Financial Liberalization, Credit Market Dynamism, and Allocative Efficiency*

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Abstract

We investigate the effects of financial liberalization on the dynamism of the credit market. We measure inter-firm credit reallocation in the U.S. states following a methodology akin to Davis and Haltiwanger (1992). We then exploit the staggered liberalization of the credit markets of the U.S. states to identify an exogenous shock to the credit reallocation process. The liberalization intensified credit reallocation in the states, even within narrowly defined groups of continuing firms, while leaving credit growth essentially unaltered. The results suggest that the increased credit market dynamism enhanced the allocation of funds to productive firms and total factor productivity growth.

JEL Codes: E44, G20.

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1 Introduction

In an economy with imperfect financial markets, the allocation of financial resources can be a primary channel through which financial reforms affect the macroeconomy. Several

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studies document that major reforms of financial markets, including regulation and liberalization episodes, influence not only the total volume of liquidity flowing to the business sector but also the allocation of liquidity across businesses. This appears to be the case for the credit market liberalization that occurred in the United States between the late 1970s and the early 1990s (Jayaratne and Strahan, 1996) and for the financial liberalizations that have taken place in several industrialized and emerging economies in the last two decades (Beck, Levine and Loayza, 2000; Galindo, Schiantarelli and Weiss, 2007; Braggion and Ongena, 2019; Varela, 2018; Howes, 2021).

In spite of the broad consensus on the importance of financial reforms for liquidity allocation, we know little about the impact of financial reforms on the continuous, dynamic process of reallocation of financial resources. This contrasts with the rich evidence on the growth of financial aggregates. Does financial liberalization intensify or attenuate the dynamic process of reallocation of liquidity across firms? In turn, does a systematically more intense reallocation of liquidity foster the dynamism of the economy, enhancing allocative efficiency, or can it instead lead to excessive instability of financial relationships, disrupting liquidity allocation? And, ultimately, through which dynamic process can financial reforms affect the efficiency of liquidity allocation?

To understand the relevance of these questions, consider first the debate about the credit booms that often follow financial liberalizations (Gourinchas and Obstfeld, 2012; Mendoza and Terrones, 2012). Policy makers preoccupied about the excessive growth of credit could adopt two very different policy stances. They could try to discourage credit extension, slowing down both credit growth and the reallocation of credit. Alternatively, they could ease the breakdown of existing credit relationships, slowing down credit growth but promoting credit reallocation. Which policy would better serve the purpose of improving liquidity allocation while moderating credit growth? Consider also the debate about the indicators to be tracked in the credit market. Many have highlighted the usefulness of looking at credit growth or credit over GDP to track the response to financial reforms. However, credit growth could provide limited information on the evolution of the allocation of liquidity. In addition to tracking credit growth, can we learn useful information from studying the dynamism with which credit is reallocated after financial reforms?

This paper takes a step towards addressing these questions. The liberalization of the credit markets of the U.S. states that occurred from the late 1970s to the early 1990s constitutes a suitable empirical laboratory for our purposes. During this period, the U.S. states relaxed the regulatory restrictions that prohibited entry by out-of-state banks, thus allowing interstate banking. Moreover, the states relaxed the restrictions on the creation of bank branches within their territory, thus permitting intrastate branching. The liberalization process deeply influenced the management of credit-granting institutions, increasing the competitive pressure on bank managers and strengthening the ties between managers' remuneration and their performance (Hubbard and Palia, 1995). It also affected the structure of credit markets, letting banks enter new geographical areas and expand branch networks, leading to larger, more ramified and allegedly more efficient banking institutions (Berger, Leusner and Mingo, 1997; Hughes, Lang, Mester and Moon, 1996).

The policy change we consider offers an ideal natural experiment to study the impact of financial liberalization on the dynamic process of credit reallocation. First, the deregulation of the credit market took place in different years across states. Jayaratne and Strahan (1996) and Morgan, Rime and Strahan (2004) document that the moment in which the states deregulated did not reflect legislators' expectations of faster investment growth or their desire to boost the volume or quality of investments. Further, they demonstrate that the moment of deregulation was not driven by contemporaneous business fluctuations in a state. We can then exploit the heterogeneous timing of the deregulation across states to identify an exogenous shock to the dynamic process of credit reallocation in the states. Second, previous work suggests that the main channel through which the deregulation affected the credit market was not an increase in credit growth but an improvement in lending quality, such as a reduction in non-performing loans (Jayaratne and Strahan, 1996; Clarke, 2004). This leads one to wonder whether changes in the process of reallocation of liquidity across firms fostered such improvements in lending quality.

To measure inter-firm credit reallocation, we adopt the approach of Herrera, Kolar and Minetti (2011), which in turn replicates the methodology developed by Davis and Haltiwanger (1992) for the measurement of job reallocation. The measurement of credit reallocation across firms requires comprehensive firm-level data. Further, one needs sufficiently long time series to exploit the staggered timing of the credit market deregulation across states. With these needs in mind, we employ firm balance sheet data from U.S. Compustat tapes and compute inter-firm annual flows of total and long-term credit in the states (excluding firms in the "finance, insurance, and real estate" sector). We then estimate a fixed effects model that projects the rate of credit reallocation in a state onto indicators of interstate and intrastate credit market regulation. We obtain that the interstate liberalization of the credit markets of the states significantly boosted credit reallocation in the states (raising the annual credit reallocation rate by more than 25%).[1] This supports the hypothesis that the liberalization enhanced the dynamism of the credit market, ameliorating frictions in the process of reallocation of credit across firms. By contrast, in

[1] We do not estimate a significant effect of intrastate liberalization on credit reallocation. In the paper, we discuss interpretations for the different impact of interstate and intrastate liberalization.

line with prior studies (e.g., Jayaratne and Strahan, 1996), we find no evidence that the liberalization boosted credit growth in the states.[2]

We then dig into the mechanisms whereby the liberalization fostered the allocative dynamism of the credit market. Three findings stand out. First, the estimated increase in credit reallocation largely reflects an intensification in the reallocation of credit across continuing firms (the intensive margin). Thus, the increase in credit reallocation goes well beyond the financing of a larger firm turnover induced by the liberalization. Second, the increase in credit reallocation does not reflect a mere reshuffling of liquidity from small to large firms, which was allegedly produced by the specialization of entering financial institutions in the financing of large businesses. Finally, the increase in credit reallocation was no less an intra-industry phenomenon as an inter-industry one. In fact, the interstate liberalization significantly promoted the reallocation of credit within industries.

In the second part of the paper, we turn to investigate whether the enhanced allocative dynamism of the credit market allowed to channel funds to productive firms in a more flexible way. We address this question in two ways. First, we construct an index measuring the efficiency of the credit reallocation process and uncover evidence that the value of this indicator significantly rose after the credit market liberalization. In a second test, we compute an aggregate measure of total factor productivity (TFP) in each state using a growth accounting methodology. Next, we project TFP growth onto the value of credit reallocation in the state defined by the regulatory indicators. The results reveal that the increase in credit reallocation after financial liberalization significantly boosted TFP growth in the states. And, notably, this holds true even if we restrict attention to the

[2]These results survive a broad array of robustness tests, such as allowing for non-linearities in the effects and controlling for measures of state-level economic conditions and fiscal policy stance.

intensive margin of credit reallocation, that is, the reallocation across continuing firms.

The paper unfolds as follows. Section 2 relates the analysis to prior literature. Section 3 describes the deregulation and the data. In Section 4, we detail the empirical methodology. Section 5 presents the main results. Section 6 focuses on dissecting the mechanisms and excluding alternative explanations. Section 7 studies the effects on firms' productivity. In Section 8, we subject our results to a broad array of robustness tests. Section 9 elaborates on the insights of the analysis for other financial reforms. Section 10 concludes. Details on the data and on robustness tests are relegated to the Online Appendices.

2 Prior literature

In the past, most empirical literature focused on measures of the total volume of credit, such as credit growth or credit over GDP (see, e.g., Tornell, Westermann and Martinez, 2003, for a discussion). There is now established evidence that, because of pronounced firm heterogeneity, the allocation of liquidity plays a role as relevant as its total volume for the macroeconomy (see, e.g., Caballero and Hammour, 2005, Eisfeldt and Rampini, 2006, Eisfeldt and Shi, 2018, and Howes, 2021, for in-depth analyses). This paper relates to three strands of literature that explore the allocative effects of financial liberalizations and reforms. A first strand takes a micro-level perspective and investigates the impact of financial liberalization on lending practices. Bertrand, Schoar and Thesmar (2007) demonstrate that, following the 1985 French banking deregulation, banks became less willing to grant credit to businesses with declining performance and firms stepped up their restructuring activity. Studying Japan in the 1990s, Caballero, Hoshi and Kashyap (2008) construct an index to capture whether firms received subsidized credit. They estimate that industries with more subsidized firms ("zombies") experienced lower productivity growth. Braggion and Ongena (2019) show that following the 1971 U.K. banking deregulation, increased banking competition resulted in greater bank debt as firms were able to establish relationships with multiple banks. Relative to these studies, we take a macroeconomic perspective. We construct an aggregate indicator of credit reallocation that, together with measures of credit growth or credit over GDP, can be employed by macroeconomists for tracking the dynamics of the credit market following financial reforms. This indicator summarizes the dynamism and flexibility with which the credit market performs its allocative function and, as noted, may also prove useful for fine-tuning credit market policies.

A second strand of literature investigates the impact of financial liberalization on ex-post, static measures of credit market performance. Jayaratne and Strahan (1996) find that the U.S. liberalization from the late seventies to the early nineties did not increase loan growth but reduced non-performing loans and raised banks' X-efficiency. Rice and Strahan (2010) demonstrate that the deregulation of interstate branching led to reduced loan rates. Our paper can help understand what changes in the dynamic process of allocation of liquidity allowed to attain the improvement in indicators of lending quality (e.g., non-performing loans) and the stimulus to economic activity detected by prior studies.

Finally, a third strand of studies investigate the effects of financial liberalization and financial development on the real sector. Beck, Levine and Loayza (2000) obtain that the development of financial intermediaries has a more substantive impact on productivity than on capital accumulation. Galindo, Schiantarelli and Weiss (2007) focus on developing economies and uncover evidence that, following financial liberalization, physical capital accumulation becomes relatively more intense in firms with higher productivity. Varela (2018) shows that the 2001 opening of the Hungarian credit market to international financial flows promoted the innovation and productivity of financially vulnerable firms. These studies do not aim at disentangling the changes in the dynamic process of reallocation of liquidity that induce the observed improvements in the allocation of physical capital.

In recent years, some works have started to study the continuous, dynamic process of reallocation of credit. Using data on U.S. banks, Dell'Ariccia and Garibaldi (2005) uncover an intense inter-bank reallocation of loans. Herrera, Kolar and Minetti (2011) find that the inter-firm reallocation of credit is a continuous, mildly procyclical process. However, Herrera et al. (2011) only characterize stylized facts of inter-firm credit reallocation and do not study the impact of financial liberalization on credit reallocation.

3 Data and measurement

This section describes the liberalization and the measurement of credit reallocation.

3.1 The credit market liberalization

The U.S. credit market deregulation that occurred between the late 1970s and the early 1990s offers a natural experiment to identify an exogenous shock to the credit reallocation process. The U.S. states removed restrictions in different years. This staggered timing enables us to separate the effect of the deregulation from macroeconomic and industry trends.

The McFadden Act of 1927 prohibited banks from opening branches outside their home state and attributed to the individual states the power to govern the intrastate opening of branches. Until the 1970s, only twelve states allowed unrestricted intrastate branching. Between 1970 and 1994, 38 states removed their intrastate branching restrictions. In addition, until the late 1970s states prohibited cross-state ownership of banks by applying the Douglas Amendment to the 1956 Bank Holding Company Act. Between 1978 and the early 1990s, almost all states removed the restrictions that prohibited out-of-state holding companies from acquiring in-state bank subsidiaries (as of 1992 only Hawaii retained the restrictions). Finally, the Riegle-Neal Interstate Banking and Branching Efficiency Act fully liberalized intrastate and interstate banking and branching for every state from 1997.

Prior studies demonstrate that the liberalization had profound effects on the efficiency and structure of the credit markets of the states. Banks entered new local markets, expanded their branch networks, and consolidated subsidiaries into branches (Hughes, Lang, Mester and Moon, 1996). This led to larger, ramified banking institutions operating across wider geographical areas. Entry displaced less efficient banks. Moreover, larger banks could exploit economies of scale and scope associated with the expansion of branch networks, achieving higher efficiency in lending activities (Jayaratne and Strahan, 1996). And, by converting subsidiaries into branches, they could reduce costs induced by multiple layers of management and multiple bank subsidiaries. These transformations allegedly reduced credit market frictions, enabling easier matching between banks and firms, better loan monitoring, and more flexible decisions in loan granting and denial.

The liberalization also affected bank managers' incentives. Bank managers faced competitive pressure for stepping up the quality of their management, especially in screening and monitoring borrowing firms (Berger, Leusner and Mingo, 1997). As noted by Hubbard and Palia (1995), the interstate banking liberalization made the bank market for corporate control more competitive. This required more efficient bank managers and more responsive compensations. Indeed, Hubbard and Palia (1995) find that bank managers' pay-performance relationship became stronger following the deregulation.

We expect that the improvement in managerial efficiency and the structural changes mitigated frictions in the reallocation of credit. Banks that are more effective at screening borrowers encounter less adverse selection problems and could then have more incentives to open new credit lines (Gorton and Winton, 2003). Moreover, a more efficient monitoring helps detect a deterioration in customers' investments, reducing the propensity to inertially roll over credit (Gorton and Khan, 2000). Finally, a denser structure of bank branches can mitigate frictions in the matching between lenders and firms, facilitating the formation and termination of credit relationships (Wasmer and Weil, 2004; Boualam, 2020).

However, not all the mechanisms point to an efficiency-enhancing increase in credit market dynamism following the liberalization. While spurring managerial efficiency, increased bank competitive pressure might squeeze the returns from performing loan screening and monitoring, thereby eroding the profitability from reallocating credit across firms (Boot and Thakor, 2000). Further, a growing literature stresses that stable credit relationships can channel funds to long-term, productive investments (Beck, Degryse, De Haas and van Horen, 2018). As noted, the liberalization led to ramified, larger banking institutions, and larger banks tend to engage less in long-term credit relationships (in fact, according to Hubbard and Palia, 1995, banks' CEO turnover increased after the deregulation). Thus, the liberalization, and any associated increase in credit market dynamism, could have harmed the stability of credit relationships and, through this channel, reduced the efficiency of liquidity allocation.

3.2 Measuring credit reallocation

Following Herrera et al. (2011), to measure inter-firm credit reallocation we use data from the Standard and Poor's Full-Coverage Compustat tapes, which provide details on the balance sheets and income statements of all publicly traded U.S. firms. We drop from the database firms in "finance, insurance, and real estate" because we want to include firms that demand rather than supply credit. Although a drawback of Compustat is that small businesses are underrepresented,[3] Compustat firms account for a very large share of economic activity in the United States. Chun, Kim, Morck and Yeung (2008) calculate that on average in 1971-2000 the sales of Compustat firms, net of the sales of intermediate products, amounted to about half of the U.S. GDP. In 1995, the firms in our sample accounted for about half of the debt of non-financial U.S. businesses. In addition to its comprehensive coverage, Compustat includes data for several contiguous years. This is key for constructing time series that span the years of staggered deregulation of the state credit markets. Compustat comprises annual data from 1950, although we will work with approximately 30 years because of discontinuities in the time series and some missing data.

Throughout, we consider the reallocation of both total debt and long-term debt (long-term credit helps finance many long-term investment plans and, hence, deserves special attention). Following an established literature in finance, and as in Herrera et al. (2011), we define debt as all forms of financial debt, thus excluding accounts payable to suppliers. Unlike other types of debt, trade credit is for transactional, rather than for financial, purposes and is based on firms' relationships with suppliers rather than with financial institutions. Moreover, it features specific contracts and, because of its high cost, it is used by firms only when they cannot obtain cheaper financing (Petersen and Rajan, 1997). These peculiar properties make trade credit a very limited substitute for other forms of debt (Petersen and Rajan, 1997; Nilsen, 2002). Nonetheless, in robustness tests (presented in Online Appendix D) we also extend the analysis to include trade credit.

We follow Herrera et al. (2011) in addressing some measurement issues. The first

[3] In 1995, for example, at the median (25th percentile), the net sales amounted to 61.11 (11.11) million dollars. In 1995, firms with less than 500 employees were 51% of the total.

regards entry and exit. Some firms that appear in Compustat for the first time are newborn while others are existing firms that file with the Securities and Exchange Commission, become incorporated, or originate from bigger firms' divestitures. As we do not want to count the debt of existing firms as additions to credit, following Ramey and Shapiro (1998) and Herrera et al. (2011), we drop firms that enter the data set and have a ratio between the end-of-period gross capital and net capital above 120%. The rationale is that generally the gross book value of physical capital of a new firm is similar to its net book value. Regarding exit, we treat exits due to bankruptcy or liquidation and to merger or acquisition as credit subtractions, while we do not count exits for other reasons (see Ramey and Shapiro, 1998, and Herrera et al., 2011).[4] There are compelling reasons to treat the exit of a merged or acquired firm as a credit subtraction. In a merger, the management and workforce of a firm acquire control over the financial resources of another firm. Thus, for the financiers this is at least partly equivalent to reallocating credit between two firms. Indeed, several studies show that mergers significantly affect the stock market value of target and acquirer and have large real effects (Malmendier, Moretti and Peters, 2018).

Another issue regards a few mismatches between fiscal year and calendar year. In line with the approach followed by Compustat, if the fiscal year ends after May 31st, the data of the firm are imputed to the corresponding calendar year as if there was no mismatch. If the fiscal year ends before May 31st, we allocate the data to the previous year. The results are virtually unchanged if we recompute credit flows apportioning fiscal year data proportionally to calendar years. Finally, since we are interested in changes in firms' real exposure to financiers, we deflate the data using the implicit GDP deflator.

A caveat is in order here. While the liberalization involved banks, our data also include

[4] Other reasons for exit include conversion to a private company, leveraged buyout, or unspecified.

non-bank credit, as noted. However, this is not a concern. Bank credit accounts for an important share of the financing of U.S. businesses, including firms in Compustat (the share appears to range between 25% and 35% during the eighties and nineties).[5] Moreover, the deregulation, and the possible consequent changes in the process of reallocation of bank credit, is very likely to have prompted a reshuffling of the whole borrowing portfolios, affecting non-bank credit as well. Finally, even if non-bank credit was not significantly affected, our results would be biased towards not finding an effect of the liberalization on credit reallocation. Thus, we do not risk overestimating its impact on credit reallocation.

3.3 Constructing inter-firm credit flows

To measure inter-firm credit flows in the states, we replicate the methodology proposed by Davis and Haltiwanger (1992) for measuring inter-firm job flows (see Herrera et al., 2011, for an application to credit). Let c_{ft} denote the average of the debt of a firm f at time $t-1$ and at time t. The debt growth rate of a firm is computed by dividing the change in debt from year $t-1$ to year t by c_{ft}. This debt growth rate takes values in the $[-2, +2]$ interval and has the advantages of boundedness and symmetry (Davis and Haltiwanger, 1992). If a firm is founded, its debt growth rate is $+2$; if it dies, it equals -2.

Exploiting information on the state in which firms have their principal location, we then construct five annual credit flows for each state by aggregating firms' debt growth rates. Credit creation (POS_{it}) in state i and year t is computed as the weighted sum of the debt growth rates of the firms with growing debt, where the weights are given by the firm debt c_{ft} over the debt C_{it} of the firms located in the state. Credit destruction (NEG_{it})

[5] Using aggregate data, this information can be inferred from the annual releases of the Flow of Funds Accounts of the United States (Board of Governors of the Federal Reserve System). See, for example, Table L.102. As for Compustat firms, see Crouzet (2020), who computes a proxy for bank debt in Compustat.

is calculated as the weighted sum of the debt growth rates of the firms with decreasing debt. Gross credit reallocation (SUM_{it}) is obtained as the sum of credit creation and credit destruction. Net credit growth (NET_{it}) is constructed as credit creation less credit destruction. Finally, excess credit reallocation (EXC_{it}) equals gross credit reallocation less the absolute value of net credit growth. Thus, EXC_{it} constitutes credit reallocation in excess of the minimum necessary to accommodate net credit growth. Formally,

$$\text{POS}_{it} = \sum_{\substack{f \in s_t \\ g_{ft} > 0}} g_{ft} \left(\frac{c_{ft}}{C_{it}} \right); \text{NEG}_{it} = \sum_{\substack{f \in s_t \\ g_{ft} < 0}} |g_{ft}| \left(\frac{c_{ft}}{C_{it}} \right), \tag{1}$$

$$\text{SUM}_{it} = \text{POS}_{it} + \text{NEG}_{it}; \quad \text{NET}_{it} = \text{POS}_{it} - \text{NEG}_{it}; \quad \text{EXC}_{it} = \text{SUM}_{it} - |\text{NET}_{it}|. \tag{2}$$

Our study investigates whether the deregulation altered the process of credit reallocation. While small firms (allegedly, the most financially vulnerable) are underrepresented in Compustat, there is extensive evidence that financial factors play a key role in Compustat firms' decisions and that financial imperfections hinder the substitutability of different kinds of finance for Compustat firms (Moyen, 2004; Murfin and Njoroge, 2012; Dimitrov and Tice, 2013; Whited, 1992; Acharya, Davydenko and Strebulaev, 2012). There is also evidence that the credit markets of the U.S. states are (partially) segmented, so that firms, including Compustat ones, are sensitive to the banking structure and efficiency of the state where they are located (Becker, 2007). Indeed, studying Compustat firms, Dass and Massa (2011), Sufi (2007) and Arena and Dewally (2012) find that banks obtain better information and grant easier access to credit when they are closer to firms. Clearly, the sensitiveness to the banking conditions in the state will differ across firms, so in our sample some firms with hundreds of employees will be less sensitive than firms with a few dozens employees. However, the flexibility with which credit flows, say, between cash-abundant firms less exposed to financial imperfections and firms more exposed to financial imperfections is an integral part of the credit market dynamism that our reallocation measures seek to reflect. Finally, if anything, the underrepresentation of small firms may lead to underestimate the impact of the liberalization on reallocation. Later in the study, we will nonetheless perform a sensitivity analysis by removing relatively large firms from our data.

4 Empirical methodology

Table B.1 in Online Appendix B reports the dates when each state lifted restrictions on interstate banking or intrastate branching (see Amel, 1993, and Jayaratne and Strahan, 1998).[6] The staggered credit market deregulation of the U.S. states allows to identify an exogenous shock to the process of credit reallocation, as we now turn to elaborate.

4.1 The empirical model

Our empirical strategy consists of estimating a model that projects the rate of credit reallocation in a state onto regulatory indicators and relevant control variables. Following studies on the U.S. deregulation (see Beck, Levine and Levkov, 2010, for a discussion), we drop Delaware and South Dakota. In fact, the credit market of these two states was deeply affected by laws that made them the centers of the credit card industry (for instance, in 1982 Delaware introduced tax incentives for credit cards). Thus, we use data for 48 states plus the District of Columbia. After accounting for missing data and some discontinuities in the time series, and after computing growth rates, we use data spanning 29 years (1969-1997). Table 1 reveals that in 1969-1997 the annual gross reallocation rate (SUM) and excess reallocation rate (EXC) of total credit (averaged across states

[6] As the table illustrates, prior to 1994 Iowa did not allow branch banking and Hawaii did not lift interstate banking regulations (both states are included in the regressions).

and years) equalled 18.43% and 8.86%, respectively. There is substantial variation in the intensity of credit reallocation: the annual gross reallocation of total credit equals 10.77% at the 25th percentile and 21.37% at the 75th percentile; the annual excess reallocation of total credit equals 4.06% at the 25th percentile and 12.91% at the 75th percentile.

The empirical fixed effects model can be expressed as follows:

$$Cre_{it} = \alpha_t + \beta_i + \sum_j \gamma_j regul_{jit} + \sum_h \delta_h share_{hit} + \varepsilon_{it}. \tag{3}$$

where Cre_{it} is the gross (SUM) or excess (EXC) credit reallocation or the credit growth (NET) in state i in year t, α_t is a time fixed effect that captures nation-wide shocks (e.g., monetary policy shocks) in year t, β_i is a state fixed effect that measures time-invariant factors that differ across states, and ε_{it} is the error term.

In equation (3), $regul_{jit}$ are our key regressors of interest and capture changes in banking regulation. The first variable, $interstate_{it}$, is a dummy that takes the value of one starting on the year when state i permitted entry by out-of-state banks, zero otherwise. The second, $intrastate_{it}$, is a dummy that takes the value of one starting on the year when state i permitted branching within the state via merger and acquisition, zero otherwise.[7] Since these dates reflect the year in which the legislation was implemented, the variables reflect treatment and not intention to treat. Finally, in equation (3) $share_{hit}$ denotes the labor share of sector h in total non-farm employment in state i in year t.[8] As discussed by Morgan, Rime and Strahan (2004), it is important to control for the sectorial labor force composition in the state because, for instance, a shift of sectorial weights towards rapidly expanding industries could accelerate credit growth in a state and, in

[7] In line with Clarke (2004), Jayaratne and Strahan (1996) and Morgan, Rime and Strahan (2004), we use the dates in which legislators allowed mergers and acquisitions within the state. These are highly correlated with the dates in which intrastate de-novo branching was allowed.

[8] We treat "agricultural services, forestry, fishing and other" as the omitted industry.

our setting, also affect credit market dynamism. Moreover, although prior literature has documented that the deregulation was driven by exogenous political economy factors, a reader might have some concern that state-level changes in sectorial composition and shifts in the industrial structure (due, e.g., to technological change) could have influenced states' choice to deregulate. In light of these considerations, we take the more conservative approach of performing regressions that control for the labor shares of the one-digit SIC sectors in total non-farm employment but also show that all the results are virtually identical if we exclude the sectorial labor shares.

In robustness checks, we augment the model with additional controls to assuage possible remaining concerns about endogeneity, as we now turn to discuss.

4.2 Methodological issues

As noted by prior studies, the granular fixed effects model in (3) is robust to various concerns about endogeneity and spurious correlation. A first concern could be that the states' decisions to deregulate could be reverse-caused by credit market dynamism. Jayaratne and Strahan (1998) document the political economy factors that determined the timing of deregulation of the states. For example, in the mid-1980s the Office of the Comptroller of the Currency allowed nationally chartered banks to branch freely in the states where the branching of savings and loans was not restricted. This induced some southern states to deregulate intrastate branching. Moreover, following the 1982 Garn-St. Germain Act, bank holding companies were allowed to acquire failed banks and thrifts, regardless of state laws. More broadly, new deposit taking technologies in the 1970s tilted the political balance from small banks and insurance companies (the main beneficiaries of the regulation) to large banks (Kroszner and Strahan, 1999). The state fixed effects absorb cross-sectional variation (such as when a state deregulates) or persistent differences across states (e.g., regulation was caused by the cross-state differences in structure between large and small banks). Changes in coefficients will then be driven by changes in variables after a state deregulates. To further assuage possible concerns, in robustness tests we augment our model with proxies for the relative importance of small banks in a state.

Another criticism could be that the model in (3) omits variables linked to credit market dynamism. While effects that are homogeneous across states would be absorbed by time fixed effects, the reader could be concerned that around the deregulation dates many states implemented reforms or state-level policies that affected credit market dynamism, confounding the effects of the banking deregulation. Jayaratne and Strahan (1996) document that no such coincident state-level reforms or policy shifts occurred. To further assuage this concern, in robustness tests we augment the model in (3) with proxies for the fiscal policy stance of the states. In addition, Jayaratne and Strahan (1996) show that there is no correlation between the phase of the business cycle (which could influence credit dynamism) and the timing of deregulation. And that the states did not deregulate anticipating faster growth or the recovery from a recession. In fact, there is no evidence that indicators of state fluctuations were unusually high or low before the states deregulated (Jayaratne and Strahan, 1996). To further address concerns, in robustness tests we add indicators of state economic conditions into the model.

Finally, another confounding effect could be that the states deregulated when their credit market dynamism was already shifting. We will assuage this concern through a parallel trend analysis.

5 Baseline results

This section presents the baseline empirical results.

5.1 Estimates

Table 2 reports coefficient estimates and (in parentheses) associated robust standard errors clustered at the state level. The results in columns 2 and 8 suggest that interstate deregulation significantly increased the gross reallocation (SUM) of total and long-term credit. In the regressions that do not include the indicator of intrastate deregulation (Panels A and B), the estimated coefficients on *interstate* imply that a state that allowed entry by out-of-state banks would have experienced a 5.8 percentage points larger annual gross reallocation of total credit and a 6.7 percentage points larger annual gross reallocation of long-term credit. And the coefficients on the indicator for interstate deregulation are virtually unchanged if we control for intrastate deregulation (Panels C and D). These effects appear to be economically sizeable, accounting for more than 25% of the average gross credit reallocation rate in the sample.[9] By contrast, we do not find a statistically significant effect on gross credit reallocation of the indicator for intrastate deregulation.

The reader could wonder whether the effects picked up by the estimates reflect inter-firm flows of credit that occur to accommodate changes in the total amount of credit granted to the business sector. We then reestimate the empirical model replacing gross credit reallocation with excess credit reallocation (EXC), which nets out the minimum reallocation needed to accommodate net credit growth. We also reestimate the model using net credit growth (NET) as the dependent variable. The results for excess credit reallocation yield similar insights as those for gross reallocation: the estimates in Table 2,

[9]The effects are persistent beyond the initial period of deregulation, as shown in Table B.2.

columns 4 and 10, suggest that interstate deregulation stimulated the excess reallocation of total and long-term credit, with a rise of 3.1 and 4.1 percentage points, respectively. Next, we experiment by inserting net credit growth in the regressions. As shown in Table 2, columns 6 and 12, neither intrastate not interstate deregulation affect net credit growth in a statistically significant manner. Even in the specifications in which interstate deregulation has a marginally significant effect, this will vanish in the robustness checks.

The results in Table 2 support the hypothesis that the liberalization made the process of credit reallocation across businesses more flexible and dynamic. For example, banks' structural transformation and the increased competitive pressure induced by the entry of out-of-state banks could have raised efficiency in financial institutions. This could have led to more effective screening, hence higher flexibility in extending credit, and more effective monitoring, hence less inertia in cutting credit. The finding that instead the liberalization did not affect credit growth is in line with that of Jayaratne and Strahan (1996) and Morgan, Rime and Strahan (2004), who document that the deregulation did not trigger an acceleration in loan growth using a model analogous to (3). All in all, the results corroborate the idea that credit growth was not the channel through which the liberalization affected the credit market but the liberalization reduced the obstacles to the dynamic process of reallocation of liquidity. The enhanced dynamism in the credit reallocation process may have been instrumental to the improvement in the ex-post measures of lending quality (e.g., non-performing loans) detected by prior studies (Jayaratne and Strahan, 1996).[10]

[10] Later in the paper, we will find evidence of an effect of credit reallocation on non-performing loans.

5.2 Interpreting the estimates

Macroeconomic models of the credit market with allocative frictions (e.g., monitoring, screening, or matching) can more formally rationalize changes in the dynamic process of credit reallocation and its response to a liberalization. In the class of models with informational frictions, banks allocate resources and loan officers to the continuous screening and selection of borrowers (credit creation) and simultaneously loan officers monitor existing customers to choose whether to roll over or curtail lending (credit destruction) (see, e.g., Hachem, 2021, Ruckes, 2004, and Fishman, Parker and Straub, 2020). In a parallel class of models, frictions on the credit creation margin take the form of a search and matching process, capturing the need for lenders and borrowers to form appropriate lender-borrower pairs (e.g., with the appropriate distance and lending technology) (Wasmer and Weil, 2004; Petrosky Nadeau and Wasmer, 2015; den Haan, Ramey and Watson, 2003). On the credit destruction margin, such models posit an exogenous credit destruction process or endogenous separation decisions of lenders and borrowers. Recently, the two classes of models have been integrated with each other, allowing for matching frictions in credit creation and a continuous monitoring governing credit destruction (see, e.g., Becsi, Li and Wang, 2013, Chamley and Rochon, 2011, and Brand, Isore and Tripier, 2019).

In the first class of models, higher efficiency in monitoring/screening and improved incentives of loan officers can stimulate more effective and rapid screening (hence, more intense credit creation) and more effective monitoring (hence, more flexible credit destruction). In Hachem (2021), for example, banks have limited monitoring/screening capacity so that more efficient banks will screen and monitor faster, making it easier to create but also to destroy credit. In the second class of models, an increased ramification and efficiency of banks can translate into an improvement of the credit matching process. In

Bethune, Rocheteau, Wong and Zhang (2022), for example, the decision whether to sustain search costs is influenced by banks' efficiency and monitoring quality. In the third class of models combining informational and search frictions, Chamley and Rochon (2011) show that a policy that reduces monitoring costs and increases monitoring incentives raises the payoff from matching with new borrowers and reallocating credit relative to that of loan rollover. In several of the above models, the boost to credit reallocation induced by a policy shock is not associated with an increase in credit growth: the acceleration of credit creation is accompanied by the intensification of credit destruction, leaving net credit growth essentially unchanged. On the other hand, the boost to creation reallocation enhances the ability to flexibly allocate credit to more productive firms.

A natural question is why we find that interstate deregulation affected the intensity of credit reallocation while we uncover no such evidence for intrastate deregulation. A first interpretation relies on credit market segmentation. Small banks were allegedly those more affected by the removal of intrastate branching restrictions. Prior research finds that small banks especially focus on serving small firms (Amel and Prager, 2013), and, as noted, small firms are underrepresented in Compustat. So, we may tend to underestimate the impact of intrastate deregulation on credit reallocation. A second interpretation points to the different channels through which the two margins of liberalization may exert an impact. Prior research finds that interstate deregulation primarily affected managers' effectiveness at screening and monitoring borrowers (Hubbard and Palia, 1995). By contrast, intrastate deregulation especially altered the diffusion of branches in the states, enhancing firms' ability to identify nearby branches (Berger, Leusner and Mingo, 1997) and improving capital allocation across branches (Berger et al., 2018). Our results can reflect the fact that it was especially managers' improved efficiency and incentives, fostered by the entry of out-of-state banks, that influenced the dynamism with which liquidity was reallocated.

6 Mechanisms and alternative explanations

In this section, we explore alternative, yet not exclusive, mechanisms whereby the liberalization could have led to a more intense credit reallocation process. In doing so, we also seek to exclude possible alternative explanations for our findings. In a first set of tests, we examine to what extent the intensification of reallocation was due to the increased firm churning sometimes induced by financial liberalizations. Then, we investigate the possible role of heightened firm volatility. Thirdly, we examine the hypothesis that, among continuing firms, the liberalization produced a credit reshuffling across size classes of firms. In particular, entering banks could have focused on serving large firms. A fourth test studies whether the intensification of reallocation is mostly an inter-industry phenomenon. Finally, we examine whether the increased reallocation is specific to selected firm categories.

The tests reveal that, while contributing to the intensification in credit reallocation, none of these channels can explain a significant portion of it. The intensification largely reflects intensive margin effects, that is, higher dynamism in liquidity reallocation across continuing firms, even within narrowly defined industries or size classes of firms.

6.1 Firm churning

Prior research finds an impact of financial liberalization on firm churning. We thus recompute the credit flows in (1)-(2) after dropping entering and exiting firms. The effect of interstate liberalization on credit reallocation is only slightly smaller when using these modified credit flows (Table 3, Panel A). To further probe this point, we compute the component due to credit reallocation within sectors, or within-index:[11]

$$W_t = 1 - \sum_j \frac{|NET_{jt}|}{SUM_{jt}}. \tag{4}$$

For a given firm classification (continuing versus entering/exiting firms, size classes, industries), a higher value of the index signals that a larger share of reallocation occurs within rather than across the defined groups of firms, with $W = 1$ implying that all the reallocation occurs within groups and $W = 0$ implying that it all occurs between groups. We then compute the index after partitioning firms into continuing firms and entering/exiting firms, and reestimate (3) using the index as the dependent variable. Since the index can take values in the [0,1] interval, we perform the estimation both by OLS and by two-limit Tobit. We find a positive effect of interstate liberalization on the index, implying that the relative share of reallocation within continuing firms –and within enterers and exiters– rose after the liberalization (Panel A of Table B.3). This confirms the importance of intensive margin effects in the increase of credit reallocation.

6.2 Firm volatility

The reader might wonder whether the impact of deregulation on reallocation captures the increase in firm volatility over the past decades. Following Comin and Philippon (2005), we compute the volatility of the growth rate of real sales ($\gamma_{i,t}$) for firm i at time t as

$$\sigma_{i,t} = \left[\frac{1}{10} \sum_{k=-4}^{5} \left(\gamma_{i,t+k} - \overline{\gamma}_{i,t} \right)^2 \right]^{1/2}$$

where $\overline{\gamma}_{i,t}$ is the average growth rate of real sales growth for firm i between $t-4$ and $t+5$. Then, we aggregate to the state level by weighting each firm's volatility by its share in the state's total (long-term) credit. Finally, we include this state-level measure of volatility

[11] See Davis and Haltiwanger (1992) for a similar index applied to the study of job reallocation.

as a control in our baseline regressions. As Table 3, Panel B illustrates, our findings are robust to controlling for firm-level volatility by state. Similar results are obtained if we use profits, instead of sales, to compute a measure of firm-level volatility (Table B.4).

In brief, while idiosyncratic volatility increased in recent decades, this does not account for the increase in credit reallocation that followed the banking deregulation. This again suggests that the increase in credit market dynamism could instead have been associated with an attenuation of the frictions impinging on the flow of credit across firms.

6.3 Reshuffling across size classes and industries

Prior studies suggest that the liberalization promoted the entry of banks specialized in financing relatively large firms. This could have caused a reshuffling of credit from smaller to larger businesses. To evaluate whether this can explain our findings, we compute the within-index in (4) using a classification based on firm size (sales) quartiles. We then reestimate (3) by treating this within-index as the dependent variable. The results in Panels B and C of Table B.3 suggest that the interstate liberalization did not alter the relative share of reallocation occurring across size classes. Since credit flows ("flights to quality") from small to large firms especially occur during recessions, we also performed the estimation by interacting the measures of deregulation with a recession dummy. The results suggest no material differences in the effects.

The higher credit reallocation following liberalization could also reflect more intense reallocation across industries. We re-run the model in (3) using the W-index for industries as the dependent variable (we drop the sectorial labor shares because they could absorb the effect of the regulatory indicators on inter-industry credit flows). When we partition firms into one-digit codes, we find that the liberalization raised within-industry reallocation relative to cross-industry reallocation, though we do not find such an effect when partitioning firms into 2-digit industries (Table B.3, Panel D). Overall, the results signal that both within-industry and cross-industry credit reallocation grew after the deregulation.

6.4 Further dimensions of firm heterogeneity

The reader can wonder whether the effect of the liberalization on credit reallocation was specific to selected firm categories. Tables B.5 and B.6, Panels A-B, show that all our results are robust to excluding relatively large firms (above the 95th, 90th or 85th percentile of employment). Interestingly, although the impact of firm size is not monotonic, we find some evidence that the effect of the liberalization on credit reallocation tends to become larger when focusing on relatively smaller firms. Panels C-D of Tables B.5 and B.6 also show that the baseline results carry through when we drop the oldest and youngest firms from the sample. Further, the deregulation resulted in a more intense excess credit reallocation both for firms above and below the median dependence on external finance (see Rajan and Zingales, 1998). While both groups experienced an increase in gross reallocation, the effect on the less dependent group is not precisely estimated (Panel E).

Lastly, given our interest in the effect of credit market dynamism on productivity, we inquire whether the impact of the deregulation on credit flows is different between firms above and below the median technology level. We use the Eurostat classification of high-tech industries to split our sample. As Panel F in Tables B.5 and B.6 show, the deregulation significantly increased reallocation among more technologically advanced firms. In particular, the increase in excess reallocation indicates a boost above what was required to accommodate any change in credit growth. Yet, long-term credit reallocation did increase also for firms below the median technology level.

7 Credit reallocation and productivity

Following interstate liberalization, the higher efficiency and the improved incentives of bank managers could have induced banks to transfer funds from low-productivity to high-productivity firms in a more flexible way. This is a natural channel through which the enhanced credit market dynamism may have affected state economic activity: more dynamic screening and monitoring of firms, and less inertia in extending and cutting loans, may improve the marginal productivity of capital (Greenwood and Jovanovic, 1990).[12] Nonetheless, as noted, a priori it is not obvious whether higher credit market dynamism necessarily leads to higher allocative efficiency. An increase in credit market dynamism could undermine the stability of credit relationships, leading to excessive destruction of productive bank-firm matches (Beck et al., 2018) and to the resulting extension of credit to lower productivity firms (Becsi et al., 2005).

7.1 Preliminary tests

We perform preliminary tests to probe whether the intensification of credit reallocation led to a more efficient credit allocation. In Table B.7, we estimate the impact of credit reallocation on the state-level asset-weighted non-performing loan (NPL) ratio. Data for banks' NPL ratio are from the Call Reports and, due to data availability, span the period 1983-2001. The banks' share of state assets are used as weights. Given the relatively short sample and the fact that the liberalization was well underway by 1983, we use least squares for these regressions. As Table B.7 illustrates, in our sample increases in credit reallocation are associated with a decline in the NPL ratio. This impact on loan portfolio performance suggests that the increased credit market dynamism induced by the deregulation could

[12]See also De Gregorio and Guidotti (1994) for the allocative channel in the finance-growth nexus.

have influenced the real sector via an improved ability of loan officers to allocate credit to more efficient firms. In what follows, we investigate this mechanism more directly.

7.2 The role of reallocative efficiency

Since we lack firm-level information on hours worked, computing firm-level total factor productivity would entail strong assumptions. We thus choose to rely on (a proxy for) firm-level capital productivity, which reflects both TFP and capital intensity. As suggested by Galindo, Schiantarelli and Weiss (2007), as a proxy for firm capital productivity, we use the sales to capital ratio because firm profits are significantly more noisy than sales. We then obtain evidence on the efficiency of the process of inter-firm credit reallocation adapting the index put forth by Galindo, Schiantarelli and Weiss (2007) to study the efficiency of investment allocation. The index is a ratio. In the numerator, in state i and year t, the ratio includes the weighted sum of the sales to capital ratios of the firms (s_{fit}/k_{fit}), where for each firm the weight is given by the contribution of the firm debt to the total debt of the firms in the state in that year (c_{fit}/C_{it}). In the denominator, the ratio includes the sum of the sales to capital ratios of the same firms weighted by the contribution of the firm debt to the total debt of the firms in the previous year (c_{fit-1}/C_{it-1}). Formally,

$$\widehat{Cre}_{it} = \frac{\sum\limits_{f \in i} \frac{s_{fit}}{k_{fit}} \frac{c_{fit}}{C_{it}}}{\sum\limits_{f \in i} \frac{s_{fit}}{k_{fit}} \frac{c_{fit-1}}{C_{it-1}}}. \tag{5}$$

A value of $\widehat{Cre}_{it}$ greater than one signals that in state i credit was allocated to more productive firms in year t than if the credit distribution had remained as in year $t-1$.

After computing the index for each year and state, we take the weighted average of the index across states (with weights given by the total sales in the state or by the state GDP). Precisely, the state average for year t prior or after liberalization is constructed using the values of the index for the states t years before or after the interstate liberalization of their credit market. We plot the index in Figure 1. The figure suggests that the efficiency of credit reallocation rose significantly after the interstate liberalization.

7.3 The TFP effect of credit reallocation

The Solow residual can reflect the efficiency with which resources are allocated across firms with heterogenous productivity (Hsieh and Klenow, 2009; Restuccia and Rogerson, 2008). In what follows, we carry out an alternative test of the impact on productivity of the increased credit reallocation. In particular, we treat equation (3) as the first stage of a model in which in the second stage we project a measure of state total factor productivity (TFP) onto the value of credit reallocation defined by the regulatory indicators:

$$\begin{aligned} \Delta TFP_{it} &= \alpha_t + \beta_i + \gamma Cre_{it} + \sum_h \delta_h share_{hit} + \varepsilon_{it}, \quad (6) \\ Cre_{it} &= \kappa_t + \eta_i + \sum_j \zeta_j regul_{jit} + \sum_h \theta_h share_{hit} + \nu_{it}, \quad (7) \end{aligned}$$

where ΔTFP_{it} is the log difference of the TFP of state i in year t. In the first stage, we employ the indicator for interstate deregulation as the instrument. The results of an F-test indicate that using both regulatory indicators as instruments can lead to weak identification in the second stage. This is to be expected as we have found that interstate, and not intrastate, deregulation played a key role in fostering the reallocation of credit.

We follow a standard growth accounting approach to compute TFP by state. We posit that the production function for each state i and year t is a Cobb-Douglas

$$Y_{it} = A_{it} K_{it}^{\alpha} N_{it}^{1-\alpha}, \quad (8)$$

where Y_{it} is the state annual real GDP, A_{it} is TFP, K_{it} is the state physical capital stock, and N_{it} measures the annual hours worked in the state. The data on annual aggregate capital by state were obtained from Peri (2012).[13] As for labor, because annual data by state on hours worked are available from 2007, we use the total number of non-farm employees by state and year from the Bureau of Labor Statistics, multiplied by the number of hours worked by an average worker in the state in the year. To compute the latter, we obtain data for each state on the number of hours worked by an average worker during a work week from the March Current Population Survey of the Census Bureau. We then multiply this by 46.2, the number of weeks worked per year by the average U.S. worker according to Alesina, Glaeser and Sacerdote (2005). In line with the literature, we set α (the share of output accruing to capital) at 1/3. TFP is then obtained by solving (8) for A_{it}.[14] The growth rate of state TFP, averaged across states and years, was 0.77 percent. This is very close to what obtained for the whole United States by Cette, Kocoglu and Mairesse (2009) and Maddison (2007).[15] Figure B.1 plots nation-wide measures of TFP growth computed as a weighted average of the state values using the GDP share or sales share as weights. On average, TFP increased in the years prior to the states' deregulation. This suggests that the deregulation was not intended to foster TFP growth.

Table 4 reports the second-stage estimates for the effect of credit reallocation on TFP growth. An increase in the annual gross reallocation of total (long-term) credit by one percentage point would have led to a 0.15 (0.13) percentage point increase in state TFP growth (columns 2 and 8).[16] Given that the average TFP growth during the sample period

[13]The construction of the capital stocks follows Garofalo and Yamarik (2002). See Online Appendix A.

[14]Due to lack of data on hours worked before 1976 for some states and before 1977 for most states, we estimate the productivity regressions on a shorter sample that starts in 1977.

[15]Cette, Kocoglu and Mairesse (2009) calculate that in the United States in 1980-2006 TFP growth equalled 0.9%. Maddison (2007) calculates that in 1973-2003 TFP growth was 0.7%.

[16]Instrumental variable standard errors (which account for the uncertainty in estimating the first stage) clustered at the state level are in parentheses.

was 0.77 percent, this effect is economically significant. Indeed, it accounts for roughly 56% of the increase in state-level real per capita GDP growth,[17] which is substantially larger than the average contribution (39%) of TFP growth to real per capita GDP growth over the sample. Next, we reestimate the model inserting separately excess credit reallocation and net credit growth. The estimates in columns 4 and 10 of Table 4 reveal that the TFP effect of excess reallocation is larger than that of gross reallocation. And interestingly the results remain similar if we restrict attention to the intensive margin by excluding entering and exiting firms (Panels C-D), which is in line with what we found in the baseline tests.

Finally, it is worth elaborating on the link between firms' operations and state economic activity. As noted, to determine firms' location we have used their headquarter location. This is the approach followed in several strands of literature when employing Compustat data and reflects the idea that a firm's core business and main activities are often performed close to its headquarter (see, e.g., Seasholes and Zhu, 2010; Giroud and Mueller, 2010; Gompers, Ishii and Metrick, 2010; Orlando, 2004; Francis, Reichelt and Wang, 2005; Ivkovic and Weisbenner, 2005, and references therein). Clearly, a share of a firm's business will take place outside the state where its headquarter is located. Again, if anything, we may then tend to underestimate the impact of credit reallocation on state TFP.

8 Robustness

In this section we subject our main results to a broad range of robustness tests.

[17] We estimate the regressions for state-level real GDP focusing on the same sub-period for which we can estimate the regressions for TFP. In the second stage, controlling for the sectorial labor shares, the estimated coefficient on the gross reallocation of total credit equals 0.27.

8.1 Parallel trends

Our fixed effects model in (3) is a differences-in-differences specification that relies on the assumption of underlying parallel trends. Specifically, it requires that, in the absence of the deregulation, the difference between the rate of credit reallocation in states that liberalized their credit market earlier and those that liberalized later was constant over time. We test this assumption by reestimating the baseline regression after replacing the interstate deregulation dummy with event-time dummies. The first four years prior to deregulation and first four years after deregulation are assigned individual year dummies, while periods more than four years before or after the deregulation are collapsed into "< -4" and "> 4" dummies, respectively. The year prior to deregulation is omitted so that the plotted coefficients can be interpreted as relative to the year before interstate liberalization. Figure 2 presents the coefficients for the event-time dummies, and confirms that the parallel trends assumption is valid.

A recent body of studies contends that estimates obtained through the standard two-way fixed effects (TWFE) differences-in-differences (DiD) estimator can be biased if treatment effects vary over time, even when the underlying parallel trend assumption holds (Goodman-Bacon, 2021; Sun and Abraham, 2021). This problem is especially likely to arise when the treatment is staggered, as is the case of the state-level liberalization of interstate banking restrictions. The intuition behind the possible bias is that standard treatment effects estimated through TWFE DiD regressions are the variance-weighted average of many individual treatment effects based on "2x2" comparison groups involving a treated group and a control group in the two periods surrounding treatment. There are three general types of "2x2" comparison groups: treated groups compared to never-treated groups, earlier treated groups compared to later treated groups, and later treated groups compared to earlier treated groups. The final comparison - later treated versus earlier treated - is where the problem can arise: if the treatment effect for the earlier treated group changes over time (beyond any underlying time trend), that evolution can end up biasing the DiD estimate for a “2x2” comparison of a later treated group versus earlier treated group. The “2x2” estimates stemming from these “bad” comparison groups can end up biasing the ultimate TWFE DiD estimate if they receive a large enough weight.

Goodman-Bacon (2021) puts forward a diagnostic to quantify the average DiD estimates stemming from each type of comparison group, along with their implicit weights in the ultimate TWFE DiD estimate. In our setting, all states end up receiving treatment (liberalizing interstate banking) so we only have two broad comparison groups, earlier treated versus later treated, and later treated versus earlier treated. Table B.8 gives the average DiD estimates and weights for all “earlier versus later comparisons” and all “later versus earlier comparisons” for the regressions corresponding to the baseline estimates of Panel A of Table 2.[18] Similar average treatment effects hold for both comparison groups, with the potentially problematic later versus earlier estimates contributing to only about one third of the ultimate TWFE DiD estimate. This reassures that the staggered nature of the liberalization is not biasing our estimates in Table 2. Further, Sun and Abraham (2021) devise an alternative estimator that seeks to avoid comparing treated units with inappropriate controls groups. We estimate (3) with this alternative estimator. The results in Table B.9 are similar to the baseline in Table 2, further alleviating concerns that our identification is being contaminated by time-varying treatment effects.

[18]The diagnostic requires a balanced panel. There are a handful of missing credit reallocation observations from Alaska in the early 1980’s, so Alaska is dropped from the sample in this exercise. As a result, the combined weighted estimates in Table B.8 do not precisely match the coefficients in Table 2.

8.2 Measurement

We perform a broad array of other robustness tests related to the measurement of our key variables. The reader could be concerned that, in states with a relatively smaller business sector, the credit flows reflect the credit changes of relatively few businesses. We reestimated the model in (3) first dropping Alaska only and then dropping the five states that, on average, have the smallest number of firms over the sample period. The results remain virtually unaltered (Table B.10, Panels A-B). Next, we accounted for the fact that some states imposed very tight regulatory restrictions ("unit banking" rules) which limited banks to a single location, that is, to have no branches. We then re-ran the model in (3) by interacting the indicators of deregulation with a dummy taking the value of one if the state was one of the sixteen unit banking states, zero otherwise. Again, we detected no difference from the baseline (Table B.10, Panel C).

One can also suspect that the increase in reallocation across continuing firms after liberalization reflects small credit adjustments due to day-to-day financing shortfalls. If it instead reflects enhanced flexibility in the reallocation of funds across medium- and long-term investments, we should detect a key role of large credit changes. In fact, a strand of studies (e.g., Eisfeldt and Muir, 2016; Bazdresch, 2013) suggest that non-convex adjustment costs prompt investing businesses to make lumpy debt adjustments rather than frequent small ones.[19] Table 5, Panel A, reports regressions that use large credit flows; the inferences we draw are unaltered.[20] We also tested whether the debt changes underlying the credit flows became less persistent after the liberalization. As shown in Table B.11,

[19]See also Minetti (2007) for a model rationalizing non-convex adjustment costs in credit changes.

[20]See Online Appendix C for details on the construction of large credit flows. Panel C of Table 5 reports the productivity results obtained using such flows.

we found no effect of the deregulation on the average persistence of debt changes.[21]

The reader may also wonder whether the effects are symmetric over the business cycle. During recessions credit imperfections can especially inhibit access to credit and this could slow down credit reallocation. On the other hand, recessions may have a “cleansing effect” and this may ease credit reallocation (Caballero and Hammour, 2005). We then interact the measures of deregulation with a recession dummy. The regressions in Panel B of Table 5 suggest that the positive impact of interstate deregulation on the gross reallocation of long-term credit was weaker during recessions. This is consistent with the hypothesis that the stronger obstacles to credit reallocation during downturns diluted the impact of interstate liberalization on the allocative dynamism of the credit market.

Further, we carried out placebo tests by pretending that the treatment (policy change) occurred in years different from the actual year of deregulation. As expected, moving the threshold date away from the year of liberalization tended to reduce the magnitude and statistical significance of the coefficient for interstate deregulation.

As noted, in Online Appendix D we also show that all the results are robust to including trade credit in the credit flows. Appendix D also discusses preliminary interesting insights on the role of trade credit in credit market dynamism.

8.3 Alternative specifications

We conduct additional robustness tests on the empirical specification in (3). First, we evaluate the effect of including labor shares as control variables. The results in the odd columns of Tables 2 and 4 (without labor shares) confirm those of the even columns (with

[21] See Online Appendix C for details on the calculation of the persistence of firm debt changes.

labor shares).[22] If anything, the estimates in Table 2 indicate a slightly larger impact of financial liberalization on credit market dynamism when labor shares are excluded (for long-term gross reallocation the difference across specifications is about 1 percentage point). This slightly larger impact suggests that the labor shares might absorb some transmission effect through a greater inter-industry reallocation.

As noted, previous research has shown that the timing of deregulation is uncorrelated with state economic conditions and policy stance. However, we re-estimated our specification in various ways to further alleviate possible concerns about endogeneity of the deregulation and occurrence of other policy changes which could have intensified credit reallocation. In Table B.14, we control for state-level personal income, employment, and population as well as state-specific time trends. In Panels A and B of Table B.15, we insert variables (small banks' share and financial health) that, as noted, can capture the propensity of a state to deregulate. In Panels C and D of Table B.15, we add indicators of the fiscal stance of the states, namely the tax receipt-to-income ratio and, as a proxy for expenditures on infrastructures, the real growth rate of highway expenditures (as in Jayaratne and Strahan, 1996). The results carry through.[23]

Finally, we reestimate (3) including a lag of the dependent variable as control. While a concern is that the estimates could suffer from a Nickell bias because T=29 is not large, the fact that the results in Table B.17 are essentially unchanged suggests that reverse causality is not a concern. Moreover, as shown below, our estimates are also consistent with the results obtained via local projections whether lags of the dependent variable are or not included as controls.

[22] As for the robustness results of Tables 3 and 5, the corresponding estimates without labor shares are in Tables B.12 and B.13. Again, they show that the inclusion of labor shares makes little difference.

[23] Table B.16 shows robustness of the TFP results to controlling for state expenditures on infrastructures.

8.4 Local projection analysis

While our main interest is in the long-term impact of the liberalization on the dynamism of the credit market, it is useful to investigate the dynamics of credit reallocation in the aftermath of the liberalization. To this end, we modify equation (3) into a local projection framework (Jordà, 2005). Specifically, we regress the h-period ahead rate of gross (SUM) or excess (EXC) credit reallocation or rate of credit growth (NET) on the interstate deregulation dummy, while controlling for industry labor share, state fixed effects, and time fixed effects. Impulse responses based on the h interstate dummy coefficients are displayed in Figure 3 with 95% and 68% confidence bands constructed from Driscoll-Kraay standard errors.[24] The increase in gross and excess credit reallocation persisted for over a decade following the onset of liberalization. While there may be a significant uptick in net credit reallocation three to four years after the deregulation, the dynamic response of NET is statistically equivalent to zero over the majority of the twelve-year horizon, which is consistent with the results from the fixed effects model.

9 Further implications

The credit market deregulation that occurred between the late 1970s and the early 1990s is ideal for our purposes in that it allows to isolate key components of geographic liberalization from other, possibly confounding, regulatory dimensions present in subsequent reforms. At the same time, our analysis can yield important insights into more recent financial reforms that occurred in the United States and in other countries and that could have affected banks' monitoring, screening and allocative dynamism. In this section, we

[24] The impulse responses are similar when controlling for lags of the dependent variable or lags of other state-level characteristics like output and employment.

focus on the U.S. 1999 repeal of the Glass-Steagall Act and on the 2009 banking deregulation of China. A discussion of other reforms is in Online Appendix E.

The Glass-Steagall repeal The Glass-Steagall repeal in 1999 lifted restrictions on the affiliation between investment and commercial banks. This led to larger financial institutions which could exploit scope economies in screening and monitoring associated with universal banking. Thus, a higher dynamism associated with higher monitoring efficiency might have occurred following this deregulation. However, this effect could have been confounded by the functional complexity of universal banks. Previous studies indeed find mixed evidence on universal banks' ability to exploit efficiencies of scope in monitoring (Barth, Dan Brumbaugh and Wilcox, 2000; Saunders and Walter, 1994).[25] Figure E.1 plots the weighted average of the credit flows of the U.S. states and shows a decline in credit reallocation in the years following the Glass-Steagall repeal. Clearly, studying the effect of the repeal is beyond the scope of this paper, as in our setting identification of causal effects hinges on the variation across states in the timing of a liberalization.

The 2009 banking deregulation of China The Chinese banking deregulation of 2009 has close resemblance to the U.S. liberalization considered in our analysis. In 2009, China lowered geographic bank entry barriers. In particular, it allowed joint equity banks to open branches freely in a city in which they had already established branches and to enter all cities in a province if they had operated branches in its capital city. Gao, Ru, Townsend and Yang (2019) find that banks entering into deregulated cities significantly increased lending. However, the soft budget constraints of state-owned enterprises (SOEs)

[25]Neuhann and Saidi (2018) find evidence of informational economies of scope after the Glass-Steagall repeal.

(due, e.g., to government guarantees) made entrant banks roll over lending to inefficient SOEs rather than extend credit to productive private firms. In such a scenario, a policy maker tracking credit reallocation after the liberalization, in addition to credit growth, could have detected limited increases in credit reallocation which were masked by the overall increase in credit growth. It could then have inferred the limited changes in credit allocation across firms which diluted the positive effects of the 2009 deregulation.

10 Conclusion

Credit growth masks a continuous, intense reallocation of credit across firms. This paper has investigated the impact of financial liberalization on the process of credit reallocation. Exploiting the staggered liberalization of the credit markets of the U.S. states from the late seventies to the early nineties, we have found that the interstate banking deregulation intensified credit reallocation. By contrast, we have found no evidence that the deregulation affected credit growth. The enhanced allocative dynamism of the credit market appears to have been especially driven by an increased intensity of reallocation across continuing firms, rather than by an acceleration in the flows of credit from exiting to entering firms. Our results also suggest that the rise in credit reallocation was not the result of higher firm-level volatility but it was driven by a reduction in frictions to the process of credit allocation. We have further studied to what extent the enhanced allocative dynamism of the credit market benefited more productive firms. The results suggest that the rise in credit reallocation triggered an increase in productivity growth in the states.

We believe that our results convey two important messages. First, they support the idea that financial regulation can have a significant impact on the reallocation of financial resources across firms. Second, more broadly, they suggest that the dynamic process of reallocation of credit is a key channel through which structural financial reforms can affect economic activity. More work is clearly needed to understand the properties and the quantitative importance of this channel.

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Table 1
Sample summary statistics
The table reports the definitions and summary statistics for the variables included in the regression analysis. The statistics are computed across all of the year-state observations in the sample period. For all variables, summary statistics refer to the 1969-1997 period, except productivity growth that refers to the 1977-1997 period. Year-state credit flows (SUM, EXC, NET) are computed from the firm-level credit changes using the methodology described in the paper.

Variables	Definitions	Obsv.	Mean	Standard deviation	First quartile	Median	Third quartile
Total SUM	gross total credit reallocation in the state, in percent	1416	18.43	5.72	10.77	14.81	21.37
Total EXC	excess total credit reallocation in the state, in percent	1416	8.86	7.13	4.06	7.63	12.21
Total NET	net total credit growth in the state, in percent	1416	3.53	17.55	-2.93	2.62	8.21
Long-term SUM	gross long-term credit reallocation in the state, in percent	1416	19.41	18.16	11.14	15.31	21.62
Long-term EXC	excess long-term credit reallocation in the state, in percent	1416	9.46	7.82	4.34	8.48	12.79
Long-term NET	net long-term credit growth in the state, in percent	1416	3.46	20.01	-3.01	1.82	8.16
Total SUMbig	gross total credit reallocation due to large credit changes in the state, in percent	1416	13.29	16.63	4.61	8.95	16.62
Total EXCbig	excess total credit reallocation due to large credit changes in the state, in percent	1416	5.22	6.52	1.01	3.32	7.31
Total NETbig	net total credit growth due to large credit changes in the state, in percent	1416	3.49	16.97	-1.29	1.50	6.38
Long-term SUMbig	gross long-term credit reallocation due to large credit changes in the state, in percent	1416	14.19	19.16	4.53	9.54	16.92
Long-term EXCbig	excess long-term credit reallocation due to large credit changes in the state, in percent	1416	5.52	7.26	1.04	3.40	7.74
Long-term NETbig	net long-term credit growth due to large credit changes in the state, in percent	1416	3.87	19.51	-0.70	1.79	6.69
Interstate	=1 starting on the year a state allowed interstate banking	1416	0.40	0.49	0	0	1
Intrastate	=1 starting on the year a state allowed intrastate branching	1416	0.54	0.50	0	1	1
Productivity growth	log difference of state total factor productivity, in percent	990	0.77	2.63	-0.64	0.78	2.18
Mining	labor share of the mining sector in total non-farm state employment, in percent	1416	1.31	2.03	0.17	0.43	1.52
Manufacturing	labor share of the manufacturing sector in total non-farm state employment, in percent	1416	15.84	7.20	11.12	15.63	21.07
Construction	labor share of the construction sector in total non-farm state employment, in percent	1416	5.51	1.21	4.73	5.49	6.17
Transportation	labor share of the transportation sector in total non-farm state employment, in percent	1416	5.09	0.90	4.47	5.06	5.66
Trade	labor share of the trade sector in total non-farm state employment, in percent	1416	21.27	2.50	20.40	21.69	22.59
Finance	labor share of the finance sector in total non-farm state employment, in percent	1416	7.15	1.39	6.17	7.01	7.92
Services	labor share of the services sector in total non-farm state employment, in percent	1416	24.51	5.27	20.39	23.82	27.63
Government	labor share of the government sector in total non-farm state employment, in percent	1416	18.37	5.55	14.68	17.20	20.37

Table 2

Credit market deregulation and credit reallocation

The table reports regression coefficients for the impact of deregulation on credit flows within states. Robust standard errors clustered at the state level are in parentheses. All coefficients and standard errors are multiplied by 100 to ease interpretation. The regressions are estimated by ordinary least squares. The dependent variables are gross credit reallocation (SUM) in columns (1), (2), (7) and (8), excess credit reallocation (EXC) in columns (3), (4), (9) and (10), and net credit growth (NET) in columns (5), (6), (11) and (12). Panels (A) and (C) refer to total credit, Panels (B) and (D) to long-term credit. Interstate (Intrastate) is an indicator variable taking the value of one starting on the year a state allowed interstate banking (intrastate branching), zero otherwise. Panels A and B include Interstate but not Intrastate; Panels C and D include both Interstate and Intrastate. Mining, manufacturing, construction, transportation, trade, finance, services, and government are the labor shares of the various sectors in total non-farm employment, in percent. All regressions include state and year effects. *, **, and *** denote statistical significance at the 10, 5 and 1% level, respectively.

	Panel A: Total Credit						Panel B: Long-term Credit					
	SUM (1)	SUM (2)	EXC (3)	EXC (4)	NET (5)	NET (6)	SUM (7)	SUM (8)	EXC (9)	EXC (10)	NET (11)	NET (12)
Interstate	6.36***	5.78***	3.17***	3.06***	4.25*	3.37	7.67***	6.70***	4.34***	4.12***	3.96**	2.97*
	(1.91)	(1.86)	(1.10)	(1.12)	(2.13)	(2.05)	(2.31)	(2.12)	(1.33)	(1.26)	(1.81)	(1.68)
Mining		-8.35***		0.30		1.84		-16.77**		-1.35		-2.45
		(3.82)		(1.31)		(3.58)		(6.68)		(1.43)		(1.87)
Manufacturing		-8.74**		-0.78		0.79		-17.17**		-2.31		-4.08**
		(4.14)		(1.26)		(3.01)		(6.80)		(1.49)		(1.97)
Construction		-7.38		-0.01		3.92		-15.68**		-1.53		-0.32
		(4.84)		(1.11)		(2.88)		(7.55)		(1.36)		(1.52)
Transportation		-8.79**		-1.75		-1.15		-16.05**		-2.80		-5.24
		(4.28)		(1.58)		(2.08)		(7.17)		(1.82)		(3.57)
Trade		-8.84**		-0.95		1.50		-16.46**		-2.24		-3.40
		(4.28)		(1.18)		(3.95)		(6.54)		(1.40)		(2.04)
Finance		-8.05**		-0.84		-0.74		-17.07**		-2.16		-6.44*
		(3.33)		(1.47)		(2.76)		(6.54)		(1.82)		(3.28)
Services		-8.81**		-0.73		0.90		-16.50**		-2.08		-3.72**
		(4.36)		(1.30)		(2.99)		(6.61)		(1.51)		(1.70)
Government		-9.29**		-0.78		0.09		-17.53**		-2.03		-4.46**
		(4.13)		(1.21)		(2.87)		(6.84)		(1.48)		(1.89)
Number of observations	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416

	Panel C: Total Credit						Panel D: Long-term Credit					
	SUM (1)	SUM (2)	EXC (3)	EXC (4)	NET (5)	NET (6)	SUM (7)	SUM (8)	EXC (9)	EXC (10)	NET (11)	NET (12)
Interstate	6.34***	5.63***	3.26***	3.09***	4.43*	3.41	7.68***	6.59***	4.37***	4.11***	4.21**	3.06*
	(1.88)	(1.83)	(1.07)	(1.10)	(2.24)	(2.09)	(2.29)	(2.05)	(1.29)	(1.23)	(1.96)	(1.74)
Intrastate	0.26	1.52	-0.83	-0.26	-1.62	-0.33	-0.09	1.02	-0.24	0.09	-2.22	-0.95
	(1.27)	(1.46)	(0.69)	(0.73)	(1.54)	(1.58)	(1.51)	(1.88)	(0.86)	(0.93)	(1.51)	(1.53)
Labor shares	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416	1416

Table 3
Mechanisms and alternative explanations
The table reports regression coefficients for the impact of deregulation on credit flows within states after accounting for intensive margin effects (Panel A) or changes in firm volatility (Panel B). Robust standard errors clustered at the state level are in parentheses. All regressions include state and year effects and the labor shares of the various sectors in total non-farm employment (coefficients and standard errors are not reported to conserve space). The dependent variables are total credit reallocation (SUM) in column (1), excess total credit reallocation (EXC) in column (2), net total credit change (NET) in column (3), long-term credit reallocation (SUM) in column (4), excess long-term credit reallocation (EXC) in column (5), and net long-term credit change (NET) in column (6). In Panel A all the flows are constructed using only credit changes of continuing firms. In Panel B firm volatility is the debt-weighted average volatility of firm sales in a state. *, **, and *** denote statistical significance at the 10, 5 and 1% level, respectively.

Panel A	Intensive Margin (Total Credit)			Intensive Margin (Long-term Credit)		
	SUMint (1)	EXCint (2)	NETint (3)	SUMint (4)	EXCint (5)	NETint (6)
Interstate	5.53***	3.00***	2.87*	6.09***	4.18***	3.19**
	(1.54)	(1.05)	(1.70)	(1.72)	(1.20)	(1.56)
Number of observations	1415	1415	1415	1415	1415	1415
Panel B	**Firm Volatility (Total Credit)**			**Firm Volatility (Long-term Credit)**		
	SUM (1)	EXC (2)	NET (3)	SUM (4)	EXC (5)	NET (6)
Interstate	5.16***	3.06**	2.35	5.76***	4.19***	2.22
	(1.74)	(1.14)	(1.60)	(1.77)	(1.28)	(1.36)
Firm Volatility	-11.37	4.36	-35.37**	-3.95	1.97	-31.30*
	(11.03)	(3.42)	(14.48)	(8.34)	(4.13)	(16.30)
Number of observations	1385	1385	1385	1385	1385	1385

Table 4
Credit reallocation and productivity (second stage results).
The table reports regression coefficients of the second stage for the impact of credit flows on the growth of state total factor productivity. Robust standard errors clustered at the state level are in parentheses. The dependent variable is the log difference of state total factor productivity. Panel A reports regressions using total credit flows, panel B reports regressions using long-term credit flows. Panels C and D report regressions using total and long-term credit flows with newly formed and dying firms excluded from the flow computations. Gross credit reallocation (SUM) is the credit flow in columns (1), (2), (7) and (8), excess credit reallocation (EXC) is the credit flow in columns (3), (4), (9) and (10), and net credit reallocation (NET) is the credit flow in columns (5), (6), (11) and (12). All regressions include state and year effects. Even columns include the labor shares of the various sectors in total non-farm employment (coefficients and standard errors are not reported to conserve space). *, **, and *** denote statistical significance at the 10, 5 and 1% level, respectively.

	Panel A: Total Credit						Panel B: Long-term Credit					
	SUM (1)	SUM (2)	EXC (3)	EXC (4)	NET (5)	NET (6)	SUM (7)	SUM (8)	EXC (9)	EXC (10)	NET (11)	NET (12)
SUM/EXC/NET	0.12*	0.16**	0.25*	0.29**	0.18	0.30	0.10*	0.14**	0.18*	0.20**	0.20	0.39
	(0.07)	(0.07)	(0.14)	(0.14)	(0.13)	(0.20)	(0.06)	(0.06)	(0.09)	(0.09)	(0.14)	(0.28)
Labor Shares	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Observations	990	990	990	990	990	990	990	990	990	990	990	990
	Panel C: Total Credit (Flows w/o Entry and Exit)						Panel D: Long-term Credit (Flows w/o Entry and Exit)					
	SUM (1)	SUM (2)	EXC (3)	EXC (4)	NET (5)	NET (6)	SUM (7)	SUM (8)	EXC (9)	EXC (10)	NET (11)	NET (12)
SUM/EXC/NET	0.13**	0.16**	0.27**	0.30**	0.23*	0.32*	0.11**	0.15**	0.17**	0.20**	0.21	0.34*
	(0.06)	(0.07)	(0.14)	(0.14)	(0.14)	(0.19)	(0.05)	(0.06)	(0.08)	(0.09)	(0.13)	(0.18)
Labor Shares	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Observations	989	989	989	989	989	989	989	989	989	989	989	989

Table 5

Credit market deregulation, credit reallocation, and productivity. Non-linearities

The table reports regression coefficients for the impact of deregulation on credit flows within states after accounting for possible non-linearities due to large credit changes (Panel A) or recessions (Panel B). Panel C reports regression coefficients of the second stage for the impact of large credit flows on the growth of state total factor productivity. Robust standard errors clustered at the state level are in parentheses. All regressions include state and year effects and the labor shares of the various sectors in total non-farm employment (coefficients and standard errors are not reported to conserve space). The dependent variables in Panels A and B are total credit reallocation (SUM) in column (1), excess total credit reallocation (EXC) in column (2), net total credit change (NET) in column (3), long-term credit reallocation (SUM) in column (4), excess long-term credit reallocation (EXC) in column (5), and net long-term credit change (NET) in column (6). In Panel A all these flows are constructed using large credit changes. Large credit change is defined at the firm level as credit creation or destruction of at least 18% in absolute value. In Panel B, recession is a dummy that takes the value of one for the six NBER recessions that occurred during the sample period, zero otherwise. In Panel C columns (1)-(3) report regressions using total credit flows and columns (4)-(6) report regressions using long-term credit flows. *, **, and *** denote statistical significance at the 10, 5 and 1% level, respectively.

Panel A	Large Credit Flows (Total Credit)			Large Credit Flows (Long-term Credit)		
	SUMbig (1)	EXCbig (2)	NETbig (3)	SUMbig (4)	EXCbig (5)	NETbig (6)
Interstate	6.67***	3.25***	3.44*	6.83***	4.10***	2.99*
	(2.00)	(1.09)	(1.81)	(2.22)	(1.29)	(1.56)
Number of observations	1416	1416	1416	1416	1416	1416
Panel B	Recessions (Total Credit)			Recessions (Long-term Credit)		
	SUM (1)	EXC (2)	NET (3)	SUM (4)	EXC (5)	NET (6)
Interstate	6.31***	3.30***	4.05*	7.51***	4.48***	4.00**
	(2.10)	(1.14)	(2.21)	(2.45)	(1.36)	(1.92)
Interstate x Recession	-3.67	-1.66	-4.67	-5.66*	-2.53	-7.16*
	(2.95)	(1.69)	(3.87)	(3.21)	(2.04)	(3.93)
Recession	5.60	-5.42**	17.14**	15.62	-1.81	16.99*
	(5.67)	(2.68)	(7.71)	(11.81)	(2.71)	(10.03)
Number of observations	1416	1416	1416	1416	1416	1416
Panel C	Large Flows (Total Credit)			Large Flows (Long-term Credit)		
	Productivity Growth			Productivity Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
SUMbig/EXCbig/NETbig	0.14**	0.27**	0.31	0.13**	0.20**	0.36
	(0.06)	(0.12)	(0.19)	(0.06)	(0.09)	(0.25)
Number of observations	990	990	990	990	990	990

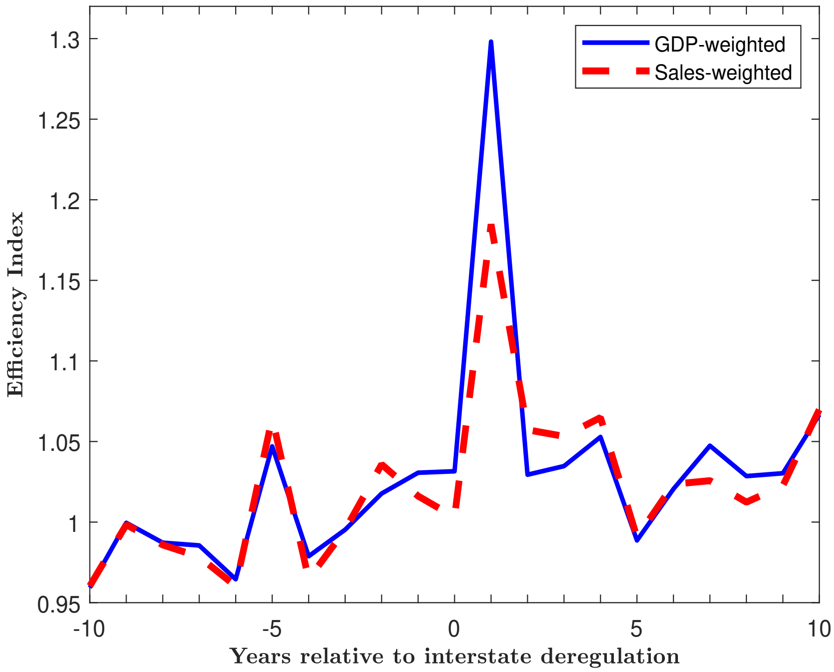

Figure 1: This figure plots the efficiency index from ten years before the interstate deregulation to ten years after the interstate deregulation. The efficiency index is defined in section 7.1 of the main text.

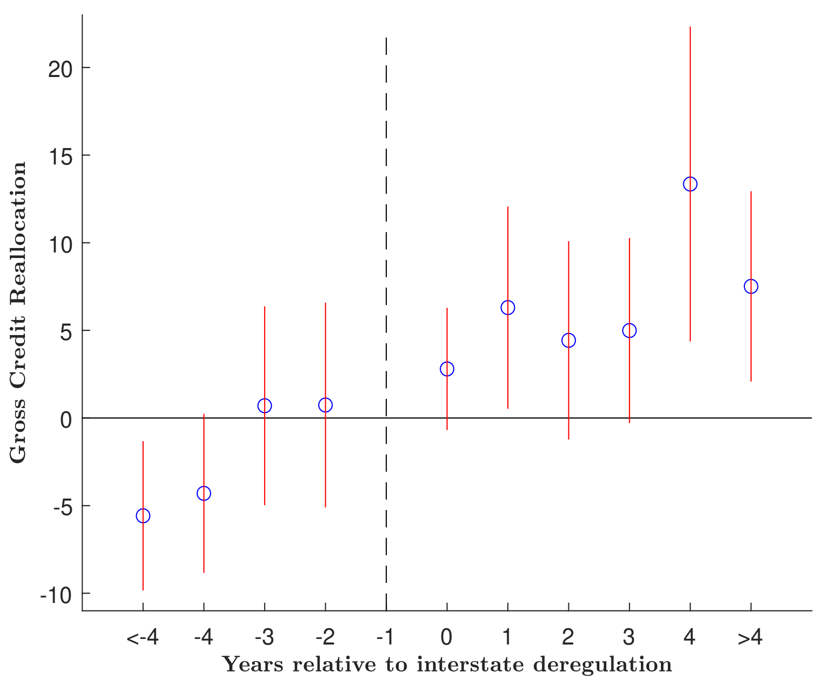

Figure 2: This figure plots the coefficients from the parallel trends event study specification, which replaces the interstate deregulation dummy with time dummies relative to the year of deregulation. 90% confidence intervals based on robust standard errors clustered at the state level are represented with the vertical lines.

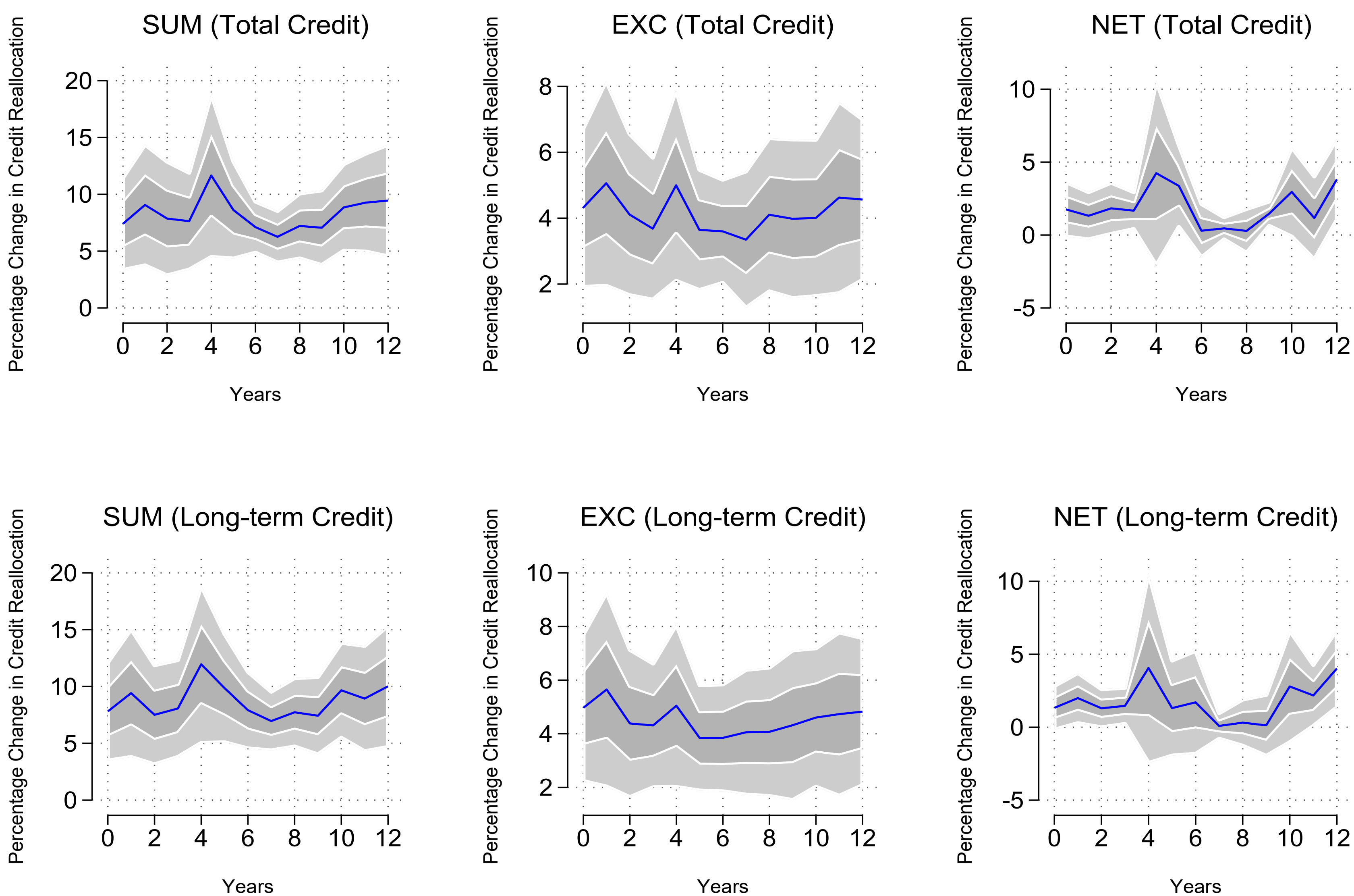

Figure 3: This figure plots impulse responses of the credit reallocation measures to the interstate liberalization. 95% (light gray) and 68% (dark gray) confidence bands are constructed from Driscoll and Kraay standard errors.