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

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Abstract

We highlight an important macro-financial linkage: Severe regulatory enforcement actions such as Formal agreements, Prompt corrective actions, and Cease and desist orders on banks trigger temporarily adverse effects for the macroeconomy. Exploiting the fact that severe regulatory enforcement actions impose shocks on bank business activities beyond the control of bank executives, we use an instrumental variables setup that shows severe regulatory actions imposed on single-market banks reduce personal income growth, the number of establishments, and increase unemployment in U.S. counties. These effects are related to contractions in bank lending and liquidity creation. Our identification is sharpened by a series of tests based on falsification tests, placebo enforcement actions, and we also rule out several other plausible alternative explanations that may confound our results.

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: Severe regulatory enforcement actions such as Formal agreements, Prompt corrective actions, and Cease and desist orders on banks trigger temporarily adverse effects for the macroeconomy. Exploiting the fact that severe regulatory enforcement actions impose shocks on bank business activities beyond the control of bank executives, we use an instrumental variables setup that shows severe regulatory actions imposed on single-market banks reduce personal income growth, the number of establishments, and increase unemployment in U.S. counties. These effects are related to contractions in bank lending and liquidity creation. Our identification is sharpened by a series of tests based on falsification tests, placebo enforcement actions, and we also rule out several other plausible alternative explanations that may confound our results.

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Do shocks to bank business activities caused by regulatory behavior affect the macroeconomy? And if so, how large is the effect? Does it persist in the long run? How do competitor banks of the institutions that are affected by regulatory actions respond? 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, examine whether these effects persist in the long run, and we investigate if competitor banks respond to these actions as well.

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 are unlikely to have a substantial impact on the local economy. Note that using lagged differences of these actions rather than the current levels excludes the possibility that our instruments are correlated with omitted variables related to local economic conditions (such as more corruption and fraud occurring in counties

with low economic growth). For instance, consider the case of a county in which, because of a period of low economic growth for several years, people are more likely to commit fraud. In such a county, the dummy for Less severe enforcement actions is more likely to be equal to one than in counties with high economic growth for the current year. However, the first lagged difference of Less severe enforcement actions (which is constructed using the previous two years) will be zero for all the cases in which the levels take on the same value (either zero or one) in two consecutive years, excluding by construction the possibility to confound periods of low local economic growth with a sudden increase in the level of regulatory monitoring (the latter being the phenomenon we intend to capture with this instrument).

Using the lagged differences of Less severe enforcement actions also ensures satisfaction of the exclusion condition even when the unit of our analysis is the individual bank in the regressions that focus on bank lending and liquidity creation. While the behavior of key employees can affect bank conduct, it is plausible that the propensity of a bank employee to commit fraud is sluggish (i.e., it is unlikely that an individual is prepared to commit fraud in 2003, but not in 2004). A switch from zero to one in the value of the lagged differences of Less severe enforcement actions affects the probability of occurrence of a Severe enforcement action purely because of increased scrutiny from the regulators on the bank: Such stronger scrutiny and monitoring leads to a greater probability that regulators discover more severe problems in the bank, relative to banks that have not been subjected to a Less severe enforcement action. Therefore, the lagged differences of Less severe enforcement actions satisfy the exclusion restriction even for our bank-level regressions. With similar arguments, we also employ as an additional instrument the lagged difference of the Severe enforcement action dummy.

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 hypothesize 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,

[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.

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.

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 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 transmit to the real sector. Bernanke (1983), Samolyk (1994), 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, 1995, 1996; Kang and Stulz, 2005; Gan, 2007; Khwaja and Mian, 2008; Paravisini, 2008; Lemmon and Roberts, 2010; Chava and Purnanandam, 2011; Schnabl, 2012). The literature on how spillover effects of regulatory efforts, that aim to mitigate shocks to banks, affect bank borrowers is very limited. Norden, Rosenboom, and Wang (forthcoming) show that capital injections in the U.S. trigger positive stock return responses by these banks' borrowers. Our paper distinguishes itself from these studies by focusing on a new type of shock arising from regulatory enforcement actions.

While the studies that hone in on the effects of enforcement actions suggest immediate effects on bank behavior, the question whether these enforcement actions affect the real economy remains an empirical issue. On one hand, regulators may issue enforcement actions to correct bad bank behavior, including excessive credit growth; on the other hand, to the extent to which these actions go too far, they may trigger unintended and possibly undesirable consequences for economic growth, suggesting a transmission mechanism by which regulatory behavior 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 7,062 banks operating in 1,898 U.S. counties (11,004 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, the number of establishments per 100 inhabitants, and the unemployment rate on the county level. The effects are substantial in terms of their economic magnitude: Severe actions are associated with a 0.6 percentage point reduction in personal income growth, and the number of establishments declines by 0.02 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 account for demand effects. To sharpen causal inference, we also generate placebo enforcement actions which confirm the main results.

An 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. Additional tests provide more details that rule out demand effects. We also show that our findings are not driven by poorly performing banks which contract lending and liquidity creation absent any enforcement action, and we document that anticipation effects of regulatory behavior and bank location in weak areas, reflected by multiple enforcement actions in the same county, play no role for our results. Our final set of tests focus on the long run and the behavior of competitor banks. We illustrate that the adverse effects for the real economy are only observable immediately after the announcement of enforcement actions but typically cannot be documented in the three subsequent years. Competitor banks, defined as those institutions located in the same U.S. county, neither increase lending nor liquidity creation.

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. Section 4 offers concluding remarks and sheds light on the policy debate.

1. Institutional Background: Enforcement Actions

In pursuing the aim to maintain a safe and sound banking system, regulatory agencies (FDIC, Federal Reserve System, and OCC) are charged with the supervision of banks. 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 that 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.

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 and publicly available. A typical civil money penalty that in our example also goes hand in hand with a removal and prohibition order reads as follows:

[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).

"WILLIAM BEN DUPREE, III ("Respondent") has received a NOTICE OF INTENTION TO REMOVE FROM OFFICE AND PROHIBIT FROM FURTHER PARTICIPATION AND NOTICE OF ASSESSMENT OF A CIVIL MONEY PENALTY, FINDINGS OF FACT AND CONCLUSIONS OF LAW, ORDER TO PAY AND NOTICE OF HEARING issued by the Federal Deposit Insurance Corporation ("FDIC") detailing the violations of law or regulation, unsafe or unsound banking practices and/or breaches of fiduciary duty for which an ORDER OF REMOVAL FROM OFFICE AND PROHIBITION FROM FURTHER PARTICIPATION AND ORDER TO PAY A CIVIL MONEY PENALTY (the "ORDER") may issue, and has been further advised of the right to a hearing on the alleged charges under sections […]."[3]

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.

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 undercapitalized 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.

A typical severe action which details the requirements for capital restoration plans, limits on capital disbursements, asset growth, and also contains restrictions on bonus payments, reads as follows:

"[…] 1. The Bank shall no later than 60 days of the date of this Directive (or such additional time as the Board of Governors may permit):

a) Increase the Bank's equity through the sale of shares or contributions to surplus in an amount sufficient to make the Bank adequately capitalized as defined in section 208.43(b)(2) of Regulation H of the Board of Governors (12 C.F.R. § 208.43(b)(2));

[3] The entire document with the Order of Removal from Office and Prohibition from Further Participation and Order to pay a Civil Money Penalty can be downloaded from https://www5.fdic.gov/edo/DataPresentation.html using docket number: FDIC-10-624k. Our manual search for a variety of other enforcement actions suggests the details of this removal and prohibition order are representative.

[…]

2. The Bank shall comply fully with the provisions of section 38(d)(1) of the FDI Act (12 U.S.C. § 1831o(d)(1)) restricting the making of any capital distributions, including, but not limited to, the payment of dividends.

3. The Bank shall not, without the prior written approval of the Federal Reserve Bank of Richmond (the "Reserve Bank") and the fulfillment of one of the requirements set forth in paragraph 1, solicit and accept new deposit accounts or renew any time deposit bearing an interest rate that exceeds the prevailing effective rates on deposits of comparable amounts and maturities in the Bank's market area.

[…]

5. The Bank shall comply fully with the provisions of sections 38(f)(4)(A)(i) and (ii) of the FDI Act (12 U.S.C. §§ 1831o(f)(4)(A)(i) and (ii)) restricting the payment of bonuses to senior executive officers and increases in compensation of such officers.

6. The Bank shall comply fully with the provisions of sections 38(e)(3) and (4) of the FDI Act (12 U.S.C. §§ 1831o(e)(3) and (4)) restricting asset growth, acquisitions, branching, and new lines of business. […]"[4]

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.[5]

[TABLE 1: Time distribution of enforcement actions]

2. Data Description and Identification Strategy

We obtain Call Report data for all commercial and savings banks in the U.S. from SNL Financial. This database also contains information about the timing and types of enforcement actions, 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 the effects of regulatory enforcement actions during normal periods rather than during crises. Moreover, we want to avoid that our results are driven by an extraordinary high frequency of enforcement actions that occur between 2008 and 2009. Nevertheless, we also performed our tests for the period 1999-2011, and the results remain very similar when the crisis years are included.[6]

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[4] The full text document with additional details for this Prompt corrective action can be downloaded from http://www.federalreserve.gov/newsevents/press/enforcement/enf20110811a1.pdf. We verify with a manual search that this example is highly representative for other severe enforcement actions.

[5] All our tests exclude banks from Delaware and South Dakota. Delaware has about 20 times more incorporations than other U.S. states due to favorable 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.

[6] Nearly 18 percent of all enforcement actions for the period 1999-2011 occur between 2008 and 2009. Including this subperiod in our regressions leaves our key inferences unchanged. The results are presented in Supplementary Appendix A.1.

Our analysis focuses on enforcement actions in banks that operate in only one 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 considers findings by Samolyk (1994) who documents the importance of conditions in the local banking sector for explaining personal income growth. Such a regional credit view highlights localized information costs which arise from the 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.

[FIGURE 1: Single-market banks]

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

2.1. Identification Strategy

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 behavior 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 explanatory 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 enforcement actions but are neither correlated with the macroeconomic setting nor with bank behavior in terms of lending, liquidity creation, and the second-stage error term. We use instrumental variables throughout this paper, and rely on a two-stage estimator, and 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 a severe enforcement action in the county at time t (zero 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. All regressions are based on annual data.

Our tests below first focus on the macroeconomic setting. For these tests, the dependent variable Y_{it} represents personal income growth deflated using the CPI, the number of establishments, and the unemployment rate.[7] For the 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 of strain in the banking system regulators may be more prone to issue enforcement actions). 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.

The 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. Cross-sectional variations 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 dived by the standard deviation of its return on assets, is included to consider bank soundness. This measure is 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 banks that operate in geographically delimited areas. Moreover, we use a Herfindahl-Hirschman Index (HHI) for deposit market shares to control for market structure, and we also account for average firm size (ln). Both variables are skewed and we therefore take the natural log. We use this variable 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 (Helpman, Melitz and Yeaple, 2004). Cetorelli and

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

Strahan (2006) argue that competition in banking affects access to finance and consequently has ramifications on industry structure in the non-financial sector. Table 2 presents summary 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 severe enforcement actions.[8] 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 by nor affect the economy as a whole, nor are such actions likely to trigger adjustments in bank behavior. Thus, they are good candidates to meet the identifying assumptions for valid 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 additional support to this claim, in Section 3.2 we provide further investigation of possible threats to the validity of this assumption. As a further instrument, we also use the first lag of the difference of the severe enforcement action.

3. Results

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 in the tests performed for the macroeconomic variables and on the bank level in the regressions for lending and liquidity creation to correct for serial correlation

[8] Our Supplementary Appendix A.2 documents that less severe enforcement actions correlate positively with severe enforcement actions but severe enforcement actions do not trigger less severe enforcement actions.

(Bertrand, Duflo, and Mullainathan, 2004). All regressions on the county level include county- and year-fixed effects, and the regressions on the bank level include bank- and year-fixed effects.

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

Our discussion focuses first on the results from the second stage. There is clear evidence that severe regulatory enforcement actions disrupt the real economy. Personal income growth, and the number of establishments are significantly reduced, and the unemployment rate increases in response to severe actions by regulators.[9] The economic magnitude of these effects is substantial: against an average growth rate of personal income of 1.7 percent, Severe enforcement actions reduce growth by 0.6 percentage points. Given an average number of 2.4 establishments per 100 inhabitants and an average unemployment rate of 6.1 percent, 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. Thus, while the number of establishments is affected only to a limited extent, adjustments are made within firms by shedding workforce. This effect ultimately shows up in reduced growth. These magnitudes appear plausible. Ashcraft (2005) documents that bank failures result in declines of real economic activity measured by county personal income growth of 1.12 percent in the year of failure. Since our analyses do not consider the closure of a bank but rather the restrictions of activities, our figures are in the right ballpark.

Next, we discuss the results for the first stage. 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.

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 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 between the Hansen statistic in 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]

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

Table 4 examines different types of enforcement actions, and presents 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, 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, the number of establishments, or unemployment. 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 cause reductions by 0.01 percentage points. 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 hardest. 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 by the bank to address the problems and respond quickly. Banks have strong incentives to avoid public attention from Cease and desist orders and are keen to portray themselves as adjusting behavior 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 to a change in conduct, Cease and desist orders may follow Formal agreements and have a lesser impact on banks.

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 percent 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 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 the ease, cost and time with which banks can dispose of their obligations in the case of assets. Second, we next assign weights of either +½, 0, or -½ to all 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 product category and maturity while loans are classified based on category due to data 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. To this end, we run instrumental variable regressions on the bank level for 7,062 banks and use the same exogenous instruments and control variables as in the tests above.[10]

Table 5 highlights that total lending growth contracts. 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 lending conditional on the types of borrowers when they decide to cut back their lending.[11] 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 standardized (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.

[10] The number of enforcement actions in Table 5 is smaller than those that enter the county-level analysis in Table 4 because the bank-level tests require that banks must have both Less severe and Severe enforcement actions to identify the effect of Severe enforcement actions.

[11] Supplementary Appendix A.3 provides an overview about the distribution of bank lending activities across key loan categories broken down by the type of borrowers.

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

Indeed, Table 5 illustrates that only considering lending behavior underestimates the effects of enforcement actions. Banks reduce their growth in liquidity creation by 13.7 percentage points when regulators issue a severe action. 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 small with 6.4 percentage points, and liquidity creation off the balance sheet is not affected.[12]

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

3.2 Identification concerns

Our examples of Less severe enforcement actions in Section 1 illustrate that such actions are not related to banks' business activities but rather relate to misconduct of staff. This assumption may be contested. Bank staff may be more prone to engage in fraudulent behavior during episodes of economic contraction which triggers civil money penalties and other Less severe enforcement actions. Specifically, 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 action in counties with bad economic conditions. To rule out the possibility that the probability of Less severe actions increases as economic conditions deteriorate, we run conditional logit models with county- and year-fixed effects

[12] To rule out that banks which received capital support from the Capital Purchase Program of the Troubled Asset Relief Program confound these results as they may have been under political pressure to increase lending, we remove in Supplementary Appendix A.4 these institutions. The results remain very similar.

using the dummy for the Less severe enforcement action as the dependent variable, and three dummies as the main explanatory variables. A dummy which takes the value one if there is a drop in personal income growth for a given county in year *t*, and zero otherwise; a dummy that takes on the value one if there is a contraction in the number of establishments for a given county in year *t*, and zero otherwise, and, finally, we use a dummy that takes on the value one if there is an increase in the unemployment rate for a given county in year *t*, and zero otherwise. These auxiliary tests, shown in Panel A of Table 6, provide additional evidence that this concern is not present in our data. Short-term changes in macroeconomic conditions at the county level bear no significant relation with the probability of Less severe actions. These results support the validity of our identification strategy.[13]

Another possible criticism may stem from the fact that fraud cases tend to peak at the end of boom periods (Povel, Singh and Winton, 2007), suggesting the possibility that we are confusing the effects of enforcement actions with business-cycle effects. We next test separately for possible confounding effects at the end of local business cycles by constructing a dummy variable, *Last year before recession*, which takes on the value one if in year *t+1* personal income growth in a county is negative *and* in year *t* it is non-negative, and zero otherwise. This dummy variable captures the last year before a local recession. Panel B of Table 6 shows that excluding observations for which *Last year before recession* is equal to one has virtually no effect on the results of our two-stage regressions on personal income growth. The coefficient on the lag of severe enforcement actions is still negative and significant. To further elucidate this issue, we also run conditional logit regressions (second and third column of Panel B) in which the dummies for less severe enforcement actions and severe enforcement actions are a function of the end of the local business cycle. The coefficients on *Last year before recession* are insignificant, providing further support for our key inferences and refuting the claim that we are confusing the impact of enforcement actions with local business-cycle effects.

Furthermore, we have already shown that our instruments are strong in the econometric sense because weak instruments can bias our coefficients. Importantly, our main regressions include control variables that provide information about market structure, the size of the borrowing firms, and bank soundness. Moreover, we also include county-fixed effects to net out time-invariant county-specific factors and year-fixed effects that capture time-varying shocks. None of these factors can therefore bias our results.

[13] One may also argue that episodes of economic contraction increase peoples' propensity to engage in criminal behavior. Our Supplementary Appendix A.5 presents additional logit regressions where we examine the effects of different types of crime on the probability of observing less severe enforcement actions.

In the next subsections, we deal with the only remaining source of concern: Omitted variables. While we cannot completely rule out omitted variable problems in observational data, we offer additional tests to mitigate concerns that omitted variables that are collinear with our instruments (i.e., variables which temporarily coincide with Less severe enforcement actions) drive the changes in the dependent variables.

3.2.1 Demand issues and spill-over effects

Next, we deal with the concern that the effects are due to demand effects rather than are causally related to severe regulatory actions. In other words, (lack of) economic growth is not the driving force behind the regulatory enforcement actions. We omit the worst performing counties in our sample in Panel C of Table 6, defined as counties where personal income growth contracts over two consecutive years prior to the severe action and replicate our regression for personal income growth from Table 3. We also present results for different enforcement actions based on the specifications from Table 4.

The dummy for severe regulatory actions 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 insignificant. Importantly, the magnitudes of the coefficients do not shrink. 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.

[TABLE 6: Threats to identification: Less severe actions, end of the business cycle, and demand issues]

We offer additional analyses in Table 7 based on placebo tests. The idea is to investigate if the responsiveness to enforcement actions we have shown is simply driven by chance (Bertrand, Duflo, and Mullainathan, 2004). Placebo tests 1, 2, and 3 examine spill-over effects of severe enforcement actions on personal income growth, the number of establishments and the unemployment rate in counties that are contiguous to those counties where regulators issued a severe action to a bank. Since we focus on placebo tests, we know that there should be no effect, unless there is a spillover effect from the county where the enforcement action is observed or, alternatively, our significant findings so far are purely random. We generate 100 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 7 presents the rejection rates at 1, 5, and 10 percent levels for these tests. This exercise indicates only very limited evidence that enforcement actions spill over into neighboring counties because the rejection rates remain very low. In only 9 out of 200 replications (i.e., 4.5 percent), we obtain *t*-statistics

significant at the 1 percent level, and even at the 10 percent significance level only 29 of the simulations yield significant effects for personal income growth. The results for the number of establishments and the unemployment rate are similar. Our final placebo test 4 takes these analyses to the bank level. We randomly assign 100 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 present the rejection rates. These falsification tests with very low rejection rates ranging from 1 to 9.5 percent for total lending and liquidity creation reinforce our belief that our key inferences are not due to chance. Rather, they are attributable to regulatory actions.

[TABLE 7: Threats to identification: Placebo tests]

3.2.2 Anticipation effects

Anticipation effects may also confound our inferences. If banks expect regulators to issue enforcement actions against them, they may contract lending and liquidity creation to avoid actions by regulators. In turn, this will trigger adverse effects for the real economy. Moreover, when banks consistently underperform, management may change loan underwriting standards which will also affect the macroeconomy via the effects on lending and liquidity creation, irrespective of any regulatory actions.

To mitigate concerns that such anticipation effects are responsible for our results, Panel A in Table 8 presents regressions where we run conditional logit regressions that model the probability of a bank being subject to a regulatory enforcement action given that other banks in the county have been subject to such actions in the previous year. The intuition is that being located in close proximity to other banks with enforcement actions gives rise to information advantages for the bank itself in terms of its awareness of regulatory monitoring and scrutiny in the vicinity (Kedia and Rajgopal, 2011). The coefficient on the interventions on other banks in the county remains insignificant, indicating that such anticipation effects play no role.

We next remove in Panel B of Table 8 banks which have declines in Z-scores (ln) and banks which display negative ROA prior to the issue of severe enforcement actions to consider the anticipation of enforcement actions due to poor performance. Our key coefficients remain very similar.

[TABLE 8: Threats to identification: Anticipation effects]

3.2.3 Clustering of enforcement actions

Clearly, in economically depressed areas, banks are likely to be weaker than elsewhere and our results may so far also reflect a clustering of enforcement actions in regions where multiple banks are subject to enforcement actions at the same time. Our regressions so far included observations where multiple

banks experienced regulatory actions in the same period. Table 9 documents that removing counties with multiple enforcement actions in the same year does not change our inferences.

[TABLE 9: Threats to identification: Multiple enforcement actions]

3.3 Long-run effects and the behavior of competitor banks

Our next test sheds light on the question whether regulatory actions correct a temporary overexpansion of bank activities to ensure safe, sound, and sustainable future banking activities, or if there are long-run damages for the real economy arising from these regulatory actions.

To this end, we replicate the main regressions from Table 3 but forward the dependent variable by one, two, and three years. Panel A in Table 10 presents the results. The coefficients on the Severe actions dummy are rendered insignificant, except for the dummy on the number of establishments at year t+1 where we are still able to document a contraction. Taken together, these tests suggest that the adverse effects from regulatory actions for the macroeconomy are only of temporary nature and do not cause long-run harm for the real sector. In other words, while regulatory enforcement actions correct bank behavior, the negative effects for the real sector recede quickly.

A possible explanation for why there are no effects of enforcement actions in the three years following their announcement could be that distressed banks' competitors pick up business opportunities. If this conjecture is true, we should be able to document positive effects of severe enforcement actions on lending and liquidity creation among the competitors. Panel B in Table 10 shows regressions of the levels and growth rates of market shares in terms of lending and liquidity creation of the competitors from the same county. Competitor institutions neither change their lending nor their liquidity creation.[14] Two explanations appear plausible. First, the relationship lending literature highlights the information sensitivity of banking. Repeated interaction between banks and borrowers may cause severe hold up problems and limits borrowers' propensity to switch (Sharpe, 1990; Ongena and Smith, 2001). Second, competitor banks may become aware of the enforcement actions at their distressed counterparts. To avoid being subject to regulatory scrutiny, they may consciously shy away from filling the gap and forego the opportunity to extend their market shares.

[14] In separate analyses, relegated to Supplementary Appendix A.6, we show that these results are marginally nuanced depending on the structure of the banking market. While we confirm the absence of substitution effects in counties above the median HHI, there is evidence that competitor banks pick up some of the business of the distressed institutions in more competitive banking markets with low HHIs. However, the economic magnitude of these effects remains negligible. Supplementary Appendix A.6 also investigates if the absence of profitable lending opportunities is responsible for the absence of substitution effects. We split the sample at the median level of non-performing loans but find no strong pattern in the data that would support this explanation.

[TABLE 10: Long-run effects and the behavior of competitor institutions]

Our final analyses in Table 11 provide some indication about the importance of regulatory enforcement actions by examining the counterfactual of what would happen to banks in the absence of interventions by regulatory authorities. These tests are important because they allow us to inform the debate about possible alternatives to these actions, and what their implications would be for bank behavior. To this end, we exploit the phenomenon in the data that a limited number of 82 banks operate with total capital ratios below 6 percent which is the threshold level for a bank to be classified as significantly undercapitalized which should trigger Prompt corrective actions. However, regulators exerted discretion and these banks did not receive Prompt corrective actions. Exploiting the benefit of hindsight in Panel A, we are also able to document what happens to these banks over a four year period. Of those institutions that received Prompt corrective actions, none was acquired and only 10 failed, whereas those banks where regulators did not intervene, six were acquired but 32 of those 82 banks failed. This result constitutes suggestive evidence that Prompt corrective actions are a useful tool to limit the disruptions arising from failure which would also affect the real economy as shown by Ashcraft (2005). While this small subsample necessitates very cautious interpretation, we also find that growth rates in lending and liquidity creation differ at weak levels of significance. Panel B shows that banks without Prompt corrective actions also contract lending and liquidity creation but less so than those banks with total capital ratios below 6 percent which are subject to Prompt corrective actions. While significantly undercapitalized banks also contract their activities, they do so to a lesser extent. Panel C offers tests based on regressions (containing bank- and year-fixed effects) that show that failing banks display substantial contractions in lending and liquidity creation, whereas subsequent acquisitions correlate positively with increases in lending growth. In sum, these tests offer suggestive evidence that enforcement actions motivate banks to change behavior beyond the adjustments they make to restore soundness in the absence of regulatory actions and avoid even bigger disruptions.

[TABLE 11: Significantly undercapitalized banks, Prompt corrective actions, and the effects on total lending growth and liquidity creation growth]

4. Concluding Remarks

The key result in this paper suggests that enforcement actions by bank regulators trigger temporarily adverse effects for the real economy in terms of decreasing personal income growth, the number of establishments, and increasing unemployment rates in U.S. counties. These effects, however, typically last for one year, and recede afterwards.

Using a careful instrumental variables estimation strategy to account for the non-random assignment of regulatory enforcement actions, we 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 0.6 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 percent between 1999 and 2011 (excluding the 2008-2009 recession) on the county level in the U.S., these effects are considerable in terms of their economic magnitude. Our more detailed analyses illustrate that these real effects are brought about by declines in bank lending, in particular standardized 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. A final analysis which examines the behavior of the distressed institutions' competitor banks shows no indication that they exploit the arising business opportunities and increase lending and liquidity creation.

Our research builds on a quickly evolving body of literature that analyses the effects of regulatory enforcement actions on distressed banks. Unlike other studies that limit their analyses to the effects of enforcement actions to the micro level and focus exclusively on bank behavior, the main innovation in our article is that we are the first to show that local economic indicators are affected by these regulatory actions. Placebo tests show that our results are unlikely to be driven by spill overs across banks or counties, and additional tests rule out the possibility that our results are driven by demand effects or other variables that are likely to coincide with the actions taken by regulators. Our empirical framework is uniquely suited to highlight this important macro-financial linkage: While enforcement actions are undoubtedly important to restore bank health, a theme we did not explore in this study, we document that they have potential to trigger adverse effects for the immediate macroeconomic environment. However, these effects are only temporary in nature. Moreover, our findings based on a small subsample of banks which fall below the threshold levels and should have received Prompt corrective actions but were not subject to these tests indicate that these banks fail in subsequent years at a higher frequency but adjust their lending and liquidity creation to a lesser extent than the banks that operate under enforcement actions. In sum, despite some short-term knock-on effects for the real economy, regulatory enforcement actions limit the occurrence of disruptive bank failures and also result in stronger adjustments of ailing banks' business operations.

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Table 1

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***	1			
	(0.00)				
Formal agreements	0.690***	0. 145***	1		
	(0.00)	(0.00)			
Cease and desist orders	0.765***	0.192***	0.145***	1	
	(0.00)	(0.00)	(0.00)		
Prompt corrective actions	0.170***	0.070***	0.104***	0.143***	1
	(0.00)	(0.59)	(0.00)	(0.00)	

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 actions. 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*
Dependent macroeconomic variables						
Real per capita personal income growth	11,004	0.017	0.036	-0.077	0.126	U.S. Bureau of Economic Analysis
# of Establishments (per 100 inhabitants)	11,004	2.448	0.780	0.662	9.403	County Business Patterns database
Unemployment rate (in %)	10,998	6.013	2.444	1.100	29.900	Bureau of Labor Statistics
Dependent bank-level variables						
Total lending growth	38,558	0.070	0.189	-0.308	0.966	SNL Financial, authors' calculation
Corporate real estate loan growth	38,558	0.142	0.392	-0.468	2.176	SNL Financial, authors' calculation
Residential real estate loan growth	38,558	0.062	0.263	-0.412	1.325	SNL Financial, authors' calculation
Commercial and industrial loan growth	38,558	0.083	0.340	-0.538	1.600	SNL Financial, authors' calculation
Consumer loan growth	38,558	-0.014	0.280	-0.541	1.320	SNL Financial, authors' calculation
Liquidity creation growth	38,558	0.112	0.389	-1.000	1.666	SNL Financial, authors' calculation
Liquidity creation growth (asset side)	38,558	0.080	1.173	-4.317	4.592	SNL Financial, authors' calculation
Liquidity creation growth (liability side)	38,558	0.099	0.226	-0.335	1.192	SNL Financial, authors' calculation
Liquidity creation growth (off balance)	38,558	0.164	0.420	-0.559	1.794	SNL Financial, authors' calculation
Interventions(lagged): county-level regressions						
Severe actions	11,004	0.080	0.271	0	1	SNL Financial, authors' calculation
Less severe actions	11,004	0.054	0.227	0	1	SNL Financial, authors' calculation
Formal agreements	11,004	0.040	0.196	0	1	SNL Financial
Prompt corrective actions	11,004	0.003	0.052	0	1	SNL Financial
Cease and desist orders	11,004	0.048	0.213	0	1	SNL Financial
Interventions(lagged): bank-level regressions						
Severe actions	38,558	0.020	0.141	0	1	SNL Financial
Less severe actions	38,558	0.015	0.122	0	1	SNL Financial
Formal agreements	38,558	0.010	0.100	0	1	SNL Financial
Prompt corrective actions	38,558	0.000	0.020	0	1	SNL Financial
Cease and desist orders	38,558	0.014	0.118	0	1	SNL Financial
Control variables (lagged)						
HHI deposits (ln)	11,004	2.547	0.564	0.000	4.580	SNL Financial, authors' calculation
Firm size (ln)	11,004	4.572	0.937	-0.871	37.468	Bureau of Labor Statistics
Z-score (ln)	11,004	-1.436	0.491	-3.157	0.000	Authors' calculation
Z-score (ln) bank-level	38,558	4.122	1.097	-3.590	38.854	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

	Panel A: Second stage			Panel B: First stage	
Dependent variable(s)	*Personal income growth*	*# of Establishments*	*Unemployment rate*	*Personal income growth # of Establishments*	*Unemployment rate*
LD.Less Severe actions				0.035**	0.035**
				(2.524)	(2.523)
L2D.Less severe actions				0.036**	0.036**
				(2.533)	(2.532)
L3D.Less severe actions				0.056***	0.056***
				(3.539)	(3.540)
LD.Severe actions				0.576***	0.576***
				(51.922)	(51.916)
L.Severe actions	-0.006**	-0.022***	0.142*	*Dependent Variable*	*Dependent Variable*
	(-2.419)	(-3.361)	(1.720)		
L.ln(Z-score)	0.001***	0.004*	-0.124***	-0.007***	-0.007***
	(3.494)	(1.734)	(-4.574)	(-2.682)	(-2.672)
L.ln(HHI)	0.004	0.028**	-0.216	-0.048**	-0.049**
	(1.605)	(2.165)	(-1.394)	(-2.067)	(-2.101)
L.ln(Firm size)	0.003*	0.026***	-0.528***	-0.038***	-0.038***
	(1.886)	(4.801)	(-7.530)	(-3.298)	(-3.293)
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Observations	11,004	11,004	10,998	11,004	10.998
R-squared	0.063	0.334	0.727	0.357	0.358
Counties	1,898	1,898	1,898	1,898	1,898
Number of actions	879	879	877		
Under-identification	298.1	298.1	298.4		
Weak-identification	687.5	687.5	687.4		
Hansen *J*-test	0.921	2.508	2.602		
p-value (Hansen)	0.820	0.474	0.457		
C-test (p-values)					
LD.Less Severe actions	0.885	0.140	0.180		
L2D.Less Severe actions	0.656	0.863	0.503		
L3D.Less Severe actions	0.480	0.634	0.170		
LD.Severe actions	0.715	0.182	0.251		

Notes. This table presents results of instrumental variable regressions of enforcement actions in single-market banks on per capita personal income growth, the number of establishments, and the unemployment rate, all measured at the county level. Panel A presents the second-stage regressions, and Panel B shows the first-stage results. Enforcement actions include Severe actions (dummy variable equal to one 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 one if enforcement actions against Personnel and individuals, and other Civil money fines are observed, or zero otherwise). Our regressions control for soundness in the local banking market, approximated by the Z-score (ln), concentration of the local banking market, measured by a county-level deposit-based Herfindahl-Hirschman Index (HHI), and average firm size in the county (Firm size). Delaware and South Dakota counties excluded. The standard errors are clustered on the county level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4

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

Panel A: Second stage									
Dependent variable	*Personal income growth*			*# of Establishments*			*Unemployment rate*		
L.Formal agreements	-0.012**			-0.047***			0.321*		
	(-2.408)			(-3.287)			(1.777)		
L.PCA		-0.200*			-0.474			7.038*	
		(-1.688)			(-1.515)			(1.789)	
L.Cease & desist orders			-0.010**			-0.038***			0.261*
			(-2.395)			(-3.231)			(1.780)
L.ln(Z-score)	0.002***	0.002***	0.001***	0.005*	0.005*	0.004*	-0.124***	-0.124***	-0.123***
	(3.533)	(3.547)	(3.412)	(1.743)	(1.880)	(1.715)	(-4.574)	(-4.544)	(-4.570)
L.ln(HHI)	0.004*	0.005*	0.004	0.029**	0.033**	0.027**	-0.217	-0.238*	-0.211
	(1.688)	(1.840)	(1.546)	(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
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,004	11,004	11,004	11,004	11,004	11,004	10,998	10,998	10,998
Counties	1,898	1,898	1,898	1,898	1,898	1,898	1,898	1,898	1,898
Number of actions	439	30	525	439	30	525	439	30	523
Under-identification	140.1	8.481	171.2	140.1	8.481	171.2	140.2	8.482	171.5
Weak-identification	61.11	2.130	84.75	61.11	2.130	84.75	61.14	2.130	84.84
Hansen J-test	0.957	2.086	0.877	2.771	6.880	2.929	2.372	0.449	2.390
p-value (Hansen)	0.812	0.555	0.831	0.428	0.076	0.403	0.499	0.930	0.495
C-test (p-values)									
LD.Less severe actions	0.857	0.219	0.802	0.110	0.009	0.111	0.203	0.881	0.204
L2D.Less severe actions	0.622	0.709	0.713	0.957	0.797	0.855	0.555	0.997	0.497
L3D.Less severe actions	0.499	0.737	0.527	0.572	0.533	0.565	0.187	0.544	0.184
LD.Severe actions	0.779	0.295	0.822	0.135	0.010	0.131	0.158	0.980	0.260
Panel B: First stage									
LD.Less severe actions	0.022*	0.009*	0.033**	0.022*	0.009*	0.033**	0.022*	0.009*	0.033**
	(1.919)	(1.664)	(2.416)	(1.919)	(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**
	(1.030)	(0.668)	(2.267)	(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***
	(2.280)	(1.103)	(2.841)	(2.280)	(1.103)	(2.841)	(2.28)	(1.103)	(2.841)
LD.Severe actions	0.265***	0.011**	0.320***	0.265***	0.011**	0.320***	0.265***	0.011**	0.321***
	(15.476)	(2.284)	(18.167)	(15.476)	(2.284)	(18.167)	(15.479)	(2.283)	(18.176)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.142	0.015	0.196	0.142	0.015	0.196	0.142	0.015	0.197
Observations	11,004	11,004	11,004	11,004	11,004	11,004	10,998	10,998	10,998
Counties	1,898	1,898	1,898	1,898	1,898	1,898	1,898	1,898	1,898

Notes. This table presents the results of IV regressions of enforcement actions in single-market banks on per capita personal income growth on the county level, total lending growth, and liquidity creation growth. Delaware and South Dakota counties excluded. The standard errors are clustered on the county level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5

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

Panel A: Second stage									
	Bank lending					*Bank liquidity creation*			
Dependent variable	*Total lending growth*	*Corporate real estate loan growth*	*Residential real estate loan growth*	*Commercial and industrial loan growth*	*Consumer loan growth*	*Liquidity creation growth*	*Liquidity creation growth (asset side)*	*Liquidity creation growth (liability side)*	*Liquidity creation growth (off balance)*
L.Severe actions	-0.098***	-0.095**	-0.053**	-0.112***	-0.114***	-0.137***	-0.225**	-0.064***	-0.050
	(-5.566)	(-2.534)	(-2.143)	(-3.715)	(-3.910)	(-3.928)	(-2.041)	(-2.838)	(-1.218)
L.ln(Z-score)	0.012***	0.006**	0.004**	0.014***	0.007***	0.015***	0.027***	0.011***	0.016***
	(7.914)	(2.299)	(2.370)	(5.543)	(3.239)	(5.679)	(3.357)	(6.653)	(5.245)
L.ln(HHI)	0.016	0.005	0.001	0.025	-0.022	0.032*	0.005	0.015	0.050***
	(1.630)	(0.242)	(0.045)	(1.507)	(-1.544)	(1.790)	(0.097)	(1.337)	(2.593)
L.ln(Firm Size)	0.005	-0.001	0.001	-0.006	-0.006	-0.004	-0.019	0.007	0.026**
	(1.067)	(-0.067)	(0.219)	(-0.681)	(-0.966)	(-0.396)	(-0.526)	(1.225)	(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	38,558	38,558	38,558	38,558	38,558	38,558	38,558	38,558	38,558
Banks	7,062	7,062	7,062	7,062	7,062	7,062	7,062	7,062	7,062
Number of actions	778	778	778	778	778	778	778	778	778
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
C tests (p-values)									
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
Panel B: First stage									
LD.Less severe actions	0.030***	0.030***	0.030***	0.030***	0.030***	0.030***	0.030***	0.030***	0.030***
	(2.634)	(2.634)	(2.634)	(2.634)	(2.634)	(2.634)	(2.634)	(2.634)	(2.634)
L2D.Less severe actions	0.035***	0.035***	0.035***	0.035***	0.035***	0.035***	0.035***	0.035***	0.035***
	(3.124)	(3.124)	(3.124)	(3.124)	(3.124)	(3.124)	(3.124)	(3.124)	(3.124)
L3D.Less severe actions	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***	0.041***
	(3.378)	(3.378)	(3.378)	(3.378)	(3.378)	(3.378)	(3.378)	(3.378)	(3.378)
LD.Severe actions	0.605***	0.605***	0.605***	0.605***	0.605***	0.605***	0.605***	0.605***	0.605***
	(45.874)	(45.874)	(45.874)	(45.874)	(45.874)	(45.874)	(45.874)	(45.874)	(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 the results of IV regressions of enforcement actions in single-market banks on per capita personal income growth on the county level, total lending growth, and liquidity creation growth. Panel A presents the second stage regressions, and Panel B shows the first stage results. The standard errors are clustered on the county level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6

Threats to identification: Less severe actions, demand issues and spillover effects

Panel A: Occurrence of Less severe actions			
	P(Less severe action) (Conditional logit model)		
Drop personal income growth	0.099		
	(1.106)		
Drop # of establishments		-0.027	
		(-0.313)	
Increase unemployment rate			0.064
			(0.634)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	4,536	4,536	4,536
Counties	416	416	416

Panel B: The impact of the end of the local business cycle			
	IV two-stage regression Personal income growth	**Conditional logit** P(Less severe actions)	**Conditional logit** P(Less severe actions)
L.Severe actions	-0.006**		
	(-2.224)		
Last year before recession		-0.135	
		(-1.309)	
Last year before recession			-0.023
			(-0.238)
Controls	Yes	No	No
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	9,057	4,536	5,223
Counties	1,877	416	481
Number of actions	743		
Under-identification	256.1		
Weak-identification	472.4		
Hansen *J*-test	0.944		
p-value (Hansen)	0.815		

Panel C: Demand effects

Instrumental variable regressions for Personal income growth

(excluding cases for which PIG is negative for both of the last two years)

L.Severe actions	-0.006**			
	(-2.295)			
L.Formal Agreements		-0.013**		
		(-2.269)		
L.Prompt corrective actions			-0.204	
			(-1.328)	
L.Cease and desist orders				-0.012**
				(-2.251)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	9,254	9,254	9,254	9,254
Counties	1,868	1,868	1,868	1,868
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

Notes. This table presents in Panel A conditional logit models with county- and year-fixed effects for the occurrence of Less severe enforcement actions. The dummy variables for the drop in personal income growth and in the number of establishments take the value one if there is a contraction in personal income growth, and in the number of establishments at the county level, and zero otherwise respectively. The dummy for the increase in the unemployment rate takes on the value one if there is an increase in unemployment rate at the county level, and zero otherwise. All regressions include, in addition to the treatment variable, county-fixed effects and year-fixed effects. In Panel B, we examine specifically the effects of the end of the local business cycle on our inferences. *Last year before recession* is a dummy that takes on the value one if for year *t* personal income growth is non-negative *and* for year *t+1* personal income growth is negative, and zero otherwise. This dummy identifies the last year prior to a local recession. We first present the results of a two-stage regression of the effect of severe enforcement actions on personal income growth. The specification of the regression is the same as for Table 3 but we exclude observations for which *Last year before recession* is equal to one. In the second and third column of Panel B, we show results for conditional logit regressions predicting the probability of *Less severe actions* and *Severe actions*, respectively, as a function of *Last year before recession*. Panel C shows tests that rule out demand effects. We present the second stage results from instrumental variable regressions for personal income growth as in Table 3 but now omit observations in countries where the personal income growth rate has been negative over two consecutive periods. Standard errors are clustered at the county level Panel C presents placebo tests. The standard errors are clustered on the county and on the bank level, respectively. The *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7

Threats to identification: Placebo tests

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 # of Establishments	**Placebo test 3** Spill-over effects of Severe EA on Unemployment rate	**Placebo test 4** Spill-over effects of Severe EA on Total Lending and Liquidity Creation growth of banks in the same county
Number of replications: 200	Number of replications: 200	Number of replications: 200	Number of replications: 200
Rejection rate at the 1% level (2-tailed test): 4.5%	Rejection rate at the 1% level (2-tailed test): 1%	Rejection rate at the 1% level (2-tailed test): 1%	Rejection rate at the 1% level (2-tailed test): Total Lending 1% Liquidity Creation 1%
Rejection rate at the 5% level (2-tailed test): 10%	Rejection rate at the 5% level (2-tailed test): 5.5%	Rejection rate at the 5% level (2-tailed test): 5.5%	Rejection rate at the 5% level (2-tailed test): 4% 7.5% Total Lending 4% Liquidity Creation 7.5%
Rejection rate at the 10% level (2-tailed test): 14.5%	Rejection rate at the 10% level (2-tailed test): 11%	Rejection rate at the 10% level (2-tailed test): 11%	Rejection rate at the 10% level (2-tailed test): Total Lending 9.5% Liquidity Creation 9.5%

Notes. This table presents placebo tests that focus on spill-over effects. Placebo tests 1, 2, and 3, we generate 100 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 enforcement action in year *t*. The rejection rates are based on the estimated *t*-statistics for each of the 200 coefficients on L.Severe actions. The focus is on personal income growth in test 1, on the number of establishments in test 2, and on the unemployment rate in test 3. For Placebo test 4, 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. The standard errors are clustered on the county and on the bank level, respectively. The *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8

Threats to identification: Anticipation effects – regulatory monitoring intensity and declines in bank performance

Panel A: Regulatory monitoring intensity in the county			**Panel B: Declines in performance**			
Dependent variable	*Severe enforcement actions*	*Severe enforcement actions*	*Total lending growth*	*Liquidity creation growth*	*Total lending growth*	*Liquidity creation growth*
			Sample excludes observations with declines in Z-scores (ln)		Sample excludes observations with declines in ROA	
L.Severe actions (county)	0.154	0.141				
	(0.751)	(0.614)				
L.Severe actions			-0.111***	-0.144***	-0.084***	-0.124***
			(-4.641)	(-2.887)	(-3.847)	(-2.708)
L.ln(Z-score)		-0.999***	0.012***	0.016***	0.013***	0.017***
		(-13.410)	(6.025)	(4.007)	(7.585)	(5.176)
L.ln(HHI)		0.253	-0.000	0.031	-0.005	0.011
		(0.686)	(-0.010)	(1.297)	(-0.441)	(0.495)
L.ln(Firm Size)		-0.898***	0.014**	0.010	0.005	-0.002
		(-2.914)	(2.316)	(0.598)	(0.950)	(-0.147)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,777	3,522	19,877	19,877	23,161	23,161
R-squared			0.042	0.021	0.040	0.019
Number of Banks	536	490	5,449	5,449	5,710	5,710
Under-identification			187.2	187.2	207.0	207.0
Weak-identification			223.5	223.5	285.3	285.3
Hansen *J*-test			3.428	2.829	5.199	5.518
p-value (Hansen)			0.330	0.419	0.158	0.138

Notes. We present conditional logit regressions that model the occurrence of a severe enforcement action as a function of any severe enforcement action in the previous year in the same county in Panel A. Panel B presents instrumental variables regressions for lending growth and liquidity creation growth as a function of severe actions and control variables as in Table 4 with the difference that we remove observations where banks experience a decline in Z-scores (ln), and a decline in ROA over two consecutive years, respectively. Unless stated otherwise, these regressions include the control variables used in Table 3 and Table 4, respectively. We only show second-stage results. First-stage results are available upon request. The standard errors are clustered on the bank (county) level and the associated t(z)-statistics are reported in parentheses.. *** p<0.01, ** p<0.05, * p<0.1

Table 9

Threats to identification: Excluding counties with multiple enforcement actions

Dependent variable	*Personal income growth*	*# of Establishments*	*Unemployment rate*
L.Severe actions	-0.007**	-0.021***	0.121
	(-2.477)	(-2.922)	(1.302)
L.ln(Z-score)	0.001***	0.004*	-0.118***
	(3.338)	(1.651)	(-4.497)
L.ln(HHI)	0.004	0.026*	-0.208
	(1.572)	(1.897)	(-1.477)
L.ln(Firm size)	0.003*	0.026***	-0.541***
	(1.839)	(4.762)	(-7.587)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	10,754	10,754	10,748
R-squared	0.063	0.330	0.723
Counties	1,892	1,892	1,806
Under-identification	261.0	261.0	261.3
Weak-identification	536.0	536.0	536.2
Hansen *J*-test	0.636	1.823	4.082
p-value (Hansen)	0.888	0.610	0.253

Notes. We present instrumental variable regressions identical to those shown in Table 3 but we exclude counties that have more than one bank per year being subject to regulatory enforcement actions. The variables are explained in the Notes to Table 3. We only show second-stage results. First-stage results are available upon request. The standard errors are clustered on the county level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10

Long-run effects and the behavior of competitor institutions

Panel A: Long-run effects

Dependent variable	*Personal income growth t+1*	*# of Establishments t+2*	*Unemployment rate t+3*	*Personal income growth t+1*	*# of Establishments t+2*	*Unemployment rate t+3*	*Personal income growth t+1*	*# of Establishments t+2*	*Unemployment rate t+3*
L.Severe actions	0.001	-0.017**	-0.008	-0.000	0.005	-0.015	0.006	-0.008	0.099
	(0.450)	(-2.332)	(-0.094)	(-0.026)	(0.528)	(-0.145)	(1.528)	(-1.008)	(0.821)
L.ln(Z-score)	0.001	0.004	-0.086***	0.000	-0.005	0.196***	-0.001	-0.007*	0.238***
	(1.285)	(1.564)	(-4.977)	(0.055)	(-1.104)	(4.621)	(-0.536)	(-1.789)	(3.915)
L.ln(HHI)	0.022***	0.042***	-0.179	0.008	0.027**	0.404***	0.010**	0.041***	-0.062
	(5.645)	(3.296)	(-1.376)	(1.446)	(2.435)	(2.596)	(2.328)	(3.335)	(-0.366)
L.ln(Firm size)	0.004*	0.014**	-0.240***	0.004	-0.007	0.073	0.001	0.006	0.070
	(1.894)	(2.399)	(-3.511)	(1.570)	(-1.085)	(0.731)	(0.211)	(0.976)	(0.564)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,697	9,697	9,691	8,280	8,280	8,274	8,284	8,284	8,281
R-squared	0.086	0.335	0.676	0.318	0.274	0.742	0.332	0.402	0.786
Number of banks	1,795	1,795	1,795	1,776	1,776	1,776	1,779	1,779	1,779
Under-identification	228.1	228.1	227.6	149.5	149.5	149.0	149.5	149.5	149.0
Weak-identification	472.7	472.7	471.8	417.6	417.6	416.7	417.6	417.6	412.1
Hansen-*J*-test	0.391	2.129	0.741	1.159	2.844	3.481	3.039	5.993	5.577
p-value (Hansen)	0.942	0.546	0.863	0.763	0.416	0.323	0.386	0.112	0.134

Panel B: Behavior of competitor banks

Dependent variable	*Total lending (market share)*			*Liquidity creation (market share)*			*Total lending growth*			*Liquidity creation growth*		
	t+1	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
L.Severe actions	-0.000**	-0.000	-0.000	-0.000	-0.000	-0.000*	-0.001	-0.001	0.000	0.001	0.006	-0.014
	(-2.257)	(-0.704)	(-0.711)	(-1.302)	(-1.106)	(-1.893)	(-1.644)	(-0.889)	(0.113)	(0.287)	(0.414)	(-1.365)
L.ln(Z-score)	-0.000**	-0.000	-0.000	-0.000**	-0.000**	-0.000**	-0.000	-0.000	-0.000	-0.000	-0.000	-0.008
	(-2.097)	(-1.129)	(-1.404)	(-2.198)	(-2.464)	(-2.457)	(-1.635)	(-0.729)	(-0.560)	(-0.869)	(-0.124)	(-1.009)
L.ln(HHI)	-0.000*	-0.000	-0.000	-0.000*	-0.000*	-0.000	-0.000	0.000	-0.000	-0.003**	-0.001	0.004
	(-1.789)	(-1.557)	(-0.903)	(-1.841)	(-1.916)	(-0.657)	(-0.946)	(1.100)	(-0.448)	(-2.102)	(-0.453)	(0.845)
L.ln(Firm size)	0.000*	0.000	0.000	0.000*	0.000*	0.000	-0.000	0.000	-0.000	-0.001	0.003	0.012
	(1.715)	(1.427)	(1.071)	(1.674)	(1.916)	(0.990)	(-1.227)	(0.963)	(-0.221)	(-0.110)	(0.776)	(1.190)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,344	28,577	27,479	32,344	28,577	27,479	32,344	28,577	27,479	32,344	28,577	27,479
R-squared	0.010	0.015	0.007	0.006	0.007	0.003	0.010	0.006	0.012	0.000	0.000	0.000
Number of banks	5,879	5,581	5,384	5,879	5,581	5,384	5,879	5,581	5,384	5,879	5,581	5,384
Under-identification	173.8	134.9	125.8	173.8	134.9	125.8	173.8	134.9	125.8	173.8	134.9	125.8
Weak-identification	370.5	348.4	353.1	370.5	348.4	353.1	370.5	348.4	353.1	370.5	348.4	353.1
Hansen-*J*-test	3.062	2.161	2.378	1.718	1.959	1.830	7.429	1.358	0.144	8.316	4.975	1.341
p-value (Hansen)	0.382	0.540	0.498	0.633	0.581	0.609	0.0594	0.715	0.986	0.0399	0.174	0.719

Notes. Panel A presents instrumental variable regressions that establish the long-run effects of severe enforcement actions on personal income growth, the number of establishments, and the unemployment rate. The dependent variables are forwarded one, two, and three years. Panel B examines the behavior of competitor banks' market shares in terms of total lending and liquidity creation, and the corresponding growth rates for one, two, and three years following the announcements of severe enforcement actions using instrumental variable regressions. The control variables discussed in the notes to Table 3 are included. We only show second-stage results. First-stage results are available upon request. The standard errors are clustered on the county and bank level respectively, and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11

Significantly undercapitalized banks, Prompt corrective actions, and the effects on total lending growth and liquidity creation growth

Panel A: Significantly undercapitalized banks, failures, acquisitions, and Prompt corrective actions

	Total capital ratio <6%	
	Banks with Prompt corrective action	*Banks without Prompt corrective action*
Failed banks	10	32
Acquired banks	0	6
Neither failed nor acquired	3	45
Total	13	82[+]

Panel B: Differences in means of total lending growth and liquidity creation growth

	Mean	*Mean*	*t-Test*
Total lending growth	-0.141	-0.062	1.923*
Liquidity creation growth	-0.132	0.006	2.228**

Panel C: Total lending growth, liquidity creation growth and subsequent acquisitions and failures

Dependent variable	*Total lending growth*	*Liquidity creation growth*
Failed bank	-0.187***	-0.142**
	(-4.829)	(-2.315)
Acquired bank	0.067**	0.059
	(2.126)	(0.936)
Year FE	Yes	Yes
Bank FE	Yes	Yes
Observations	38,558	38,558
R-squared	0.034	0.019
Number of banks	7,062	7,062

Notes. Panel A presents an overview of significantly undercapitalized banks, defined as banks whose total capital ratio below 6 percent, and the occurrence of Prompt corrective actions in any one of the last four years. Panel B presents *t*-tests for the differences in means in terms of Total lending growth and Liquidity creation growth between these two types of banks. The *t*-tests are performed with unequal variances. In Panel C, we show auxiliary regressions (with both bank- and year-fixed effects) that document how future bank failure and future acquisitions of those significantly undercapitalized banks affect total lending growth and liquidity creation growth. Standard errors are clustered on the bank level. We report robust *t*-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [+] One bank was acquired, but subsequently failed – for this reason the total number of banks, 82, does not match the sum of the three sub-categories.

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.

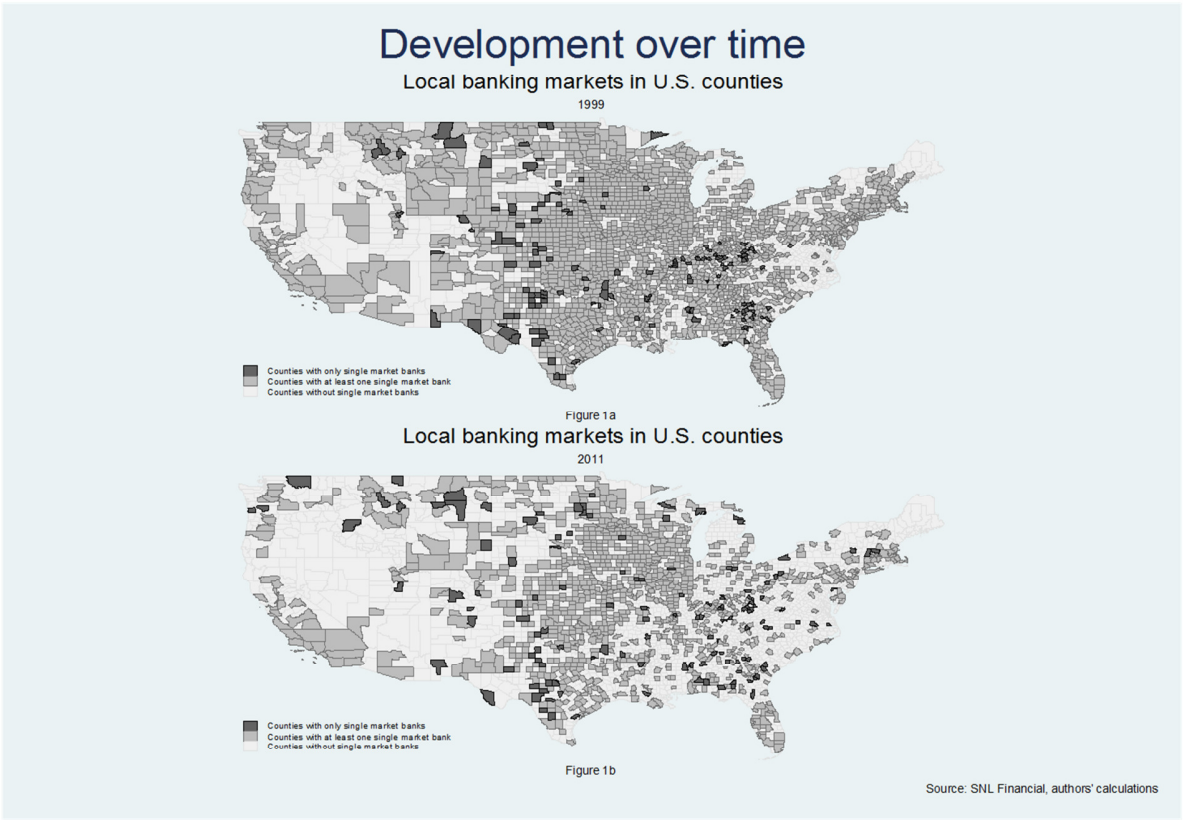
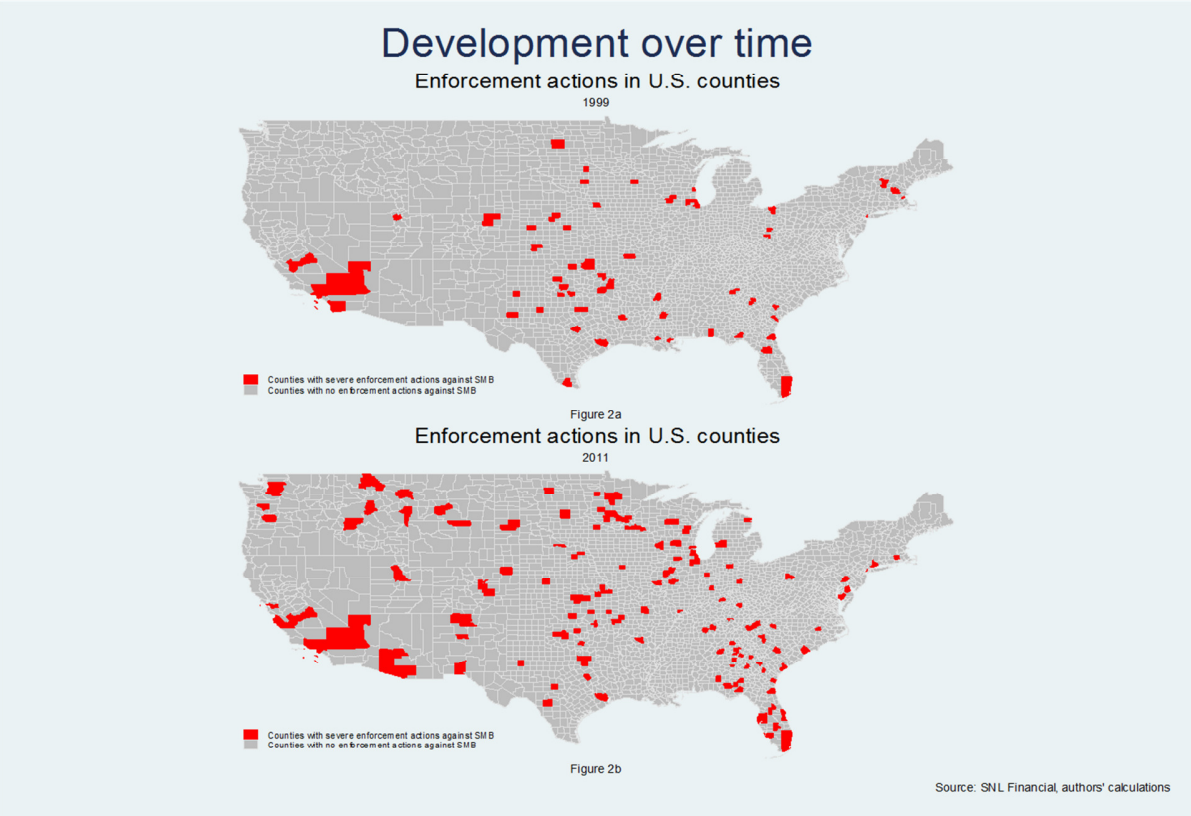


Figure 2

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.



Supplementary Appendix

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

- not for publication –

Supplementary Appendix: The real effects of regulatory enforcement actions: Evidence from U.S. counties *(not for publication)*

Table A.1 Additional Robustness Test - Including the crisis years 2008 and 2009

Dependent variable	*Personal income growth*	*# of Establishments*	*Unemployment rate*
L.Severe actions	-0.003	-0.021***	0.171***
	(-1.395)	(-4.086)	(2.625)
L.ln(Z-score)	0.001***	0.002	-0.123***
	(3.596)	(1.231)	(-4.345)
L.ln(HHI)	0.005**	0.023**	-0.159
	(1.966)	(1.971)	(-1.107)
L.ln(Firm size)	0.003*	0.022***	-0.522***
	(1.754)	(4.409)	(-7.631)
Crisis Dummy	-0.048***	0.055***	0.487***
	(-31.368)	(16.867)	(9.904)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	13,992	13,992	13,986
R-squared	0.214	0.321	0.727
Counties	1,903	1,903	1,903
Under-identification	343.1	343.1	343.4
Weak-identification	1233	1233	1231
Hansen *J*-test	0.178	1.768	4.066
p-value (Hansen)	0.981	0.622	0.254

Supplementary Appendix: The real effects of regulatory enforcement actions: Evidence from U.S. counties *(not for publication)*

Table A.2 Relationship between less severe and severe enforcement actions

Dependent variable	*Less severe enforcement actions*	*Severe actions*
D.Severe actions	0.009	
	(0.756)	
D.Less severe enforcement action		0.035***
		(3.019)
Year FE	Yes	Yes
Bank FE	Yes	Yes
Observations	38,558	38,558
R-squared	0.002	0.016
Number of banks	7,062	7,062

Notes. This table presents linear probability regressions for the probability of observing Less severe enforcement actions as a function of changes (i.e. first-difference of) in severe enforcement actions, and of the probability of observing severe enforcement actions as a function of changes (i.e. first-difference of) Less severe enforcement actions to understand the sequencing of different types of enforcement actions. The standard errors are clustered on the bank level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Supplementary Appendix: The real effects of regulatory enforcement actions: Evidence from U.S. counties *(not for publication)*

Table A.3 Summary statistics for selected 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. The different types of loans are scaled by total loans per year.

Supplementary Appendix: The real effects of regulatory enforcement actions: Evidence from U.S. counties *(not for publication)*

Table A.4 Additional Robustness Test - Excluding banks which received TARP capital support

Dependent variable	*Total lending growth*	*Liquidity creation growth*
L.Severe actions	-0.096***	-0.135***
	(-5.488)	(-3.884)
L.ln(Z-score)	0.012***	0.015***
	(7.960)	(5.745)
L.ln(HHI)	0.016	0.032*
	(1.600)	(1.794)
L.ln(Firm Size)	0.004	-0.005
	(0.972)	(-0.419)
Year FE	Yes	Yes
Bank FE	Yes	Yes
Observations	37,812	37,812
R-squared	0.032	0.018
Number of banks	6,358	6,358
Under-identification	303.4	303.4
Weak-identification	527.5	527.5
Hansen-*J*-test	4.107	7.313
p-value (Hansen)	0.250	0.0626

Notes. This table presents additional results of instrumental variable regressions of enforcement actions on single-market banks on per capita personal income growth, total loan growth, and liquidity creation growth, all measured at the county level in Panel A. These regressions are identical to those shown in Table 3 but we included the crisis period 2008-2009. The explanatory variables are explained in the Notes to Table 3. Panel B presents bank level regressions identical to those shown in Table 5 but we exclude banks which received capital support from the Troubled Asset Relief Programme. The explanatory variables are explained in the Notes to Table 3. We only show the second-stage results. The standard errors are clustered on the county level and the associated *t*-statistics are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Supplementary Appendix: The real effects of regulatory enforcement actions: Evidence from U.S. counties *(not for publication)*

Table A.5 Exogeneity of Less severe enforcement actions: Additional tests using crime data

Dependent variable	*Less severe enforcement actions*	*Less severe enforcement actions*	*Less severe enforcement actions*	*Less severe enforcement actions*	*Less severe enforcement actions*
Number of motor vehicle thefts known to police	13.778				
	(0.214)				
Number of larceny-thefts known to police		6.054			
		(0.554)			
Number of burglaries known to police			6.057		
			(0.271)		
Number of property crimes known to police				3.498	
				(0.483)	
Number of robberies known to police					-271.232
					(-1.021)
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Observations	2,939	2,939	2,939	2,939	2,939
Counties	328	328	328	328	328

Notes. This table presents conditional logit regressions to provide further evidence that Less severe enforcement actions are not driven by poor conditions in the local economy, reflected by crime data. The dependent variable is a dummy that takes on the value of one if a Less severe enforcement action was observed in the county in a given year or zero otherwise. The explanatory variables provide information about different types of crimes. The regressions also include year- and county-fixed effects. We report z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.6 Additional tests that examine the absence of substitution effects by competitor banks

Panel A: Counties with high concentration (HHI)					**Panel B: Counties with low concentration (HHI)**			
Dependent variable	*Total lending (market share)*	*Liquidity creation (market share)*	*Total lending growth*	*Liquidity creation growth*	*Total lending (market share)*	*Liquidity creation (market share)*	*Total lending growth*	*Liquidity creation growth*
L.Severe actions	0.000	-0.000	-0.000	-0.012	0.000***	0.000**	0.001**	0.003***
	(1.050)	(-0.083)	(-0.151)	(-0.802)	(2.868)	(2.351)	(2.247)	(2.734)
L.ln(Z-score)	-0.000	-0.000	-0.001***	0.000	-0.000	-0.000	-0.000**	-0.000**
	(-0.562)	(-1.265)	(-4.732)	(0.161)	(-0.671)	(-0.248)	(-2.469)	(-2.354)
L.ln(HHI)	-0.000*	-0.000	-0.000	-0.006	0.000	0.000	-0.000	-0.007
	(-1.701)	(-0.851)	(-0.341)	(-1.175)	(1.194)	(0.215)	(-0.899)	(-1.537)
L.ln(Firm Size)	0.000*	0.000	0.000	0.023	0.000**	0.000**	0.000*	0.021
	(1.701)	(1.060)	(0.956)	(0.571)	(2.548)	(2.101)	(1.650)	(1.637)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,534	16,534	16,534	16,534	19,989	19,989	19,989	19,989
R-squared	0.007	0.004	0.019	0.001	0.020	0.011	0.010	-0.004
Number of banks	3,039	3,039	3,039	3,039	3,764	3,764	3,764	3,764
Under-identification	119.6	119.6	119.6	119.6	152.9	152.9	152.9	152.9
Weak-identification	196.6	196.6	196.6	196.6	270.0	270.0	270.0	270.0
Hansen-*J*-test	3.776	3.145	3.293	1.833	3.800	3.819	8.755	7.906
p-value (Hansen)	0.287	0.370	0.349	0.608	0.284	0.282	0.0327	0.0480
Panel C: Counties with high levels of non-performing loans					**Panel D: Counties with low levels of non-performing loans**			
L.Severe actions	-0.000	-0.000	0.000	0.000	0.000	0.000	-0.000	-0.060*
	(-0.081)	(-0.165)	(0.657)	(0.229)	(0.650)	(0.766)	(-0.141)	(-1.908)
L.ln(Z-score)	-0.000	-0.000	-0.000***	-0.002	-0.000***	-0.000***	-0.000	-0.001
	(-1.071)	(-1.192)	(-6.402)	(-0.886)	(-2.748)	(-2.720)	(-1.058)	(-0.987)
L.ln(HHI)	-0.000	-0.000	-0.000	0.000	-0.000	-0.000	0.000	0.000
	(-1.307)	(-0.682)	(-0.637)	(0.043)	(-1.467)	(-1.305)	(0.665)	(0.033)
L.ln(Firm Size)	0.000	0.000	0.001**	0.021	0.000**	0.000	0.000	-0.018
	(0.685)	(0.671)	(2.357)	(1.043)	(2.188)	(0.522)	(0.418)	(-0.935)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,614	18,614	18,614	18,614	17,464	17,464	17,464	17,464
R-squared	0.009	0.004	0.018	0.000	0.010	0.008	0.009	0.000
Number of banks	4,572	4,572	4,572	4,572	4,326	4,326	4,326	4,326
Under-identification	247.0	247.0	247.0	247.0	47.95	47.95	47.95	47.95
Weak-identification	402.2	402.2	402.2	402.2	46.89	46.89	46.89	46.89
Hansen-*J*-test	2.458	4.291	3.026	2.633	0.389	1.117	4.750	2.928
p-value (Hansen)	0.483	0.232	0.388	0.452	0.942	0.773	0.191	0.403

Notes. We present regressions of the effect of severe enforcement actions on the market shares of total lending and liquidity creation and the corresponding growth in these two variables for the distressed banks' competitors. These regressions are identical to those presented in the lower panel of Table 10. Panel A shows results for a subsample of banks located in counties with high concentration, i.e., above the median HHI in the sample, and Panel B shows the results for a subsample of banks from counties with low levels of concentration. Panels C and D split the sample at the median level of non-performing loans to examine whether lending opportunities drive our finding. We only show the second-stage results. The standard errors are clustered on the bank level and the associated *t*-statistics are reported in parentheses.. *** p<0.01, ** p<0.05, * p<0.1