

The real effects of regulatory enforcement actions: Evidence from U.S. counties

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Abstract

We highlight an important macro-financial linkage: Regulatory enforcement actions on banks trigger significant adverse effects for the macroeconomy. Exploiting the fact that regulatory enforcement actions impose shocks on bank business activities beyond the control of bank executives, we use an instrumental variables setup to account for the non-random assignment of regulatory enforcement actions and document that severe actions such as Formal agreements and Cease and desist orders imposed on single-market banks reduce personal income growth in rural U.S. counties by 0.06 percentage points. These effects are causally related to contractions in bank lending and liquidity creation. Our identification is sharpened by a series of tests based on placebo enforcement actions.

Keywords: *bank distress, enforcement actions, local economic growth, macro-financial linkages*

JEL codes: *G21; G28; O43*

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Abstract

We highlight an important macro-financial linkage: Regulatory enforcement actions on banks trigger significant adverse effects for the macroeconomy. Exploiting the fact that regulatory enforcement actions impose shocks on bank business activities beyond the control of bank executives, we use an instrumental variables setup to account for the non-random assignment of regulatory enforcement actions and document that severe actions such as Formal agreements and Cease and desist orders imposed on single-market banks reduce personal income growth in rural U.S. counties by 0.06 percentage points. These effects are causally related to contractions in bank lending and liquidity creation. Our identification is sharpened by a series of tests based on placebo enforcement actions.

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Do shocks to bank business activities caused by regulatory behaviour affect the macroeconomy? And if so, how large is the effect? Answering these questions is key to understanding macro-financial linkages. Moreover, it is also central to understanding how banks react to the regulatory environment, a timely question of relevance for academics, policymakers, and the public alike against the background of far-reaching changes in banking regulation following the signing into law of the Dodd–Frank Wall Street Reform and Consumer Protection Act.

Empirically, establishing these effects is challenging because of well-known identification problems. Economic growth, unemployment, and other macroeconomic variables as well as bank lending, and bank health are endogenously determined. It is easy to observe that bank lending responds to economic conditions, and that bank health depends on the economic environment. Reverse causality issues are omnipresent.

In this paper, we use shocks imposed on bank business activities via severe regulatory enforcement actions such as Formal agreements, Prompt corrective actions, and Cease and desist orders issued by the Federal Deposit Insurance Corporation (FDIC), the Federal Reserve System (Fed), and the Office of the Comptroller of the Currency (OCC) to identify their effect on a set of macroeconomic variables in U.S. counties through lending and liquidity creation. We also establish the economic magnitude of these effects.

Our identification strategy focuses on enforcement actions imposed on banks that operate in geographically delimited markets, defined by county borders. The problems arising from reverse causality and the lack of random assignment of regulatory enforcement actions pose a serious econometric challenge. Since we do not have a natural experiment, identification of causal effects requires the use of an instrumental variables estimator. As detailed further below, we exploit plausibly exogenous variation in the one, two, and three year lagged differences of the assignment of less severe enforcement actions which exclusively relate to bank personnel such as civil money penalties, and suspension, removal, and prohibition orders for the identification strategy. These types of actions aim to deal with fraud or individual bank staff failing to fulfil fiduciary duties and therefore do not relate to banks' conduct. They are consequently unlikely to affect macroeconomic variables. It is also unlikely that these actions have ramifications for bank lending and liquidity creation. As an additional instrument, we use in the spirit of Arellano and Bond (1991) the lagged difference of the severe enforcement action. Consistent with the intuition that banks whose senior staff is subject to fines and other regulatory enforcement actions in previous years, we document a greater likelihood that those institutions are issued with severe enforcement actions such as Formal agreements, Prompt corrective actions, and Cease and desist orders.

To the extent that bank credit has no close substitutes as argued by Diamond (1984) and James (1987), we hypothesise that enforcement actions which affect the scope and scale of bank activities affect the macroeconomy.¹ This hypothesis reflects that enforcement actions typically reduce banks' ability to intermediate loans and deposits. Since banking markets are local in nature (Adams, Breevort, and Kiser, 2007), we focus on enforcement actions in single market banks that operate within clearly defined geographical boundaries because we expect the macroeconomic effects to be more pronounced there.

¹ Event studies suggest bank credit lacks substitutability. Slovin, Sushka, and Polonchek (1993) and Ongena, Smith, and Michalsen (2003) report adverse valuation effects for corporate borrowers when their banks experience distress. Their results indicate a reduction in bank durability affects borrower welfare.

A few studies examine direct effects of enforcement actions which can range from civil money penalties to restrictions on services such as deposit taking or provision of credit. Peek and Rosengren (1995, 1996) demonstrate that loan portfolios shrink, especially real estate loans, following enforcement actions in the U.S. Berger, Bouwman, Kick, and Schaeck (2012) document that regulatory interventions disrupt liquidity creation in Germany. However, this literature has so far not yet examined the real effects of enforcement actions beyond the micro level.

Our work is also related to the literature on the real effects of bank distress, and studies that analyse how credit and liquidity supply shocks to banks impact the real sector. Bernanke (1983) and Calomiris and Mason (2003) show loan supply shocks reduce local economic output, and Ashcraft (2005) suggests bank failures reduce county income. A growing literature focuses on how corporate investment and access to credit deteriorate when banks' liquidity supply contracts (Gibson, 1995; Peek and Rosengren, 1997, 2000; Kang and Stulz, 2005; Gan, 2007; Khwaja and Mian, 2008; Paravisini, 2008; Lemmon and Roberts, 2010; Chava and Purnanandam, 2011; Schnabl, 2012).

While the studies that hone in on the effects of enforcement actions suggest immediate effects on bank behaviour, the question whether these enforcement actions affect the real economy remains an empirical issue. To the extent they do, they have potential to give rise to unintended and possibly undesirable consequences for economic growth, suggesting a transmission mechanism by which regulatory behaviour has real economic impact. This is the subject of our research. We are not aware of any other study that identifies such effects.

To address this phenomenon, we combine data for 6,374 banks operating in 1,812 U.S. counties (10,918 county-year observations), with 879 regulatory enforcement actions, and macroeconomic variables for the period 1999 to 2011. In separate tests that explore the transmission channel through which enforcement actions affect growth, we additionally examine their effects on different lending categories and liquidity creation. The latter is based on a new measure of bank liquidity creation (Berger and Bouwman, 2009). The basic intuition of this measure is that banks create liquidity in the economic system by transforming illiquid assets into liquid liabilities.

Our main result suggests that severe enforcement actions which impose restrictions on bank activities such as deposit taking and origination of credit exert significantly negative effects on real per capita personal income growth, firm size (ln), the number of establishments per capita, and the unemployment rate on the county level. The effects are substantial in terms of their economic magnitude: Severe actions are associated with 0.06 and 0.07 percentage point reductions in personal income growth and firm size (ln), respectively, and the number of establishments also declines by 0.2 percentage points when regulators issue severe enforcement actions. The unemployment rate increases by 0.14 percentage points. These results are robust to the inclusion of control variables, and we also include county, and year fixed effects to consider demand effects. To sharpen causal inference, we also generate placebo enforcement actions which we find to have no effect on growth.

Our exploration of the transmission mechanism suggests that these macro-financial linkages are attributable to contractions in bank lending. In particular, we observe considerable reductions in consumer lending, in credit supply to the commercial and industrial sector, and commercial real estate lending also contracts. Since bank lending neither considers the full scale of banks' intermediation activities nor considers off balance sheet activities such as lines of credit and

guarantees, we additionally examine banks' liquidity creation. This analysis highlights that liquidity creation, especially on the asset side of the balance sheet, contracts even more in response to severe enforcement actions than does bank lending.

Section 1 discusses the institutional background. Section 2 describes the dataset, and offers a preliminary investigation of basic statistics and our identification strategy. We present results in Section 3, including robustness tests and extensions. Section 4 offers concluding remarks.

1. Institutional Background: Enforcement Actions

In pursuing the aim to maintain a safe and sound banking system, regulatory agencies (FDIC, Fed, and OCC) are charged with the supervision of financial intermediaries.

One of the key tools to achieve this aim are on-site examinations. In instances when these on-site examinations suggest unsafe, unsound, or illegal practices which violate laws, enforcement actions are used to restore safety and soundness by altering bank practices, stabilising the institution, and averting losses to the deposit insurer (Curry, O'Keefe, Coburn, and Montgomery, 1999). Typical reasons for the initiation of enforcement actions are management problems (poor loan administration, insufficient corporate planning, poor internal controls), and financial problems (inadequate capital and inadequate loan loss reserves, poor asset quality, clustering of loan portfolio risks, failure to charge off loan losses, poor liquidity, insider payments, failure to file with regulators). Noncompliance with enforcement actions can result in termination of deposit insurance. Since banks understand their asset choices determine regulatory closure rules, enforcement actions are likely to trigger changes in conduct (Mailath and Mester, 1994).

Several different enforcement actions exist. For our analyses, we group them together into *Less severe actions* and *Severe actions* based on their seriousness, disclosure requirements, whether they can be enforced in court, and based on whether they have potential to affect the scope and scale of bank activities (Curry et al., 1999; Ioannidou, 2005). Our grouping reflects both supervisory practice in the U.S., and also considers the Basel Core Principles for Effective Banking Supervision, issued in 2012. We now list the actions in ascending order.

The following types of actions are classified as *Less severe actions*.

Civil money penalties are imposed for violations of laws, regulations, Cease and desist orders, or Formal Agreements. They are publicly known. Typical penalties relate to violations of the Bank Secrecy Act, the Home Mortgage Disclosure Act, and to Call Report infractions. This form of enforcement action carries charges from \$1,000 to \$1 million per day, depending on the severity of the penalty.

Suspension, removal, and prohibition orders allow regulators to bar individuals from associating with a bank due to violation of laws, regulations, or other written agreements.² These actions are disclosed.

We consider the following enforcements as *Severe actions*, all of them are disclosed.

Formal (written) agreements are bilateral agreements between the bank and the regulator which set out details on how to correct conditions which are the basis for the agreement. This type of enforcement action is not followed by a federal court case verdict.

² Note that when illegal actions of individuals threaten the safety and soundness of the bank itself, a Cease and desist order or a Formal agreement will be issued against the institution as well (Ioannidou, 2005).

Cease and desist orders are issued following hearings. Unlike Formal agreements, they are imposed on the bank by the regulator. Cease and desist orders, can come in the form of restrictions on bank activities, e.g., on asset growth and the prohibition of asset disposals. Moreover, Cease and desist orders go beyond the restriction of activities and usually require remedial actions to correct violations of laws and improve safety and soundness of the institution. Unlike Formal agreements, they can be enforced in court.

Prompt corrective actions are imposed on undercapitalised banks. This action demands corrective measures to restore adequate levels of capital, and requires submission of a capital restoration plan within a predetermined time period. Depending on the level of undercapitalisation, Prompt corrective actions can trigger dismissals of senior executives, and carry restrictions on executive pay, asset growth, and prohibition of: acquisitions, establishing new branches, issuing new lines of credit, selling company shares, and disposing assets.

Deposit insurance threats are the most severe enforcement action before a bank is placed in receivership, which leads to termination of the banks' charter or sale to other investors.

Table 1 provides an overview about enforcement actions. In total, we observe 1,129 Less severe actions and 1,530 Severe actions. We record 744 Formal agreements. Cease and desist orders account for 911 observations, and there are 46 Prompt corrective actions (some of the banks received actions simultaneously). Regulators have not issued Deposit insurance threats during the sample period for single market banks.³

[TABLE 1: Time distribution of enforcement actions]

2. Data Description and Identification Strategy

We obtain Call Report data for commercial and savings banks in the U.S. from SNL Financial. This database also contains information about the timing and types of enforcement actions by regulators, branch location information, and deposit market shares from the Summary of Deposits from the FDIC. Our main regressions focus on the period 1999-2011, excluding the 2008-2009 crisis as we are interested in potentially unintended effects of regulatory enforcement actions during normal periods rather than during crises.

This analysis hones in on enforcement actions in banks that operate in only one small market, referred to as single-market banks, to allow a better demarcation of the boundaries of the relevant market for which we try to establish the real effects of enforcement actions. Our choice is predicated on the basis that where enforcement actions cause economic disruption, their real effects will be more pronounced within the county where the single-market bank operates, reflecting geographic market segmentation. This segmentation is due to low spatial mobility of bank customers and the information asymmetries inherent in lending relationships (Adams et al., 2007; Felici and Pagnini, 2008). Consequently, the natural unit of analysis is the county. We define a single-market county as a county that has at least one single-market bank in each year. Figure 1 illustrates that the majority of counties has at least one single-market bank and the number of counties with single-market banks is increasing over time. Figure 2 shows that there is no systematic clustering of counties in which regulatory enforcement actions took place.

³ All our tests exclude banks from Delaware and South Dakota. Delaware has about 20 times more incorporations than other U.S. states due to favourable legal treatment of incorporations, and South Dakota has a very large number of credit card banks incorporated resulting in a skewed distribution of measures of banking system structure there.

[FIGURE 1: Single-market banks]

[FIGURE 2: Enforcement actions in single-market banks]

2.1. Identification Strategy

The above mentioned endogeneity concerns between the macroeconomic environment and bank health, lending, and liquidity creation which arise primarily from the lack of random assignment of regulatory enforcement actions and the possibility of omitting time-varying, county-specific variables that may be coincident with the assignment of enforcement actions pose an identification problem: macroeconomic variables, bank behaviour and regulatory actions are jointly determined. Naïvely regressing macroeconomic variables and bank lending and liquidity creation on enforcement actions will yield biased coefficients on the variables for the enforcement actions because the error terms will be correlated with the dependent variable. This problem would render causal inference impossible because we do not observe the counterfactual. Even in the absence of actions by regulators, banks may recognize possible problems and alter their lending and liquidity creation. In the absence of a natural experiment, establishing causality therefore requires variables that explain regulatory enforcement actions but are neither correlated with the macroeconomic setting nor with bank behavior in terms of lending and liquidity creation. We therefore use instrumental variables estimators throughout this paper, and rely on a two stage least squares estimator. Specifically, we estimate the following equations.

$$EA_{it} = \alpha_i + \beta Z_{it} + \delta X_{it} + \gamma_i + \gamma_t + \varepsilon_{it}, \quad (1)$$

$$Y_{it} = a_i + \lambda EA_{it} + \delta X_{it} + \gamma_i + \gamma_t + \varepsilon_{it} \quad (2)$$

where EA_{it} denotes the regulatory enforcement action, represented by a dummy variable that takes on the value of one if a single-market bank was subject to an enforcement action in the county at time t (0 otherwise); Z_{it} is a set of instruments; X_{it} is a vector of control variables, defined in detail below; γ_i and γ_t are county (bank) and year dummies, respectively. The term ε_{it} is the error term. We estimate the first stage with a linear probability model.

Our regressions below first focus on the macroeconomic setting. For these tests, the dependent variable Y_{it} represents personal income growth deflated using the CPI, firm size (ln), the number of establishments, and the unemployment rate.⁴ For the subsequent tests on the bank level, the dependent variable represents bank lending, liquidity creation, and their respective components. There exist a number of difficult-to-observe variables which may affect both regulatory conduct and the macroeconomy over time (e.g., during times strain in the banking system regulators may be more prone to issue enforcement actions, urban counties may have higher growth rates). We capture these time-invariant omitted variables by including county dummies, γ_i , and business cycle fluctuations common to all counties are captured by year dummies γ_t .

Our vector of control variables X_{it} contains variables that can affect the macroeconomy and also matter for bank lending and liquidity creation. Demand effects clearly play a role. They are differenced out via our county (bank) fixed effects which net out differences in taxation across states that affect entrepreneurial activity, and we additionally use year fixed effects that take out the variation in demand conditions across the business cycle (Black and Strahan, 2002). The Z-score, defined as the ratio between a banks' return on assets and its capital ratio divided by the standard deviation of its return on assets, is included to consider bank soundness. This measure is

⁴ We obtain these variables from the Bureau of Economic Analysis, the Bureau of Labor Statistics, and the County Business Patterns database.

an accounting based measure of a bank's distance to default (Laeven and Levine, 2009). We use this variable because bank soundness is reflective of the location of a bank, in particular when dealing with small banks that operate in a geographically delimited area, and bank soundness is also highly correlated with the probability of regulatory enforcement actions (Berger, Bouwman, Kick, and Schaeck, 2010). Moreover, we use a Herfindahl-Hirschman Index (HHI) for deposit market shares to control for the structure in local banking markets, and – with the exception of the regressions where firm size (\ln) is the dependent variable - we also account for average firm size. Both variables are skewed and we therefore take the natural log. We use these two variables to control for the firm structure of the non-financial sector because counties with on average larger firms are likely to grow faster as these are typically high-productivity firms. Cetorelli and Strahan (2006) argue that competition in banking affects access to finance and consequently has ramifications on industry structure in the non-financial sector.⁵ Table 2 presents descriptive statistics.

[TABLE 2: Summary statistics]

2.2 *Instrumental variables*

Our identification strategy relies on four plausibly exogenous, yet simple instruments which vary across time and across our cross-sectional units. We argue that banks that were subject to less severe regulatory enforcement actions that extend exclusively to banks' personnel such as fines, civil money penalties, suspension, removal, and prohibition orders which bar individuals from associating with a bank due to violation of laws, regulations, or other written agreements are good precursors to more severe bank problems. We use the first, second, and third lags of the first-differences of a dummy variable for these less severe enforcement actions as instruments for the lag of the severe enforcement actions. We use lagged first-differences rather than levels of the dummy for the less severe enforcement actions to capture switches between states, which suggest an increase/decrease in the level of regulatory monitoring on the bank. Because the effect of an increase in monitoring can persist for several years, we employ three lags of the first-difference of less severe actions. Since these types of enforcement actions only relate to individuals, they are neither affected nor affect the economy as a whole, nor are such actions likely to trigger observable adjustments in bank behaviour. In this sense, they are good candidates to meet the identifying assumption for instruments. Moreover, while less severe actions may be more likely in counties with worse economic conditions (i.e., in poorer counties, banks' personnel may be more likely to breach regulations), it is unlikely that switches between states are related to short-term changes in local economic conditions. To provide further support to this claim, in Section 3.2 we provide further investigation of possible threats to the validity of this assumption. In addition, we also use the first lag of the difference of the severe enforcement action. This approach resembles the idea in Arellano and Bond (1991) of an 'internal instrument': The lagged difference of the dummy for severe enforcement actions is correlated with the lag of severe enforcement actions, satisfying the relevance condition, but is uncorrelated with the current value of the residuals (i.e., it is a pre-determined variable rather than an endogenous variable), satisfying the exclusion condition.

3. Main results

⁵ Claessens and Laeven (2004) show that concentration within the banking market has important implications on competition between financial institutions which in turn may affect the availability and cost of credit.

Table 3 presents our main results for the effect of severe regulatory enforcement actions on the macroeconomy using annual data. Our instrumental variables regressions cluster heteroskedasticity-adjusted standard errors on the county level to correct for serial correlation within counties (Bertrand, Duflo, and Mullainathan, 2004). We include counties where regulators impose actions on multiple banks in the same year, although our results are not affected when we exclude these observations (as shown in Appendix B). All regressions on the county level include county and year fixed effects, and our regressions on the bank level include bank and year fixed effects. For three out of four of these macrovariables, the results remain unchanged when the 2008-2009 crisis period is included (as shown in Appendix C).

[TABLE 3: Main results - The macroeconomic effects of regulatory enforcement actions]

Our discussion focuses first on the results from the second stage regressions. There is clear evidence that severe regulatory enforcement actions disrupt the real economy. Personal income growth, firm size (ln), and the number of establishments per capita are all significantly reduced, and the unemployment rate increases in response to severe actions by regulators.⁶ The economic magnitude of these effects is substantial against an average growth rate of personal income of 1.7%. Severe enforcement actions reduce growth by 0.06 percentage points. Given an average number of 2.4 establishments per 100 people and an average unemployment rate of 6.1%, the coefficients indicate that severe actions reduce the former rate by 0.022 percentage points, and increase the latter rate by 0.142 percentage points. Given an average firm size (ln) of 2.386, the coefficients indicate that severe actions reduce firm size (ln) by 0.069; equivalent to one employee or a 10% reduction in the average firm's workforce. The control variables, if significant, display intuitive signs.

Next, we discuss the results for the first stage.⁷ The coefficients of all instruments enter at conventional levels of significance. The lags of the differences of the less severe enforcement actions are all positive, and so is the lagged difference of the severe enforcement actions dummy. The control variables also display intuitive signs.

To verify the choice of our instruments, we investigate several diagnostics. These tests reject underidentification of our models, and the Kleibergen Paap F-tests for weak identification likewise do not suggest that we suffer from a problem of weak instruments. The Hansen J-tests for the correlation between the residuals and the instruments indicates the exogeneity of our instruments. In addition, we also present C-tests (or difference-in-Hansen tests) to test for the exogeneity of each of the instruments. This test is defined as the difference of the Hansen statistic of the equation with the smaller set of instruments and the equation including the instrument whose validity we want to test. Under the null hypothesis that both the smaller set of instruments and the suspect instrument are valid, the C-statistic follows a Chi-squared distribution. The results for these tests show that our instruments are valid.

[TABLE 4: Main results - The macroeconomic effects of different types of enforcement actions]

In Table 4, we turn to the role of different types of enforcement actions. Specifically, we present the coefficients for the effects of Formal agreements, Prompt corrective actions, and Cease and desist orders. These regressions highlight that the magnitude of the effect on the macroeconomic environment depends on the type of enforcement action. Prompt corrective actions, despite being the type of action with the lowest frequency (46 instances) in our dataset,

⁶ We lose 6 observations in the regressions for the unemployment rate because data for these counties are missing.

⁷ Note that the first stage regression for Personal income growth and the number of establishments are identical.

consistently display the largest effect, followed by Formal agreements, and Cease and desist orders. This pattern persists irrespective of whether we examine personal income growth, firm size, the number of establishments per capita, or the unemployment rate. To illustrate, while a Prompt corrective action in a single market bank triggers a reduction in personal income growth in the average county of 0.20 percentage points, Formal agreements result in declines of only 0.012 percentage points, and Cease and desist orders correlate with 0.01 percentage point reductions, respectively. These results are not surprising. Prompt corrective actions are the strongest types of enforcement actions, only occur rarely during the sample period, and ‘hit’ banks the most. The stronger effect of Formal agreements in comparison to Cease and desist orders is also intuitive. Formal agreements are issued with the consent of the institution, suggesting a strong commitment of the bank to address the problems and respond quickly. Banks have therefore a strong incentive to avoid public attention arising from Cease and desist orders and are keen to portray themselves as adjusting behaviour in line with mutual agreements with the regulator rather than being subject to Cease and desist orders. Moreover, Formal agreements tend to occur prior to Cease and desist orders, and many problem banks are examined at a high frequency which results in additional enforcement actions following Formal agreements. Thus, these two coefficients may pick up compounding effects. While Formal agreements are bilaterally agreed and are the first step that triggers changes in conduct, Cease and desist orders may follow Formal agreements and therefore have a lesser impact on bank behaviour.

A quick inspection of the diagnostics for the instrumental variables reinforces the choice of our instruments for Formal agreements and Cease and desist orders. Only for Prompt corrective actions the test statistics point towards a problem of weak instruments. The Hansen J-test also displays weak significance at the ten % level. This problem is due to the very low number of these actions.

[TABLE 5: Mechanism: The effects of enforcement actions on bank lending and liquidity creation]

3.1 Mechanism: Bank lending and liquidity creation

What might underlie these findings? Two key candidates that have potential to trigger macroeconomic contractions are bank lending and liquidity creation.

The latter measure is based on Berger and Bouwman (2009), who propose a three-step procedure to compute bank liquidity creation. First, using information on the category and maturity of banks’ assets and liabilities, we classify bank assets, liabilities and equity as liquid, semi-liquid, or illiquid depending on ease, cost, and time it takes customers to obtain liquid funds from the bank in case of liability items, and based on ease, cost and time with which banks can dispose of their obligations in the case of assets. Second, we next assign weights of either $+\frac{1}{2}$, 0, or $-\frac{1}{2}$ to all bank activities classified in the first step. The weights correspond to liquidity creation theory, which asserts that banks create liquidity by converting illiquid assets into liquid liabilities, whilst by transforming liquid assets into illiquid liabilities or equity banks destroy liquidity. In the final step, we calculate how much liquidity each bank creates by combining and multiplying the activities classified in step 1 with the weights from step 2.

Our measure of liquidity creation is the preferred measure from Berger and Bouwman (2009) which classifies all activities other than loans by both product category and maturity while loans are only classified based on category due to data availability constraints. We also include off-balance sheet items, so that our measure of liquidity creation is identical to the measure termed ‘cat fat’ by Berger and Bouwman (2009).

The additional tests in Table 5 report results for these possible mechanisms through which the severe actions by regulators might affect the macroeconomy. We first analyze bank lending and then liquidity creation which we compute in the style of Berger and Bouwman (2009).

To this end, we run instrumental variable regressions on the bank level for 6,374 banks and use the same exogenous instruments and control variables as in the tests above.

Table 5 highlights that total loan growth contracts substantially, it decreases by 9.8 percentage points in response to severe actions. A question that naturally arises then is whether all lending categories are affected similarly or whether banks adjust their lending behaviour conditional on the types of borrowers when they decide to cut back their lending.⁸ The intuition is that loan categories reflect differences in risk choices and the most risky lending activities are likely to be most affected. To understand whether banks react in an intuitive manner, the next four columns show regressions with Corporate real estate loan growth, Residential real estate loan growth, Commercial and industrial loan growth, and with Consumer loan growth. The most standardised and (Consumer lending), and the most risky lending activities (Commercial and industrial lending) are affected most prominently. The former contracts by 11.4 percentage points and the latter is reduced by 11.2 percentage points. Corporate lending activities are reduced by 9.5 percentage points. In contrast, residential lending only contracts by 5.3 percentage points.

While contractions in bank lending appear to be a prime suspect, it only captures bank activities incompletely because off-balance sheet activities and lines of credit that are typically drawn down during crisis are omitted when we examine bank lending activities (Ivashina and Scharfstein, 2010). If bank lending contracts in response to severe regulatory actions, the more comprehensive measure which captures the intermediation activities more comprehensively should not only also contract but the magnitude of the effect should be even more pronounced. We consequently also examine banks' liquidity creation.

Indeed, Table 5 illustrates that only considering bank lending behavior underestimates the effects of regulatory enforcement actions. Banks reduce their growth in liquidity creation by 13.7 percentage points when regulators issue a severe action against them. Our subsequent decomposition into growth in liquidity creation on the asset side, on the liability side, and off the balance sheet indicates that the key driver behind this economically large effect is a contraction in liquidity creation on the asset side of the balance sheet with a large magnitude of minus 22.5 percentage points. The coefficient for liquidity creation on the liability side is relatively small with 6.4 percentage points, and liquidity creation off the balance sheet does not seem to be affected.

Among the control variables, we find that bank soundness correlates positively with both more lending and more liquidity creation. More concentrated markets have a positive effect on growth in banks' liquidity creation, and firm size also displays a significantly positive sign for growth in bank liquidity creation off the balance sheet.

The first stage results, reported at the bottom of Table 5, confirm that the lagged differences of less severe enforcement actions and also the lagged difference of severe enforcement actions are significantly affecting the probability of severe regulatory actions. The diagnostics for the instruments confirm that our instruments are strong, and with the exception of the Hansen J-test

⁸ Appendix A provides an overview about the distribution of bank lending activities across different loan categories broken down by the type of borrowers.

in the regressions for Corporate real estate loans and Liquidity creation growth where it is significant at the 10 % level they are also exogenous.

3.2 Demand issues and spill-over effects

We now deal with a key concern in our paper that our effects are due to demand effects rather than causally related to severe actions by bank regulators. In other words, (lack of) economic growth is not the driving force behind the regulatory enforcement actions. To this end, we omit the worst performing counties in our sample, defined as counties where personal income growth contracts over two consecutive years prior to the severe action.

Table 6 presents the results. The dummy variable for the severe regulatory action in the first column as well as the tests of Formal agreements and Cease and desist orders all yield statistically significant reductions in personal income growth. The dummy for Prompt corrective actions also retains its negative sign but it is rendered insignificant in this test. Importantly, the magnitudes of the coefficients do not shrink in magnitude. Instead, they either retain their magnitude (for severe regulatory actions), or even increase in the case of Formal agreements, Prompt corrective actions, and Cease and desist orders.

As said in Section 2.2, one may argue that using variables related to less severe actions as instruments may invalidate the exclusion restriction, since the regulator may be more likely to issue this type of actions in counties with bad economic conditions. To rule out the possibility that the probability of less severe actions increases as economic growth deteriorates, we run conditional logit models (with county and year fixed effects) using the dummy for the less severe regulatory action as the dependent variable, and the following dummies as main explanatory variables:

- $PIG(-)$ takes on the value one if there is a contraction in PIG for a given county in year t , and zero otherwise;
- $NE(-)$ takes on the value one if there is a contraction in the number of establishments per capita for a given county in year t , and zero otherwise;
- $FS(-)$ takes on the value one if there is a contraction in the log of firm size for a given county in year t , and zero otherwise.
- Finally, $UR(+)$ takes on the value one if there is an increase in the unemployment rate for a given county in year t , and zero otherwise.

The results reported in Table 6 suggest that short-term changes in macroeconomic conditions at the county level bear no significant relation with the probability of a less severe action. This result lends further support to the validity of our identification strategy.

We offer two additional analyses in Table 6 based on placebo tests. We first run a placebo test to examine spill over effects of severe enforcement actions on personal income growth of counties that are contiguous to those counties where regulators issued a severe actions to a bank. Specifically, we generate 200 randomly assigned placebo enforcement actions for year t for counties where we are sure that no enforcement action was issued during the sample period but these counties share a common border with the county where a bank was subject to such regulatory actions in year t . Table 6 presents the rejection rates at 1, 5, and 10 % levels for these tests. This exercise indicates only very limited evidence that the enforcement action spill over into neighbouring counties because the rejection rates remain very low. Our final test takes these placebo tests to the bank level. Here, we randomly assign 200 placebo enforcement actions to banks which did not receive enforcement actions located in the same county where a bank was

subject to such regulatory action in year t , and we again present the rejection rates. These falsification tests with very low rejection rates reinforce our belief that our key inferences are not due to chance but rather are attributable to the actions taken by regulators against banks.

[TABLE 6: Robustness tests: Demand issues and spillover effects]

4. Concluding Remarks

The key result in this paper suggests that regulatory enforcement actions on banks trigger adverse effects for the real economy.

Using a careful instrumental variables estimation strategy to account for the non-random assignment of regulatory enforcement actions, we robustly document that Severe enforcement actions such as Formal agreements and Cease and desist orders imposed on single-market banks reduce personal income growth rates by 0.06 percentage points, firm size (\ln) by 0.07 percentage points, the number of establishments by 0.02 percentage points, and the unemployment rate increases by 0.14 percentage points, respectively. With growth rates averaging 1.7% between 1999 and 2011 (excluding the 2008-2009 recession) on the county level in the U.S., these effects are noteworthy in terms of their economic magnitude. Our more detailed analyses illustrate that the reduction in growth is brought about by declines in bank lending, in particular standardised consumer lending and the most risky type of lending to the commercial and industrial sector. We also document contractions in bank liquidity creation in response to these regulatory actions. In short, these enforcement actions constitute shocks to banks' intermediation activities that spill over into the real economy.

Our research builds on a quickly evolving body of literature that hones in on the potentially unintended effects of regulatory enforcement actions on distressed banks for the real economy. Unlike other studies that limit their analyses to the effects of enforcement actions to the micro level and focus exclusively on bank behaviour, the main innovation in our article is that we are the first to show that local economic growth is causally affected by these regulatory actions. Placebo tests show that our results are unlikely to be driven by spillovers across banks or counties, and additional tests rule out the possibility that our results be driven by local recessions causing enforcement actions. Our empirical framework, therefore, is uniquely suited to highlight this important macro-financial linkage: While enforcement actions are undoubtedly important to restore bank health, they also have potential to trigger considerable adverse effects for the immediate macroeconomic environment.

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Appendix A: Summary Statistics for Loan Categories

Year	Corporate Real Estate Loans	Residential Real Estate Loans	C&I Loans	Consumer Loans
	(% of total loans)	(% of total loans)	(% of total loans)	(% of total loans)
1999	0.156	0.308	0.162	0.147
2000	0.166	0.303	0.166	0.139
2001	0.173	0.303	0.166	0.133
2002	0.184	0.298	0.162	0.124
2003	0.199	0.295	0.157	0.114
2004	0.212	0.287	0.154	0.105
2005	0.217	0.284	0.151	0.095
2006	0.221	0.275	0.149	0.088
2007	0.222	0.267	0.149	0.082
2008	0.223	0.261	0.150	0.078
2009	0.237	0.268	0.146	0.072
2010	0.252	0.277	0.139	0.070
2011	0.260	0.281	0.137	0.067
Average	0.208	0.286	0.153	0.102

Notes. This table presents statistics for the distribution of lending. Types of loans are scaled by total loans in a given year. Total loans is the sum of loans to agricultural production, commercial and industrial loans, construction loans, real estate loans (both commercial and residential), and loans to other depository institutions.

Appendix B: Additional Robustness Test – Counties with multiple interventions in the same year excluded.

Dependent variable(s)	Second stage			
	<i>Personal income growth</i>	<i>Firm size</i>	<i>Establishments per capita</i>	<i>Unemployment rate</i>
L.Severe actions	-0.007** (-2.477)	-0.065*** (-3.128)	-0.021*** (-2.922)	0.121 (1.302)
L.ln(Z-score)	0.001*** (3.338)	0.011*** (3.041)	0.004* (1.651)	-0.118*** (-4.497)
L.ln(HHI)	0.004 (1.572)	0.034 (1.141)	0.026* (1.897)	-0.208 (-1.477)
L.ln(Firm size)	0.003* (1.839)		0.026*** (4.762)	-0.541*** (-7.587)
Year FE	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Observations	10,668	10,668	10,668	10,662
R-squared	0.063	0.036	0.330	0.723
Counties	1,806	1,806	1,806	1,806
Under-identification	261.0	260.6	261.0	261.3
Weak-identification	536.0	529.9	536.0	536.2
Hansen J-test	0.636	0.992	1.823	4.082
p-value (Hansen)	0.888	0.803	0.610	0.253

Appendix C: Additional Robustness Test – 2008-2009 crisis period included.

Dependent variable(s)	Second stage			
	<i>Personal income growth</i>	<i>Firm size</i>	<i>Establishments per capita</i>	<i>Unemployment rate</i>
L.Severe actions	-0.003 (-1.395)	-0.042** (-2.535)	-0.021*** (-4.086)	0.171*** (2.625)
L.ln(Z-score)	0.001*** (3.596)	0.004 (1.392)	0.002 (1.231)	-0.123*** (-4.345)
L.ln(HHI)	0.005** (1.966)	0.039* (1.659)	0.023** (1.971)	-0.159 (-1.107)
L.ln(Firm size)	0.003* (1.754)		0.022*** (4.409)	-0.522*** (-7.631)
<i>Crisis Dummy</i>	-0.048*** (-31.368)	-0.138*** (-12.448)	0.055*** (16.867)	0.487*** (9.904)
Year FE	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Observations	10,668	10,668	10,668	10,662
R-squared	0.063	0.036	0.330	0.723
Counties	1,806	1,806	1,806	1,806
Under-identification	261.0	260.6	261.0	261.3
Weak-identification	536.0	529.9	536.0	536.2
Hansen J-test	0.636	0.992	1.823	4.082
p-value (Hansen)	0.888	0.803	0.610	0.253

Table 1*Descriptive statistics for enforcement actions*

Descriptive statistics for enforcement actions

Panel A: Time distribution of enforcement actions in U.S. counties						
Year	Any action	Severe actions	Less severe actions	Breakdown of Severe actions		
				Formal agreements	Cease and desist orders	Prompt corrective actions
1999	121	66	55	31	35	3
2000	159	79	80	44	38	3
2001	169	91	78	53	45	3
2002	185	109	76	64	54	2
2003	196	117	79	60	66	2
2004	201	117	84	60	65	1
2005	201	104	97	55	53	1
2006	189	86	103	49	39	0
2007	179	78	101	37	47	0
2008	198	100	98	45	62	0
2009	275	174	101	76	120	8
2010	369	259	110	114	181	19
2011	217	150	67	56	106	4
Total	2659	1530	1129	744	911	46
Panel B: Correlations between enforcement actions						
	Severe actions	Less severe actions	Formal agreements	Cease and desist orders	Prompt corrective actions	
Severe actions	1					
Less severe actions	0.211*** (0.00)	1				
Formal agreements	0.690*** (0.00)	0.145*** (0.00)	1			
Cease and desist orders	0.765*** (0.00)	0.192*** (0.00)	0.145*** (0.00)	1		
Prompt corrective actions	0.170*** (0.00)	0.070*** (0.59)	0.104*** (0.00)	0.143*** (0.00)	1	

Notes. Panel A presents the number of enforcement actions issued by bank regulators in U.S. counties in the years 1999-2011. We report the total number of enforcement actions (Severe and Less severe actions), the number of Severe actions (Formal agreements, Cease and desist orders, and Prompt corrective actions), and the number of Less severe actions. This latter category consists of Actions against personnel and individuals, and other Civil money fines. During our sample period, we observe no single Deposit insurance threat in single-market banks. Deposit insurance threats would also be classified as Severe action. Enforcement actions in single market banks in Delaware and South Dakota states are excluded. Panel B presents a correlation matrix for the different types of enforcement actions. *** p<0.01, ** p<0.05, * p<0.1.

Table 2*Summary statistics*

Variable	Observations	Mean	S.D.	Min	Max	Source
<i>Dependent macroeconomic variables</i>						
Real per capita personal income growth	16904	0.017	0.035	-0.077	0.126	U.S. Bureau of Economic Analysis
Firm Size (ln)	16904	2.483	0.557	-1.386	5.432	County Business Patterns database
Establishments per capita (in %)	16904	2.386	0.863	0.419	12.203	County Business Patterns database
Unemployment rate (in %)	16890	6.077	2.522	1.500	29.90	Bureau of Labor Statistics
<i>Dependent bank-level variables</i>						
Total loan growth	49242	0.076	0.195	-0.308	0.966	SNL Financial, authors' calculation
Corporate real estate loan growth	48263	0.156	0.422	-0.468	2.176	SNL Financial, authors' calculation
Residential real estate loan growth	48790	0.068	0.269	-0.412	1.325	SNL Financial, authors' calculation
Commercial and industrial loan growth	48294	0.095	0.361	-0.538	1.600	SNL Financial, authors' calculation
Consumer loan growth	48906	-0.007	0.283	-0.541	1.320	SNL Financial, authors' calculation
Liquidity creation growth	43324	0.110	0.396	-1.000	1.666	SNL Financial, authors' calculation
Liquidity creation growth (asset side)	42932	0.091	1.167	-4.317	4.592	SNL Financial, authors' calculation
Liquidity creation growth (liability side)	42931	0.096	0.226	-0.335	1.192	SNL Financial, authors' calculation
Liquidity creation growth (off balance)	42744	0.166	0.416	-0.559	1.794	SNL Financial, authors' calculation
<i>Interventions(county-level regressions)</i>						
Severe actions	19487	0.046	0.209	0	1	SNL Financial, authors' calculation
Less severe actions	19487	0.033	0.178	0	1	SNL Financial, authors' calculation
Formal agreements	19487	0.021	0.145	0	1	SNL Financial
Cease and desist orders	19487	0.001	0.037	0	1	SNL Financial
Prompt corrective actions	19487	0.028	0.166	0	1	SNL Financial
<i>Interventions(bank-level regressions)</i>						
Severe actions	53126	0.022	0.147	0	1	SNL Financial
Less severe actions	53126	0.015	0.120	0	1	SNL Financial
Formal agreements	53126	0.010	0.097	0	1	SNL Financial
Cease and desist orders	53126	0.000	0.019	0	1	SNL Financial
Prompt corrective actions	53126	0.013	0.115	0	1	SNL Financial
<i>Control variables</i>						
HHI deposits (ln)	16904	-1.289	0.519	-3.157	0.000	SNL Financial, authors' calculation
Firm size (ln)	16904	2.483	0.557	-1.386	5.432	Bureau of Labor Statistics
Z-score (ln)	16904	4.562	0.981	0.459	37.882	Authors' calculation
Z-score (ln) bank-level	52280	4.134	1.149	-5.426	38.981	Authors' calculation

Notes. The table presents summary statistics, means, standard deviations, minima, maxima and the data sources. Sample period: 1999-2011 (2008-2009 excluded).

Table 3*Main results: The macroeconomic effects of regulatory enforcement actions*

Dependent variable(s)	Second stage				First stage		
	<i>Personal income growth</i>	<i>Firm Size (ln)</i>	<i>Establishments per capita</i>	<i>Unemployment rate</i>	<i>Personal income growth</i> <i>Establishments per capita</i>	<i>Unemployment rate</i>	<i>Firm Size (ln)</i>
LD.Less Severe actions					0.035** (2.524)	0.035** (2.523)	0.035** (2.523)
L2D.Less severe actions					0.036** (2.533)	0.036** (2.532)	0.036** (2.547)
L3D.Less severe actions					0.056*** (3.539)	0.056*** (3.540)	0.057*** (3.552)
LD.Severe actions					0.576*** (51.922)	0.576*** (51.916)	0.577*** (51.590)
L.Severe actions	-0.006** (-2.419)	-0.069*** (-3.696)	-0.022*** (-3.361)	0.142* (1.720)	<i>Dependent Variable</i>	<i>Dependent Variable</i>	<i>Dependent Variable</i>
L.ln(Z-score)	0.001*** (3.494)	0.011*** (2.957)	0.004* (1.734)	-0.124*** (-4.574)	-0.007*** (-2.682)	-0.007*** (-2.672)	-0.007*** (-2.688)
L.ln(HHI)	0.004 (1.605)	0.046 (1.559)	0.028** (2.165)	-0.216 (-1.394)	-0.048** (-2.067)	-0.049** (-2.101)	0.050** (-2.122)
L.ln(Firm size)	0.003* (1.886)		0.026*** (4.801)	-0.528*** (-7.530)	-0.038*** (-3.298)	-0.038*** (-3.293)	
Year FE	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES
Observations	10,918	10,918	10,918	10,912	10,918	10,912	10,918
R-squared	0.063	0.033	0.334	0.727	0.357	0.358	0.356
Counties	1,812	1,812	1,812	1,812	1,812	1,812	1,812
Number of actions	879	879	879	877			
Under-identification	298.1	297.1	298.1	298.4			
Weak-identification	687.5	678.3	687.5	687.4			
Hansen J-test	0.921	0.797	2.508	2.602			
p-value (Hansen)	0.820	0.850	0.474	0.457			
<i>C-test (p-values)</i>							
LD.Less Severe actions	0.885	0.345	0.140	0.180			
L2D.Less Severe actions	0.656	0.742	0.863	0.503			
L3D.Less Severe actions	0.480	0.506	0.634	0.170			
LD.Severe actions	0.715	0.289	0.182	0.251			

Notes. This table presents results of IV regressions of enforcement actions in single-market banks on per capita personal income growth on the county level, total loan growth, and liquidity creation growth. Enforcement actions include Severe actions (dummy variable equal to 1 if Formal agreements, Cease and desist order, and/or Prompt corrective action is observed and zero otherwise); Less severe action is a dummy variable equal to 1 if enforcement actions against Personnel and individuals, and other Civil money fines are observed, or zero otherwise). Our regressions control for concentration of the local banking market, measured by a county-level deposit-based Herfindahl-Hirschman Index (HHI), average firm size in the county (Firm size). Delaware and South Dakota counties excluded. We report robust t-statistics in parentheses, and errors are clustered on the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4

Main results: The macroeconomic effects of different types of enforcement actions

Second stage												
Dependent variable	<i>Personal income growth</i>			<i>Firm size (ln)</i>			<i>Establishments per capita</i>			<i>Unemployment</i>		
L.Formal agreements	-0.012**			-0.150***			-0.047***			0.321*		
	(-2.408)			(-3.646)			(-3.287)			(1.777)		
L.PCA		-0.200*			-2.456**			-0.474			7.038*	
		(-1.688)			(-2.170)			(-1.515)			(1.789)	
L.Cease & desist orders			-0.010**			-0.124***			-0.038***			0.261*
			(-2.395)			(-3.676)			(-3.231)			(1.780)
L.ln(Z-score)	0.002***	0.002***	0.001***	0.011***	0.011***	0.011***	0.005*	0.005*	0.004*	-0.124***	-0.124***	-0.123***
	(3.533)	(3.547)	(3.412)	(2.959)	(2.848)	(2.935)	(1.743)	(1.880)	(1.715)	(-4.574)	(-4.544)	(-4.570)
L.ln(HHI)	0.004*	0.005*	0.004	0.048*	0.061*	0.044	0.029**	0.033**	0.027**	-0.217	-0.238*	-0.211
	(1.688)	(1.840)	(1.546)	(1.658)	(1.655)	(1.511)	(2.216)	(2.437)	(2.126)	(-1.418)	(-1.726)	(-1.355)
L.ln(Firm size)	0.003*	0.001	0.003*				0.025***	0.023***	0.026***	-0.523***	-0.454***	-0.527***
	(1.764)	(0.634)	(1.856)				(4.636)	(3.473)	(4.782)	(-7.441)	(-5.516)	(-7.521)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,912	10,912	10,912
Counties	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812
Number of actions	439	30	525	439	30	525	439	30	525	439	30	523
Under-identification	140.1	8.481	171.2	140.5	8.840	171.1	140.1	8.481	171.2	140.2	8.482	171.5
Weak-identification	61.11	2.130	84.75	61.52	2.222	84.95	61.11	2.130	84.75	61.14	2.130	84.84
Hansen J-test	0.957	2.086	0.877	0.706	1.346	0.552	2.771	6.880	2.929	2.372	0.449	2.390
P-value (Hansen)	0.812	0.555	0.831	0.872	0.718	0.907	0.428	0.076	0.403	0.499	0.930	0.495
<i>C-test (p-values)</i>												
LD.Less severe actions	0.857	0.219	0.802	0.381	0.633	0.391	0.110	0.009	0.111	0.203	0.881	0.204
L2D.Less severe actions	0.622	0.709	0.713	0.692	0.621	0.766	0.957	0.797	0.855	0.555	0.997	0.497
L3D.Less severe actions	0.499	0.737	0.527	0.544	0.940	0.543	0.572	0.533	0.565	0.187	0.544	0.184
LD.Severe actions	0.779	0.295	0.822	0.300	0.685	0.330	0.135	0.010	0.131	0.158	0.980	0.260
First stage												
LD.Less severe actions	0.022*	0.009*	0.033**	0.022*	0.009	0.033**	0.022*	0.009*	0.033**	0.022*	0.009*	0.033**
	(1.919)	(1.664)	(2.416)	(1.914)	(1.661)	(2.451)	(1.919)	(1.664)	(2.416)	(1.918)	(1.664)	(2.415)
L2D.Less severe actions	0.011	0.002	0.029**	0.011	0.002	0.029**	0.011	0.002	0.029**	0.011	0.002	0.029**
	(1.030)	(0.668)	(2.267)	(1.045)	(0.685)	(2.277)	(1.030)	(0.668)	(2.267)	(1.03)	(0.668)	(2.267)
L3D.Less severe actions	0.031**	0.006	0.042***	0.031**	0.006	0.042***	0.031**	0.006	0.042***	0.031**	0.006	0.042***
	(2.280)	(1.103)	(2.841)	(2.290)	(1.109)	(2.846)	(2.280)	(1.103)	(2.841)	(2.28)	(1.103)	(2.841)
LD.Severe actions	0.265***	0.011**	0.320***	0.267***	0.011**	0.322***	0.265***	0.011**	0.320***	0.265***	0.011**	0.321***
	(15.476)	(2.284)	(18.167)	(15.526)	(2.387)	(18.194)	(15.476)	(2.284)	(18.167)	(15.479)	(2.283)	(18.176)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.142	0.015	0.196	0.140	0.013	0.195	0.142	0.015	0.196	0.142	0.015	0.197
Observations	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,918	10,912	10,912	10,912
Counties	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812	1,812

Notes. This table presents results of IV regressions of placebo enforcement actions in single-market banks on per capita personal income growth on the county level, total loan growth, and liquidity creation growth. We create placebo treatments by assuming that the enforcement actions occurred two years prior to their actual occurrence, their coding follows the exposition reported above in the notes to Table 3. All control variables reported in Table 3 are also included. Delaware and South Dakota counties excluded. We report robust t-statistics in parentheses, and errors are clustered on the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5

Mechanism: The effects of enforcement actions on bank lending and liquidity creation

Second stage									
Dependent variable	<i>Bank lending</i>					<i>Bank liquidity creation</i>			
	<i>Total loan growth</i>	<i>Corporate real estate loan growth</i>	<i>Residential real estate loan growth</i>	<i>Commercial and industrial loan growth</i>	<i>Consumer loan growth</i>	<i>Liquidity creation growth</i>	<i>Liquidity creation growth (asset side)</i>	<i>Liquidity creation growth (liability side)</i>	<i>Liquidity creation growth (off balance)</i>
L.Severe actions	-0.098*** (-5.566)	-0.095** (-2.534)	-0.053** (-2.143)	-0.112*** (-3.715)	-0.114*** (-3.910)	-0.137*** (-3.928)	-0.225** (-2.041)	-0.064*** (-2.838)	-0.050 (-1.218)
L.ln(Z-score)	0.012*** (7.914)	0.006** (2.299)	0.004** (2.370)	0.014*** (5.543)	0.007*** (3.239)	0.015*** (5.679)	0.027*** (3.357)	0.011*** (6.653)	0.016*** (5.245)
L.ln(HHI)	0.016 (1.630)	0.005 (0.242)	0.001 (0.045)	0.025 (1.507)	-0.022 (-1.544)	0.032* (1.790)	0.005 (0.097)	0.015 (1.337)	0.050*** (2.593)
L.ln(Firm Size)	0.005 (1.067)	-0.001 (-0.067)	0.001 (0.219)	-0.006 (-0.681)	-0.006 (-0.966)	-0.004 (-0.396)	-0.019 (-0.526)	0.007 (1.225)	0.026** (2.040)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,870	37,870	37,870	37,870	37,870	37,870	37,870	37,870	37,870
Banks	6,374	6,374	6,374	6,374	6,374	6,374	6,374	6,374	6,374
Number of actions	807	807	807	807	807	807	807	807	807
Under-identification	305.3	305.3	305.3	305.3	305.3	305.3	305.3	305.3	305.3
Weak-identification	533.6	533.6	533.6	533.6	533.6	533.6	533.6	533.6	533.6
Hansen J-test	4.266	6.284	1.014	6.729	0.184	7.460	3.578	3.792	4.806
P-value (Hansen)	0.234	0.099	0.798	0.081	0.980	0.059	0.311	0.285	0.187
<i>C tests (p-values)</i>									
LD.Less severe actions	0.066	0.271	0.538	0.119	0.792	0.243	0.766	0.501	0.331
L2D.Less severe actions	0.222	0.092	0.755	0.052	0.747	0.029	0.582	0.278	0.125
L3D.Less severe actions	0.067	0.031	0.368	0.086	0.790	0.536	0.098	0.314	0.777
LD.Severe actions	0.027	0.040	0.443	0.012	0.677	0.386	0.945	0.714	0.632
First stage									
LD.Less severe actions	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)	0.030*** (2.634)
L2D.Less severe actions	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)	0.035*** (3.124)
L3D.Less severe actions	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)	0.041*** (3.378)
LD.Severe actions	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)	0.605*** (45.874)
R-squared	0.375	0.375	0.375	0.375	0.375	0.375	0.375	0.375	0.375
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table presents results of IV regressions of placebo enforcement actions in single-market banks on per capita personal income growth on the county level, total loan growth, and liquidity creation growth. We create placebo treatments by assuming that the enforcement actions occurred two years prior to their actual occurrence, their coding follows the exposition reported above in the notes to Table 3. All control variables reported in Table 3 are also included. We report robust t-statistics in parentheses, and errors are clustered on the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6

Robustness tests: Demand issues and spillover effects

Personal Income Growth (excluding cases for which PIG is negative for both of the last two years)					Less Severe actions (logit model)					Placebo test 1 Spill-over effects of Severe EA on Personal Income Growth of contiguous counties	Placebo test 2 Spill-over effects of Severe EA on Total Lending and Liquidity Creation growth of banks in the same county
<i>Second stage</i>										Number of replications: 200 Rejection rate at the 1% level (2-tailed test): 4.5% Rejection rate at the 5% level (2-tailed test): 10% Rejection rate at the 10% level (2-tailed test): 14.5%	Number of replications: 200 Rejection rate at the 1% level (2-tailed test): Total Lending Liquidity Creation 1% 1% Rejection rate at the 5% level (2-tailed test): Total Lending Liquidity Creation 4% 7.5% Rejection rate at the 10% level (2-tailed test): Total Lending Liquidity Creation 9.5% 9.5%
L.Severe actions	-0.006** (-2.295)				PIG(-) dummy	0.099 (1.106)					
L.Formal Agreements		-0.013** (-2.269)			UR(+) dummy		0.064 (0.634)				
L.Prompt corrective actions			-0.204 (-1.328)		NE(-) dummy			-0.027 (-0.313)			
L.Cease and desist orders				-0.012** (-2.251)	FS(-) dummy				-0.022 (-0.261)		
Controls	YES	YES	YES	YES							
Year FE	YES	YES	YES	YES	Year FE	YES	YES	YES	YES		
County FE	YES	YES	YES	YES	County FE	YES	YES	YES	YES		
Observations	9,145	9,145	9,145	9,145	Observations	4,536	4,536	4,536	4,536		
Counties	1,759	1,759	1,759	1,759	Counties	416	416	416	416		
Number of actions	681	340	18	396							
Under-identification	237.3	117.4	6.656	131.2							
Weak-identification	498.5	52.22	1.663	61.13							
Hansen J-test	2.093	2.143	3.453	2.162							
P-value (Hansen)	0.553	0.543	0.327	0.539							

Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include, in addition to the treatment variable, county (or bank, for column 3) fixed effects and year fixed effects, as well as controls. For column 1 and 3, standard errors are clustered at the county level, while for column 4 they are clustered at the bank level. For column 2, we run conditional logit models with county fixed effects. The dummies PIG(-), NE(-), and FS(-) take on the value one if there is a contraction in PIG, the number of establishments per capita, and the log of firm size at the county level, respectively, and zero otherwise. UR(+) takes on the value one if there is an increase in unemployment rate at the county level, and zero otherwise. For “Placebo test 1”, we generate 200 randomly-assigned placebos for year t for counties where there are no severe enforcement actions throughout the sample period but are contiguous to counties where there is a Severe EA in year t. The rejection rates are based on the estimated t-statistics for each of the 200 coefficients on L.Severe actions. For “Placebo test 2”, we generate 200 randomly-assigned placebos for year t for banks that were in a same county where a severe enforcement action in occurred year t, but which did not receive an enforcement action themselves.

Figure 1

Local banking markets in the U.S. (Development over time)

Figure 1 presents the location and number of counties in which single-market banks operate, at the beginning and the end of the sample period (1999 and 2011). Counties shaded in dark blue represent counties in which all operating banks are single market banks. Counties shaded in blue color are counties where at least one single market is located. All other counties are shaded in light blue color.

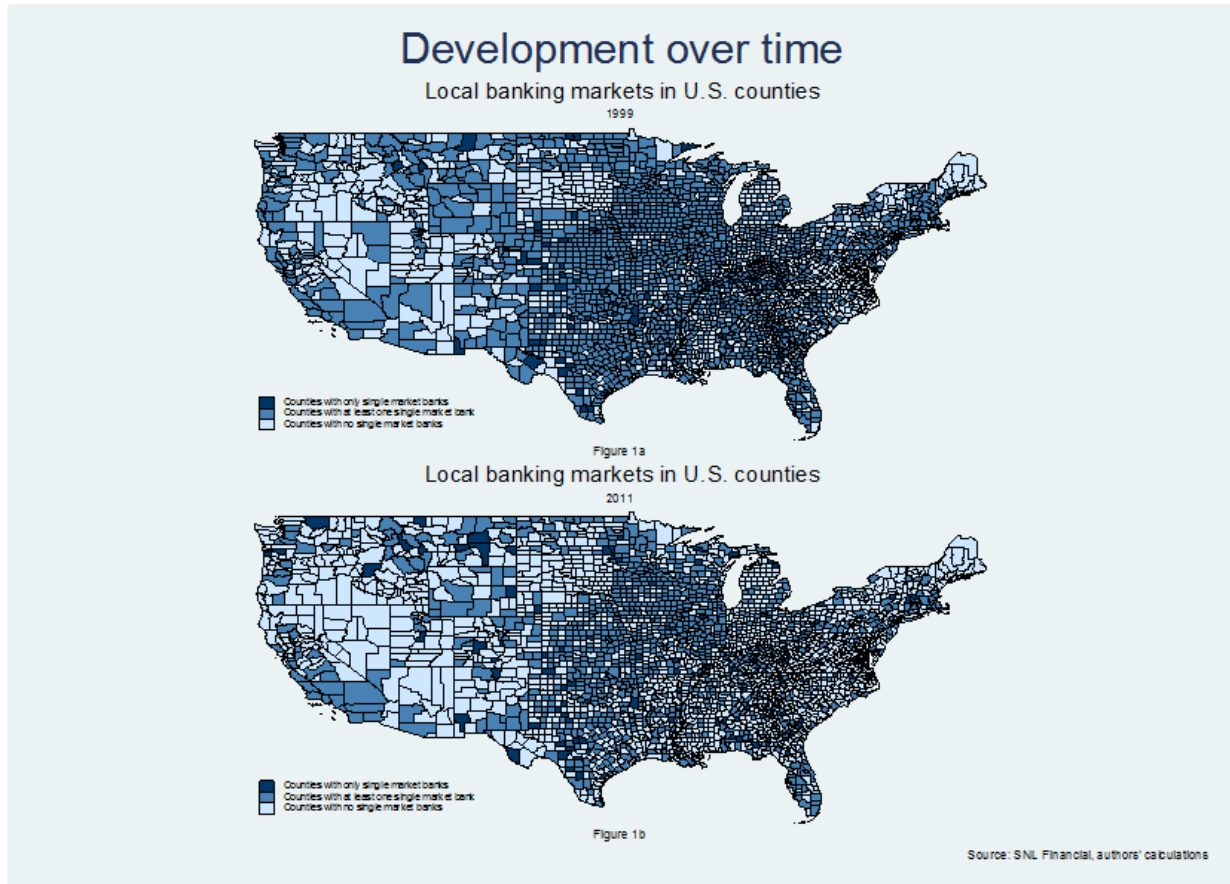


Figure 2

Enforcement actions in U.S. counties (Development over time)

Figure 2 presents the location and number of counties in which single market banks were subject to regulatory enforcement actions. We present their location and number at the beginning and the end of the sample period (year 1999 and 2011). Counties shaded in dark blue represent intervention counties. All other counties are shaded in light blue color.

