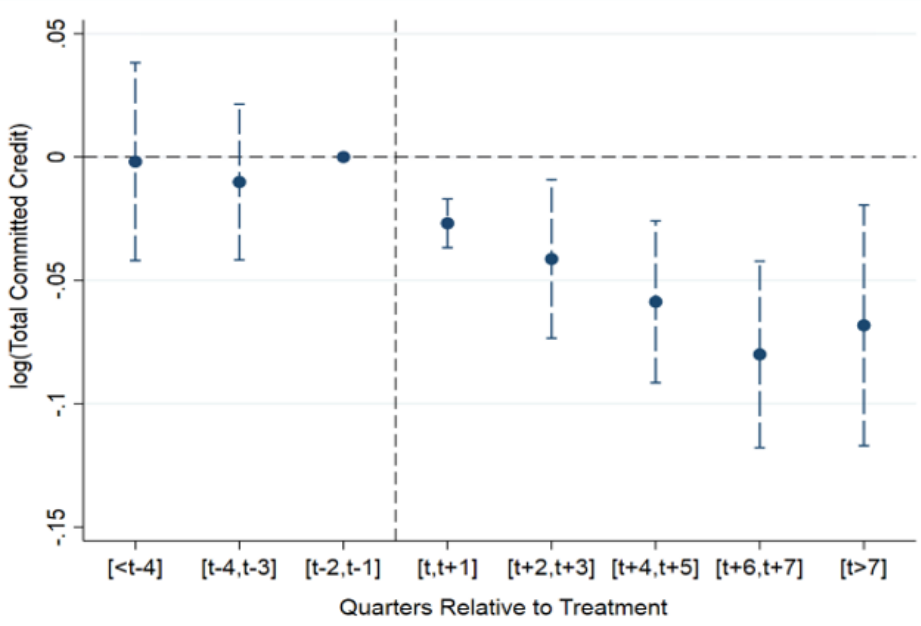

TABLE 1: **SUMMARY STATISTICS**

The table reports the summary statistics for firm-bank-quarter-level and firm-quarter-level characteristics relating to bank lending between the thirty largest bank holding companies and U.S. firms. The sample period covers 2012 Q2 to 2016 Q2. Data Source: Federal Reserve Y-14Q and Y-9C.

	N	Mean	p50	SD
	(1)	(2)	(3)	(4)
Firm-Bank-Quarter:				
Total Committed Exposure	50,724	29.891	6.000	90.349
Total Utilized Exposure	50,724	9.035	2.030	25.028
Spread	29,957	2.253	2.127	1.115
Maturity	39,709	46.130	33.000	57.543
Collateral	18,885	0.983	0.000	27.371
Bank Capital	50,724	8.997	9.167	1.214
Bank Profitability	50,724	0.979	1.006	0.502
Bank Liquidity	50,724	13.310	11.318	10.655
Bank Size	50,724	20.075	19.713	1.281
Firm-Quarter:				
SME	41,616	0.847	1.000	0.360
Large Firm	41,616	0.153	0.000	0.360
Relationship Creation with Any Bank	41,616	0.116	0.000	0.320
Relationship Termination with Any Bank	41,616	0.048	0.000	0.214
Relationship Creation with Treated Bank	41,616	0.086	0.000	0.280
Relationship Termination with Treated Bank	41,616	0.034	0.000	0.181
Relationship Creation with Control Banks	41,616	0.033	0.000	0.179
Relationship Termination with Control Bank	41,616	0.015	0.000	0.123
Probability of Default	18,512	0.026	0.009	0.077
Total Debt to Assets	41,616	0.311	0.260	0.337
Return on Assets	41,616	0.096	0.063	0.156
Non-performing Loans	41,616	0.005	0.000	0.072
Tangible Assets to Assets	40,542	0.891	0.996	0.203
Fixed Assets to Assets	40,542	0.891	0.996	0.203
Trade Credit to Assets	41,616	0.152	0.102	0.165
Sales to Assets	41,616	2.506	1.873	8.491
Capital Expenditures to Assets	37,336	0.029	0.002	0.327

TABLE 2: **SELECTION MODEL**

The table reports the coefficient estimates of a proportional hazard model (Cox, 1972) where the dependent variable is the time until the targeting of a bank by OCP, or the "event". For the banks in our sample that are not targeted, the model takes the "event" as not occurring (censored). The explanatory variables are defined at the bank-holding company level and are measured prior to the first targeting. Standard errors are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Bank Targeted									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bank Size	0.458									
	(0.299)									
Bank Capital		0.105								
		(0.291)								
Bank Liquidity			-0.641							
			(0.473)							
Bank Profitability				0.310						
				(0.395)						
Bank Share of Lending to Targeted Industries					0.189					
					(0.267)					
Bank Share of Firm Relationships in Target Industries						-0.173				
						(0.292)				
Average Short-term Debt of Firms in a Bank's Portfolio							-0.961			
							(1.651)			
Average Long-term Debt of Firms in a Bank's Portfolio								0.331		
								(0.275)		
Average Total Debt of Firms in a Bank's Portfolio									-0.015	
									(0.300)	
Average Profitability of Firms in a Bank's Portfolio										0.272
										(0.326)
Observations	30	30	30	30	30	30	30	30	30	30

TABLE 3: **IMPACT OF OPERATION CHOKE POINT ON EXISTING LENDING**

This table reports the impact of operation choke point on existing lending. The regression uses firm-bank-quarter level data to compare lending between treated and control banks, within the same treated industries. The regressions use three different dependent variables to estimate the effects on lending: Columns (1)-(3) use the natural logarithm of committed capital; Columns (4)-(6) use the natural logarithm of utilized credit. All regressions include *Bank* and *Year* fixed effects. Estimates in columns (1) and (4) include *Quarter* × *FirmSize* × *Industry* fixed effects, as well as *Quarter* × *State* fixed effects. Columns (2), (3), (5) and (6) include *Quarter* × *FirmSize* × *Industry* × *State* fixed effects. Standard errors are double clustered at the bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed Credit)			Log(1+Utilized Credit)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Banks × Post	-0.037**	-0.049***		-0.141	-0.155	
	(0.014)	(0.017)		(0.120)	(0.137)	
Treated Banks × Post × SME			-0.095***			-0.156
			(0.024)			(0.184)
Treated Banks × Post × Large Firm			0.013			-0.153
			(0.039)			(0.264)
Quarter × Firm Size Quartiles × Industry FE	Y			Y		
Quarter × State FE	Y			Y		
Quarter × Firm Size Quartiles × Industry × State FE		Y	Y		Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Observations	50,724	50,724	50,724	50,724	50,724	50,724

TABLE 4: **CHANGE IN LENDING TERMS**

This table reports the impact of operation choke point on lending terms. The regression uses firm-bank-quarter level data to compare lending terms between treated and control banks, within the same treated industries. The regressions use three different dependent variables to estimate the effects on lending terms: Columns (1)-(2) use the loans interest spread; Columns (3)-(4) use loan maturity; Columns (5)-(6) use the log of the loan collateral. All regressions include *Bank*, *Year*, and *Quarter* × *FirmSize* × *Industry* × *State* fixed effects. Standard errors are double clustered at the bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Interest Rate Spread		Maturity		Log(1+Collateral)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Banks × Post	0.023		-1.288		0.178***	
	(0.026)		(0.937)		(0.028)	
Treated Banks × Post × SME		0.051		-2.178**		0.192***
		(0.036)		(1.036)		(0.032)
Treated Banks × Post × Large Firm		-0.021		0.288		0.118
		(0.035)		(1.082)		(0.077)
Quarter × Firm Size Quartiles × Industry × State FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Observations	25,973	25,973	37,294	37,294	17,218	17,218

TABLE 5: **ROBUSTNESS**

This table reports the impact of operation choke point on existing lending using various robustness tests. The dependent variable in columns (1)-(5) use the natural logarithm of committed capital. Standard errors are double clustered at the bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed Credit)				
	Robustness Tests		Falsification Tests		
	Adding Firm × Time FE	Committed Credit Exposure Above $5m Only	Non-Affected Industries	Non-Affected Cash-Intensive Industries	Random Treatment Date (1000 reps)
	(1)	(2)	(3)	(4)	(5)
Treated Banks × Post	-0.085**	-0.042**	0.005	0.015	0.001
	(0.034)	(0.019)	(0.017)	(0.038)	(0.018)
Time × Firm Size Quartiles × Industry × State FE	Y	Y	Y	Y	Y
Firm FE	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y
Quarter × Firm FE	Y	N	N	N	N
Observations	12,732	11,929	290,444	14,657	50,724

TABLE 6: **TERMINATION OF BANKING RELATIONSHIPS**

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) focuses on relationships with treated banks, while columns (5)-(6) focuses on relationships with control banks. All regressions include *Quarter* × *Industry* × *FirmSize* × *State*, *MainBank*, and *Firm* fixed effects. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Relationship Termination					
	Any Bank		Treated Banks		Control Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	0.004		0.043***		-0.037***	
	(0.011)		(0.007)		(0.008)	
Firm Exposed to Treated Bank × Post × SME		0.007		0.043***		-0.035***
		(0.011)		(0.006)		(0.008)
Firm Exposed to Treated Bank × Post × Large Firm		-0.011		0.043***		-0.049***
		(0.015)		(0.012)		(0.012)
Quarter × 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	41,616	41,616	41,616	41,616	41,616	41,616

TABLE 7: **CREATION OF BANKING RELATIONSHIPS**

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) focuses on relationships with treated banks, while columns (5)-(6) focuses on relationships with control banks. All regressions include *Quarter* × *Industry* × *FirmSize* × *State*, *MainBank*, and *Firm* fixed effects. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Relationship Creation					
	All Banks		Treated Banks		Control Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	0.015**		-0.036***		0.052***	
	(0.007)		(0.010)		(0.011)	
Firm Exposed to Treated Bank × Post × SME		0.016*		-0.035***		0.052***
		(0.008)		(0.011)		(0.011)
Firm Exposed to Treated Bank × Post × Large Firm		0.010		-0.039*		0.053***
		(0.018)		(0.020)		(0.016)
Quarter × 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	41,616	41,616	41,616	41,616	41,616	41,616

TABLE 8: **CHANGE IN FIRM LEVEL BORROWING**

This table reports the impact on firm level borrowing. The regressions use firm-quarter level data to compare between treated firms of treated and control banks. The dependent variable of columns (1)-(3) is the natural logarithm of committed capital, while columns (4)-(6) use the natural logarithm of utilized credit. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Log(Committed Credit)			Log(1+Utilized Credit)		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	0.053***	0.046**		0.020	-0.032	
	(0.018)	(0.018)		(0.107)	(0.199)	
Firm Exposed to Treated Bank × Post × SME			0.024**			-0.072
			(0.011)			(0.254)
Firm Exposed to Treated Bank × Post × Large Firm			0.168**			0.193
			(0.068)			(0.425)
Time × Industry FE	Y	N	Y	N	Y	N
Time × Industry × Firm Size Quartiles × State FE	N	Y	N	Y	N	Y
Main Bank FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Observations	41,616	41,616	41,616	41,616	41,616	41,616

TABLE 9: **CHANGE IN FIRM LEVEL LENDING TERMS**

This table reports the impact on firm level lending terms. The regressions use firm-quarter level data to compare between treated firms of treated and control banks. The regressions use three different dependent variables to estimate the effects on lending terms: Columns (1)-(2) use the loans interest spread; Columns (3)-(4) use loan maturity; Columns (5)-(6) use the log of the loan collateral. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

	Interest Rate Spread		Maturity		Log(1+Collateral)	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	0.028		-2.380*		0.069	
	(0.033)		(1.267)		(0.065)	
Firm Exposed to Treated Bank × Post × SME		0.035		-2.467*		0.081
		(0.033)		(1.325)		(0.060)
Firm Exposed to Treated Bank × Post × Large Firm		-0.030		-1.725		-0.005
		(0.074)		(2.019)		(0.057)
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	25,286	25,286	33,859	33,859	16,102	16,102

TABLE 10: **CHANGE IN FIRM PERFORMANCE**

This table reports the impact of operation choke point on firm performance. The dependent variables are total debt to assets (columns 1); total trade credit to assets (column 2); return on assets (column 3); total capital expenditures to assets (column 4); probability of default (column 5); and non-performing loans (column 6). All regressions include *Quarter* × *Industry* × *FirmSize* × *State*, *MainBank*, and *Firm* fixed effects. Standard errors are double clustered at the firm's main bank and state level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: Federal Reserve Y-14Q and Y-9C.

Panel A: Firm Performance

	Total Debt/Assets		Trade Credit/Assets		ROA	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	0.004		-0.004		0.001	
	(0.007)		(0.004)		(0.005)	
Firm Exposed to Treated Bank × Post × SME		0.004		-0.003		0.002
		(0.008)		(0.005)		(0.005)
Firm Exposed to Treated Bank × Post × Large Firm		0.004		-0.010		-0.002
		(0.015)		(0.007)		(0.007)
Quarter × 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	41,616	41,616	18,512	18,512	37,336	37,336

Panel B: Firm Performance Continued

	Capex/Assets		Pr(Default)		NPLs	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposed to Treated Bank × Post	-0.000		0.002		-0.000	
	(0.002)		(0.004)		(0.002)	
Firm Exposed to Treated Bank × Post × SME		-0.001		0.001		-0.001
		(0.002)		(0.004)		(0.002)
Firm Exposed to Treated Bank × Post × Large Firm		0.004		0.002		0.001
		(0.002)		(0.005)		(0.007)
Quarter × 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	37,336	37,336	18,512	18,512	41,616	41,616

Defunding Controversial Industries

INTERNET APPENDIX
FOR ONLINE PUBLICATION

TABLE IA1: **FDIC LIST OF MERCHANTS INVOLVED IN "HIGH-RISK" ACTIVITIES**

This table reproduces the lists of thirty merchants categories the FDIC's advisory notice identified as being involved in "high-risk" activities.

Merchants Categories Listed By the FDIC	
(1) Ammunition Sales	(16) Life-Time Memberships
(2) Cable Box De-scramblers	(17) Lottery Sales
(3) Coin Dealers	(18) Mailing Lists/Personal Info
(4) Credit Card Schemes	(19) Money Transfer Networks
(5) Credit Repair Services	(20) On-line Gambling
(6) Dating Services	(21) PayDay Loans
(7) Debt Consolidation Scams	(22) Pharmaceutical Sales
(8) Drug Paraphernalia	(23) Ponzi Schemes
(9) Escort Services	(24) Pornography
(10) Firearms Sales	(25) Pyramid-Type Sales
(11) Fireworks Sales	(26) Racist Materials
(12) Get Rich Products	(27) Surveillance Equipment
(13) Government Grants	(28) Telemarketing
(14) Home-Based Charities	(29) Tobacco Sales
(15) Life-Time Guarantees	(30) Travel Clubs

TABLE IA2: **KEY DATES OF OPERATION CHOKE POINT**

This table summarises the key dates of Operation Choke Point and provides key dates of its initiation and termination. The initial supervisory insight article that contained the 30 merchant categories was generated in the Summer of 2011, while the initial inception of the OCP started on November 2012.

Date	Event
Summer 2011	**FDIC issues a Supervisory Insight Article** Article warning banks of high risks activities associated with doing business with a list of 30 merchant categories, including payday lenders, firearm sellers, etc.
January 2012	**FDIC Issues New Guidance** Document indicating that banks could face consequences for failing to adequately manage relationships involving borrowers that engage in industries with higher incidences of consumer fraud and potentially illegal activities.
November 2012	**Inception of Operation Choke Point** Attorneys within the DOJ's Civil Division proposed an internal initiative intended to protect consumer from fraud perpetrated by fraudulent merchant, financial institutions, and financial intermediaries. Initiative named Operation Choke Point.
February – August 2013	**Initial Waves of Subpoenas** DOJ issued 60 administrative subpoenas to entities for which the Department determined it had evidence of potential consumer fraud.
2013 – 2016	**Continuation of Operation Choke Point** Banks are targeted by the DOJ for their lending relationships with specific industries.
August 2017	**Official Termination of Operation Choke Point** Operation choke point officially ended in August 2017. FDIC commits to Congress to provide additional training for its examiners, and to cease issuing similar information and unwritten suggestions to banks it regulates.

TABLE IA3: **INDUSTRY NAICS CODES**

This table lists the the industries that were targeted as part of Operation Choke Point. Column (1) lists the the industries that were outlined in the DOJ bulletin. Column (2) lists the search terms used to find the relevant NAICS codes. Column (3) lists the related NAICS codes that were identifies as corresponding to the respective industries.

Industry (1)	**Search Terms** (2)	**NAICS Codes** (3)
Ammunition/Firearm Sales	ammunition, firearm, gun	332992, 332993, 339920, 325920, 321920, 424690, 332994, 332439, 332994, 423910, 423990
Coin Dealers	coin	339910, 423940, 453310, 453998
Credit Repair Services	credit repair	541990
Drug Paraphernalia	drug, paraphernalia	446110, 325412, 446199, 325411
Escort Services/Pornography	escort, dating, porn, adult	812990
Firework Sales	firework	325998, 423920, 453998, 713990
Lottery Sales	lottery	713290, 334118
Mailing List/Personal Info.	mailing list	511140, 541860, 561431
Online Gambling	gambling, online gambling	713290, 519130
Pharmaceutical Sales	pharmaceutical	424210, 325412, 325411, 325199
Surveillance Equipment	surveillance, monitor, monitoring	334511, 561621, 334290, 453998
Telemarketing	telemarketing	561422
Tobacco Sales and Tobacco Paraphernalia	tobacco, cigarette, nicotine	424940, 312230, 111910, 453991, 453998, 424590, 339910, 321920, 333249, 115114, 333111, 339999, 326299, 316998