Employment effects of unconventional monetary policy: Evidence from QE*

Stephan Luck[†] and Tom Zimmermann[‡]

Federal Reserve Board, QuantCo

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Abstract

This paper investigates the effect of the Federal Reserve's unconventional monetary policy on employment via a bank lending channel. We find that while banks with higher mortgage-backed securities holdings issued relatively more loans after the first and third rounds of quantitative easing (QE1 and QE3), additional volume is concentrated in refinanced mortgages after QE1, and in originated home purchase mortgages and commercial and industrial lending after QE3. Using spatial variation, we show that regions with a high share of affected banks experienced stronger lending and overall employment growth after QE3, but only weak employment effects after QE1. While the ability of households to refinance mortgages after QE1 spurred local demand, the resulting employment growth was confined to the non-tradable goods sector. In contrast, the increase in overall employment after QE3 can be attributed to the supply of additional credit to firms. To support this finding, we use new confidential loan-level data to show that firms with stronger ties to affected banks increased employment and capital investment more after QE3. Altogether, our findings suggest that unconventional monetary policy can, similar to conventional monetary policy, affect real economic outcomes, but its effect depends on the type of targeted assets and on market forces outside the central bank's authority.

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[†]Federal Reserve Board, stephan.luck@frb.gov

[‡]QuantCo, Tom.Zimmermann.work@gmail.com

1 Introduction

Does unconventional monetary policy affect the real economy? And if so, what are the channels? Over the course of the 07-08 Financial Crisis, monetary policy reached the zero lower bound in several countries. As a consequence, central banks have turned toward unconventional monetary policies such as forward guidance and large-scale asset purchases (LSAPs). Because the channels through which unconventional monetary policy affects the real economy are yet not fully understood and because conclusive empirical evidence is scarce, the efficacy and desirability of such policies has been controversial.[1]

Against this background, this study investigates the real effects of the Federal Reserve's unconventional monetary policy. Our study is the first to document a link between the Federal Reserve's quantitative easing (QE) and employment outcomes. This is a particularly important finding as previous research has highlighted an increase in lending to households and businesses subsequent to the first and third rounds of QE (QE1 and QE3; Darmouni and Rodnyansky (2017) and Di Maggio et al. (2016)) but has not been able to speak to the crucial question of the extent to which such lending translated into employment effects.

In particular, we show that while QE1 led to increased refinancing activity in the mortgage market by commercial banks, this additional lending activity had only weak effects on employment growth, confined to the non-tradable goods sector. In contrast, QE3 induced additional commercial and industrial (C&I) lending as well as increased origination of home purchase mortgages, which in turn translated into economically sizable growth in overall employment. Our evidence suggests that LSAPs can, similar to reductions of interest rates in times of conventional monetary policy, affect real economic outcomes via a bank lending channel. However, the efficacy depends on the types of assets targeted as well as market forces outside of a central bank's authority.

The empirical assessment of macroeconomic policy is generally difficult, given the natural absence of a control group.[2] In order to overcome this inherent challenge, we proceed in three steps. First, we exploit cross-sectional variation in the exposure of commercial banks to QE. Second, we exploit spatial

[1]For instance, after the implementation of QE1 a number of prominent economists wrote an open letter to Ben Bernanke (published in the *Wall Street Journal* on November 15 2010): "We believe the Federal Reserve's large-scale asset purchase plan (so-called 'quantitative easing') should be reconsidered and discontinued. We do not believe such a plan is necessary or advisable under current circumstances. The planned asset purchases risk currency debasement and inflation, and we do not think they will achieve the Fed's objective of promoting employment."

[2]The problem is best summarized by Ben Bernanke in his memoir, *The Courage to Act*: "We can't know exactly how much of the U.S. recovery can be attributed to monetary policy, since we can only conjecture what might have happened if the Fed had not taken the steps that it did."

variation in the activity of banks that were more affected by QE, allowing us to trace the effect of QE on local credit and labor markets. Third, we provide evidence from confidential loan-level data that allows us to directly link lending decisions by banks to investment and employment decisions by firms.

In the first step, we investigate the link between QE and lending at the bank level. We follow existing contributions that exploit the cross-sectional variation of banks' mortgage-backed securities (MBS) holdings in order to identify an effect of QE on bank lending, see Darmouni and Rodnyansky (2017). Our analysis confirms the positive effect of QE1 and QE3 on total bank lending. But our analysis goes further than existing work: When we decompose total lending into mortgages and C&I loans, we find that QE1 mainly affected the refinancing volume of existing mortgages (Di Maggio et al. (2016) and Beraja et al. (2016)), and QE3 affected the origination of home purchase mortgages at more exposed banks. We further show that QE3 spurred additional C&I lending. This finding turns out to be crucial for understanding how QE affected employment.

In the second and central part of the paper, we trace the effects documented at the bank-level to the county level. We exploit that, on top of the cross-sectional variation of MBS holdings, there is geographical variation in banks' activity. Importantly, a bank's regions of activity are very stable over time and highly predictable. Measuring banks' local activity by either the historical amount of small business lending or the historical amount of deposits issued in a given county, we construct an exposure measure at the county level: We treat counties that have historical activity from banks with more MBS holdings as exposed counties and those with less activity from such banks as non-exposed counties.

Notably, employment growth in counties with high and low exposure to banks with large MBS holdings evolved very similarly during the Financial Crisis, suggesting that these counties were on similar trajectories absent QE. To be precise, with respect to employment growth, exposed and non-exposed counties follow the same trend for more than 18 consecutive quarters before the implementation of QE3. After QE3, however, we find that exposed counties experience higher aggregate employment growth. The effects are economically sizable: Our estimates suggest that counties in the upper tercile of the MBS–bank distribution experienced 60 basis points higher employment growth than counties in the lower tercile of the distribution.[3] In contrast, after QE1, no such differential effect is observed. While exposed counties do experience higher employment growth in the non-tradable goods sector during

[3]If taken at face value, our findings suggests that the Federal Reserve's policy added an additional 200,000 new jobs per year in affected counties after QE3 via inducing additional bank lending. However, given the cross-sectional nature of our analysis, one cannot easily draw conclusions with respect to aggregate employment. Such an interpretation would only be valid under a number of important and strong assumptions, such as ruling out the possibility of distributive effects, i.e., from unaffected regions to affected regions.

QE1, the effect is weak, and aggregate employment growth is identical across affected and unaffected counties.

Finally, in the third step, we use confidential loan level data to trace how credit availability for firms directly translates into employment and investment decisions.[4] In particular, we use newly available confidential loan–level data,[5] which requires large banks with more than $50 billion assets to report any loan on their balance sheet with a commitment of 1 million USD or more. Data availability, however, constrains the analysis to C&I loans issued by larger banks for the period around QE3.

Using the loan-level database, we provide evidence that the increase in lending after QE3 is not driven by increases in demand for loans by firms, but rather by additional supply of bank loans. To account for loan demand by firms, we control for firm-specific demand in the spirit of Khwaja and Mian (2008) in bank–firm–level regressions. We also show that the increased volume of C&I lending comes from the issuance of additional term loans rather than from an expansion of credit lines, consistent with term loans more likely being used to fund new investment. Finally, using firm-level data on investment and employment, we provide evidence that, after QE3, firms that historically tended to borrow from affected banks increased capital investment and employment by more than firms that had been borrowing from unaffected banks.

Given that the loan-level data only include the largest U.S. banks, we also provide evidence on the relationship between bank lending and employment growth from more aggregate data that include all U.S. commercial banks. First, we find that the lending effects documented at the bank level can also be found at the regional level: Small business lending and origination of home purchase mortgages increased in more exposed counties after QE3, while only mortgage refinancing activity increased in those counties after QE1. Second, similar to controlling for demand at the firm level, we also control for demand at the regional level. We show that our findings with respect to C&I and mortgage lending remain unchanged when controlling for county-specific demand at the bank–county level, further strengthening the importance of bank credit supply.

While our results show that the increase in lending stems from an increase in credit supply to households as well as firms, the employment effect could be the result of two separate channels: It could either result from an increase in local credit supply to firms, or from increased local credit supply

[4]Note that in an ideal experiment, one would also observe which households are borrowing from affected and which from unaffected banks, and observe how their net-worth and consumption responds to changes in credit availability.

[5]Data are collected by U.S. regulators to assess capital adequacy and to support stress-test models. We focus on the loan schedule of the data collection (Y-14Q, H1).

to households, which in turn affects employment through increasing demand for local consumption. While generally both channels may be relevant, our data gives us some sense of which type of lending is more important for employment. In particular, we exploit that increases in demand are more likely to affect the non-tradable goods sector than the tradable goods sector. Hence, if increased demand was driving the increase in economic activity, additional employment would more likely be generated in the non-tradable goods sector (Mian and Sufi (2014)).

We find that the aggregate change in employment after QE3 is driven by changes of employment in sectors other than the non-tradable goods sector. In contrast, even though there is no aggregate employment effect during QE1, employment in the non-tradable sector increases subsequent to the implementation of the program. Our findings thus suggest that the ability of households to refinance mortgages after QE1 spurred local demand by increasing household net-worth, resulting in increased employment in the non-tradable goods sector. However, the effect is weak and cannot be documented in other sectors. In contrast, the significant increase in employment after QE3 is more likely to be a consequence of the increased supply of C&I loans rather than increased mortgage origination. In line with this interpretation and consistent with our findings at the bank-firm level, we also document that the increase in employment after QE3 is driven by employment growth in industries that are more dependent on external financing, pointing to increased credit supply to firms being the more relevant channel.

Our results survive a large number of robustness checks. As our identification strategy relies on spatial variation in bank activity, it requires banking markets to be sufficiently local. If there were no frictions in lending across regions, our bank-level results should not extend to the regional level, as a bank with a high MBS share would be equally likely to increase lending in any region.[6] Throughout the main body of our analysis, we define a county as a local market. However, we show that our main findings also hold at the level of a metropolitan statistical area (MSA), which tends to coincide with commuting zones and is sometimes used as an alternative way of defining local labor markets. Furthermore, we also show that results are not driven by particular regions of the United States: Our results hold when we exclude counties with a very high population or regions with a high concentration of banks with large MBS holdings. We also show that the findings are robust to alternative definitions

[6]Existing evidence suggests that the market for C&I and small business loans are local, see Brevoort et al. (2010), Greenstone et al. (2014), and Nguyen (2015). In the case of mortgage lending activity, the existing evidence is ambiguous. Beraja et al. (2016) do not find regional frictions in mortgage lending, while Scharfstein and Sunderam (2014) find that mortgage lending is characterized by local markets.

of the exposure measure.

Altogether, our findings suggest that unconventional monetary policy can, similar to conventional monetary policy, spur economic activity via a bank lending channel.[7] In particular, LSAPs can induce commercial banks to expand lending which may translate into additional employment. However, our analysis also shows that the effects are less predictable than those of conventional monetary policy and seemingly depend on a number of forces that lie outside of the central bank's authority. While the success of QE3 shows that central banks can affect real economic outcomes at the zero lower bound, the absence of an employment effect during QE1 shows that the success depends not only on the central bank's actions, but also on a complex interplay of market forces, e.g., in the credit market, that lead to additional employment in the private sector.[8]

We proceed as follows. After a brief summary of the existing literature, Section 2 discusses data used in our study. Section 3 presents the bank-level analysis, while the core contribution of the paper is the county-level analysis in Section 4. Section 5 provides evidence that the results are driven by an increase in credit supply and not by an increase in local demand and uses loan level data to establish a direct link between the banks' lending decisions and employment by firms. Section 6 provides robustness checks before we conclude in Section 7.

1.1 Related literature

Our study contributes to the literature on the bank lending channel of monetary policy. During times of conventional monetary policy, the conventional wisdom is that tighter monetary policy is associated with decreased bank lending and increases in unemployment (see Bernanke and Blinder (1992), Kashyap et al. (1996), and Kashyap and Stein (2000)).[9] Existing evidence on unconventional monetary policy, however, is scarce, and, prior to the 07-08 Financial crisis, mainly focused on the case of Japan (see Bernanke et al. (2004) and Bernanke and Reinhart (2004)).

[7]Conventional monetary policy assumes a relationship between inflation and real economic variables in the short-run, which implies that a central bank can, by determining the rate of inflation via the target nominal interest rate, affect real economic outcomes such as employment. The theoretical foundation for this type of policy is given by models in which frictions, such as price adjustments cost, make it desirable for a central bank to mitigate the consequences of those frictions which prevent economic variables from attaining their "natural level" (see Woodford 2003, Gali 2015, Walsh 2017). In principle, monetary policy can work through a number of different channels; see Mishkin (1996) for an overview.

[8]Given that QE1 was implemented directly after the climax of the 07-08 Financial Crisis, one may argue that even though QE1 may have induced banks to supply additional credit, there was likely little or no demand for additional borrowing due to the pessimistic economic outlook. In particular, firms may not have been willing to conduct additional investment. Likewise, while households used the lower interest rates to refinance their existing mortgages, they may not have purchased new homes in expectation of further falling prices or because of falling net-worth.

[9]The theoretical channels are pioneered by Bernanke and Gertler (1995) and Kashyap and Stein (1994), and Bernanke et al. (1999).

Existing empirical papers on the effect of unconventional monetary policy in the United States[10] have so far focused on outcomes other than employment.[11] For instance, existing contributions have investigated the effects of QE on aggregate financing cost (Hancock and Passmore (2011) and Gilchrist et al. (2015)) or on outcomes in the housing market and at the household level (see, e.g., Di Maggio et al. (2016), Beraja et al. (2016)), at the bank level (see, e.g., Darmouni and Rodnyansky (2017), Kurtzman et al. (2017)), or at the firm level (Chakraborty et al. (2016)). To the best of our knowledge, our paper is the first to document a bank lending channel of unconventional monetary policy that has a positive employment effect.

Our analysis at the bank level is closely related to Darmouni and Rodnyansky (2017) and Di Maggio et al. (2016), who exploit cross-sectional variation in MBS holdings. Darmouni and Rodnyansky (2017) document a positive effect of QE1 and QE3 on bank lending. Importantly, our bank-level analysis goes further by studying the type of lending affected in more depth. In particular, we decompose mortgage lending and C&I lending to isolate the exact lending channel. Consistent with Di Maggio et al. (2016), who study the effects on the mortgage market in more detail, we find that the increased lending after QE1 is due to increased refinancing activity, and the additional lending subsequent to QE3 increased origination of home purchase mortgages. Moreover, we further find that QE3 also led to additional C&I lending by affected banks, in particular to small businesses, and this additional insight turns out to be key to understanding the employment effects of QE in our main analysis.

When studying the effects of QE on households' investment and consumption behavior, Di Maggio et al. (2016) find a sizable effect of QE1 on equity extraction and an increase in consumption. Moreover, Beraja et al. (2016) argue that refinancing is more pronounced in regions in which households have a lower leverage. Both is consistent with the employment increase in the non-tradables sector that we document. Importantly, however, this employment effect is modest and does not translate into an effect for overall employment. In contrast, we further document that the additional lending subsequent to QE3 had a significant effect on overall employment. This point is particularly important, as employment

[10]Several papers investigate the effects of the ECB's unconventional monetary policy. Acharya et al. (2016) find that the QE program implemented by the ECB did not have real effects in the euro zone. The difference from our results can arise because of the different institutional backgrounds of the euro zone and the United States as well as the differences in the actual implementation of unconventional monetary policy. While existing evidence does not suggest that the ECB's unconventional monetary policy had real effects, existing contributions indicate that it did spur bank lending. In particular, see Carpinelli and Crosignani (2017) for the case of the long-term refinancing options (LTRO), Acharya et al. (2016) for the announcement of outright monetary transactions (OMT), and Acharya et al. (2016) on both.

[11]Note that several contributions in the macroeconomic literature provide estimates for the effect. For instance, Engen et al. (2015), assess the effects of QE by estimating a DSGE model of the US economy and find the effects on employment an inflation peaked only in early 2015 and early 2016, respectively.

is one of the Federal Reserve's statutory objectives and high unemployment was one of the driving forces behind the implementation of QE3.[12]

While we find that QE3 induced additional C&I lending, in particular to small businesses, Chakraborty et al. (2016) find that QE, rather than increasing C&I lending, crowds out C&I lending and led to a reduction of investment by firms. The differences in findings can be explained by the difference in the research design as well as by the data used. First, while Chakraborty et al. (2016) conduct a panel analysis in which a bank's MBS holdings are interacted with the monthly amount of assets purchased by the Federal Reserve, our paper is an event study that allows us to compare lending patterns before and after a respective round of QE using a difference-in-differences approach. In particular, their findings suggest a negative relationship of mortgage lending and C&I lending over a longer period. In contrast, our findings suggest that, specifically around QE3, banks conduct additional mortgage lending as well as additional C&I lending. Second, our confidential loan-level data are not restricted to syndicated loans alone, and our sample at the bank-firm level is hence substantially larger and likely more representative of the C&I lending landscape.[13]

More generally, our analysis of the employment effects of QE contributes to the literature on how financial conditions affect real economic outcomes (see, e.g., Bernanke (1983), Peek and Rosengren (2000), Driscoll (2004), Khwaja and Mian (2008)). In particular, a series of recent papers show how the 2007-08 financial crisis affected bank lending (see, e.g., Ivashina and Scharfstein (2010)), and real economic outcomes via various lending channels (see, e.g., Chodorow-Reich (2014), Duygan-Bump et al. (2015), Haltenhof et al. (2014), Bord et al. (2015), Benmelech et al. (2017), Greenstone et al. (2014)). Our empirical strategy is closest to Benmelech et al. (2017), Bord et al. (2015), or Greenstone et al. (2014), who exploit spatial variation in the exposure to the financial crisis. Unlike our paper, which is concerned with the effect of unconventional monetary policy on employment, Benmelech et al. (2017) trace the effect of the run in the asset-backed commercial paper market on auto sales, and Bord et al. (2015) and Greenstone et al. (2014) analyze the employment effects of the financial crisis through reductions in small business lending.

Our findings about the relevance of small business lending for aggregate fluctuations in employment

[12]See the FOMC statement of September 13, 2012 that announced QE3, stating: "If the outlook for the labor market does not improve substantially, the Committee will continue its purchases of agency mortgage-backed securities, undertake additional asset purchases, and employ its other policy tools as appropriate until such improvement is achieved in a context of price stability."

[13]Our results at the bank–firm level that allow for firm–time fixed effect are based on more than 45000 observations, while the corresponding results in Chakraborty et al. (2016) are based on 400 observations.

is also in line with existing evidence. Hurst and Pugsley (2011) show that more than 90% of all firms in the Unites States have fewer than 99 employees, and that these firms comprised roughly 35% of aggregate paid employment. Moreover, small businesses generally tend to be more dependent on external financing and, in particular, on bank financing (see, e.g., Petersen and Rajan (1994)). Indeed, Duygan-Bump et al. (2015) show that workers in small firms that were more dependent on external financing were more likely to become unemployed during the great recession. Moreover, Chen et al. (2017) show that the decrease of small business lending by large banks subsequent to the 07-08 Financial Crisis had an adverse effect on employment in regions with historically high activity of large banks.

2 Data

Our study brings together data from different sources at the bank, regional,[14] and firm level.

We take bank balance sheet and income statement information from the Consolidated Reports of Condition and Income for commercial banks in the United States (Call Reports, based on Forms FFIEC 031 and FFIEC 041). All items are adjusted for bank mergers. Given the focus of our paper on local activity, we run all bank-level specifications at the commercial bank level, but the results are robust to aggregating to the bank holding company level. The data provided by the call reports are quarterly for all variables used in our analysis, except for small business lending, which is only reported at an annual frequency prior to 2010:Q1.[15]

We make use of three further administrative sources that collect data at the bank level.

The Home Mortgage Disclosure Act (HMDA) requires most banks to report on their mortgage lending activity. Data are reported at the mortgage-level and include information on the mortgage amount, whether the amount refers to the origination of a mortgage for a home purchase or the re-financing of an existing loan, and the geographic location of the property down to the census tract level. For the remainder of the paper, we refer to origination of home purchase mortgages as "mortgage origination" and to the origination of an existing refinanced as a "refinanced mortgage". Virtually all banks in the Unites States that do any mortgage lending in an MSA report on thei activity, so the HMDA data are considered a near census of mortgage lending.[16] Importantly, the public version of the HMDA data only identifies the year of the observation. We use a confidential version of the data, maintained by

[14]"Regional" encompasses the county as well as MSA level.

[15]Prior to 2010:Q1, small business lending is reported in the second quarter of every year.

[16]The FFIEC publishes the criteria for reporting requirements: https://www.ffiec.gov/hmda/reporter.htm. In 2009, every depository bank with assets above $39m that originated at least one mortgage had to report.

the Board of Governors of the Federal Reserve, that reports exact application and action dates for each observation and therefore enables us to measure the timing of potential lending effects more precisely. For our analysis, we aggregate the HMDA data to the bank-quarter level and to the county-quarter level.

Similar to the HMDA data, the Community Reinvestment Act (CRA) requires banks to report on small business lending activity by geographic area.[17] Each bank breaks down data by geographic region down to the census tract[18] and by loan size bins. In addition, banks report total lending to businesses with revenues of less than $1m. The CRA data are fairly representative of the universe of small business lending by commercial banks: Greenstone et al. (2014) estimate that small business lending in the CRA data captures 86% of total small business lending in the U.S. We aggregate the CRA data to the bank level or to the county level for most of our analysis, but we use census tract information and bank-county level data in some regressions.

In addition, we use data from the Summary of Deposits (SoD) collected by the Federal Deposit Insurance Corporation (FDIC). The SoD data consist of annual branch-level reports of total deposits by bank branch, and we aggregate the data to the bank–county or bank–census tract level in our analysis.

County-level data come from different sources. Employment data are from the Quarterly Census of Employment and Wages (QCEW) that is collected by the Bureau of Labor Statistics. The data provide quarterly counts of employment and wages by industry as reported by employers, and they cover more than 95% of U.S. jobs. In addition to employment, the data contain information on the number of establishments by industry in a given county. We complement employment data with the County Business Patterns (CBP), an annual data collection by the U.S. Census that includes the number of establishments and number of employees by industry and county in the first week of March each year.[19] As controls in our analysis, we use information on local economic conditions: Regional housing prices come from Moody's, income comes from Haver Analytics, and population comes from the U.S. Census.

To investigate directly the link between banks' MBS holdings, corporate lending and real outcomes for a subset of banks and firms, we make use of newly available administrative data on bank loans. Since 2012, regulators have collected loan-level data on bank lending from any bank with total assets of more than 50 billion USD on a quarterly basis. The purpose of the data collection is to assess capital

[17]The reporting thresholds are different for the CRA data than for the HMDA data. In 2005, every bank with total assets of at least $1b has to report, and the reporting threshold has been adjusted each year. The size threshold rose to 1.221 billion in 2015, and the data cover approximately 700 banks during the period that we consider.

[18]The census tract identifier is removed in the publicly available data for confidentiality reasons but is available internally at the Federal Reserve Board.

[19]The CBP data often report employment only in brackets. We use the method in Autor et al. (2013) to estimate the employment numbers within brackets. The code is available here: `http://www.ddorn.net/data.htm`

adequacy and to support stress test models, and the loan schedule (Y14Q-H1) requires banks to report any loan on their balance sheet with a commitment of 1 million USD or more. Data include loan characteristics such as interest rates and the date at which those can be re-set, collateral requirements, and the purpose of the loan. Moreover, the data allow to distinguish between term loans and credit lines; and include borrower characteristics, such as total assets, total debt, and capital investment. Firms can be followed across banks in the data set by their tax identification numbers. Importantly, relative to other data sets (such as DealScan), this data set includes syndicated loans but also many smaller and/or non-syndicated loans, and therefore provides for a much broader coverage of firms than has been available in previous studies.

When we look at employment effects at the firm-level, we match the loan registry data with firms' annual employment figures from Compustat based on tax identification numbers. Analysis of firm-level employment effects is therefore restricted to firms that can be matched in both datasets, and the overlap amounts to roughly 2000 companies.

3 Bank-level analysis

3.1 Empirical Strategy

The first step of our analysis provides evidence of the effect of the Federal Reserve's LSAPs on the lending of commercial banks. Our analysis in this section essentially extends existing work by Darmouni and Rodnyansky (2017) and Kurtzman et al. (2017), who exploit the cross-sectional variation in banks' MBS holdings in order to identify the effect of QE on bank behavior. While the empirical strategy applied at the bank level is similar to those in existing contributions, we provide additional insights using different data on mortgages and small business lending. These additional insights turn out to be crucial when explaining the employment effects of QE.

The Federal Reserve's first and third round of quantitative easing contained large-scale purchases of MBS.[20] Previous studies have shown that the Fed's actions had a considerable effect on MBS prices and operated through a "narrow channel" (Krishnamurthy and Vissing-Jorgensen (2013)). These stylized findings pave the way for an identification strategy based on the cross-sectional variation of MBS holdings, with a bank's MBS holdings being used as a measure of the bank's exposure to the Fed's policy.

[20] A detailed account of the events can be found in Appendix A

We measure this exposure as the ratio of the sum of a bank's MBS available for sale and MBS intended to be held until maturity over a bank's total assets.[21] The average share of a bank's assets in MBS is about 8% during our sample period; see Table 1. Moreover, there is considerable variation across commercial banks' MBS holdings, as depicted in Figure 1.

[TABLE 1 AND FIGURE 1 ABOUT HERE]

In order to assess the effect of the Fed's actions on bank lending, we employ a difference-in-differences (DiD) design with a continuous treatment variable. The unit of observation is the commercial bank and the main specification is given by:

$$\ln(y_{bt}) = \alpha + \beta \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b^{(j)} \text{QE}_t^{(j)} + \theta X_{bt} + \gamma_b + \tau_t + \epsilon_{bt} \tag{1}$$

Here, $y_{b,t}$ is the amount of lending of bank b at time t. We use different categories of lending in our regressions, including total lending, mortgage lending, and C&I lending. Moreover, we distinguish between newly originated and refinanced mortgages as well as between small business loans of different sizes. We estimate the regression for each episode of quantitative easing, $j = 1, 2, 3$, with a time window of four quarters before and after the introduction of the respective program.[22] $\text{QE}_t^{(j)}$ is an indicator variable equal to 1 after the introduction of the j-th round of quantitative easing.[23] We measure bank b's MBS-to-assets ratio, $\left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)}$, as the average ratio over the 4 quarters prior to the j-th round of QE.

Additionally, our regression includes bank-fixed effects, γ_b, and time-fixed effects, τ_t, to control for fixed differences between banks and for differences over time that affect all banks. We include bank-specific time-varying controls X_{bt} to control for remaining differences between banks. Controls included in the regression are listed in Table 1.

Two main concerns are associated with the above specification: On the one hand, banks could anticipate the Fed's actions and strategically build up MBS holdings. This concern is, however, alleviated by the fact that MBS holdings are extremely rigid over time, as shown by Kurtzman et al. (2017). On the other hand, a more serious concern is that banks with higher MBS holdings are generally distinct than those with lower MBS holdings. Table 2 gives a sense of the differences across the two groups across the

[21] In our main analysis, we focus on agency-MBS as only those MBS were eligible for purchases by the Federal Reserve.

[22] All results are robust to changing the time window as well as to pooling events in a single regression.

[23] Given that the data on the commercial bank level is quarterly, we choose 2009Q1 as the event date for QE1, and 2010Q4 and 2012Q4 as the event dates for QE2 and QE3, respectively.

entire sample period, splitting the sample by the median MBS holdings in the 4 quarters prior to QE1.

[TABLE 2 ABOUT HERE]

Table 2 reveals that while banks with higher MBS holdings tend to be larger and tend to a have a higher leverage than low MBS banks, the two groups of banks are otherwise very similar. Regressions below include time-varying controls in X_{bt} in all specifications to alleviate concerns that results could be driven by differences in observables.

3.2 Results

Table 3 estimates Equation (1) for three different dependent variables: the natural logarithm of the overall amount of lending, of the amount of mortgage lending, and of the amount of C&I lending.

[TABLE 3 ABOUT HERE]

We make three observations about the results depicted in Table 3. First, for total lending volume, the interaction term between QE and a bank's MBS-to-asset ratio prior to QE is positive and statistically significant for QE1 and QE3. This suggests that banks with large MBS holdings issued relatively more loans after QE1 and QE3. Second, focusing on real estate lending and C&I lending, we find that real estate related lending increased more at affected banks after QE1 and QE3, while C&I lending increased only after QE3. Third, there is no effect on lending during QE2. [24] Finally, the results are economically sizable. For instance, moving a bank from the 25th percentile of the MBS distribution to the 75th percentile implies an increase in C&I lending of about 1.3% after QE3. The overall pattern of our results confirms the findings of Darmouni and Rodnyansky (2017) of a stimulating effects of QE1 and QE3 on bank lending.

However, our analysis goes further. Using data collected under the HMDA and aggregated to the bank-level, we provide further evidence on the effect on lending for residential housing. Importantly, the

[24] This is consistent with the focus of the different programs on MBS purchases (QE1, QE3) and U.S. Treasury purchases (QE2). We should not expect QE2 to affect banks with high and low MBS shares differently via a narrow channel. Note that banks generally hold less Treasuries than MBS, and the price effect on Treasuries is generally weaker. Note also that lowering Treasury yields, as done by QE2, can yet affect bank lending: mortgage rates move with Treasury rates, and hence QE2 could spur refinancing in the mortgage market. Indeed, the positive coefficient for overall lending and mortgage lending, Column (2) and Column (5), point to a weak effect on mortgage-related lending. However, the absence of a significant effect can be rationalized by the high refinancing activity after QE1 and the relatively weaker effect on long-term rates of QE2 compared to QE1 (see Gilchrist et al. (2015)).

(confidential) version of HMDA data that we use, allows us to estimate regressions at the quarterly level (rather than the annual level which is the frequency in public HMDA data) to get a more precise sense of the timing of effects. Table 4 shows the estimates from Equation (1), but uses refinanced mortgages as well as newly issued mortgages as dependent variables. Columns (1) and (2) confirm the results for total real estate lending from the Call Report data in Table 3. The additional specifications distinguish between new origination of mortgages and refinancing of existing mortgages. Even though aggregate lending related to housing increased during QE1 and QE3, the underlying type of lending is different. In particular, results in columns (3) and (4) reveal that the effect during QE1 is driven by increased refinancing activity of affected banks, consistent with the findings by Di Maggio et al. (2016), who show that QE1 spurred refinancing activity in the mortgage market. Columns (5) and (6) show that the effect of QE3 is driven by origination of mortgages for new home purchases and by refinancing of existing mortgages.

[TABLE 4 ABOUT HERE]

Using additional data collected under the Community Reinvestment Act (CRA) on small business lending, we estimate the main specification using four related types of small business lending as dependent variables: loans to small business with face value of $ 0 to 100k, $ 100k to 250k, and $ 250k to 1m, as well as loans to businesses with an annual revenue of less than $ 1 million. Table 5 shows results. Consistent with the results in Table 3, small business lending does not respond in any category after QE1 and estimated coefficients fluctuate widely (columns (1), (3), (5), and (7) of Table 5). For QE3, however, we find consistent effects across all categories (columns (2), (4), (6), and (8)): Coefficients are only significant in two of the categories but are positive and of similar magnitude across all of three of them. In particular, note that the coefficient for loans with a face value between 250k to 1m, the category with the highest aggregate volume, is significant.[25]

[TABLE 5 ABOUT HERE]

A bank that holds 12% of its assets in MBS instead of having no MBS holdings, increased the issuance of small business loans with a face value between 250k and 1m by about 4% after QE3. The magnitude

[25] Note that similar results are found when using the Call Report data on small business loans, which has quarterly data around QE3. To this end, see Table 15 in the appendix.

is comparable to the magnitude of the effect on total C&I lending in Table 3.[26]

The overall picture that emerges is that QE1 is associated with an increased refinancing of existing mortgages, and QE3 with additional lending to firms, in particular small businesses, and additional origination of mortgages for home purchases. Two important questions arise: First, what is the channel that led to additional bank lending? Second, why are the effects different across the different rounds of QE? With respect to the first question, the most convincing argument is the portfolio re-balancing channel: given that low-yield assets, such as reserves, are not perfect substitutes to higher yielding assets, such as MBS, large increases in the supply of central bank money can induce banks to invest in more higher yielding assets. This can be done, for instance, by issuing new loans or mortgages.[27]

With respect to the second question, note that the additional C&I lending and additional mortgage lending during QE3 is fully consistent with a portfolio re-balancing channel. Moreover, the refinancing effect during QE1 is somewhat unsurprising, given that the steep decline in interest rates before QE1 gave strong incentives for household and banks to refinance existing mortgages: refinancing allowed households to benefit from lower interest rates (Di Maggio et al. (2017)) and banks to create MBS eligible for purchase from the Federal Reserve (Di Maggio et al. (2016)). In contrast, from a portfolio re-balancing channel perspective, it is somewhat more surprising that QE1 did not lead to additional C&I lending. One can only speculate about the absence of this effect. One explanation could be that the pessimistic economic outlook may have led to a depressed demand for C&I loans and mortgages for home purchases. This could explain the absence of additional C&I lending and mortgage origination even if banks were willing to supply more credit.

4 County-level Analysis

4.1 Empirical Strategy

The central question in this study concerns the effect of unconventional monetary policy on real outcomes such as employment. Any answer to this question immediately faces a challenging identification problem: At the national level, realized outcomes in the absence of unconventional monetary policy are not observed. Without making strong assumptions, it is therefore impossible to identify a causal effect of unconventional monetary policy on the economy from aggregate data alone.

[26]Note that the CRA data reports flow but the call reports stock. The comparison is based on the coefficient reported in the regression using Call Report data,Table 15 in the appendix.

[27]See Bernanke and Reinhart (2004) on a general discussion on how a central bank can operate at the zero-lower bound.

Motivated by the bank-level results in the previous section, we proceed in two steps. We start by analyzing whether the activity of affected bank correlates with increased employment growth at the local level. In order to analyze the effect on local employment, we exploit spatial variation in the presence of banks with different MBS shares prior to quantitative easing episodes. In particular, we define a county's exposure to QE as

$$\text{Exposure}_c^{(j)} = \sum_b w_{bc}^{(j)} \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b^{(j)} . \tag{2}$$

In eq. (9), $w_{bc}^{(j)}$ is the activity share of bank b in county c prior to QEj, where a bank's activity is measured by either its total deposits in county c, its total small business lending in county c , or its total amount of mortgage lending in county c prior to QEj. The activity share is computed as bank b's deposits (or small business loans, or mortgage lending) in county c relative to the total deposits (or total small business loans, or total mortgage lending) of all banks active in county c. Finally, a county's exposure is a bank-activity weighted average of bank-specific MBS shares. Since MBS are held by the respective commercial bank, the MBS share varies only at the bank-level and not at the bank-county level.

A few comments about our measure are in order. First, the measure's main degree of freedom is the number of periods before QE over which the activity share is calculated. In our robustness checks, we show that results are invariant to this choice. Second, to the extent that other bank characteristics are correlated with its MBS share, these will be picked up by our measure as well. However, this would not change the interpretation of our results: We are merely interested in constructing a proxy that generates spatial variation in QE exposure. Third, for these reasons, our measure, while continuous in nature, is probably best thought of as producing an ordinal ranking of counties' exposures. In our main specifications, we report results based on the continuous measure above as well as results based on grouping counties into terciles of the exposure measure.[28]

Figure 2 shows our measure based on small business lending across U.S. counties before QE1.[29] The spatial distribution does not seem to follow a systematic pattern, except for a cluster of counties with higher values of the exposure variable in the north east. In robustness checks, we show that results do not depend on including the north east in the estimations.

[28]Note that the main results are robust to using quartiles or quintiles instead of terciles.

[29]Since MBS shares are very persistent within banks over time (see Darmouni and Rodnyansky (2017) or Kurtzman et al. (2017)) and spatial variation in geographical activity of banks is very stable over time as well, the map looks qualitatively very similar at other points in time.

[FIGURE 2 ABOUT HERE]

We run the following difference-in-differences specifications around each quantitative easing episode:[30]

$$y_{ct} = \alpha + \beta \times \text{Exposure}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \theta X_{ct} + \epsilon_{ct} \tag{3}$$

$$y_{ct} = \alpha + \beta \times \text{Treat}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \theta X_{ct} + \epsilon_{ct} \tag{4}$$

Here, y_{ct} is an outcome in county c at time t, γ_c and τ_t denote county and time fixed effects, and X_{ct} denote county-level time-varying controls. As time-varying controls, we use the natural logarithm of the population and the median income, and the level of housing prices.[31] Equation (3) uses the continuous exposure measure directly, while Equation (4) uses a binary version with $\text{Treat}_c^{(j)}$ equal to 1 if a county's exposure is in the upper tercile of the exposure distribution over all counties, and 0 if it is in the lower tercile of that distribution.

Our outcome variable is a county's employment growth. County-level employment is measured quarterly in the QCEW data and annually in the CBP. To exploit the timing of quantitative easing episodes as precisely as possible, we use quarterly data in the main body of the analysis but confirm the results with the annual CBP data in the appendix. We compute the four-quarter harmonized growth rate as

$$\Delta \text{Emp}_{ct} = \frac{\text{Emp}_{ct} - \text{Emp}_{c,t-4}}{0.5(\text{Emp}_t + \text{Emp}_{c,t-4})}. \tag{5}$$

The success of our empirical strategy depends on a number of assumptions. First, as in any difference-in-differences design, outcomes need to evolve similarly absent treatment, i.e., follow parallel trends. Below, we provide suggestive evidence that counties with high and low MBS exposure followed similar trends before quantitative easing episodes.

Second, relevant in our specific setup, our measure of a county's MBS exposure is a proxy for its actual exposure, and any definition that we use might introduce measurement error in the regressor, leading to attenuation bias in the estimated coefficients. While our results are largely unaffected by the exact definition, this concern is an additional reason to focus on eq. (4), our specification with a binary

[30] We use 13 quarters for each regression, six quarters before to six quarters after the event.

[31] We collect county population and county income from Haver Analytics and county-level housing prices from Moody's. In our main specification at the county-level, we restrict to counties with a population no less than 2500 registered inhabitants and no more than 1m.

treatment: Even if there is measurement error in the continuous variable, that should not affect the ordinal ranking of counties much, especially if, as we do, one compares counties in the highest tercile of the MBS exposure distribution to the ones in the lowest tercile.

Third, and related to the previous point, the success of our strategy also depends on the extent to which bank lending markets are local. Throughout the main body of our analysis, we define each county as a local market. If there were no frictions in lending across regions, our bank-level results in section 3 should not extend to the regional level, as a bank with a high MBS share would be, conditional on local loan demand, equally likely to increase lending in any region. Existing evidence suggests that the markets for C&I and small business loans tend to be local (see, e.g., Brevoort et al. (2010), Greenstone et al. (2014), and Nguyen (2015).) In the case of mortgage lending activity, it is more difficult to argue that markets are local and the existing evidence is ambiguous. Beraja et al. (2016) do not find regional frictions in mortgage lending, while Scharfstein and Sunderam (2014) find that mortgage lending is characterized by local markets.

4.2 Main Results

Figure 3 shows the employment growth of counties with high and low MBS exposure, defined as the upper and lower tercile of the distribution, during our sample period, with the dashed vertical lines denoting the QE1 and QE3 events, respectively. The figure reveals, that both types of counties experienced very similar employment growth rates during the recession and throughout much of the recovery. This helps to validate our empirical design in eq. (3) and eq. (4) as the parallel trends assumption appears to hold. Employment growth rates diverge, however, two quarters after the start of QE3. At up to 40 basis points per quarter, the difference is sizable. Note that accumulation of the quarterly differences in employment growth rates adds up to even larger differences in employment levels.

[FIGURE 3 ABOUT HERE]

Panel A of Table 6 presents our main results and provides a more rigorous analysis of the visual patterns in Figure 3. We estimate eq. (3) and eq. (4) around QE1 and QE3, controlling for county- and time-fixed effects and county-level time-varying characteristics. In line with Figure 3, we do not find any effect around QE1; the coefficient estimates are small and insignificant. Effects around QE3, though, are larger and significant. Estimates based on the binary treatment specification suggest that county's in the upper tercile of the MBS distribution experienced 60 basis points higher employment growth than counties in the lower tercile of the distribution, after accounting for other time-varying and time-invariant county characteristics. Panel B of Table 6 confirms the results when we use the lending-based measure of exposure instead of the deposits-based measure in the regressions.[32]. Finally, consistent with the result at the bank-level, no employment effects can be documented for QE2, see Table 17 of the appendix.

[TABLE 6 ABOUT HERE]

While the results in table 6 report average effects, before and after quantitative easing episode, Figure 4 illustrates the timing of the effect more precisely. To this end, we interact time effects with the exposure variable and estimate the following regression:

$$\Delta \text{Emp}_{ck} = \alpha + \sum_{k=-6}^{6} \beta_k^{(j)} \text{Treat}_c^{(j)} \tau_k + \theta X_{ck} + \gamma_c + \tau_k + \epsilon_{ck}. \tag{6}$$

In eq. (6), time (k) is measured relative to the start of each QE episode, and the regression includes county controls and county and time fixed effects as before. We normalize coefficients to 0 in the period before the start of QE (i.e. in $k = -1$).

Figure 4 shows estimates for QE1 and QE3. First, note that there is no discernible pre-trend before either QE1 or QE3, providing support to the parallel trends assumption. In line with the result on the average effect for QE1, coefficient estimates are small and insignificant in each quarter following the start of QE1. For QE3, we see an uptick of employment growth that becomes statistically significant two quarters after QE3 started.

[FIGURE 4 ABOUT HERE]

Recall that QE3 was implemented in September 2012. The employment effect therefore becomes apparent between 6 and 9 months after the implementation. The literature has found a delayed effect of conventional monetary policy on real outcomes, with the delay typically estimated to be between 6 and 24 months (see Christiano et al. (1999) for an overview, and Olivei and Tenreyro (2007) for a recent study). Our estimate for unconventional monetary policy is at the lower end of that range. The question

[32] Findings are also unchanged when we use annual employment data from the CBP instead of the quarterly QCEW data. Results are in Figure 6 and Table 16 in the supplementary appendix.

of whether this is true for unconventional monetary policy more generally can only be answered from a larger set of episodes of unconventional monetary policy implementation.

4.3 Magnitude

To summarize, we find that small business lending and mortgage origination increased in arguably more exposed areas after QE3, while only mortgage refinancing activity increased in those areas after QE1. Our estimates imply that employment growth was approximately 60 basis points higher in more exposed counties, while small business lending growth was about 13 percentage points higher and growth in mortgage origination was about 4 percentage points higher.

To put our estimates in context, we compare them to the literature on bank lending and employment. Haltenhof et al. (2014) find that a 1 percentage point increase in business lending standards is associated with a 4 basis points decrease in employment for more bank-dependent industries. Extrapolating their estimates, a 13 percentage point increase in business lending standards would be associated with a 52 basis points decrease in employment, roughly in line with our estimates for lending volume and employment growth during QE3.

To get a better sense of the economic significance, we put our estimates in the historical context. Recall that the U.S. economy was in a recovery phase from the financial crisis during 2012. Our estimates suggest that employment growth was 60 basis points higher in counties that were more affected by QE. Total employment in the upper tercile of counties by MBS exposure before QE3 was roughly 37.6m in September of 2012 and 38.3m a year later, resulting in an employment growth rate of 1.7 percent, such that roughly a third of employment growth in more affected counties (or, equivalently, 200k new jobs) can be attributed to the additional lending induced by QE3.

Note that, due to the cross-sectional nature of our analysis, we cannot conclude we are measuring an aggregate employment effect of QE. To illustrate why this is the case, consider two extreme cases. At one extreme, the effect could be merely redistributive: jobs that would have been created in unaffected counties, were, due to QE, created in affected regions instead. At another extreme, the aggregate effect may be larger than what we are observing as there may be complementarity between different regions: QE induced job creation in some affected regions that could have spurred additional economic activity in unaffected regions. Observe that the results also hold at the MSA level (see Section 6). Assuming that labor market mobility is relatively high within MSA's but low across MSA's, the documented effects are thus unlikely to be purely redistributive.

5 Channel

In this section, we attempt to shed light on whether the increase in employment can be attributed to increases in bank lending. To support the link between the increases in bank lending and employment, we start out with showing that, around QE1 and QE3, lending expanded more in more exposed counties, suggesting that the channel from lending to employment was operative during that period. We then show that the increased lending at the county-level can indeed be attributed to an increase in bank supply of credit rather than county-specific demand. Moreover, we provide additional evidence suggesting that the employment increase is more likely to stem from additional C&I lending rather than from additional origination of home purchase mortgages. Finally, we turn towards the analysis of confidential loan-level data that allow us to observe bank-firm relationships directly and to analyze how firm investment and employment change as a response to QE3.

5.1 Linking Bank Lending and Employment

We start out by showing that the lending effect at the bank level passes through to the county level. To this end, the next set of tables shows estimates of our main regression specifications, Equation (3) and Equation (4), for lending outcomes at the county-level. We consider changes in three different types of lending: small business loans with face value between $250k and 1 million, mortgage origination, and refinancing of existing mortgages, denoted as ΔC&ILending, ΔOrigination, and ΔRefinancing, respectively. As above, the data on mortage lending is at a quarterly frequency, while the data on small business lending is at an annual frequency. We compute the annual/quarterly harmonized growth rate similar to the growth rate of employment in Equation (5).

Table 7 estimates difference-in-difference regressions using small business lending growth as the outcome variable for both the lending-based (Panel A) and the deposit-based exposure measures (Panel B). Mirroring results for employment, we do not find any effect on small business lending around QE1. This is unsurprising, given that Section 3 showed that banks did not extend small business lending after QE1. Hence, there should be no regional variation in small business lending if markets are local. The fact that the non-result extends from the bank- to the county-level is re-assuring in that it is consistent with treating county's as separate banking markets. For QE3, bank-level results extend to the county-level as well: Effects around QE3 are sizable and significant. Counties in the upper tercile of the exposure distribution experienced roughly 5-6 percentage point higher lending growth than counties in the lower tercile of the distribution after QE3.

[TABLE 7 ABOUT HERE]

Table 8 displays the same regression specifications with mortgage lending variables as outcomes. In line with the bank-level results, we find that mortgage refinancing activity increases more in more exposed counties after QE1 but we do not find consistent effects on mortgage origination activity. The pattern reverses around QE3: We find that mortgage origination increased in more exposed counties but refinancing activity was unaffected. Counties in the upper tercile of the exposure distribution experienced roughly 2.5-4 percentage point higher mortgage origination growth than counties in the lower tercile of the distribution after QE3.

[TABLE 8 ABOUT HERE]

The evidence presented so far hence indicates that the increased lending during QE1, which consisted mostly of refinancing of existing mortgages did not coincide with additional employment growth. In contrast, the increased lending during QE3, which consisted mostly of new mortgage origination and small business loans, did coincide with additional employment growth.[33]

5.2 Separating supply and demand in the credit market

The results so far do not separate credit supply effects from credit demand effects. In other words, the increase in employment might stem from firms having better access to local credit or from increased demand for credit, for instance, because QE affected household net-worth (Beraja et al. (2016)), which in turn could have stimulated local consumer demand.

To address this issue, we start out by controlling for county-specific credit demand by estimating county-bank level regressions, in the spirit of Khwaja and Mian (2008). Since multiple banks are active in the same county, we can identify supply effects from the following regression:

$$y_{bct} = \alpha + \beta \times \left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)} \times \text{QE}_t^{(j)} + \gamma_{ct} + \delta_{bc} + \theta X_{bt} + \epsilon_{bct} \tag{7}$$

[33] The correlation between growth in different types of lending and employment is also supported by the fact that, historically, local employment increases are correlated with growth in local lending. Consider evidence from a panel regression that relates employment and different types of lending for the period from 2000 to 2014, reported in Table 18 in the appendix. The results show that there is a positive correlation between local increases in small business lending and mortgage origination on the one hand, and employment on the other hand. The relationship between employment and mortgage refinancing is weaker and non-significant once other lending types are controlled for. While this result is a mere correlation and does not speak to the direction of the link, several studies document the importance of small business lending for employment, see, e.g., Hurst and Pugsley (2011).

Here, the outcome y_{bct} is the amount of lending by bank b in county c at time t. δ_{bc} is a bank-county fixed effect. γ_{ct} is a county-time fixed effect that absorbs all local economic conditions, including county-specific credit demand. We use three different categories of lending as outcomes: the growth of small business lending, the growth of refinanced mortgages refinanced and the growth of newly originated mortgages. As above, the regression is estimated for a time-window around the respective round of QE.

[TABLE 9 ABOUT HERE]

Table 9 shows results. Regressions in columns (2) and (4) include county-time fixed effects. First, note that, across all panels, coefficient estimates do not change much when county-time fixed effects are included, suggesting that the potential bias from local demand effects is low. Consistent with our main results, we find that C&I lending (panel A) and mortgage origination (panel C) increased at more affected banks after QE3 but only mortgage refinancing increased after QE1 (panel B). Overall, Table 9 suggests that local demand is not alone driving our results. Instead, QE1 and QE3 led banks to expand local lending via a credit supply channel.[34]

5.3 Separating supply and demand in the labor market

Even if the increase in local mortgage origination and small business lending can be attributed to an increase in credit supply, it is not clear which one of the two is more important for generating additional employment.While both channels may be relevant at the same time, the former would work via changes in local demand for consumption,[35] and the latter would work via improved financing conditions for local firms.[36] In other words, QE may operate via spurring local demand if the improved access to mortgages for households increases housing net-worth, or QE may operate via spurring labor supply if improved access to credit for local firms increases their labor (and capital) investment.

In order to shed light on which of the two channels is more important, we consider the employment effect of QE3 by industry. Mian and Sufi (2014) show that an increase in local demand, e.g., related to

[34] Note that while the effect of additional credit supply for refinancing of mortgage refinancing is somewhat surprising, as refinancing in low interest environments is more likely to be demand-driven, as households have strongest incentives to refinance, see Di Maggio et al. (2017). However, note that the refinancing subsequent to QE1 also allowed banks to issue new MBS that were eligible for sale to the Federal Reserve. To this end, see Kandrac and Schlusche (2015).

[35] See, for instance, Mian and Sufi (2014) on the importance on household net-worth for employment. Moreover, see Garcia (2017) for evidence on how the decline in mortgage lending affected employment during the Great Recession.

[36] See, e.g., Chodorow-Reich (2014).

household net worth, is more likely to affect non-tradable goods sector employment than employment in other sectors. The underlying idea is that non-tradable employment relies heavily on local demand, whereas tradable goods related employment is related to aggregate demand. Using that insight, we distinguish between employment growth in the non-tradable goods sector and employment growth in other sectors. If increases in local consumer demand rather than improvements of financing conditions for local firms are driving the results, we should see a stronger effect for non-tradable employment (which is arguably more correlated with rises in household net-worth).

We first estimate the main specification, Equation (3) and Equation (4), for other sectors, and non-tradable employment (denoted by ΔEmpTradOther and ΔEmpNonTrad) separately, and use the definitions of non-tradable goods sectors and other sectors from Mian and Sufi (2014).[37] Because the QCEW data report many missing values for employment by industry, we focus on the annual CBP data in this part of the analysis.[38] The results are reported in Table 10, Panel A.

[TABLE 10 ABOUT HERE]

The results show that the employment effect during QE3 is mostly driven by employment growth in sectors other than the non-tradable goods sector. To see this, compare columns (5) and (6) with (7) and (8). We interpret this as suggestive evidence, that the increase in employment is more likely linked to improved financing conditions for local firms rather than to increases in local demand.

Notably, while the increased refinancing activity during QE1 is not associated with an increase in the aggregate growth of employment, it is associated with an increase of the growth rate of employment in the non-tradable goods sector. This findings is in line with the logic of Mian and Sufi (2014), as described above. The additional refinancing of existing mortgages positively affects household net-worth and therewith employment in the non-tradable goods sector. However, even though the effect is statistically significant, the effect does not translate into additional growth in aggregate employment. This can be attributed to the fact that only 20% of the work force are employed in the non-tradable good sector.

As another piece of evidence, we investigate whether the employment change is concentrated in industries that are more dependent on external financing. If so, that would also suggest that credit supply forces were more relevant than local demand forces to drive the effect. As a measure of external

[37]Mian and Sufi (2014) calculate that in 2007, around 20% of all employment is in the non-tradable goods sector, 10% are in the tradable goods, 60% are defined as "'others", which mainly contains the service sector that does not offer non-tradable goods, and another 10% are in construction.

[38]All results are robust to using the quarterly QCEW data.

financial dependence, we use the definition of Almeida et al. (2010), which gives an ordering of financial dependence of all industries. We define an industry as dependent on external financing if it is in the upper tercile of the financial dependence distribution and non-dependent if it is in the lower tercile. Using the industry codes in the CBP data, we construct the change in employment by county and financial dependence, denoted ΔEmpFin and ΔEmpNonFin.

Panel B of Table 10 reports results from estimating our main specification, Equation (3) and Equation (4), using these outcome variables. The results in column (5) and (6) indicate that employment growth subsequent to QE3 comes from employment growth in financially dependent industries, as opposed to financially independent industries, see column (7) and (8). Again, this suggests that the main employment effect is driven by improved credit supply. Moreover, in line with the absence of an aggregate employment effect during QE1, no effect on employment in either industry is detected during QE1.

5.4 Evidence from confidential loan-level data

In this section, we provide further evidence for the link from QE to bank lending to firms from confidential loan-level data that are part of the Y14 data collection. The advantage of using this dataset is that it provides a direct link between banks and firms, and firms can be followed across banks via their tax identification numbers, which allows us to control for firm-specific credit demand when analyzing lending at the bank-firm level. Moreover, the data include information on firms' capital investment and therefore allow us to trace the impact of lending on real firm activity directly. The drawback of the data is that collection started only in 2012, too late for QE1 (and QE2) and only includes corporate lending from the largest banks. Still, total volume of C&I lending covered is roughly 75% of total C&I lending by banks.

We start with quarterly bank-firm level regressions that allow for bank-firm and firm-time fixed effects in the spirit of Khwaja and Mian (2008). Exploiting the fact that banks borrow from multiple banks in the same time period makes it feasible to control for firm-specific credit demand as follows:

$$y_{bit} = \alpha + \beta \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b^{(j)} \text{QE}_t^{(j)} + \gamma_{it} + \delta_{ib} + \theta X_{bt} + \epsilon_{bit} \tag{8}$$

where y_{bit} is the (quarterly) of the amount firm i borrows from bank b from time $t-1$ to time t, γ_{it} is a firm-time fixed effect that absorbs firm-specific demand, and δ_{ib} is a bank-firm fixed effect that controls for relationship-specific unobservables between bank b and firm i. Equation (8) requires multiple observations of the same firm within one time period and hence the sample is restricted to firms that borrow from multiple banks. Given the credit-registry like nature of our data, our datasets includes several thousand such firms, an order of magnitude larger than in previous applications that relied on publicly available data of syndicated loans in the United States.

Table 12 shows results for total lending, term loans and credit lines for each bank-firm pair. Given the log-linear specification, coefficients can be interpreted as percentage changes. For instance, the estimate in column (1) implies that the total lending volume to a firm from a bank with an MBS share of 10% (.1) increases by 3.4%. The specification in column (3) includes firm-time fixed effects and suggests that about half of the effect can be attributed to credit demand, but the credit supply effect is still sizable and statistically significant. The remaining columns split total lending into term loans and credit lines. The estimates suggest that the entire effect can be attributed to an increase in term loan volume rather than credit lines.

TABLE 12 ABOUT HERE

Using additional information on capital expenditures from the Y14 C&I schedule, we can further analyze how QE3 affected firms' investment decisions. We also analyze firms' employment decisions for the subset of firms that we can match with Compustat.

We first calculate a firm's exposure to QE3 as

$$\text{Exposure}_i = \sum_b w_{bi} \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b , \tag{9}$$

where $\left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b$ is the average MBS share of bank b over the four quarters prior to QE3, and w_{bi} is the average total lending volume from bank b to firm i over the same period.[39]

We then run a cross-sectional regression of investment or employment changes from before to after QE3 on firm i's exposure:

[39]We have checked that results are robust to alternative exposure measures, e.g. calculating the exposure as the MBS share of only the most important bank/lead bank for firm i.

$$y_i = \alpha + \beta \text{Exposure}_i + \theta X_i + \epsilon_i \quad (10)$$

where y_i is either the annual growth in investment at the firm level from 2012Q4 to 2013Q4, or the annual and the two-year growth rate of employment at the firm level.[40]

Panel A and B of Table 13 show results for investment and employment, respectively. Starting with Panel A, firms that were more exposed to banks with a higher MBS share, increased investment more after QE3 for both the subsample of public firms from Compustat and for the larger sample of firms in the Y14 data. As the interquartile range of the exposure measure is about .035, the coefficient estimates imply that a firm at the 75th percentile of the exposure distribution increased investment by about 4 percentage points more than a firm at the 25th percentile of the distribution.

Panel B shows that more exposed firms also increased employment relatively more after QE3. Because employment is not available in the Y14 data, results are based on those firms that can be matched to Compustat. Effects in the first three columns show employment growth over the four quarters after QE3, and the remaining columns show employment growth over eight quarters. The employment effect is already detectable in the four quarters after QE3: Employment growth is about 1.5 percentage points higher for a firm at the 75th percentile of the employment distribution than for a firm at the 25th percentile of the distribution. Given that it takes time to hire, estimates are somewhat higher for employment growth over two years.

[TABLE 13 ABOUT HERE]

6 Robustness checks

This section provides additional evidence to show that results are robust across different specifications and data sets. First, we show that the employment effect of QE3 is robust to a number of sample

[40] As before, we calculate the harmonized annual growth rates as

$$\Delta \text{Investment}_i = \frac{\text{Investment}_{2013Q4} - \text{Investment}_{2012Q4}}{(0.5 * (\text{Investment}_{2013Q4} + \text{Investment}_{2012Q4}))}$$

and

$$\Delta \text{Emp}_i = \frac{\text{Emp}_{2013Q4} - \text{Emp}_{2012Q4}}{(0.5 * (\text{Emp}_{2013Q4} + \text{Emp}_{2012Q4}))}.$$

restrictions and to different definitions of the exposure variable. We estimate our main specifications, Equation (3) and Equation (4), using the change in employment as the dependent variable, while varying the sample and the exposure measure.

[TABLE 14 ABOUT HERE]

The results are reported in Table 14. Columns (1) and (2) report results for our main regression when we restrict the sample to counties that are relatively small as our banking market definition is arguably more likely to hold for smaller counties. The visual evidence in Figure 2 suggests that the concentration of banks with high MBS is particularly high in the Northeast corridor of the U.S. . Columns (3) and (4) show that results are unchanged when this region is excluded from the estimation. In columns (5) and (6), we calculate the exposure measure by using the MBS holdings of banks in the four quarters prior to QE1 instead of those prior to QE3. Results are robust to this change, which is unsurprising as MBS holdings are typically highly autocorrelated within bank over time. Finally, columns (7) and (8) report results for calculating the exposure measure based on MBS over total securities instead of MBS over total assets.

Moreover, we show that our main results hold when we define local markets at the MSA level. Table 19 shows the main employment effect, Table 20 documents that small business lending increased in more affected MSA's after QE3, and Table 21 shows that QE1 induced additional refinancing, while induced QE3 additional mortgage origination in affected MSA's (all tables are in the appendix). This addresses the concern that firms may borrow and employees may work across county-lines, and a county may not be a local credit and labor markets.

7 Conclusion

How can monetary policy affect real economic outcomes at the zero-lower bound? Almost a decade after the start of quantitative easing in the United States, the channels by which such monetary policies can affect employment are not yet fully understood and controversial. While existing evidence shows how LSAPs can affect bank lending as well as activity in the mortgage market, the effect of QE on economic activity and employment remains unclear. However, such evidence is particularly important, given that the Federal Reserve's statutory objective is concerned with not only price stability, but also employment.[41] Against this background, our study brings to bear cross-sectional variation across banks and regions to shed light on the effect of unconventional monetary policy on employment.

We show that banks with a higher share of assets in MBS increased lending more after QE1 and QE3, and exploit spatial variation in banks' activity to trace the effect of lending on employment. Our results indicate that while QE1 and QE3 affected bank lending, only QE3 affected employment. While QE1 and QE3 were both successful in spurring bank lending in general, we show that there is considerable variation in what type of lending was expanded. Importantly, we go further than existing work by showing that the type of lending that is expanded is crucial for the ultimate effect on the real economy. In particular, we show that only QE3 led to additional employment, which can be attributed bank extending additional C&I loans. In contrast, the additional refinancing activity following QE1, likely increased household net-worth and spurred local demand. However, this translated only into some additional growth in the non-tradable goods sector, but did not lead to a larger effect on aggregate employment growth.

Altogether, our results indicate that LSAPs as conducted by the Federal Reserve affected employment via a bank lending channel. The evidence presented lends to the view that the Federal Reserve's action, in particular QE3, were helpful for the economic recovery.[42] However, our results also show that the effects of unconventional monetary policy are less predictable than conventional policies and depend on forces that lie outside a central bank's authority. Our findings indicate that LSAPs can induce banks to provide more credit. However, for actual credit and economic activity to expand, the increased supply needs to meet with sufficient demand. Seemingly, this was not the case during QE1, when the economy was still in a downturn.[43] Since the policy tools of the central bank are too broadly defined in order to target specific allocations such as firm investment and household consumption directly, there is no simple mapping between unconventional monetary policy and its ultimate, distributive and aggregate, effects on the economy. Therefore, our results indicate that while the bank lending channel is not entirely impaired at the zero-lower bound, it is less predictable.

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[41] Note that the Federal Reserve Act also explicitly mentions moderate long-term interest rates as a statutory objective.

[42] Interestingly, the Federal Reserve hence achieved the stated goal of QE3. See for instance Ben Bernanke in *The Courage to Act*: "[QE3] was risky. Either we reached our goal of substantial labor market improvement or we would have to declare the program a failure and stop the purchases, a step sure to rattle confidence".

[43] For instance, household balance sheets may have been sufficiently impaired or QE1 may have only affected banks that lend to households with a relatively low marginal propensity to consume.

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A Figures

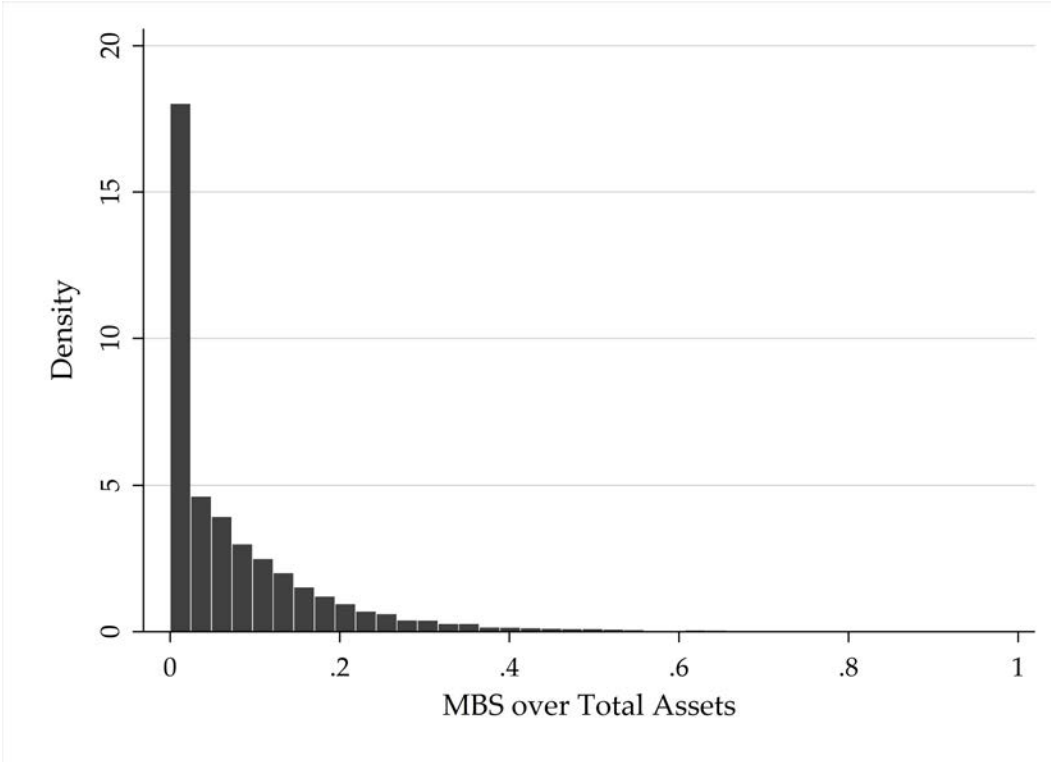


Figure 1: *Distribution of the average MBS shares in the four quarters prior to QE1 over banks.*

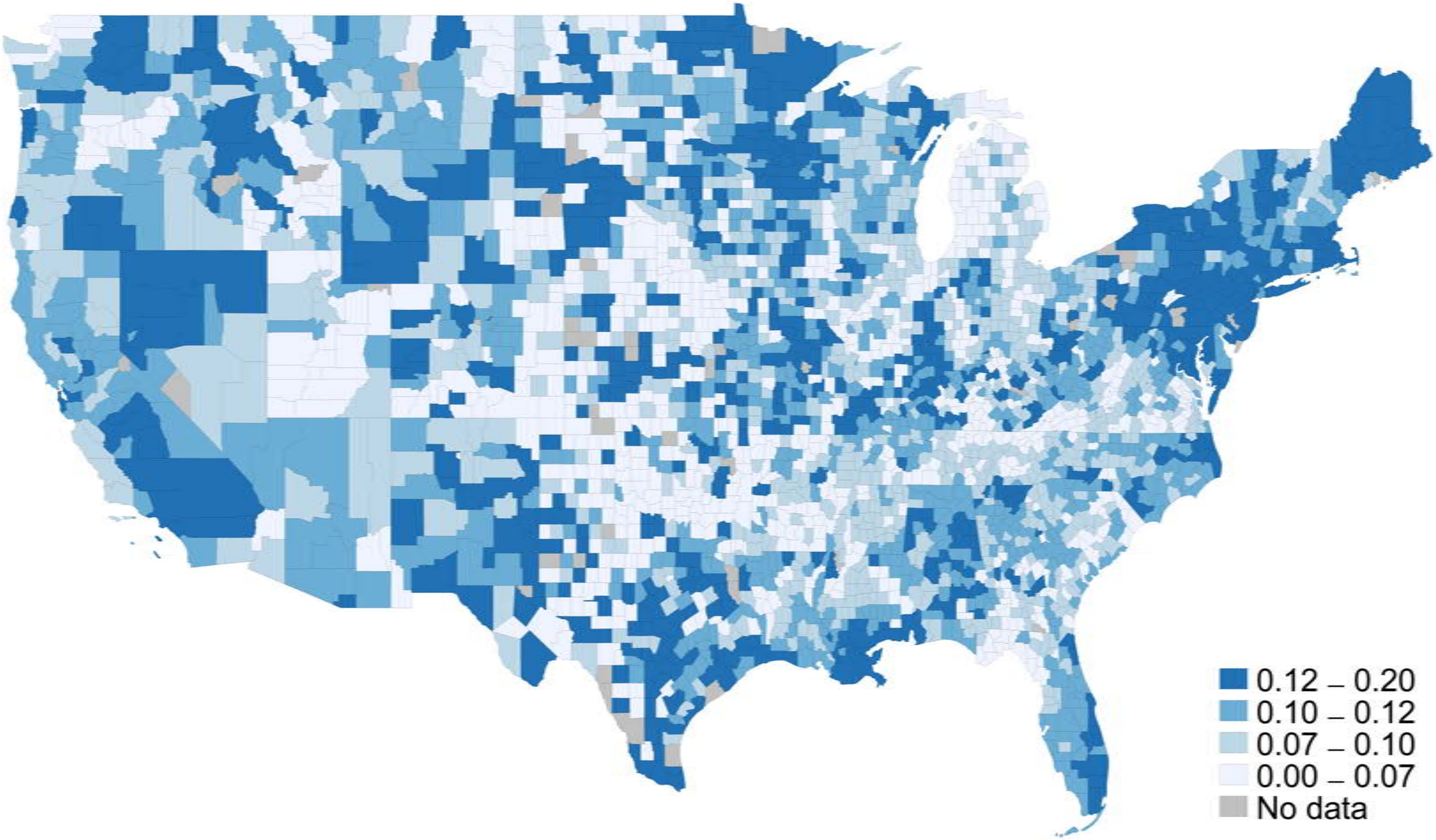


Figure 2: *Spatial variation of* $Exposure_c^{(j)}$ *prior to QE1.*

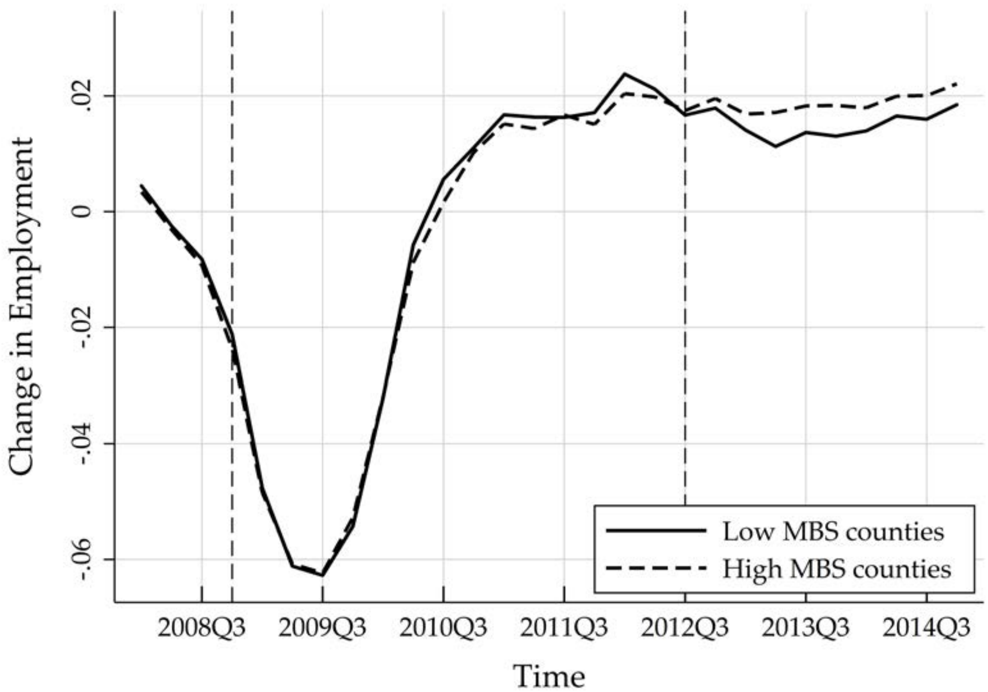
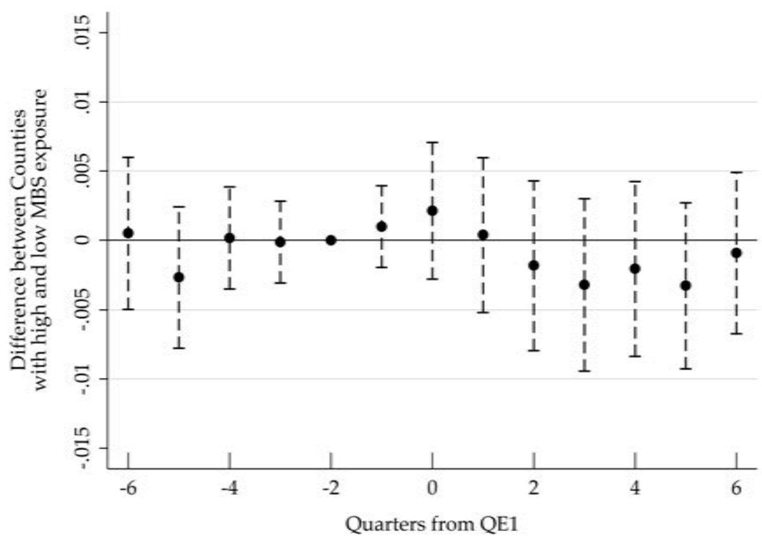
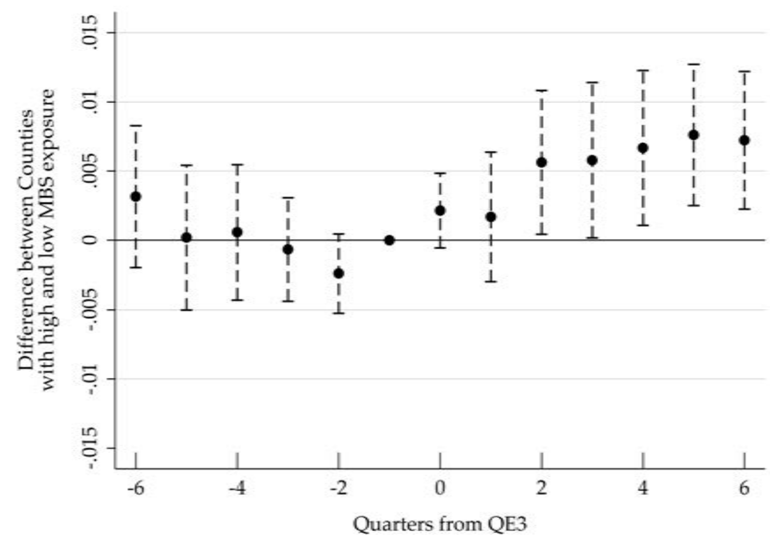


Figure 3: *The figure depicts the annual rate of growth ΔEmp_{ct} in QCEW data for splitting counties into the upper tercile of counties with a high exposure prior to QE3 $Exposure_c^{(3)}$, and the lower tercile. The two vertical lines depict the quarters of the implementation of QE1 and QE3, respectively.*



(a) *QE1*



(b) *QE3*

Figure 4: *This figure plots coefficient and their confidence bands from interacting time effects with the exposure variable and estimating the following regression:* $\Delta Emp_{ck} = \alpha + \sum_{k=-4}^{4} \beta_k^{(j)} Treat_c^{(j)} \tau_k + \theta X_{ck} + \gamma_c + \tau_k + \epsilon_{ck}$. *Time (k) is measured relative to the start of QE1 (QE3), which is in 2009:Q1 (2012:Q4), and the regression includes county controls, county and time fixed effects as in the main specification. We normalize coefficients to 0 in the quarter before the start of QE. Dashed lines denote 95% confidence bands based on standard errors clustered by county and quarter.*

B Tables

Table 1: *Summary Statistics for bank-level control variables*

	Mean	Std	10th Perc	25th Perc	Median	75th Perc	90th Perc	N
MBS/TotalAssets	0.08	0.11	0.00	0.00	0.04	0.12	0.21	145868
MBS/TotalSecurities	0.36	0.34	0.00	0.01	0.31	0.61	0.85	142036
log(assets)	12.00	1.35	10.52	11.16	11.87	12.66	13.57	145868
Equity/TotalAssets	0.12	0.09	0.08	0.09	0.10	0.12	0.15	145868
Deposit Ratio	0.82	0.12	0.74	0.80	0.85	0.88	0.90	145868
Short-term Ratio	0.33	0.14	0.16	0.23	0.32	0.41	0.51	144675
Trading Book Ratio	0.00	0.01	0.00	0.00	0.00	0.00	0.00	145868
Profitability	0.00	0.15	-0.00	0.00	0.00	0.01	0.01	145868
Overhead Ratio	0.83	10.73	0.52	0.61	0.71	0.83	1.01	145855
Net interest margin	0.02	0.26	0.01	0.01	0.02	0.03	0.04	145716
Non-performing Loans Ratio	0.01	0.02	0.00	0.00	0.01	0.02	0.04	145868
Delinquency Ratio	2.22	1.79	0.33	0.91	1.82	3.06	4.59	142202
C&I Loan Ratio	0.15	0.10	0.05	0.08	0.13	0.19	0.27	143985
Real Estate Ratio	0.69	0.19	0.43	0.59	0.73	0.83	0.90	143985
Tier 1 Ratio	0.22	1.89	0.10	0.11	0.14	0.18	0.25	145866

This table reports the distribution of MBS-to-assets and MBS-to-securities ratios over banks and quarters, and of variables used as controls in bank-level regressions. The dataset runs from 2007Q1 to 2015Q2, and includes up to around 7000 banks.

Table 2: *Comparison of banks with high and low MBS shares*

	Low MBS Share		High MBS Share		Difference	
	Mean	Std	Mean	Std	Diff	t-stat
log(assets)	11.748	1.251	12.284	1.407	0.539	17.279
Equity/TotalAssets	0.126	0.109	0.114	0.076	-0.015	-7.709
Deposit Ratio	0.823	0.140	0.824	0.098	0.003	1.334
Trading Book Ratio	0.000	0.010	0.001	0.009	0.000	0.395
Profitability	0.004	0.169	0.004	0.018	-0.001	-1.174
Overhead Ratio	0.769	1.482	0.840	12.475	-0.002	-0.192
Net interest margin	0.022	0.458	0.023	0.079	0.001	0.283
Non-performing Loans Ratio	0.012	0.021	0.012	0.019	-0.000	-1.299
Delinquency Ratio	2.045	1.754	1.998	1.668	-0.040	-1.387
C&I Loan Ratio	0.145	0.108	0.148	0.098	0.004	1.598
Tier 1 Ratio	0.231	1.270	0.215	2.433	-0.027	-0.763

This table compares control variables prior to QE1 from banks with "high MBS" (ratio of MBS over assets) and "low MBS". Banks are split into groups based on the median of their average MBS share in the four quarters prior to the first round of QE.

Table 3: *Bank-level analysis, effect on lending, real-estate loans and C&I loans*

Dependent variable	log(Lending)			log(RE lending)			log(C&I)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	0.071***			0.090***			-0.008		
	(0.024)			(0.032)			(0.037)		
$QE_t^{(2)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(2)}$		0.134			0.031			-0.008	
		(0.109)			(0.052)			(0.095)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$			0.098***			0.090***			0.121***
			(0.026)			(0.027)			(0.036)
R_a^2	0.667	0.493	0.540	0.593	0.487	0.414	0.256	0.159	0.132
No. Banks	6947	6370	6014	6919	6344	5995	6910	6335	5984
N	60931	43941	53081	60666	43755	52903	60520	43668	52787
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The table depicts the results for estimating Equation (1) for using three different dependent variables: the natural logarithm of a banks quarterly total lending as reported by banks the call reports, the total amount of real estate lending (RE lending) and the total amount of lending to commercial and industrial corporations (C&I lending). We restrict the time window in each regression to 9 quarters: 4 quarters before and after the respective start of a respective round of QE. Standard errors in parentheses are two-way clustered at the bank-quarter level and stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 4: *Bank-level analysis, effect on mortgage refinancing and origination of mortgages for home purchases*

Dependent variable	log(Total)		log(Refinance)		log(Origination)	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	0.358**		0.556***		0.238	
	(0.162)		(0.153)		(0.195)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$		0.301***		0.099		0.398***
		(0.112)		(0.169)		(0.092)
R_a^2	0.075	0.077	0.099	0.099	0.091	0.171
No. Banks	4380	3871	4288	3806	4322	3810
N	14512	16351	13844	15652	14088	15847
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The table depicts the results for estimating Equation (1) for three different dependent variables: the natural logarithm of banks' annual total amount of real estate lending as reported in the HMDA, banks' annual total amount of mortgage refinancing and banks' total amount mortgage origination. We restrict the time window in each regression to 9 quarters: 4 quarters before and after the respective start of a respective round of QE. Standard errors in parentheses are two-way clustered at the bank-quarter level and stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 5: *Bank-level analysis, effect on small business lending*

Dependent variable	[0, 100k]		[100k, 250k]		[250k, 1m]		Rev<1mil	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	-0.041		0.044		0.150		0.290	
	(0.142)		(0.141)		(0.143)		(0.190)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$		0.056		0.102		0.390***		0.292**
		(0.105)		(0.117)		(0.121)		(0.128)
R_a^2	0.286	0.101	0.291	0.155	0.271	0.165	0.275	0.109
No. Banks	810	665	810	667	810	667	809	664
N	2517	2225	2514	2227	2512	2235	2517	2211
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The table depicts the results for estimating Equation (1) for four different dependent variables collected under the Community Reinvestment Act (CRA): a banks' amount C&I loans with a lending volume of less than 100 thousand dollars, between 100 and 250 thousand dollars, between 250 and 1 million dollars as well as loans to firms with an annual revenue of less than 1 million dollars. All dependent variables are in logs. We restrict the observations used in each regression to 4 years, 1 year before the starting year of a respective round of QE as well as two years after the year of the event. Standard errors in parentheses are two-way clustered at the bank-year level and stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 6: *The effect of QE1 and QE3 on employment growth at the county-level: measuring exposure by bank-county lending activity (Panel A) and deposit base (Panel B)*

Panel A: Exposure measured with bank-county small business lending activity

Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.029	0.027						
	(0.025)	(0.025)						
$QE_t^{(1)} \times Treat_c^{(1)}$			-0.000	0.000				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.061**	0.061***		
					(0.024)	(0.023)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.006***	0.006***
							(0.002)	(0.002)
R^2	0.274	0.394	0.270	0.384	0.276	0.279	0.282	0.285
No. Counties	2826	2826	1883	1883	2820	2820	1887	1887
N	36071	36071	23970	23970	36117	36117	24133	24133

Panel B: Exposure measured with bank-county deposit activity

Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)DEP}$	-0.027	-0.027						
	(0.020)	(0.020)						
$QE_t^{(1)} \times Treat_c^{(1)DEP}$			-0.003	-0.003				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)DEP}$					0.036**	0.036**		
					(0.015)	(0.015)		
$QE_t^{(3)} \times Treat_c^{(3)DEP}$							0.006***	0.005***
							(0.002)	(0.002)
R^2	0.274	0.396	0.267	0.376	0.278	0.281	0.287	0.290
No. Counties	2819	2819	1872	1872	2785	2785	1861	1861
N	35976	35976	23785	23785	35682	35682	23812	23812
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table estimates Equation (3) and Equation (4), using the quarterly change in employment in a county, ΔEmp, as the dependent variable. We restrict the observations in each regression to 13 quarters: 6 quarters before and 6 quarters after a respective round of QE. In Panel A, we measure banks' activity in a county by the average amount of small business loans provided in the 4 years prior to a respective round of QE. In Panel B, we measure a banks' activity by the average amount of deposits over the 4 year prior to a respective round of QE that are associated with the respective banks' branches in a county. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by county and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 7: *The effect of QE1 and QE3 on lending to small businesses at the county-level: measuring exposure by bank-county C&I lending activity (Panel A) and by bank-county deposit base (Panel B)*

Panel A: Exposure measured with bank-county small business lending activity

Dependent variable	ΔC&ILending							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.561	0.594						
	(0.477)	(0.494)						
$QE_t^{(1)} \times Treat_c^{(1)}$			0.065	0.069				
			(0.040)	(0.043)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.467**	0.503**		
					(0.204)	(0.212)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.050***	0.054***
							(0.012)	(0.011)
R^2	0.136	0.165	0.141	0.170	0.134	0.135	0.141	0.142
No. Counties	2782	2782	1837	1837	2776	2776	1841	1841
N	10457	10457	6830	6830	10488	10488	6869	6869

Panel B: Exposure measured with bank-county deposit activity

Dependent variable	ΔC&ILending							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)DEP}$	-0.009	0.125						
	(0.325)	(0.327)						
$QE_t^{(1)} \times Treat_c^{(1)DEP}$			0.013	0.026				
			(0.038)	(0.039)				
$QE_t^{(3)} \times Exposure_c^{(3)DEP}$					0.706**	0.680**		
					(0.319)	(0.320)		
$QE_t^{(3)} \times Treat_c^{(3)DEP}$							0.066*	0.061*
							(0.035)	(0.036)
R^2	0.136	0.166	0.144	0.171	0.133	0.135	0.136	0.139
No. Counties	2750	2750	1812	1812	2714	2714	1811	1811
N	10341	10341	6699	6699	10282	10282	6808	6808
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table estimates Equation (3) and Equation (4), using the annual change of small business loans originated in a county as the dependent variable, ΔC&ILending. We restrict the observations used in each regression to 4 years, 1 year before the starting year of a respective round of QE as well as two years after the year of the event. In Panel A, we measure banks' activity in a county by the average amount of small business loans provided in the 4 years prior to a respective round of QE. In Panel B, we measure a banks' activity by the average amount of deposits over the 4 year prior to a respective round of QE that are associated with the respective banks' branches in a county. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by county and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 8: *The effect of QE1 and QE3 on lending related to mortgages at the county-level*

Panel A: Mortgage refinancing								
Dependent variable:	ΔRefinancing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.296*	0.322**						
	(0.152)	(0.147)						
$QE_t^{(1)} \times Treat_c^{(1)}$			0.023*	0.025**				
			(0.012)	(0.011)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					-0.051	-0.101		
					(0.095)	(0.091)		
$QE_t^{(3)} \times Treat_c^{(3)}$							-0.011	-0.017
							(0.011)	(0.011)
R^2	0.125	0.586	0.125	0.562	0.104	0.198	0.098	0.186
No. Counties	2759	2759	1835	1835	2737	2737	1827	1827
N	18307	18307	12082	12082	18540	18540	12352	12352
Panel B: Mortgage origination								
Dependent variable	ΔOrigination							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.073	0.078						
	(0.168)	(0.169)						
$QE_t^{(1)} \times Treat_c^{(1)}$			0.013	0.014				
			(0.013)	(0.013)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.364**	0.253*		
					(0.150)	(0.148)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.040***	0.025*
							(0.014)	(0.014)
R^2	0.064	0.251	0.061	0.236	0.043	0.209	0.044	0.186
No. Counties	2754	2754	1830	1830	2759	2759	1843	1843
N	18801	18801	12433	12433	18664	18664	12365	12365
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table estimates Equation (3) and Equation (4), using the quarterly growth of the amount of mortgages refinanced in a county, ΔHMDARefinancing, and mortgages originated in a county, ΔHMDAorigination, as the dependent variable. We restrict the observations used in each regression to 7 quarters, 3 quarters before a respective round of QE as well as three quarters after the event. In Panel A, we use the growth of mortgages refinanced and in Panel B the growth of mortgages originated as dependent variable. In all specification, we measure banks' activity in a county by the average amount of mortgage originated in the 4 years prior to a respective round of QE. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors in parentheses are clustered by county and time. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 9: *County-bank level analysis: controlling for demand by using county-time fixed effects*

Panel A				
Dependent variable	ΔC&ILending			
	(1)	(2)	(3)	(4)
$QE_t^{(1)} \times \left(\frac{MBS}{Assets}\right)_b^{(1)}$	-0.106	-0.133		
	(0.104)	(0.093)		
$QE_t^{(3)} \times \left(\frac{MBS}{Assets}\right)_b^{(3)}$			0.399***	0.362***
			(0.120)	(0.104)
R^2	.31	.33	.28	.31
N	54527	45309	55355	46514
No Banks	734	660	641	582
No Counties	2694	1887	2695	1811
Panel B				
Dependent variable	ΔRefinancing			
	(1)	(2)	(3)	(4)
$QE_t^{(1)} \times \left(\frac{MBS}{Assets}\right)_b^{(1)}$	0.766***	0.753***		
	(0.235)	(0.287)		
$QE_t^{(3)} \times \left(\frac{MBS}{Assets}\right)_b^{(3)}$			0.130	0.141
			(0.086)	(0.087)
R^2	.17	.29	.04	.14
N	154582	152481	199456	197688
No Banks	3240	3025	2880	2702
No Counties	3029	2834	3051	2856
Panel C				
Dependent variable	ΔOrigination			
	(1)	(2)	(3)	(4)
$QE_t^{(1)} \times \left(\frac{MBS}{Assets}\right)_b^{(1)}$	0.227*	0.206		
	(0.117)	(0.132)		
$QE_t^{(3)} \times \left(\frac{MBS}{Assets}\right)_b^{(3)}$			0.110*	0.110*
			(0.059)	(0.065)
R^2	.075	.24	.061	.23
N	115909	111824	134605	125806
No Banks	3295	3041	2962	2734
No Counties	2989	2606	2948	2546
Bank-County FE	Yes	Yes	Yes	Yes
Time FE	Yes	No	Yes	No
County-Time FE	No	Yes	No	Yes
Bank Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes

This table shows estimates of eq. (7) for three different outcome variables at the county-bank level. In Panel A, we restrict data to 4 years, in Panel B & C to 7 quarters. Standard errors are two-way clustered at the bank and time levels. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 10: *Employment effect by industry, non-tradable vs. others (Panel A) and financial dependence of industries (Panel B)*

Panel A: Employment in tradable/other and nontradable goods sector								
Dependent variable	ΔEmpTradOther		ΔEmpNonTrad		ΔEmpTradOther		ΔEmpNonTrad	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.024		0.165***					
	(0.059)		(0.018)					
$QE_t^{(1)} \times Treat_c^{(1)}$		-0.000		0.011**				
		(0.005)		(0.005)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.080*		-0.051	
					(0.046)		(0.031)	
$QE_t^{(3)} \times Treat_c^{(3)}$						0.008*		-0.005
						(0.005)		(0.004)
R^2	0.251	0.241	0.204	0.202	0.247	0.248	0.184	0.192
No. Counties	2907	1942	2907	1942	2904	1942	2904	1942
N	11605	7742	11601	7742	11598	7747	11594	7747

Panel B: Employment by financial dependence of industry								
Dependent variable	ΔEmpFin		ΔEmpNonFin		ΔEmpFin		ΔEmpNonFin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.008		-0.053					
	(0.041)		(0.041)					
$QE_t^{(1)} \times Treat_c^{(3)}$		0.001		-0.005				
		(0.003)		(0.003)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.120**		-0.067	
					(0.059)		(0.049)	
$QE_t^{(3)} \times Treat_c^{(3)}$						0.011*		-0.006
						(0.006)		(0.005)
R^2	0.208	0.214	0.296	0.238	0.194	0.192	0.224	0.237
No. Counties	2858	1902	2856	1900	2855	1912	2855	1912
N	11196	7427	10684	7083	11238	7512	10939	7291

This table estimates Equation (3) and Equation (4), using the annual hange in employment and change in the number of establishments in a county as the dependent variables, distinguishing by tradable or nontradable goods sector (Panel A) as well as financial dependence of industry (Panel B). We use the definition of non-tradable industries as used in Mian and Sufi (2014). We define financial dependence by using the measure from Almeida et al. (2010), defining the upper tercile as financially dependent and the lower tercile and non-dependent. We restrict the observations in each regression to 5 years: 2 year prior and 2 years subsequent to a respective round of QE. We measure banks' activity in a county by the average amount of mortagage lending provided in the 4 years prior to a respective round of QE. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by county and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 11: *The effect of QE1 and QE3 onchange in auto purchases at the county-level: measuring exposure by bank-county mortgage activity*

Exposure measured with bank-county mortgage lending activity								
Dependent variable	ΔAuto							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)}$ X $Exposure_c^{(1)HMDA}$	1.147***	0.335**						
	(0.359)	(0.165)						
$QE_t^{(1)}$ X $Treat_c^{(1)HMDA}$			0.091***	0.024**				
			(0.028)	(0.012)				
$QE_t^{(3)}$ X $Exposure_c^{(3)HMDA}$					0.107	0.125		
					(0.092)	(0.090)		
$QE_t^{(3)}$ X $Treat_c^{(3)HMDA}$							0.007	0.010
							(0.008)	(0.008)
R^2	0.110	0.463	0.102	0.442	0.143	0.173	0.138	0.165
No. Counties	2613	2613	1741	1741	2613	2613	1758	1758
N	31979	31979	21409	21409	32948	32948	22103	22103

This table estimates Equation (3) and Equation (4), using the quarterly change in auto purchases in a county, ΔAuto, as the dependent variable. We restrict the observations in each regression to 13 quarters: 6 quarters before and 6 quarters after a respective round of QE. We measure banks' activity in a county by the average amount of mortgages provided in the 4 years prior to a respective round of QE. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by county and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 12: *Bank-firm level analysis using Y14-Q schedule H data: firm-time FE control for demand*

Dependent variable	ΔC&I lending			ΔC&I $\text{lending}_{\text{Syndicated}}$			ΔC&I $\text{lending}_{\text{NonSyndicated}}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$QE^{(3)} \times \left(\frac{MBS}{TotSec}\right)_b^{(3)}$	0.092**	0.106***	0.061**	0.037	0.033	0.039	0.520**	0.462***	0.612**
	(0.042)	(0.014)	(0.030)	(0.059)	(0.031)	(0.031)	(0.249)	(0.018)	(0.233)
R_a^2	0.006	-0.049	-0.010	0.003	-0.044	0.063	0.026	-0.047	-0.342
No Banks	25	25	25	25	25	25	25	25	25
No Firms	141386	141386	9511	10019	10019	5540	131367	131367	3971
No Bank-Firm-Relationships	169433	169433	32693	29629	29629	23322	139804	139804	9371
No obs	738054	738054	164316	150021	150021	122509	588033	588033	41807
Bank-Firm FE	Yes	No	No	Yes	No	No	Yes	No	No
Time FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm-Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Bank Controls	No	No	Yes	No	No	Yes	No	No	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FirmControl	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No

Bank-firm level analysis with firm-time fixed effect. Observation are restricted to 9 quarters, 4 quarters before and 4 quarters after QE3. Standard errors in parentheses are clustered at the bank-firm level and stars indicate significance at the 10%, 5%, and 1% level, respectively. We use the computationally efficient estimator of linear models with multiple levels of fixed effects proposed by Correia (2017).

Table 13: *Firm-level regression: Investment and employment effects for firms in Y14 and for firms matched to Compustat*

Panel A: Firm-level investment				
Dependent variable	$\Delta \text{Investment}_{Compustat}$		$\Delta \text{Investment}_{Y14}$	
	(1)	(2)	(3)	(4)
$\text{Exposure}_i^{(3)}$	1.950***	1.274*	0.992***	1.168***
	(0.665)	(0.726)	(0.178)	(0.193)
R^2	.055	.088	.029	.03
No Firms	1848	1581	36716	30635
Panel B: Firm-level employment				
Dependent variable	$\Delta \text{Emp}_{2012Q4:2013Q4}$		$\Delta \text{Emp}_{2012Q4:2014Q4}$	
	(1)	(2)	(3)	(4)
$\text{Exposure}_i^{(3)}$	0.458**	0.431**	0.640**	0.665**
	(0.180)	(0.200)	(0.273)	(0.290)
R^2	.014	.02	.029	.064
No Firms	1894	1578	1775	1466
Firm Controls	No	Yes	No	Yes

Cross-sectional analysis of investment growth and employment. Panel A: ΔInvestment is the annual growth rate of firm *i*'s investment from 2012Q4 to 2013Q4. In column (1)-(3) the investment data is as reported in Compustat, in column (4)-(6) it is as reported in the Y-14 data. Panel B: $\Delta \text{Emp}_{2012Q4:2013Q4}$ described the one year growth rate from 2012Q to 2013Q4, $\Delta \text{Emp}_{2012Q4:2013Q4}$ the two-year growth rate of the number of employees of firm *i*. Firm controls include the firms's leverage, z-Score, current income, and the size. Robust standard errors in parentheses and stars indicate significance at the 10%, 5%, and 1% level, respectively.

Table 14: *Main employment effect in various robustness checks*

Dependent variable	ΔEmp							
	Population		Exclude Northeast		QE1 exposure		MBS Exposure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(3)} \times Exposure_c^{(3)}$	0.057**		0.083***					
	(0.022)		(0.025)					
$QE_t^{(3)} \times Treat_c^{(3)}$		0.005**		0.008***				
		(0.002)		(0.002)				
$QE_t^{(3)} \times Exposure_c^{(1)}$					0.046**			
					(0.023)			
$QE_t^{(3)} \times Treat_c^{(1)}$						0.003*		
						(0.002)		
$QE_t^{(3)} \times Exposure_c^{(3)MBS}$							0.024***	
							(0.006)	
$QE_t^{(3)} \times Treat_c^{(3)MBS}$								0.008***
								(0.002)
R_a^2	0.312	0.316	0.292	0.294	0.286	0.298	0.291	0.295
No. Counties	1770	1117	2579	1732	2886	1930	2764	1846
N	20878	13116	30488	20403	34506	23011	32659	21834
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table show results for estimating Equation (3) and Equation (4), using the quarterly employment data from the QCEW and the change in employment, ΔEmp, as the dependent variable. All estimates are for a time window of 13 months, 6 month prior to and after QE3. In Column (1) and (2), the sample to counties with population of more than 15000 and no more than 250000. In Column (3) and (4), we exclude the states from the Northeast: Connecticut, Delaware, Massachusetts, Maine, New York, New Jersey, New Hampshire, Pennsylvania, Rhode Island, Vermont. In column (5) and (6), we calculate the exposure measure based on the MBS holdings of banks in the 4 quarters prior to QE1. Finally, in column we calculate the exposure measure by using MBS over total securities instead of MBS over total assets. Standard errors are clustered by time and county and displayed in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

A Background on QE

In this subsection, we briefly lay out the details of the LSAPs in the United States. An excellent account of the events is also provided by Kandrac and Schlusche (2016).

LSAPs became an important tool for the Federal Open Market Committee (FOMC) once the target federal funds rate was reduced to zero at the end of 2008. Quantitative easing expanded the balance sheet of the Fed from about $800 billion (about 5 percent of nominal GDP) prior to the financial crisis to over $4 trillion (about 1/4 of nominal GDP) by the Fall of 2014. The first asset purchase program (QE1) was announced on November 25, 2008. The FOMC announced that if would buy up to $100 billion of direct debt obligations issued by Fannie Mae and Freddie Mac, and an additional $500 billion of agency-MBS.[44]

At the December meeting, the FOMC cut the target federal funds rate to 0 to 0.25 percent and effectively reached the zero lower bound. At this point the FOMC decided to expand the balance sheet of the Federal Reserve with an intent of lowering long-term interest rates. The Trading Desk at the Federal Reserve Bank of New York was directed to purchase $200 billion of debt issued by the housing-related GSEs and up to $1.25 trillion of agency MBS by the end of 2009. Furthermore, the FOMC decided that the Desk should buy an additional $300 billion of longer-term Treasury securities. During QE1, the Federal Reserve bought $1.25 trillion in MBS, $ 175 billion in Federal Agency debt, and $300 billion in U.S. Treasuries. By the end of QE1 (March 2010), the Fed held about one quarter of available MBS.

On August 10, 2010, the Federal Reserve indicated a second round of quantitative easing. This indication was reinforced in Chairman Bernanke's speech at the Jackson Hole Symposium on August 29, 2010. The FOMC statement on September 21, 2010 confirmed that the Federal Reserve intended to hold on to the securities purchased so far. The FOMC statement on November 3, 2010 announced that the Federal Reserve would further expand its Treasury holding. QE2 dictated the purchase of $600 billion in long-term U.S. Treasuries between November 3, 2010 and June 30, 2011.

In between QE2 and QE3, the Federal Reserve implemented a maturity extension program, often referred to as "Operation Twist". This included the sale of short-term U.S. Treasuries and the purchase of long-term Treasuries. Operation Twist was intended to lower yields at the long end of the yield curve, and worked to raise the average duration of the Federal Reserve's portfolio of Treasury holdings.

Eyeing still low economic activity and still high unemployment, the FOMC announced QE3 in its

[44] We refer to agency-MBS as those MBS that are guaranteed by Fannie Mae, Freddie Mac, or Ginnie Mae. Moreover, note that the Federal Reserve mostly bought assets via the to-be-announced market.

statement on September 13, 2012. QE3 was flow-based, open ended and largely unanticipated. QE3 initially dictated the purchase of $40 billion in agency MBS per month, and another $45 billion in U.S. Treasuries (added in December to the policy). The FOMC famously stated: "If the outlook for the labor market does not improve substantially, the Committee will continue its purchases of agency mortgage-backed securities, undertake additional asset purchases, and employ its other policy tools as appropriate until such improvement is achieved in a context of price stability." After improvements in the job market became apparent, the June 19, 2013 FOMC statement indicated that asset purchases may be tapered later in the year and ended altogether by mid-2015. The Federal Reserve reduced the purchase amounts to $35 billion in agency MBS and $40 billion in U.S. Treasuries, respectively in December, 2013. The program formally ended October 29, 2014.

B Supplementary Figures

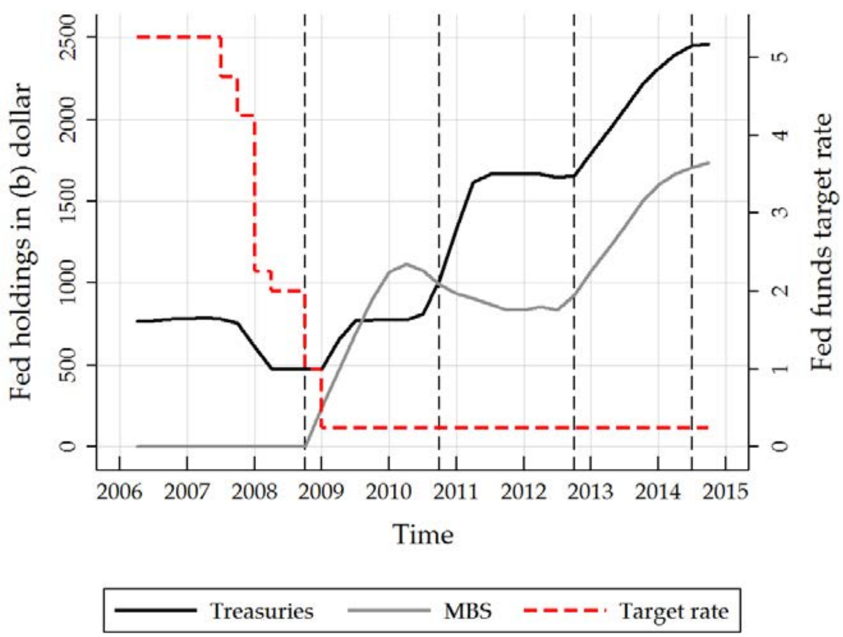


Figure 5: *Treasury and MBS holdings of the Federal Reserve and the federal funds target rate. The four vertical lines mark the event date of QE1, QE2, QE3, and the Tapering of QE3.*

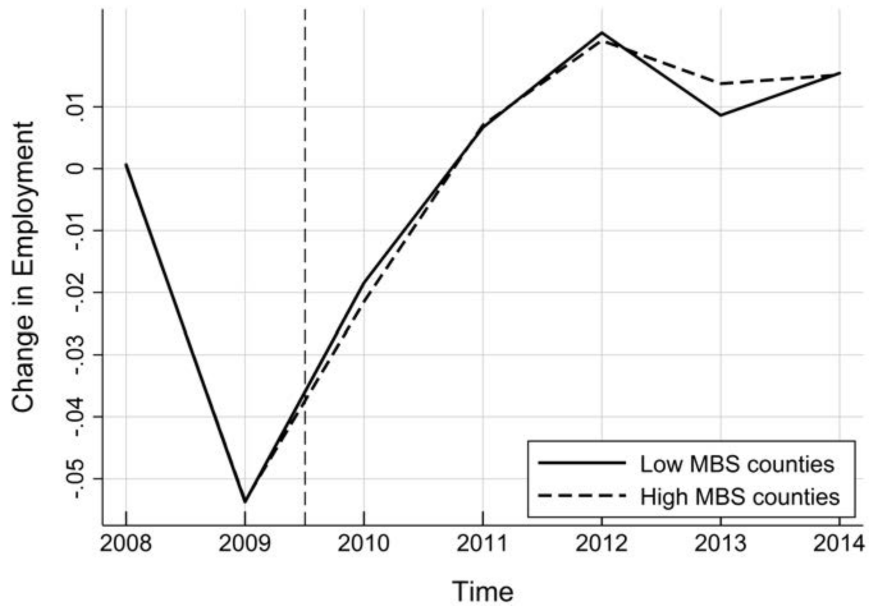


Figure 6: *The figure depicts the annual rate of growth ΔEmp_{ct} in the CBP data for splitting counties into the upper tercile of counties with a high exposure $Exposure_c^{(3)}$, and the lower tercile.*

C Supplementary Tables

Table 15: *Bank-level analysis, C&I loans for small businesses using Call Report data*

Dependent variable	[0, 100k]		[100k, 250k]		[250k, 1m]	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	0.026		-0.092		-0.069	
	(0.062)		(0.080)		(0.089)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$		0.062*		0.056		0.119**
		(0.034)		(0.038)		(0.053)
R_a^2	0.174	0.065	0.142	0.051	0.109	0.033
No. Banks	6445	5640	6374	5602	6236	5481
N	17551	37524	17334	37054	16876	35982
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The table depicts the results for estimating Equation (1) for four different dependent variables collected under the Community Reinvestment Act (CRA): a banks' amount C&I loans with a lending volume of less than 100 thousand dollars, between 100 and 250 thousand dollars, between 250 and 1 million dollars as well as loans to firms with an annual revenue of less than 1 million dollars. All dependent variables are in logs. We restrict the observations used in each regression to 4 years, 1 year before the starting year of a respective round of QE as well as two years after the year of the event. Standard errors in parentheses are two-way clustered at the bank-quarter level and stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 16: *The employment effect of QE3 in the annual data of the CBP*

Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	0.002	0.008						
	(0.025)	(0.023)						
$QE_t^{(1)} \times Treat_c^{(1)}$			-0.001	-0.001				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.081***	0.083***		
					(0.027)	(0.027)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.010***	0.010***
							(0.003)	(0.003)
R^2	0.210	0.296	0.210	0.289	0.351	0.353	0.372	0.373
No. Counties	2854	2854	1976	1976	2780	2780	1923	1923
N	11229	11229	7871	7871	8223	8223	5763	5763
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table show results for estimating Equation (3) and Equation (4), using the annual employment data from the CBP and the change in employment, ΔEmp, as the dependent variable. The regressions are for a time window of four years. One year prior to the respective round of QE as well as two years after. Standard errors are clustered by time and county and displayed in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 17: *The effect of QE2 on employment growth at the county-level: measuring exposure by bank-county lending activity (Panel A) and deposit base (Panel B)*

Dependent variable	ΔEmp			
	(1)	(2)	(3)	(4)
$QE_t^{(2)} \times Exposure_c^{(2)}$	-0.011	-0.017		
	(0.030)	(0.031)		
$QE_t^{(2)} \times Treat_c^{(2)}$			-0.001	-0.002
			(0.003)	(0.003)
R^2	0.334	0.458	0.327	0.437
No. Counties	2807	2807	1877	1877
N	25041	25041	16710	16710
Dependent variable	ΔEmp			
	(1)	(2)	(3)	(4)
$QE_t^{(2)} \times Exposure_c^{(2)DEP}$	0.022	0.025		
	(0.021)	(0.021)		
$QE_t^{(2)} \times Treat_c^{(2)DEP}$			0.003	0.003
			(0.002)	(0.002)
R^2	0.333	0.458	0.332	0.452
No. Counties	2807	2807	1876	1876
N	25045	25045	16695	16695
County FE	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes

This table estimates Equation (3) and Equation (4), using the quarterly change in employment in a county, ΔEmp, as the dependent variable. We restrict the observations in each regression to 13 quarters: 6 quarters before and 6 quarters after QE2. In Panel A, we measure banks' activity in a county by the average amount of small business loans provided in the 4 years prior to a respective round of QE. In Panel B, we measure a banks' activity by the average amount of deposits over the 4 year prior to a respective round of QE that are associated with the respective banks' branches in a county. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by bank and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 18: *Employment and bank lending at the county level*

Dependent variable	ΔEmp			
	(1)	(2)	(3)	(4)
$\Delta\text{Refinance}_{ct}$	0.006*			0.003
	(0.003)			(0.004)
$\Delta\text{Origination}_{ct}$		0.010***		0.012***
		(0.002)		(0.002)
$\Delta\text{C\&ILending}_{ct}$			0.001***	0.001**
			(0.001)	(0.001)
$\Delta\text{Emp}_{c,t-1}$	-0.159***	-0.158***	-0.163***	-0.167***
	(0.022)	(0.021)	(0.020)	(0.019)
R^2_a	0.042	0.046	0.057	0.065
No. Counties	2831	2834	2789	2786
N	21306	21285	19671	19657
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes

This table reports results for estimating the following regression

$$y_{ct} = \alpha + \beta \times \text{Exposure}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \theta X_{ct} + \epsilon_{ct}$$
$$y_{ct} = \alpha + \beta \times \text{Treat}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \theta X_{ct} + \epsilon_{ct}$$

using annual data from 2000 until 2014. γ_c, τ_t are county and time fixed effects. X_{ct} contains time-varying county specific controls, $L_{c,t-1}$ contains lags of the changes in local lending, $\Delta\text{Refinancing}_{c,t-1}$, $\text{Origination}_{c,t-1}$, $\Delta\text{C\&ILending}_{c,t-1}$. Standard errors are clustered by time and county and displayed in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 19: *The employment effect of QE at the level of Metropolitan Statistical Area's (MSA)*

Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	-0.037	-0.028						
	(0.026)	(0.025)						
$QE_t^{(1)} \times Treat_c^{(1)}$			-0.003	-0.002				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.053**	0.053**		
					(0.022)	(0.022)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.005***	0.005***
							(0.002)	(0.002)
R^2	0.326	0.577	0.313	0.555	0.339	0.343	0.352	0.356
No. MSA's	951	951	633	633	930	930	618	618
N	12013	12013	7973	7973	11912	11912	7917	7917

This table show results for estimating Equation (3) and Equation (4) at the MSA level, using the quarterly employment data from the QCEW and the change in employment, ΔEmp, as the dependent variable. The regressions are for a time window of four years. One year prior to the respective round of QE as well as two years after. Standard errors are clustered by time and county and displayed in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 20: *The effect of QE1 and QE3 on lending to small businesses at the MSA level: measuring exposure by bank-MSA C&I lending activity (Panel A) and by bank-MSA deposit base (Panel B)*

Dependent variable	ΔC&ILending							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	-0.375	0.115						
	(0.307)	(0.257)						
$QE_t^{(1)} \times Treat_c^{(1)}$			-0.026	0.024				
			(0.026)	(0.022)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					1.104***	1.136***		
					(0.364)	(0.365)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.106***	0.107***
							(0.023)	(0.023)
R^2	0.140	0.231	0.138	0.238	0.146	0.152	0.145	0.152
No. MSA's	927	927	611	611	907	907	606	606
N	3488	3488	2279	2279	3522	3522	2349	2349

This table estimates Equation (3) and Equation (4) at the MSA level, using the annual change of small business loans originated in a county as the dependent variable, ΔC&ILending. We restrict the observations used in each regression to 4 years, 1 year before the starting year of a respective round of QE as well as two years after the year of the event. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors are clustered by bank and time and in parentheses. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 21: *The effect of QE1 and QE3 on lending related to mortgages at the MSA level*

Panel A: Mortgage refinancing

Dependent variable:	ΔRefinancing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	1.357***	0.592***						
	(0.266)	(0.126)						
$QE_t^{(1)} \times Treat_c^{(1)}$			0.116***	0.042***				
			(0.028)	(0.013)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					-0.332	-0.068		
					(0.237)	(0.179)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.002	0.002
							(0.019)	(0.015)
R^2	0.344	0.756	0.339	0.751	0.481	0.877	0.479	0.869
No. MSA's	927	927	611	611	907	907	606	606
N	3495	3495	2284	2284	3526	3526	2353	2353

Panel B: Mortgage origination

Dependent variable	ΔOrigination							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)}$	-0.494*	-0.142						
	(0.258)	(0.186)						
$QE_t^{(1)} \times Treat_c^{(1)}$			-0.049*	-0.023				
			(0.026)	(0.022)				
$QE_t^{(3)} \times Exposure_c^{(3)}$					0.277*	0.316**		
					(0.152)	(0.156)		
$QE_t^{(3)} \times Treat_c^{(3)}$							0.023*	0.025*
							(0.012)	(0.013)
R^2	0.465	0.667	0.432	0.644	0.303	0.378	0.303	0.373
No. MSA's	893	893	584	584	907	907	606	606
N	2919	2919	1896	1896	3519	3519	2348	2348

This table estimates Equation (3) and Equation (4) at the MSA level, using the annual growth of the amount of mortgages refinanced in a county, ΔHMDARefinancing, and mortgages originated in a county, ΔHMDAorigination, as the dependent variable. We restrict the observations used in each regression to 4 years, 1 year before the starting year of a respective round of QE as well as two years after the year of the event. In Panel A, we use the growth of mortgages refinanced and in Panel B the growth of mortgages originated as dependent variable. In all specification, we measure banks' activity in a county by the average amount of small business loans provided in the 4 years prior to a respective round of QE. Column (1)-(2) and (5)-(6) report the coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) for the indicator treatment variable. Standard errors in parentheses are clustered by bank and time. Stars indicate significance at the 10%, 5% and 1% levels, respectively.