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Employment effects of unconventional monetary policy: Evidence from QE*

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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 banks with higher mortgage-backed securities holdings made relatively more loans after the first and third rounds of quantitative easing (QE1 and QE3). While additional volume is concentrated in refinanced mortgages after QE1, increases are driven by newly originated home purchase mortgages and additional commercial and industrial lending after QE3. Using spatial variation, we show that regions with a high share of affected banks experienced stronger employment growth after both QE1 and QE3. While the ability of households to refinance mortgages after QE1 spurred local demand, the resulting additional employment growth was relatively weak and confined to the non-tradable goods sector. In contrast, the increase in employment after QE3 is sizable and 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.

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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 effect of QE on bank lending (Darmouni and Rodnyansky, 2017), and an effect of QE on household net-worth and consumption via the mortgage market (Di Maggio et al., 2016; Beraja et al., 2018), but has not been able to speak to the crucial question of its effect on improving broader economic conditions such as employment outcomes.

In particular, we show that while the first round of QE (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, the third round of QE (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 total 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.[2]

The empirical assessment of macroeconomic policy is generally difficult, given the natural absence of a control group.[3] In order to overcome this inherent challenge, we proceed in three steps. First, we

[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]Given that we do not identify strong employment effects of QE1 and QE2 does not imply that these programs didn't have considerable impact on the real economy via different channels not considered in this paper. Likewise, given the cross-sectional nature of our analysis, one cannot easily draw conclusions with respect to aggregate employment.

[3]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."

exploit cross-sectional variation in the exposure of commercial banks to QE. Second, we exploit spatial 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 use confidential loan-level and mortgage-level credit registry data that allow us to shed light on whether additional credit results from changes in credit supply or credit demand.

In the first step, we exploit a link between QE and bank lending that has recently been established in several papers (see, e.g. Darmouni and Rodnyansky, 2017). This literature uses cross-sectional variation of banks' mortgage-backed securities (MBS) holdings in order to identify an effect of QE on bank lending. Complementary to these findings, we show that more exposed banks experienced higher stock returns on QE announcement days, controlling for many potentially confounding bank characteristics. In addition, we find that, within the mortgage market, QE1 mainly affected the refinancing volume of existing mortgages (Di Maggio et al., 2016; Beraja et al., 2018), and QE3 affected the origination of home purchase mortgages at more exposed banks. Within the C&I loan market, only QE3 increased lending, in particular in to smaller firms. These findings turn out to be important for understanding how QE affected employment.

In the second and central part of the paper, we study the link between bank lending and employment at 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, mortgage origination, or deposit volume 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.

A concern with our approach is that banks with higher MBS might systematically select into markets that are most economically developed, or that are expected to display high future economic growth. Our identification addresses this concern in two ways. First, while banks' location choices are systematic, they are also highly persistent. It is hence unlikely that banks selected locations in anticipation of quantitative easing, and it is such correlation with the timing of unconventional monetary policy that would be of most concern. Second, our difference-in-differences approach allows for systematic differences across markets as long as markets would have co-moved in absence of QE. We provide evidence in support of this assumption.

In particular, 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, employment growth in exposed and non-exposed counties followed the same trend for more than 18 consecutive quarters before the implementation of QE3. After QE3, however, we find that exposed counties experience higher overall employment growth. The effects are economically sizable: Our estimates suggest that counties in the upper tercile of the MBS–bank distribution experienced 40 basis points higher employment growth than counties in the lower tercile of the distribution. 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 QE1, the effect is weak, and overall employment growth is statistically indistinguishable between affected and unaffected counties.

Finally, in the third step, we analyze whether additional credit is driven by changes in credit supply as opposed to credit demand. In order to control for demand in the C&I loan market, we use newly available confidential loan–level data from the Y-14 data collection, which, since 2011, requires large banks with more than $50 billion in assets to report any C&I loan on their balance sheet with a commitment of 1 million USD or more.

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. Moreover, 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. Notably, our results in this part contrast to results documented by Chakraborty et al. (2016), who find a negative effect of QE3 on C&I credit and investment by large firms. The difference in findings can largely be explained by the fact that our data also contains medium-sized firms that are not active in the syndicated loan market. In particular, we show that the increased volume of C&I lending is more pronounced for non-syndicated borrowing by medium sized firms than for syndicated borrowing from larger firms, consistent with larger firms being less bank dependent.

In order to control for credit demand in the mortgage market, we use confidential mortgage-level data. In particular, we exploit that multiple banks are active in the same county and account for local credit demand by households by controlling for county-specific demand in county–bank–level regressions. We show that after QE1 was implemented, affected banks are more likely to refinance mortgages irrespective of whether they are GSE-conforming or non-conforming. However, when controlling for local demand, the effect becomes weaker in its magnitude, in particular for GSE-conforming mortgages, but remains statistically significant. We interpret this as evidence that the increase in refinancing activity after QE1 is partially driven by increased credit supply, complementary to the findings by Di Maggio et al. (2016) and Beraja et al. (2018), who emphasize the demand of households to refinance mortgages after QE1.

While our results indicate 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 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).

Even though we do not find an effect on total employment during QE1, employment in the non-tradable sector increases subsequent to the implementation of the program. Moreover, the increase in employment in the non-tradable sector coincides with an increase in auto sales, which are a proxy for overall household consumption. Our findings thus suggest that the ability of households to refinance mortgages after QE1 spurred local demand for consumption by increasing household net-worth, resulting in increased employment in the non-tradable goods sector.

In contrast, we find that the overall change in employment after QE3 is driven by changes of employment in sectors other than the non-tradable goods sector. We argue that 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, consistent with increases in lending, investment and employment at firms with relationships to more exposed banks as discussed above.

Our results survive a large number of additional tests. First, the second round of QE (QE2) consisted entirely of Treasury purchases, and hence acts as a natural placebo test: Indeed, we do not find a significant relation between MBS exposure and bank lending or employment following QE2. Second, if QE3 induced bank lending and employment, the tapering of QE3 should have effects in the opposite direction: We provide evidence that supports this conjecture. Finally, we also show that our main results are robust to minor variations in the empirical specification and sample restrictions.

Altogether, our findings suggest that unconventional monetary policy can, similar to conventional monetary policy, spur economic activity via a bank lending channel. In particular, LSAPs can induce commercial banks to expand lending which may translate into additional employment. However, differences in findings across the different rounds of QE suggest that the effects are time varying, mirroring findings on the effects of conventional monetary policy (Vavra, 2014).

We proceed as follows. After a brief summary of the existing literature, Section 2 discusses data used in our study. Section 3 presents the empirical strategy and discusses results on bank lending and employment.Section 4 separates supply and demand effects in the C&I loan and mortgage markets using loan-level data. Section 5 discusses evidence from additional events (QE2 and tapering) and robustness of results before we conclude in Section 6.

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 more accommodative monetary policy is associated with an increase in bank lending and a decrease in unemployment (see, e.g. Bernanke and Blinder, 1992; Kashyap and Stein, 1994, 2000; Drechsler et al., 2017). Previous studies on the effect of *un*conventional monetary policy in the United States have mostly focused on outcomes other than employment. For instance, recent studies have investigated the effects of QE on aggregate financing cost (Hancock and Passmore, 2011; Gilchrist et al., 2015) or on outcomes in the housing market and at the household level (Di Maggio et al., 2016; Beraja et al., 2018), at the bank level (Darmouni and Rodnyansky, 2017; Kurtzman et al., 2017), or at the firm level (Foley-Fisher et al., 2016; Chakraborty et al., 2016).[4]

Our analysis of bank lending 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 using Call Report data. While the focus of our study is on employment outcomes, we also provide complementary bank-level evidence beyond their study by distinguishing more granularly between different types of lending, and by investigating stock market reactions to QE announcements.

Di Maggio et al. (2016) and Beraja et al. (2018) study the effect of QE on the mortgage market in

[4]There are also several papers that investigate the effects of unconventional monetary policy in Europe (see, e.g., Acharya et al., 2016; Carpinelli and Crosignani, 2017; Crosignani et al., 2018; Cahn et al., 2018; Cumming, 2018).

more detail. In particular, Di Maggio et al. (2016) show that the Federal Reserve's purchases of MBS are associated with a refinancing boom after QE1, which in turn triggered significant equity extraction and an increase in consumption. Beraja et al. (2018) show that refinancing and the subsequent growth in consumption is more pronounced in regions in which households have lower leverage and are hence less constrained in their refinancing decision. 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. As we document, only the additional lending subsequent to QE3 had a significant effect on overall employment and is arguably driven by credit supply rather than credit demand.

Foley-Fisher et al. (2016) and Chakraborty et al. (2016) study the effect of unconventional monetary policy on firm behavior. In particular, Foley-Fisher et al. (2016) investigate the effects of the monetary expansion program (MEP), on non-financial firms and they find that firms that historically relied more on long-term debt issued more long-term debt after the MEP and that those firms increased investment.

Chakraborty et al. (2016) study the effect of QE on bank lending and firm investment. While we find that QE3 induced additional C&I lending, Chakraborty et al. (2016) find that QE crowded out C&I lending and led to a reduction of investment by large firms. The differences in findings can be explained by the difference in the empirical specification and by differences in data coverage. First, their findings suggest a negative relationship between mortgage lending and C&I lending over a longer period, in line with more general trends documented by Chakraborty et al. (2018). 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 less restricted in two important aspects: Our C&I lending data are not restricted to syndicated loans alone, and our sample at the bank-firm level is substantially larger and likely more representative of the C&I lending landscape. We find that the effect of QE3 is stronger for smaller firms that are not active in the syndicated loan market and that are arguably more bank dependent. Second, our data on C&I lending as well as our data on mortgages is at the quarterly rather than annual frequency and therefore allows us to capture the timing of QE effects more precisely.

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., 2015). Our empirical strategy is closest to Benmelech et al. (2017), Bord et al. (2015), or Greenstone et al. (2015), 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. (2015) analyze the employment effects of the financial crisis through reductions in small business lending.

2 Data

Our study brings together data from different sources at the bank, county, and firm level.

We take commercial 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). Information on bank holding companies are obtained from the the Consolidated Financial Statements for Holdings Companies (FR Y-9C). All items are adjusted for bank mergers. We match publicly traded BHC's with daily stock returns from the Center for Research in Security Prices (CRSP) using the publicly available crosswalk provided by the Federal Reserve Bank of New York.

We make use of three further administrative sources that collect data on bank lending. 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. Virtually all banks in the Unites States that conduct any mortgage lending in an MSA report on their activity, so the HMDA data are considered a near census of mortgage lending. 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 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. Each bank breaks down data by geographic region down to the census tract 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. (2015) estimate that small business lending in the CRA data captures 86% of total small business lending in the U.S.

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 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. 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. The CBP data often report employment only in brackets. We use the method in Autor et al. (2013) to estimate employment numbers within brackets. As additional data on local economic conditions, we obtain regional house prices from Moody's, median income from Haver Analytics, auto sales data from Polk, and population 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 adequacy and to support stress test models, and the loan schedule (Y-14Q-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 dates on which those rates 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 whether a loan is syndicated or not; 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 or SNC), this data set includes syndicated loans but also many smaller and/or non-syndicated loans, and therefore has 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 2700 companies.

3 Evidence on lending and employment

3.1 Empirical Strategy

We exploit two sources of variation to study the effect of unconventional monetary policy on employment via bank lending: First, we use the fact that banks were arguably differentially exposed to the Fed's MBS purchases during QE1 and QE3, which led to differential lending responses at the bank level. Second, banks' business activities vary across different regions, allowing us to investigate how local changes in bank lending correlate with changes in employment.

We start out by briefly[5] reviewing the Federal Reserve's unconventional monetary policy before laying out our empirical strategy in more detail.

3.1.1 The Federal Reserve's LSAPs

By the end of 2008, the federal funds target rate effectively hit the zero lower bound, and LSAPs became an important tool for the Federal Open Market Committee (FOMC) to conduct monetary policy. On November 25, 2008, the FOMC announced what came to be known as QE1: the Federal Reserve 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. The program was extended in the FOMC meeting on March 18, 2009, and, by the end of QE1 (March 2010), the Federal Reserve had bought $1.25 trillion in MBS, $175 billion in Federal Agency debt, and $300 billion in U.S. Treasuries. In March 2010, the Fed held about one quarter of all available MBS.

Over the course of 2010 and 2011 the Federal Reserve implemented two additional programs focused solely on the purchase of Treasuries. First, on August 10, 2010, the FOMC indicated the start of a second round of quantitative easing (QE2), which ultimately led to the total purchase of $778 billion in long-term Treasury securities. Second, on September 21, 2011, the Federal Reserve announced the

[5]For a more detailed chronology of events, see Table 1 in Gilchrist et al. (2015).

maturity extension program (MEP), which involved the sale of short-term U.S. Treasuries and the purchase of long-term Treasuries.

Given disappointing economic activity and still relatively high unemployment, the FOMC announced a third round of quantitative easing (QE3) in its statement on September 13, 2012. Largely unanticipated, QE3 initially dictated the purchase of $40 billion in agency MBS per month, and another $45 billion in U.S. Treasuries (added to the policy on December 12, 2012). After improvements of the economy became apparent, Chairman Ben Bernanke first indicated the Tapering of QE3 in his testimony to the Joint Economic Committee on May 22, 2013. The potential tapering was confirmed in the FOMC statement on June 19, 2013, and the Federal Reserve reduced the purchase amounts to $35 billion in agency MBS and $40 billion in U.S. Treasuries, respectively in December, 2013, and the program formally ended on October 29, 2014.

3.1.2 Variation at the bank level

In this study, we focus on the effect of the Federal Reserve's purchases of MBS during QE1 and QE3. We use cross-sectional variation of banks' mortgage-backed securities (MBS) holdings in order to identify an effect of QE on bank lending. In particular, we argue that those banks that held more MBS prior to QE were relatively more affected by the MBS purchases.

There are several reasons to believe that large-scale purchases of agency MBS affected banks with relatively larger MBS holdings more than banks with relatively lower MBS holdings. First, differences in MBS holdings may capture differences in banks' business models. Some banks tend to be more active in the mortgage market and are thereby more exposed to housing in general. Hence, when the Federal Reserve purchases agency MBS and the prospects of the housing market arguably rise, banks that are more exposed to the housing market may benefit more.

Second, large-scale purchases of MBS may directly raise the values and liquidity of banks' MBS holdings; for theoretical mechanisms see, e.g., Gertler and Karadi (2011) and Brunnermeier and Sannikov (2014). Krishnamurthy and Vissing-Jorgensen (2013) show that the Fed's actions had a considerable effect on MBS prices, especially during QE1. Moreover, they argue that QE operated through a "narrow channel" and MBS prices changed more than other asset prices. Thus, one may argue that banks with higher MBS shares benefited relatively more from the Fed's actions. In line with this evidence, Darmouni and Rodnyansky (2017) find that additional lending after QE1 stems from an improved capital position of affected banks, and that additional lending after QE3 was driven by an improved liquidity position.

Third, the increase in lending may be the result of a more general portfolio re-balancing channel: given that low-yield assets, such as reserves, are not perfect substitutes for 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 achieved, for instance, by issuing new loans or mortgages.

In a first step, we test the empirical connection between banks' MBS holdings and MBS purchase announcements by the Federal Reserve. Mirroring the approach used by Foley-Fisher et al. (2016) to analyze the effect of the MEP on non-financial firms, we investigate stock returns of bank holding companies on the day of the announcement of a given round of QE using the following model:

$$r_{bt} = \alpha + \beta \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b^{(j)} + \theta X_{bt} + \epsilon_{bt} \tag{1}$$

where r_{bt} is the (risk-adjusted) stock return of bank b on day t. We proxy a bank's exposure to QE by a bank's share of agency MBS holdings relative to total assets, $\left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)}$, averaged over the four quarters prior to round $j = 1, 2, 3$ of QE. X_{bt} is a vector of bank-level characteristics available at time t.

On average, around 8% of all bank assets are MBS (see Section A in the appendix for details). In 2008, prior to QE1, more than a quarter of all commercial banks held no MBS at all, and the average MBS-to-assets ratio was around 12% in the upper quartile of the cross-sectional distribution across banks. Among the set of publicly traded bank holding companies, the average MBS-to-assets ratio is around 6.5 percent in the 25th percentile and around 17 percent in the 75th percentile. Moreover, bank with larger MBS holdings tend to be larger, more leveraged, and more exposed to the housing market in general – observable characteristics we control for in Equation (1).

Table 1 shows estimates of equation (1) on the announcement days of QE1 and QE3, respectively. A higher MBS share is associated with higher raw and risk-adjusted returns: In particular, the stock return of a bank at the 75th percentile of the cross-sectional MBS share distribution exceeded the stock return of a bank at the 25th percentile of the distribution by about 78 basis points when QE1 was announced and by about 25 basis points when QE3 was announced, controlling for other observable bank characteristics. These findings suggest that the market valued MBS exposure beyond e.g. bank size or leverage on QE announcement days.

[TABLE 1 ABOUT HERE]

Figure 1 plots coefficient estimates of β when we estimate equation (1) for five days before and after the announcements of QE1 and QE3. In line with QE1 occurring in a period of higher volatility, coefficients in QE1 are less precisely estimated. Both panels suggest that banks with a higher MBS share outperformed banks with a lower MBS share on or shortly after the day of the announcement of QE1 and QE3, respectively.

[FIGURE 1 ABOUT HERE]

Further evidence for the empirical connection between banks' MBS holdings and the MBS purchases of the Federal Reserve comes from the response of bank balance sheets after the implementation of the policy. In appendix A, we find that banks that held more MBS prior to QE expanded their lending more after the implementation of QE1 and QE3. Our analysis there, which builds on a previous study by Darmouni and Rodnyansky (2017), shows that overall bank lending volumes increased after both, QE1 and QE3, but C&I lending increased only after QE3 and not after QE1. Within the mortgage market, QE1 mainly affected the refinancing volume of existing mortgages, in line with the findings of Di Maggio et al. (2016), and QE3 affected the origination of home purchase mortgages at more exposed banks.

3.1.3 Variation at the regional level

Building on the empirical link between banks and the Federal Reserve's MBS purchases described above, we exploit that there is spatial variation in the local activity of banks with different MBS holdings. This allows us to test whether the activity of affected bank correlates with increased local lending subsequent to a round of QE, and whether an increase in lending correlates with employment and consumption growth.

For our main specification, we observe counties at different points in time (quarterly or annually) and we calculate the exposure of each county c to a round QE j as

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

Here, $w_{bc}^{(j)}$ describes the market share of bank b in county c prior to QEj, where market share is computed as bank b's deposits, its volume of small business lending or its volume of mortgage lending in county c prior to QEj relative to the total deposits (or total small business loans, or total mortgage lending) of all banks active in county c. In other words, a county's exposure is a bank-activity weighted average of banks' exposure to QE where a bank's exposure is measured by its MBS holdings. Note that 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.

In our main specifications, we use the exposure measure that is most closely related to the outcome of interest, e.g. when the outcome is mortgage growth, we use the exposure measure based on mortgage volume. In general, results are robust to using different exposure measures. As one would expect, all three measures are highly correlated, with raw correlations being above .5 and rank correlations being above .8. Figure B.2 in the appendix shows scatter plots of the three exposure measures based on deposits, small business lending and mortgage lending, respectively.

Figure 2 shows our measure based on small business lending across U.S. counties before QE1. 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. Moreover, note that since MBS shares are very persistent within banks over time (see, e.g., Kurtzman et al., 2017) and spatial variation in geographical activity of banks is very stable over time as well, the map looks very similar at other points in time.

[FIGURE 2 ABOUT HERE]

Note that our exposure measure is, however, correlated with several observable county characteristics. Splitting the sample into the upper and lower tercile, Table 2 shows that those counties that are arguably more affected tend to be more urban counties. Hence they have a higher population, a higher median income, higher housing price levels and higher recent population growth. However, reassuring for our identification strategy, both types of counties experienced comparable declines in house prices and income during the financial crisis.

[TABLE 2 ABOUT HERE]

In our main analysis we estimate the following difference-in-differences specifications around the first and third round of quantitative easing:

$$y_{ct} = \alpha + \beta \times \text{Exposure}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \sum_{k=1}^{K} \theta_k^{(0)} X_{ct}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{ct}^{(k)} \text{QE}_t^{(j)} + \epsilon_{ct} \quad (3)$$

$$y_{ct} = \alpha + \beta \times \text{Treat}_c^{(j)} \times \text{QE}_t^{(j)} + \gamma_c + \tau_t + \sum_{k=1}^{K} \theta_k^{(0)} X_{ct}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{ct}^{(k)} \text{QE}_t^{(j)} + \epsilon_{ct} \quad (4)$$

Here, y_{ct} is an outcome in county c at time t, and γ_c and τ_t denote county and time fixed effects. $\text{QE}_t^{(j)}$ is a dummy variable equal to 1 in all time periods after QEj and 0 otherwise. 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. $X_{ct}^{(k)}$ is a vector of county-level time-varying controls, including levels and annual growth of population and median income. We allow for changes in the relation between controls and outcome variables in response to QE by interacting control variables with QE event dummies. Also note that we restrict data to counties with a population no less than 2500 registered inhabitants, as data from such small counties are less reliable.

We estimate our main equation for three different types of outcomes at the county level. First, we consider the effect on local lending, distinguishing between mortgage lending to households and lending to firms, the latter proxied by small business lending. Second, in our main analysis, we consider the effect on local employment. Finally, we investigate the effect on local employment by different industries and the effect on local consumption, proxied by auto sales. Since our outcomes variables may not immediately react to the implementation of a round of QE, we estimate each regression with using quarterly data from three quarters before to three quarters after each respective QE event. In annual regressions we use the data from two years, the year in which a specific round of QE was started as well as the subsequent year.

Our main outcome variable of interest is a county's employment growth. County-level employment is measured quarterly in the QCEW data and we compute the four-quarter harmonized growth rate as

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

We further calculate growth in small business loans with face value between $250k and 1 million reported in the CRA data, mortgage origination, and refinancing of existing mortgages reported in the HMDA data, and auto sales as reported in the Polk data. We denote them as ΔC&ILending, ΔOrigination,

ΔRefinancing, and ΔAuto, respectively. Note that the data on mortgage lending are available at a quarterly frequency, while the data on small business lending are only available at an annual frequency.

Before turning towards results, note that 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 Equation (4), our specification with a binary 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, an expansion of lending at the bank level 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. Hence, all of our regressions at the regional level are a joint test: we do not just test whether there is an effect of QE on bank lending and employment but also whether banking markets are sufficiently local.

Note that 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., 2015; Nguyen, 2015). In the case of mortgage lending activity, market locality is more debatable and the existing evidence is ambiguous. Beraja et al. (2018) do not find regional frictions in mortgage lending, while Scharfstein and Sunderam (2014) find that mortgage lending is characterized by local markets.

3.2 Results

3.2.1 Local lending

We start out by testing whether more affected counties experience stronger lending growth subsequent to QE1 and QE3. Table 3 displays estimates of Equation (3) and Equation (4) with mortgage lending variables as outcomes, distinguishing between refinancing of existing mortgages (Panel A) and origination of home purchase mortgages (Panel B). In both panels, we calculate exposure to QE using weights given by banks' historical mortgage lending activity in each county.

We find that mortgage refinancing activity increases more in more exposed counties after QE1. This finding is in line with the findings in Di Maggio et al. (2016) and Beraja et al. (2018), who show that when long-term interest rates were reduced during QE1, a refinancing boom followed. Counties in the upper tercile of the exposure distribution experienced roughly 3 percentage points higher refinancing activity than counties in the lower tercile of the distribution after QE1. Moreover, while we do not find consistent effects on mortgage origination activity during QE1, 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 2.6-2.7 percentage points higher mortgage origination growth than counties in the lower tercile of the distribution after QE3.

[TABLE 3 ABOUT HERE]

Table 4 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). Counties in the upper tercile of the exposure distribution experienced roughly 6-7 percentage points higher lending growth than counties in the lower tercile of the distribution after QE3, depending on how the exposure is calculated. Point estimates after QE1 are modestly lower and effects are less precisely estimated. This finding could reflect the fact that the time period around QE1 was a period of generally higher volatility, making it harder to detect lending effects.

These findings at the county level are also largely in line with evidence at the bank level. In Section A in the appendix, we use Call Report, HMDA, and CRA data to show that additional overall lending after QE1 by banks with higher MBS shares is mostly driven by an increase in the refinancing of existing loans. In addition, we show that increases in lending after QE3 stem mostly from origination of home purchase mortgages and additional C&I lending.

[TABLE 4 ABOUT HERE]

While the results in Table 3 and Table 4 report average effects, before and after quantitative easing episodes, we can study the dynamics of the effect in more detail. We interact time effects with the exposure variable and estimate the following regression:

$$y_{ck} = \alpha + \sum_{k=-3}^{3} \beta_k^{(j)} \text{Treat}_c^{(j)} \tau_k + \sum_{k=1}^{K} \theta_k^{(0)} X_{ct}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{ct}^{(k)} \text{QE}_t^{(j)} + \gamma_c + \tau_k + \epsilon_{ck}. \tag{6}$$

In Equation (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. Given that only mortgage lending data, but not C&I lending data, are available at the quarterly level, we estimate the model for mortgage lending only.

[FIGURE 3 ABOUT HERE]

Figure 3 shows estimates for mortgage refinancing around QE1 and for mortgage origination around QE3. First, note that, reassuringly, there are no differential trends prior to either QE1 or QE3. In line with the result on the average effects above, coefficient estimates are large and significant in the three quarters immediately after the start of QE1. Similarly, for QE3, we see an uptick of mortgage origination growth quickly after the start of QE3. In line with estimates of the average effect, the magnitude of the effect is slightly weaker than the magnitude of the effect for the refinancing boom after QE1.

Taken together, our evidence suggests that exposed regions experienced higher growth in refinancing after QE1, and stronger growth in C&I lending and mortgage origination after QE3. These differences in lending responses across the two rounds of QE turn out to be crucial when explaining the employment effects of the respective rounds of QE.

3.2.2 Employment

Figure 4 shows the employment growth of counties with high and low MBS exposure, defined as being in, respectively, the upper or lower tercile of the cross-sectional exposure distribution, with the dashed vertical lines denoting the start of QE1 and QE3. 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 Equation (3) and Equation (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.

[FIGURE 4 ABOUT HERE]

Panel A of Table 5 presents our main results and provides a more rigorous analysis of the visual patterns in Figure 4. We estimate Equation (3) and Equation (4) around QE1 and QE3, controlling for county- and time-fixed effects and county-level time-varying characteristics. In line with Figure 4, we do not find any effect around QE1; the coefficient estimates are small and insignificant, irrespective of how we calculate the exposure measure. 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 40 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 and Panel C of Table 5 confirm the results when we use the deposits-based and mortgage-based exposure measures with similar magnitudes.

[TABLE 5 ABOUT HERE]

As above, we can again study the timing of the effects in more detail by estimating Equation (6), now using the change in county-level employment as dependent variable. Figure 5 shows estimates for QE1 and QE3. First, note that there is again no discernible pre-trend before either QE1 or QE3, again, 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 5 ABOUT HERE]

Given that QE3 was implemented in mid September 2012, Figure 5 suggests that 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. The classic study by Bernanke and Blinder (1992) finds that employment reacts around 8 to 12 month after the monetary policy shock. More generally, the literature typically estimates the delay 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 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.

Magnitude To summarize, we find that small business lending and mortgage origination increased in arguably more exposed counties after QE3, while only mortgage refinancing activity increased in those counties after QE1. Our estimates imply that, after QE3, employment growth was approximately 30-50 basis points higher in more exposed counties, while small business lending growth was about 4-6 percentage points higher and growth in mortgage origination was about 2.7 percentage points higher.

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 50 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 that 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. Note, however, that our results also hold at the MSA level (see Section 5): 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.

3.2.3 Non-tradable goods, auto sales, and industrial financial dependence

In this section, we present additional evidence on the employment effect by industry. This allows us to derive two additional insights. First, it reveals that even though there is no effect of QE1 on overall employment, employment in the non-tradable goods sector increases. Second, it allows us to shed some light on whether the additional local economic activity results from additional demand by households or additional investment and employment by firms.

We estimate the main specification, Equation (3) and Equation (4), for employment growth in the non-tradable goods sector as well as for employment growth in manufacturing and the service sector (denoted by ΔEmpNonTrad and ΔEmpTradOther) separately. We define sectors using the definitions of Mian and Sufi (2014).[6] 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.

[TABLE 6 ABOUT HERE]

Results reported in Table 6, Panel A, reveal two important findings. First, even though there is no overall employment effect of QE1, there is an economically and statistically significant expansion of employment in the non-tradable goods sector, see columns (3) and (4). Counties in the upper tercile of the exposure distribution experienced roughly 1.6 percentage points higher employment growth in the non-tradable goods sector than counties in the lower tercile of the distribution after QE1.

Second, the overall employment effect of QE3 documented above results from both, an expansion of employment in the manufacturing and services sector, see columns (5) and (6), as well as an expansion of employment in the non-tradable goods sector, see columns (7) and (8). According to the estimates in Table 6, counties in the upper tercile of the exposure distribution experienced roughly 1.4 percentage point higher employment growth in the service industry and tradable goods sector, and 0.5 percentage point higher employment growth in the non-tradable goods sector. Note, however, that around QE3 the estimates for the non-tradable goods sector are relatively imprecisely estimated and not statistically significant.

Distinguishing the employment effect by industry also allows to shed light on whether additional local economic activity is driven by increases in local demand from households or by increased credit supply for firms. Assume for the moment that the increase in local mortgage origination and C&I lending can be attributed to an increase in credit supply. This will be discussed in more depth in the next two sections. While in theory both channels, lending to household and lending to firms, may be relevant for determining the level of employment at the same time, the former would work via changes in local demand for consumption, and the latter would work via improved financing conditions for local firms. In other words, QE may operate via spurring local demand if the improved access to mortgages for households increases housing net-worth (Mian and Sufi, 2014), or QE may operate via spurring labor supply if improved access to credit for local firms increases their labor (and capital) investment (Chodorow-Reich, 2014), or both.

[6] 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/manfuactring sector, 60% are defined as "'others", which mainly contains the service sector that does not offer non-tradable goods, and another 10% are in construction. In our analysis, we distinguish between non-tradable good industries and industries that produce tradable goods or services.

During QE1, we find that there is little or no additional lending to firms. Hence, any effect on employment should be driven by increases in local demand. The fact that employment increases in the non-tradable sector is in line with the findings by Mian and Sufi (2014), who show that increases in local demand, e.g., changes in economic activity due to changes in household net-worth, are 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.

Building on this insight, our evidence suggests that the additional refinancing of existing mortgages during QE1 indeed positively affected household net-worth and therewith demand: Given that households face lower interest payments on their outstanding debt, they are wealthier and can sustain a higher level of consumption. The additional consumption is reflected in a higher demand for non-tradable goods, leading to an increase of employment in the non-tradable goods sector. However, even though the effect is statistically significant and economically sizable within the non-tradable goods sector, the effect does not translate into additional growth in overall employment. This can be attributed to the fact that only 20% of the work force are employed in the non-tradable good sector.

To test the effect of QE1 on local consumption more directly, we provide complementary evidence on how QE1 affected auto sales. Auto sales represent a good proxy for consumption of durables and they are available at the county level. Table 7 shows that sales of new and used cars increased subsequent to QE1. Counties in the upper tercile of the exposure distribution experienced roughly 3.5 percentage points higher auto sales growth than counties in the lower tercile of the distribution after QE1. The evidence is in line with consumption increasing after a positive shock to household net worth after QE1, resulting in additional employment in the non-tradable goods sector.

[TABLE 7 ABOUT HERE]

This interpretation is further supported by studying the timing of the effect on auto sales in more detail. Figure 6 plots coefficient for estimating Equation (6) during QE1 and QE3, using the growth in auto sales as the dependent variable. Panel A, which shows results for the episode of QE1, suggesting an immediate response that fades over the subsequent quarters. Importantly, the timing of the growth in auto sales in exposed counties also closely follows the increase in local refinancing documented in Figure 3 over time.

[FIGURE 6 ABOUT HERE]

Recall that in contrast to QE1, QE3 led to an expansion of bank lending not only to households but also to firms. It is a priori not clear and empirically challenging to analyze which one of the two types of lending is more important for generating additional employment. While both channels may be important, consider the fact that the employment effect of QE3 seems strongest outside the non-tradable goods sector. To see this, compare again columns (5) and (6) with (7) and (8) in Table 6. Hence, unlike during QE1, employment growth does not result only from growth in the those industries that are more likely to be affected by local demand, indicating that lending to households was less relevant relative to the lending to firms to generate employment. Likewise, the effect of QE on the growth of auto sales documented in Table 7 is substantially weaker after QE3 than after QE1. Correspondingly, the dynamics of the auto sales response in Panel B of Figure 6 are also dampened relative to the response after QE1. These findings suggest that the additional employment after QE3 likely results at least in part from a stimulus to credit supply.

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

Altogether, our evidence suggests that the increase in employment in the non-tradable goods sector after QE1 was due to the additional refinancing activity subsequent to QE1, with positive effects on household net-worth and local demand. In contrast, additional overall employment growth subsequent to QE3 was less likely to be driven by additional lending to households but rather related to additional credit provision to firms.

4 Supply and demand in the credit market

Results in the previous section show that the expansion of bank credit to firms and households subsequent to QE3 is associated with an effect on overall employment. Moreover, the additional refinancing of existing mortgages after QE1 is associated with an increase of employment only in the non-tradable goods sector. Additional lending and employment subsequent to QE1 and QE3 could result from additional credit supply by banks, but also from additional credit demand by firms and households.

This section provides additional evidence to disentangle supply and demand in the C&I loan market and in the mortgage market. We first use confidential loan-level data to analyze supply and demand in the C&I loan market, Section 4.1, before using confidential mortgage-level data to discuss the role of demand and supply in the mortgage market, Section 4.2.

4.1 C&I loan market: Evidence from confidential loan-level data

Using confidential loan-level data that are part of the Y14 data collection allows us to provide direct evidence of a link from QE3 to bank lending, and from bank lending to firms. The advantage of using this data set is that it provides a direct link between banks and firms, and firms can be followed across banks via their tax identification numbers. In particular, the latter allows us to control for firm-specific credit demand by comparing loan amounts from different banks to the same firm in the same period. Moreover, the data include information on firms' capital expenditures and therefore allow us to trace the impact of lending on real firm activity directly.

There are, however, also two main drawbacks of the data. First, the data collection started only in 2012; too late to cover the episodes of QE1 and QE2. Second, it only includes corporate lending from the largest banks, and to the extent that firms borrow from other banks, these loans would not show up in our data. Note, however, that the banks covered by the Y-14 data collection conduct roughly 75% of total C&I lending by banks reported in the Call Reports.

As above, we measure a bank's exposure to QE by its MBS holdings relative to its total assets. Table C.2 in the appendix gives a sense of the variation of Y14-banks' MBS holdings: While the average BHC covered in the Y14 data holds around 9.5% of its assets as MBS, the average share is 7% below the median and 15% above the median. Moreover, Table C.1 shows that banks with higher MBS shares tend to be very similar to banks with relatively low MBS shares across a number of observables. Nonetheless, regressions below include a set of time-varying controls (X_{bt}) in all specifications to alleviate concerns that results could be driven by differences in observables.

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). Given the credit-registry like nature of our data, our data includes several thousand firms with a relationship with more than one banks, an order of magnitude larger than in previous applications that relied on publicly available data of syndicated loans in the United States which could not observe loans by medium-sized firms, see for instance Chodorow-Reich (2014) or Chakraborty et al. (2016). However, firms that are active in the syndicated loan market are typically larger and hence are more likely to have the ability to substitute bank financing with other types of financing such as the bond market or public equity issuance. Figure B.4 in the appendix shows that not only large firms but firms of all sizes in the data set tend to have relationships with multiple banks.

Exploiting the fact that banks borrow from multiple banks in the same time period makes it feasible to control for firm-specific credit demand. To this end, we estimate the following two equations:

$$y_{bit} = \alpha + \beta \left(\frac{\text{MBS}}{\text{Total Assets}} \right)_b^{(3)} \text{QE}_t^{(3)} + \gamma_{it} + \delta_{ib} + \sum_{k=1}^{K} \theta_k^{(0)} X_{bt}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{bt}^{(k)} \text{QE}_t^{(3)} + \epsilon_{bit} \tag{7}$$

$$y_{bit} = \alpha + \beta \text{Treat}_b^{(3)} \text{QE}_t^{(3)} + \gamma_{it} + \delta_{ib} + \sum_{k=1}^{K} \theta_k^{(0)} X_{bt}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{bt}^{(k)} \text{QE}_t^{(3)} + \epsilon_{bit} \tag{8}$$

where y_{bit} is the quarterly change in the amount firm i borrows from bank b between time $t-1$ and t. In Equation (7) we measure bank b's exposure as a continuous variable given by the average MBS holding prior to the implementation of QE3, $\left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(3)}$. In Equation (8) we use a binary dummy variable that takes the value one if a bank has MBS holdings above the median MBS holdings among all banks that report in the Y14 data, $\text{Treat}_b^{(3)}$.

Further, 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. Due to the inclusion of γ_{it}, estimation requires multiple observations of the same firm within one time period and hence the sample is restricted to firms that borrow from multiple banks. We also include K bank-level controls $X_{bt}^{(k)}$ as individual regressors and interacted with the QE3 time dummy. The bank-level control variables are listed in Table C.2 in the appendix.

Table 8 shows results. Panel A uses the continuous treatment variable and Panel B uses the binary treatment variable. The specifications in column (2), (4), and (6) include firm-time fixed effects and suggest that even when controlling for credit demand, the overall effect on lending growth is still sizable and statistically significant. In particular, as shown in column (2) in Panel B, lending by banks with an above median MBS share grew about 3 percentage points more than lending by banks with a below median MBS share after QE3.

The remaining columns split total lending into firms that report and do not report in Compustat. While the coefficient for bank lending to Compustat firms is positive and statistically significant (see column (6)), it is lower than for firms that do not report in Compustat: lending by banks with an above median MBS exposure grows at an approximately 1.7 percentage points higher rate after the implementation of QE3. In contrast, credit to firms that do not report in Compustat grew around 3 percentage points faster for banks that have an above median MBS exposure (see column (4)).

[TABLE 8 ABOUT HERE]

The difference in credit outcomes between the different types of firms reflects the relationship between firm size and bank dependence. The effect on firms that do not report in Compustat is about twice the effect of lending to Compustat firms. This can be rationalized by the difference in firm size and bank dependence: Compustat firms are typically larger, tend to borrow via the syndicated loan market and have access to the corporate bond market, and are arguably less bank dependent than smaller firms that are not public, and do not have the same access to sources of outside financing.

In sum, the analysis reveals that bank lending to firms increased subsequent to QE3, even when controlling for demand. This evidence suggests that additional C&I lending subsequent to QE3 stemmed from additional credit supply. Table C.11 in the appendix shows that effects are somewhat larger in magnitude in non-syndicated lending than in syndicated lending, the latter of which is also considered in Chakraborty et al. (2016).

Firm-level evidence on investment and employment As indicated above, the Y-14 data has an additional significant advantage: it allows us to link banks' credit supply directly to firm investment and employment decisions. Using additional information on capital expenditures from the Y14 C&I schedule, which is reported for a subset of around one third of the firms documented in the data, we can further analyze how QE3 affected firms' investment decisions. Moreover, for a subset of those firms that we match with Compustat, we do not only have information on capital expenditures, but we also obtain information on firm-level employment decisions.

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. Note 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 over the last four quarters.

To investigate the effect of QE3 on bank credit, investment, and employment, we then run the following cross-sectional regression:

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

$$y_i = \alpha + \beta \text{Treat}_i + \theta X_i + \epsilon_i \tag{11}$$

where y_i is either the one-year growth in bank credit or investment at the firm level from 2012Q4 to 2013Q4, or the one- or two-year growth rate of employment at the firm level. As above, we estimate the model both with a continuous exposure measure, Equation (10), as well as a binary treatment that takes the value of 1 if firms i's exposure is in the upper tercile of the exposure distribution and 0 if in the lower tercile, Equation (11). Note that while we calculate the change in the stock variables (credit and employment) as growth rates, we calculate the change of investment as the increase in capital expenditures normalized by the total assets of the firm, i.e.

$$\Delta\text{Investment}_i = \frac{\text{CapEx}_{2013Q4} - \text{CapEx}_{2012Q4}}{\text{Total Assets}_{2012Q4}}.$$

[TABLE 9 ABOUT HERE]

Panels A, B and C of Table 9 show results for credit, investment, and employment, respectively. Starting with Panel A, firms that were more exposed to banks with a higher MBS share, increased bank borrowing more after QE3 for both the subsample of public firms from Compustat and for the larger sample of private firms in the Y14 data. In particular, moving from the lower tercile of the exposure distribution at the firm-level to the upper tercile implies a 1.3 percentage point higher growth in bank credit for firm reporting in Compustat and a 1.7 percentage point higher growth in bank borrowing for firms not in Compustat. This is consistent with the fact that firms that report in Compustat tend to be larger and have access to relatively more sources of financing than firms that do not report in Compustat.

Panel B of Table 9 shows that the growth in bank credit is associated with an increase in investment: For firms reporting in Compustat, Columns (2) and (4) report relatively imprecise estimates suggesting that moving from the lower to the upper tercile of the exposure distribution has a positive effect on growth of capital expenditures over assets. For firms that do not report in Compustat, growth in capital expenditures is also larger, and estimated to be around 21 basis points higher when controlling for other firm characteristics.

Finally, Panel C shows that more exposed firms were also relatively more active in hiring new employees after QE3. As noted above, 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 four 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 percentage point higher for a firm in the upper tercile of the exposure distribution compared to a firm in the lower tercile. Given that it takes time to hire, estimates are somewhat higher for employment growth over two years, but the estimates show that relative employment growth is slowing down over time.

To get a sense of the relative magnitudes of all three effects, consider the median firm reporting in Compustat that we are able to match with the Y-14 data. The median firm in the Compustat subsample reports around $2 billion in total assets (see Table C.3 in the appendix). Moreover, firms in our Compustat sample finance on average 20 percent of their assets with bank loans. Our estimate in panel A implies that a firm in the upper tercile of the exposure distribution borrows an additional $5.2 million relative to a firm in the lower tercile of the distribution (20% of $2b times .013). Our estimates in panels B and C suggest that the more exposed firm increases capital expenditure by 7-14 basis points relative to total assets, i.e. by about $1.4m - $2.8m, and that it hires an additional 34 workers (with median employment at 3,400 workers and a 1 percentage point higher growth rate). Of course, these estimates come with relatively large standard errors. In general, though, the three estimates appear to have the same order of magnitude. While the estimated number of additional employees relative to additional bank credit might seem high, its important to keep in mind that the firms considered have several other ways of raising additional funds.

4.2 Mortgage market

We now turn to analyzing demand and supply in the mortgage market. Recall that the additional employment in the non-tradable goods sector subsequent to QE1 is correlated with additional borrowing from households. As with C&I lending, we cannot easily conclude that the additional lending to household results from additional supply of credit by banks. Therefore, we provide additional evidence to test whether the additional refinancing after QE1 and the additional origination of mortgages subsequent to QE3 can at least partially be attributed to additional supply by banks.

Observe that separating supply and demand empirically in the mortgage market is considerably more difficult than in the C&I loans market. This is due to the fact that a household, unlike firms, typically does not have a (mortgage borrowing) relationship with more than one bank. Hence, we can only control for local demand for mortgage financing by exploiting that multiple bank are active in the same county. I.e., we can control for county-specific credit demand by estimating the following county-bank level regressions:

$$y_{bct} = \alpha + \beta \times \left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)} \times \text{QE}_t^{(j)} + \gamma_{ct} + \delta_{bc} + \sum_{k=1}^{K} \theta_k^{(0)} X_{bt}^{(k)} + \sum_{k=1}^{K} \theta_k^{(1)} X_{bt}^{(k)} \text{QE}_t^{(j)} + \epsilon_{bct}, \quad (12)$$

where y_{bct} is the harmonized annualized growth of refinancing or mortgage origination of bank b in county c from quarter $t-4$ to t, $\left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)}$is bank b's MBS exposure prior to round j of QE, and δ_{bc} is a bank-county fixed effect, andγ_{ct} is a county-time fixed effect that absorbs all local economic conditions, including county-specific credit demand. X_{bt} contains all time-varying bank-level controls, also interacted with the QE event dummy. We distinguish between GSE-conforming mortgages and non-conforming mortgages.[7]

[TABLE 10 ABOUT HERE]

Table 10 shows results. In line with findings in Section 3.2.1, we find that additional lending to households after QE1 results from mortgage refinancing. Interestingly, additional refinancing takes place in the conforming as well as non-conforming segment of the market (Panel A), even though the non-conforming part of the market is relatively small by historical standards (also documented in Di Maggio et al. (2016)). Also in line with finding in Section 3.2.1, we find that the origination of mortgages for home purchases (Panel B) increased after QE3 at more affected banks. This is, however, entirely driven by an expansion of lending in the non-conforming segment of the market, which recovered relative to the period around QE1.

Most important for our purposes, Table 10 suggests that local demand is not the sole driver of our results. In particular, when county-time fixed effects are included in columns (2), (4), (6), and (8), the overall effects remain statistically and economically significant.

How credible is the finding that the refinancing subsequent to QE1 is at least partially driven by additional credit supply? At first glance, the structure of the US mortgage market implies that credit demand rather than credit supply should be driving observed increases in mortgage lending, in particular during QE1. Mortgages are typically highly standardized products that can be securitized and sold in secondary markets. Moreover, when interest rates were lowered during QE1, the incentives to refinance mortgages were strongest for households. In line with that logic, existing evidence by Di Maggio et al. (2016) and Beraja et al. (2018) document a refinancing boom subsequent to QE1. Moreover, Di Maggio et al. (2017) show that refinancing is more likely to be demand-driven as those household with the strongest incentive to refinance also have the highest propensity to refinance.

However, credit supply may nonetheless be important. First, note that originating GSE-conforming mortgages, even if the ultimate default risk does not lie with the originating bank, may yet be associated with significant risks. For instance, GSE-conforming mortgages are typically associated with a repurchase risk that realizes when a mortgage is returned to the bank as non-eligible. This risk had been particularly high in the period after the Financial Crisis. Moreover, non-conforming mortgages are often kept on the banks balance sheets and hence the default risk is likely to remain with the issuing bank.

[7] We define a mortgage as conforming if the amount of lending lies below the conforming amount specified on fhfa.gov (one unit limit). Observe that a conforming mortgage also needs to fulfill other requirements, such as minimum LTV ratios and FICO scores, unobserved in the HMDA data. We hence falsely define some non-conforming mortgages as conforming.

In line with this logic, observe that around QE1, the potential bias from local demand effects should be relatively lower for non-conforming mortgages than for conforming ones: Comparing the coefficient for conforming mortgages in the specification with and in the specification without county-time fixed effects, columns (1) and (2) in Panel A, we observe that the coefficient is reduced significantly when controlling for demand. In contrast, the change in coefficients for non-conforming mortgages across specifications, columns (5) and (6) in Panel A and columns (7) and (8) in Panel B, is relatively small.

5 Robustness

This section provides additional evidence to corroborate our main findings. First, we consider effects of QE2 and the Tapering of QE3 on bank lending and employment. Second, we show robustness of our main results to different specifications and exposure measures.

5.1 QE2 and the Tapering of QE3

QE2 was announced at the FOMC meeting of August 10, 2010. Given that the program consisted entirely of Treasury purchases, it acts as a natural placebo test: If the effects of the Federal Reserve's LSAPs were a result of MBS purchases rather than Treasury purchases, one would expect to find no significant relation between MBS exposure and bank lending or employment following QE2.[8]

The Tapering of QE3 was first indicated by chairman Bernanke's opening remarks in the Federal Reserve's semiannual report to Congress ("Humphrey Hawkins hearing") on May 22, 2013 and formally announced at the FOMC meeting of June 19, 2013. Likewise, if additional bank lending and employment was a result of MBS purchases, the Tapering should have a negative effect on lending and employment, thereby providing additional support for our QE1 and QE3 findings from a different event.

To analyze both programs in more detail, we estimate our main specification, Equation (4), for the three quarters before and after the announcement of QE2 and of the Tapering of QE3, using mortgage lending or employment as outcome variables. In addition, we estimate Equation (7) at the bank-firm-quarter level to assess effects on C&I lending for the three quarters around the start of the Tapering of QE3.

Table 11 shows results for mortgage origination and mortgage refinancing. Columns (1) - (4) in both panels indicate that there is little difference in mortgage related lending across counties with different

[8]As QE2 focused on treasury purchases, we re-ran all QE2 specifications reported below with treasury exposure instead of MBS exposure. Results are very similar to the ones discussed below and are available on request.

exposure to QE2. These findings are consistent with bank-level results in Section A in the appendix that show there is no differential mortgage lending response after QE2 across banks with different MBS shares.

[TABLE 11 ABOUT HERE]

In contrast, columns (5) - (8) in panel A show a significant negative response of mortgage refinancing growth in more exposed counties after the Tapering of QE3. In addition, columns (5) - (8) in Panel B suggest lower growth in the origination of home purchase mortgages after the Tapering of QE3 in more exposed counties, although the effects are imprecisely estimated. Overall, the Tapering of QE3 is associated with a reduction in mortgage-related bank lending.

Table 12 shows results for employment growth. Consistent with the muted lending response after QE2, it is not surprising that there is no considerable employment effect of QE2, regardless of which exposure measure is used (columns (1) - (4) in all panels). In contrast, after the Tapering of QE3, coefficient estimates are consistently negative although not precisely estimated across all exposure measures (columns (5) - (8) in all panels). The negative point estimates are consistent with the negative effects of Tapering on bank lending.

[TABLE 12 ABOUT HERE]

Table 13 reports estimates of the effect of tapering on C&I lending using bank-firm level data and the specification in Equation (7). Recall that, as above, this specification is particularly attractive, because we can include firm-time fixed effects that absorb changes in credit outcomes resulting from changes in credit demand rather than credit supply. However, data availability restricts the analysis to events after late 2011 and hence to the Tapering of QE3 only.

Across all specifications and subsamples in panels A and B of Table 13, our estimates suggest a significant negative effect of credit supply to firms. In particular, column (2) of Panel B shows that, in the three quarters after the Tapering of QE3, bank-firm credit growth is on average 1.3 percentage points lower at banks with above median MBS exposure. Columns (4) and (6) show that this effect is somewhat larger for non-public firms and somewhat weaker for public firms.

[TABLE 13 ABOUT HERE]

Altogether, the absence of an effect of QE2 on bank lending or employment, and the negative effects of the Tapering of QE3 on bank lending and employment reinforce our main finding that the Federal Reserve's MBS purchases (QE1 and QE3) were associated with additional bank lending that affected the real economy.

5.2 Specifications and sample restrictions

Table C.4 in the Appendix shows that the employment effect of QE3 is robust to a number of sample restrictions and to different definitions of the exposure measure. Columns (1) and (2) report results for our main regression (equation (4)) 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 United States. 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 holdings prior to QE3. Results are robust to this change, which is unsurprising as MBS holdings are highly autocorrelated within bank over time. Finally, columns (7) and (8) report results for calculating the exposure measure as MBS over total securities instead of MBS over total assets.

A related concern is that quantitative easing led not only to a decline of MBS yields but also to a decline of long-term treasury yields (see, e.g., Krishnamurthy and Vissing-Jorgensen, 2011). As such, a bank's exposure to quantitative easing might be better captured by the sum of MBS and long-term treasury holdings. While we cannot measure long-term treasury holdings in the call reports separately, we re-estimated our main specifications using the sum of MBS and *all* treasury holdings as exposure measure. Given that only a small share of banks' assets is invested in treasuries (the average share during our sample period is less than 1%), the alternative exposure measure is very similar to our original one and, as a result, estimations with the alternative measure had little effect on conclusions (results available upon request).

Our main results are also robust to defining markets at the MSA level instead of the county level. Firms may borrow and employees may work across county-lines, and therefore a county might not comprise a local credit or labor market. Table C.7 in the appendix shows that employment results are robust to conducting the analysis at the MSA level.

6 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.[9] 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 we exploit spatial variation in banks' activity to trace the effect of lending on employment. While QE1 and QE3 were both successful in spurring bank lending in general, we show that there is considerable variation in the type of lending that expanded. The differential lending response is crucial to understanding the ultimate effect on the real economy. In particular, we show that only QE3, after which banks extended C&I lending, led to additional overall employment. In contrast, QE1, after which mortgage refinancing activity picked up, likely increased household net-worth and spurred local demand. This effect, however, did not translate into overall employment growth but only into additional employment in the non-tradable goods sector.

Altogether, our results indicate that LSAPs as conducted by the Federal Reserve can generally affect employment via a bank lending channel. Our evidence suggests that the Federal Reserve's action, in particular QE3, were helpful for the economic recovery, in line with the stated goals of the program.[10] However, the absence of a strong employment effect after QE1 also indicates that the ultimate real effects of LSAPs depend on other economic variables that are not under the control of the central bank.Hence, while our results indicate that transmission from unconventional monetary policy to the real economy is not entirely impaired at the zero-lower bound, its efficacy – analogous to the efficacy of conventional monetary policy (Vavra, 2014) – varies over time.

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

[10]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".

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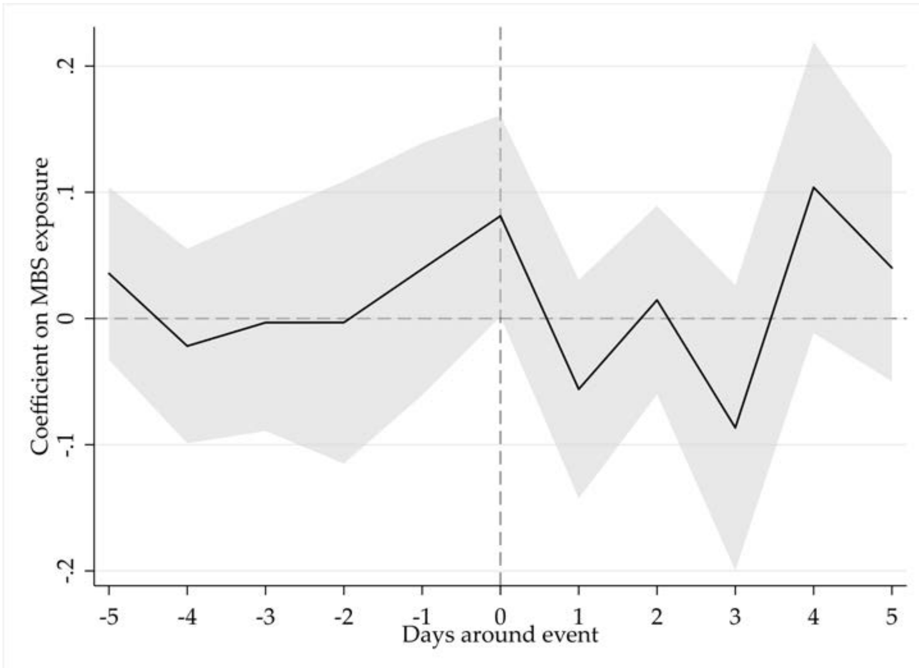
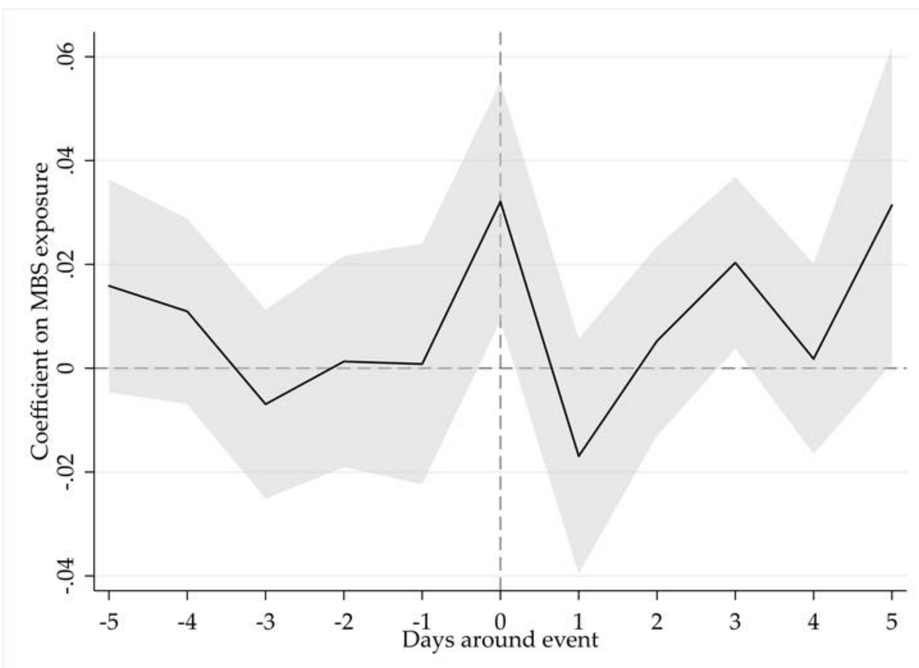
Figures



(a) *QE1: November 25, 2008*



(b) *QE3: September 13, 2012*

Figure 1: *Coefficient estimates around event dates. This figure plots MBS exposure point estimates and 95%confidence bands obtained from a series of daily regressions around QE announcement dates (November 25, 2008, for QE1 and September 13, 2012, for QE3), indicated by vertical lines at 0. Results are based on a regression of abnormal stock returns for a given day on a bank's MBS exposure and other bank characteristics, see eq.* (1).

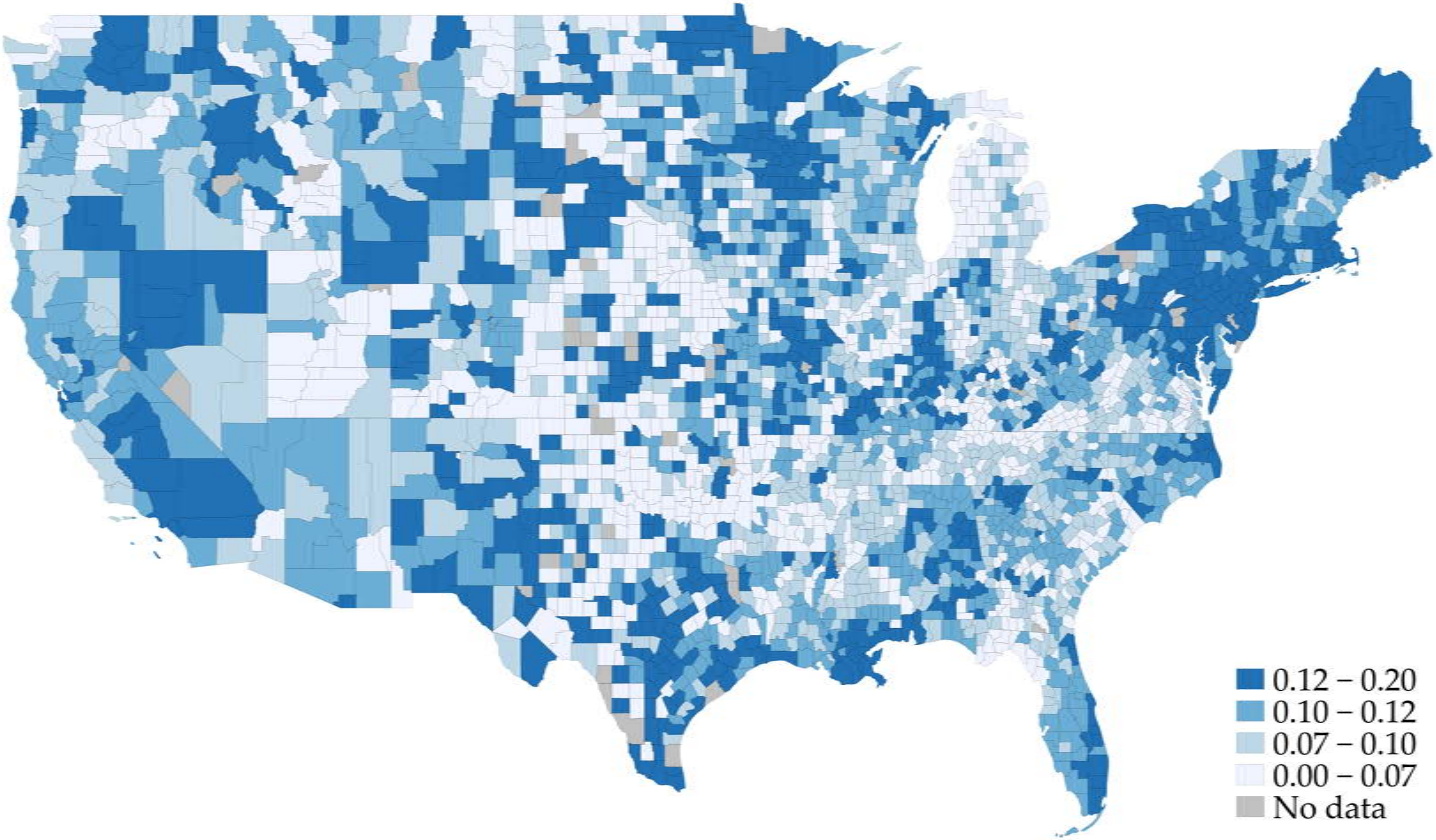
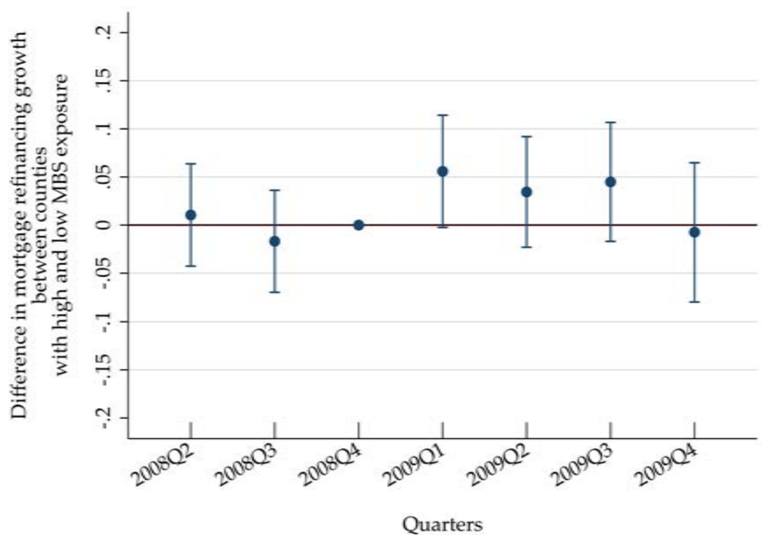
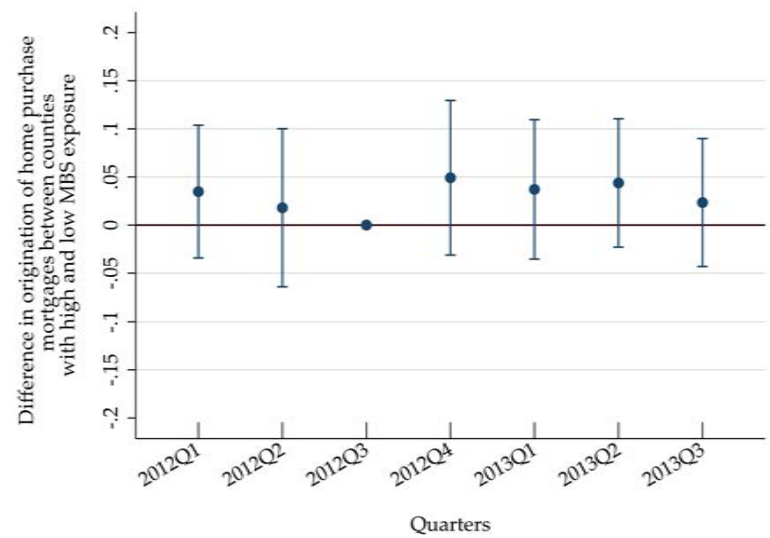


Figure 2: *MBS exposure, $Exposure_c^{SBL(1)}$, prior to QE1 over counties.*



(a) *QE1: Mortgage refinancing growth*



(b) *QE3: Mortgage origination growth*

Figure 3: *Coefficient estimates around event dates. This figure plots regression coefficients and confidence intervals for the difference in mortgage lending growth between counties in the upper and the lower tercile of the cross-county MBS exposure distribution in each quarter, i.e.* $\beta_k^{(j)}$ *from eq.* (6). *The coefficients are normalized to 0 in the quarter before the respective QE event. The outcome variable is either mortgage refinancing growth or mortgage origination growth. Vertical lines denote 95% confidence intervals based on standard errors clustered by county and quarter.*

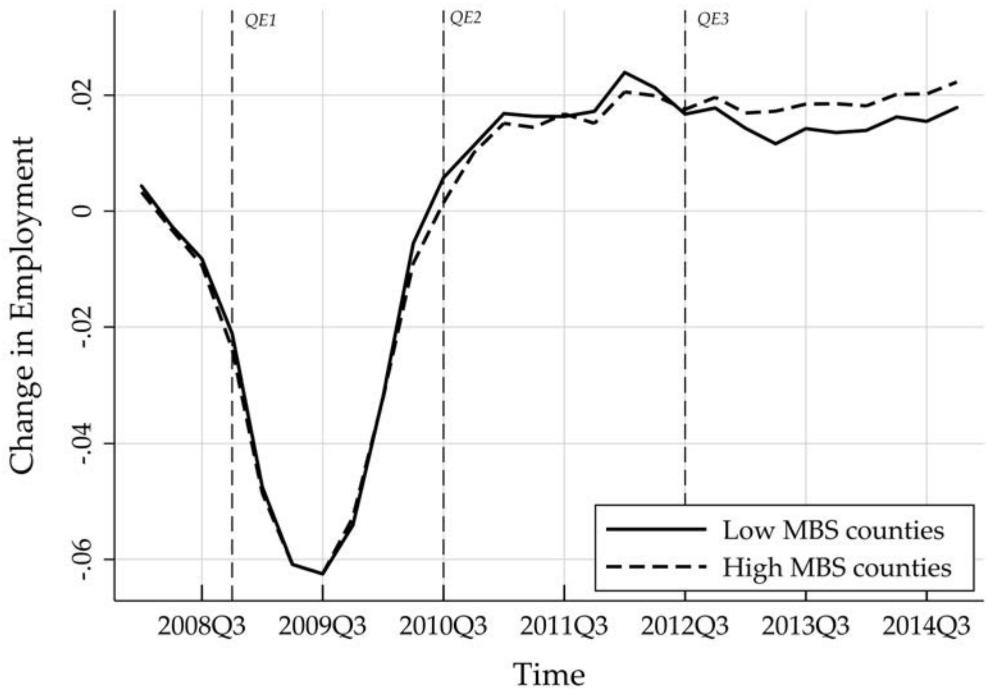
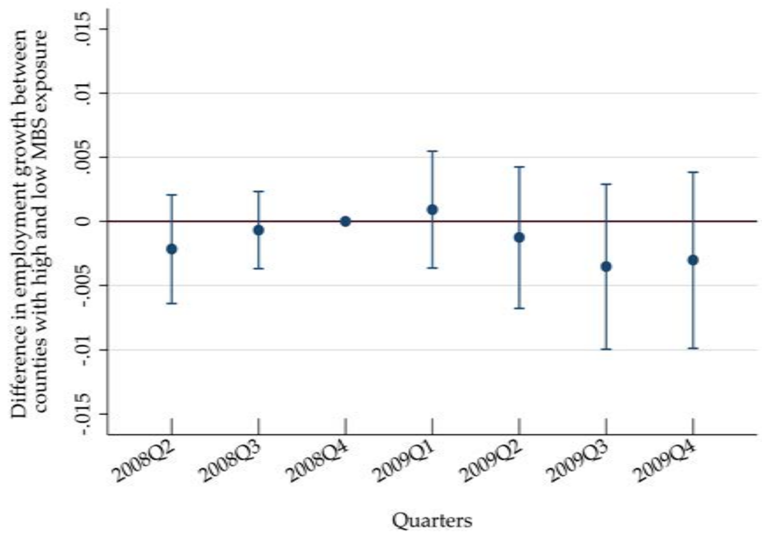
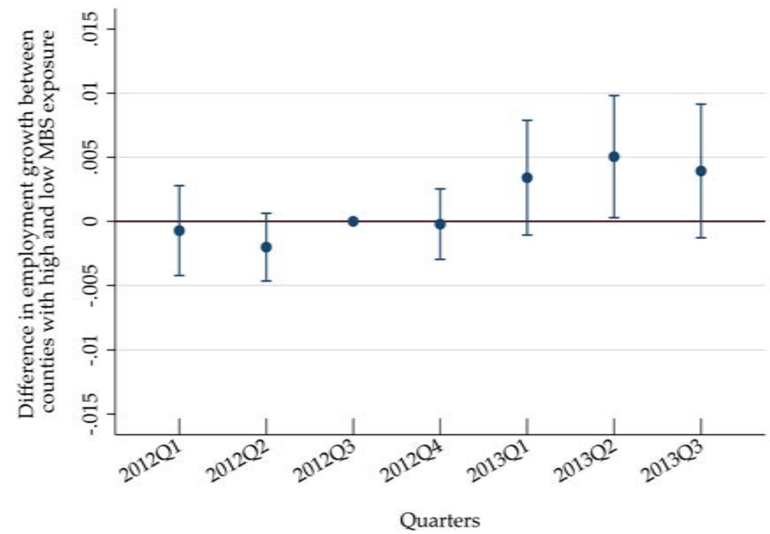


Figure 4: *Employment growth between 2008 and 2015. Annual employment growth rate, ΔEmp_{ct}, averaged separately over counties in the upper and in the lower terciles of the cross-county MBS exposure distribution prior to QE3. The two vertical lines indicate the quarters of the implementation of QE1 and QE3, respectively.*

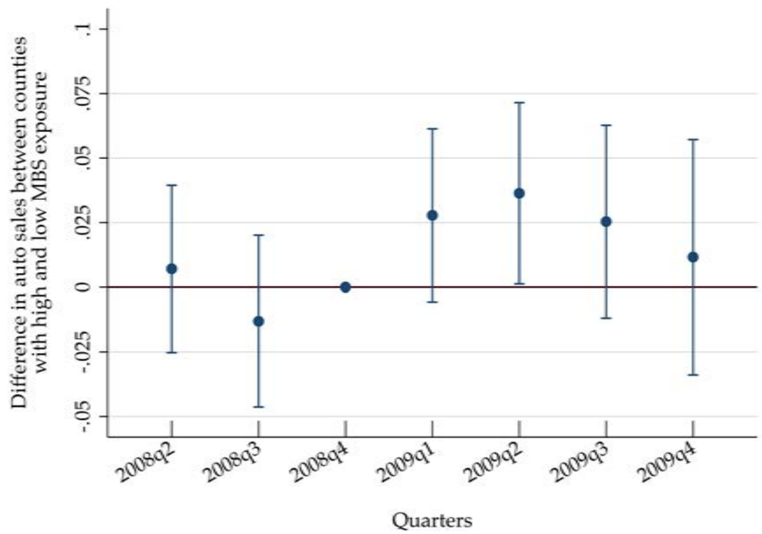


(a) *QE1*

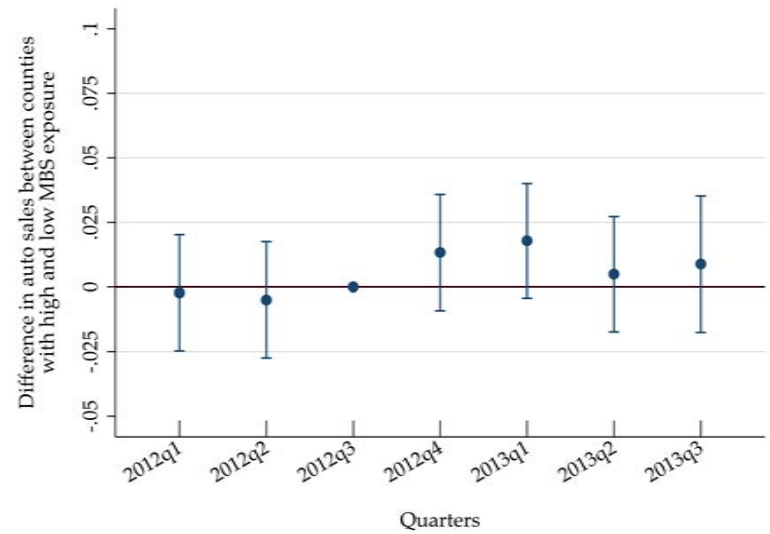


(b) *QE3*

Figure 5: *Coefficient estimates around event dates. This figure plots regression coefficients and confidence intervals for the difference in employment growth between counties in the upper and the lower tercile of the cross-county MBS exposure distribution in each quarter, i.e.* $\beta_k^{(j)}$ *from eq.* (6). *The coefficients are normalized to 0 in the quarter before the respective QE event. Vertical lines denote 95% confidence intervals based on standard errors clustered by county and quarter.*



(a) *QE1*



(b) *QE3*

Figure 6: *Coefficient estimates around event dates. This figure plots regression coefficients and confidence intervals for the difference in car registration growth between counties in the upper and the lower tercile of the cross-county MBS exposure distribution in each quarter, i.e.* $\beta_k^{(j)}$ *from eq.* (6). *The coefficients are normalized to 0 in the quarter before the respective QE event. Vertical lines denote 95% confidence intervals based on standard errors clustered by county and quarter.*

Tables

Table 1: *The announcement effect of QE1 (25 November 2008) and QE3 (13 September 2012)*

Dependent variable:	Daily return				Daily risk-adjusted Return			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\left(\frac{\text{MBS}}{\text{TotAssets}}\right)_b^{(1)}$	0.069**	0.081**			0.055*	0.078*		
	(0.033)	(0.041)			(0.033)	(0.042)		
$\left(\frac{\text{MBS}}{\text{TotAssets}}\right)_b^{(3)}$			0.019**	0.032***			0.014	0.025**
			(0.009)	(0.012)			(0.008)	(0.011)
log(Assets)		0.003		0.003***		0.002		0.002**
		(0.003)		(0.001)		(0.003)		(0.001)
Equity/Assets		0.164		0.081***		0.080		0.076**
		(0.139)		(0.030)		(0.139)		(0.030)
Profitability		0.071		-0.127		0.111		-0.105
		(0.125)		(0.082)		(0.130)		(0.085)
Real Estate Ratio		-0.017		-0.007		-0.003		-0.007
		(0.038)		(0.009)		(0.038)		(0.008)
R_a^2	0.012	0.049	0.012	0.115	0.008	0.035	0.007	0.066
N	325	325	315	315	325	325	315	315
Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table studies the impact of MBS exposure on stock returns on QE announcement days. The table reports coefficients from a cross-sectional regression of banks' daily stock returns on bank characteristics on either November 25, 2008 (QE1) or on September 13, 2012 (QE3). The outcome variable is either the bank holding company-level raw daily return or the risk-adjusted return that controls for the market return using a one-factor model, see eq. (1) and section 3.1.2 for details. Standard errors allow for heteroskedasticity. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 2: *Counties with high and low MBS exposure*

	Low MBS exposure		High MBS exposure		Difference	
	Mean	Std	Mean	Std	Diff	t-stat
Population (in thousand)	40.611	186.277	134.527	364.124	135.311	8.364
Median Income	41035.527	9181.616	44032.547	10519.178	4228.495	9.000
HousingMoodys	97.755	46.440	124.612	64.390	34.980	12.513
$\Delta Population_{2007Q4:2008Q4}$	0.001	0.010	0.005	0.010	0.004	8.058
$\Delta Housing_{2007Q4:2008Q4}$	-0.042	0.049	-0.041	0.047	0.000	0.125
$\Delta Income_{2007Q4:2008Q4}$	-0.027	0.053	-0.030	0.051	-0.001	-0.450

This table compares county characteristics prior to QE1 for counties in the upper and in the lower tercile of the cross-country MBS exposure distribution. MBS exposure is measured as the weighted average of bank-specific MBS ratios averaged over the four quarters prior to QE1, with weights given by banks' deposit shares per country, see eq. (2).

Table 3: *QE and county-level mortgage lending*

Panel A: Mortgage refinancing								
Dependent variable:	%ΔRefinancing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)HMDA}$	0.337**	0.327**						
	(0.144)	(0.144)						
$QE_t^{(1)} \times Treat_c^{(1)HMDA}$			0.031***	0.030***				
			(0.011)	(0.011)				
$QE_t^{(3)} \times Exposure_c^{(3)HMDA}$					0.048	0.060		
					(0.147)	(0.147)		
$QE_t^{(3)} \times Treat_c^{(3)HMDA}$							-0.010	-0.009
							(0.010)	(0.010)
R^2	0.593	0.593	0.563	0.563	0.189	0.189	0.174	0.174
No. Counties	2854	2854	1899	1899	2855	2855	1922	1922
N	19065	19065	12637	12637	19505	19505	13112	13112
Panel B: Mortgage origination								
Dependent variable	%ΔOrigination							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)HMDA}$	-0.149	-0.159						
	(0.156)	(0.157)						
$QE_t^{(1)} \times Treat_c^{(1)HMDA}$			-0.019	-0.020				
			(0.012)	(0.012)				
$QE_t^{(3)} \times Exposure_c^{(3)HMDA}$					0.219	0.228		
					(0.160)	(0.161)		
$QE_t^{(3)} \times Treat_c^{(3)HMDA}$							0.026**	0.027**
							(0.012)	(0.012)
R^2	0.239	0.239	0.220	0.220	0.213	0.213	0.192	0.192
No. Counties	2804	2804	1865	1865	2807	2807	1896	1896
N	19047	19047	12597	12597	19109	19109	12882	12882
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on mortgage lending. The outcome variable is county-level quarterly growth of mortgage refinancing volume in panel A, and county-level quarterly growth in mortgage origination volume in panel B. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average mortgage origination volume in a county in the four quarters prior to each QE event. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 4: *QE and county-level small business lending*

Panel A: Exposure measured with bank-county small business lending activity								
Dependent variable	%ΔC&ILending							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)SBL}$	0.663	0.693						
	(0.740)	(0.743)						
$QE_t^{(1)} \times Treat_c^{(1)SBL}$			0.071	0.061				
			(0.059)	(0.059)				
$QE_t^{(3)} \times Exposure_c^{(3)SBL}$					0.923***	0.923***		
					(0.325)	(0.325)		
$QE_t^{(3)} \times Treat_c^{(3)SBL}$							0.071**	0.070**
							(0.030)	(0.030)
R^2	0.292	0.293	0.278	0.280	0.326	0.326	0.329	0.330
No. Counties	2794	2794	1829	1829	2729	2729	1764	1764
N	5588	5588	3658	3658	5458	5458	3528	3528
Panel B: Exposure measured with bank-county deposit activity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)DEP}$	0.282	0.242						
	(0.452)	(0.454)						
$QE_t^{(1)} \times Treat_c^{(1)DEP}$			0.058	0.051				
			(0.052)	(0.053)				
$QE_t^{(3)} \times Exposure_c^{(3)DEP}$					0.258	0.256		
					(0.251)	(0.252)		
$QE_t^{(3)} \times Treat_c^{(3)DEP}$							0.064**	0.066**
							(0.031)	(0.031)
R^2	0.298	0.300	0.312	0.313	0.325	0.325	0.317	0.317
No. Counties	2788	2788	1828	1828	2735	2735	1796	1796
N	5576	5576	3656	3656	5470	5470	3592	3592
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on small business lending using the annual CRA data. The outcome variable is county-level annual growth of small business lending. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a county (panel A) and weighted by banks' average deposit volume in a county (panel B) prior to each QE event. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Data is restricted to two annual data points. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 5: *QE and county-level employment*

Panel A: Exposure measured with bank-county small business lending activity								
Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)SBL}$	0.017	0.026						
	(0.023)	(0.024)						
$QE_t^{(1)} \times Treat_c^{(1)SBL}$			0.001	0.002				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)SBL}$					0.051**	0.050**		
					(0.020)	(0.020)		
$QE_t^{(3)} \times Treat_c^{(3)SBL}$							0.004**	0.004**
							(0.002)	(0.002)
R^2	0.624	0.626	0.613	0.615	0.483	0.483	0.479	0.479
No. Counties	2947	2947	1964	1964	2947	2947	1964	1964
N	20205	20205	13436	13436	20293	20293	13478	13478

Panel B: Exposure measured with bank-county deposit activity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)DEP}$	-0.036	-0.031						
	(0.023)	(0.023)						
$QE_t^{(1)} \times Treat_c^{(1)DEP}$			-0.003*	-0.002				
			(0.001)	(0.001)				
$QE_t^{(3)} \times Exposure_c^{(3)DEP}$					0.014	0.013		
					(0.018)	(0.018)		
$QE_t^{(3)} \times Treat_c^{(3)DEP}$							0.003***	0.003***
							(0.001)	(0.001)
R^2	0.599	0.600	0.588	0.589	0.501	0.501	0.507	0.507
No. Counties	2946	2946	1965	1965	2952	2952	1968	1968
N	20597	20597	13730	13730	20636	20636	13741	13741

Continued on next page

Table 5: *(continued)*

Panel C: Exposure measured with bank-county mortgage lending activity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)HMDA}$	0.012	0.025						
	(0.031)	(0.031)						
$QE_t^{(1)} \times Treat_c^{(1)HMDA}$			0.002	0.003				
			(0.002)	(0.002)				
$QE_t^{(3)} \times Exposure_c^{(3)HMDA}$					0.059**	0.060**		
					(0.026)	(0.026)		
$QE_t^{(3)} \times Treat_c^{(3)HMDA}$							0.005***	0.005***
							(0.002)	(0.002)
R^2	0.605	0.607	0.590	0.592	0.485	0.485	0.488	0.488
No. Counties	2954	2954	1972	1972	2945	2945	1966	1966
N	20204	20204	13457	13457	20284	20284	13505	13505
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on employment. The outcome variable is county-level quarterly growth of employment. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a county (panel A), weighted by banks' average deposit volume in a county (panel B) and weighted by banks' average mortgage origination volume (panel C) prior to each QE event. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients for the indicator treatment variable. Standard errors in parentheses are clustered by county and quarter. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 6: *QE and county-level employment: Industry effects*

Panel A: Employment in tradable/other and nontradable goods sector

Dependent variable	ΔEmpTradOther		ΔEmpNonTrad		ΔEmpTradOther		ΔEmpNonTrad	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)SBL}$	-0.026		0.218**					
	(0.072)		(0.089)					
$QE_t^{(1)} \times Treat_c^{(1)SBL}$		-0.005		0.016**				
		(0.006)		(0.006)				
$QE_t^{(3)} \times Exposure_c^{(3)SBL}$					0.095*		-0.001	
					(0.056)		(0.043)	
$QE_t^{(3)} \times Treat_c^{(3)SBL}$						0.014**		0.005
						(0.006)		(0.005)
R^2	0.476	0.484	0.440	0.427	0.475	0.490	0.425	0.425
No. Counties	2959	1970	2959	1970	2951	1964	2951	1964
N	5918	3940	5918	3940	5902	3928	5902	3928

Panel B: Employment by financial dependence of industry

Dependent variable	ΔEmpFin		ΔEmpNonFin		ΔEmpFin		ΔEmpNonFin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)SBL}$	0.015		-0.098					
	(0.125)		(0.106)					
$QE_t^{(1)} \times Treat_c^{(3)SBL}$		0.003		-0.011				
		(0.010)		(0.008)				
$QE_t^{(3)} \times Exposure_c^{(3)SBL}$					0.116		0.066	
					(0.089)		(0.076)	
$QE_t^{(3)} \times Treat_c^{(3)SBL}$						0.013		0.006
						(0.008)		(0.008)
R^2	0.435	0.430	0.478	0.473	0.454	0.463	0.424	0.426
No. Counties	2959	1970	2959	1970	2960	1973	2960	1973
N	5918	3940	5918	3940	5920	3946	5920	3946
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 reports estimates of the effect of MBS exposure on employment by industry splits using the annual QCEW data. In panel A, the outcome variable is county-level annual employment growth by tradable or non-tradable industry. We use the definition of non-tradable industries of Mian and Sufi (2014). In panel B, the outcome variable is county-level annual employment growth by industry's financial dependence. We measure financial dependence as in Almeida et al. (2010). MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a county prior to each QE event. All specifications include county and time fixed effects and county-level controls, interacted with QE event dummies, see Equation (3) and Equation (4) for details. Data is restricted to two annual data points. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 7: *QE and county-level auto purchases*

Exposure measured with bank-county mortgage lending activity								
Dependent variable	ΔAuto							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)HMDA}$	0.505***	0.471***						
	(0.105)	(0.107)						
$QE_t^{(1)} \times Treat_c^{(1)HMDA}$			0.036***	0.035***				
			(0.009)	(0.009)				
$QE_t^{(3)} \times Exposure_c^{(3)HMDA}$					0.054	0.057		
					(0.087)	(0.088)		
$QE_t^{(3)} \times Treat_c^{(3)HMDA}$							0.014*	0.013*
							(0.007)	(0.007)
R^2	0.551	0.551	0.529	0.530	0.206	0.206	0.200	0.200
No. Counties	2683	2683	1794	1794	2685	2685	1795	1795
N	18775	18775	12544	12544	18790	18790	12560	12560
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on auto purchases. The outcome variable is county-level quarterly growth of car registrations. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average mortgage origination volume prior to each QE event. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 8: *QE and bank-firm-level lending*

Panel A: Continuous treatment						
Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in Compustat		Compustat	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$	0.319***	0.354***	0.326**	0.424***	0.151***	0.130**
	(0.021)	(0.052)	(0.119)	(0.074)	(0.053)	(0.054)
R^2	0.207	0.528	0.213	0.559	0.174	0.486
No Banks	25	25	25	25	25	25
No Firms	127305	9768	124729	8005	2638	1779
No Bank-Firm-Relationships	152803	33642	141636	23349	11231	10318
No obs	641048	145669	587074	96160	53696	49288

Panel B: Binary treatment						
Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in Compustat		Compustat	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE^{(3)} \times Treat_b^{(3)}$	0.025***	0.030***	0.024***	0.031***	0.021***	0.017***
	(0.002)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)
R^2	0.207	0.528	0.213	0.558	0.175	0.486
No Banks	25	25	25	25	25	25
No Firms	127305	9768	124729	8005	2638	1779
No Bank-Firm-Relationships	152803	33642	141636	23349	11231	10318
No obs	641048	145669	587074	96160	53696	49288
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	No	Yes	No	Yes	No
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interacted Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of banks' MBS exposure on bank-firm-level lending. The outcome variable is growth of total lending from each bank to each firm. MBS exposure is a bank's average MBS-to-assets ratio in the four quarters prior to QE3. All specifications include bank-firm fixed effects and bank-level controls. Specifications include either time fixed effects or firm-time fixed effects, see eq. (7) for details. We use the computationally efficient estimator of linear models with multiple high-dimensional fixed effects proposed by Correia (2017). Exposure is continuous in panel A and binary in panel B, i.e. in panel B, treatment is equal to 1 if a bank's MBS exposure is above the median of the cross-bank MBS exposure distribution. In columns (1) and (2), the data consist of all bank-firm relationships identified in the Y14 data. Columns (3) and (4) report estimates for firms within the Y14 data that cannot be matched to Compustat, and columns (5) and (6) report estimates that can be matched to Compustat. Standard errors in parentheses are clustered at the firm-time level and stars indicate significance at the 10%, 5%, and 1% level, respectively.

Table 9: *QE and firm-level lending, investment and employment*

Panel A: Firm-level borrowing								
Dependent variable	ΔCredit							
Sample	Compustat				Not in Compustat			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Exposure}_i^{(3)}$	0.255		0.334*		0.190***		0.203***	
	(0.158)		(0.201)		(0.036)		(0.040)	
$\text{Treat}_i^{(3)}$		0.012*		0.013*		0.017***		0.017***
		(0.006)		(0.008)		(0.002)		(0.003)
R^2	.0014	.0028	.0045	.0097	.00048	.0018	.00067	.0021
No Firms	1852	1171	1493	919	40319	27576	33623	23067
Panel B: Firm-level investment								
Dependent variable	ΔInvestment (in ppt)							
Sample	Compustat				Not in Compustat			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Exposure}_i^{(3)}$	3.628**		1.641		13.461***		12.599***	
	(1.703)		(1.910)		(0.846)		(0.832)	
$\text{Treat}_i^{(3)}$		0.141**		0.075		0.431***		0.217***
		(0.071)		(0.082)		(0.046)		(0.047)
R^2	.0021	.0033	.014	.015	.0068	.003	.0096	.047
No Firms	1852	1171	1690	1050	40319	27576	33623	23067
Panel C: Firm-level employment								
Dependent variable	$\Delta \text{Emp}_{2012Q4:2013Q4}$				$\Delta \text{Emp}_{2012Q4:2014Q4}$			
Sample	Compustat							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Exposure}_i^{(3)}$	0.458**		0.431**		0.640**		0.665**	
	(0.180)		(0.200)		(0.273)		(0.290)	
$\text{Treat}_i^{(3)}$		0.010*		0.011*		0.011		0.016*
		(0.006)		(0.006)		(0.009)		(0.010)
R^2	.014	.01	.02	.018	.029	.026	.064	.067
No Firms	1894	1256	1578	1045	1775	1173	1466	970
Firm Controls	No	No	Yes	Yes	No	No	Yes	Yes

This table reports estimates of the effect of firms' MBS exposure (via relationships to exposed banks) on lending, investment and employment. The outcome variable is a firm's lending growth between 2012Q4 and 2013Q4 in panel A, the difference in capital expenditure between 2012Q4 to 2013Q4 over assets in 2012Q4 in panel B, and employment growth from 2012Q to 2013Q4 or from from 2012Q to 2014Q4 in panel C. Exposure is measured as banks' MBS-to-asset ratios averaged over four quarters prior to QE3, weighted by lending volume at the bank-firm level averaged over four quarters prior to QE3, see eq. (9) for details. Panels A and B report estimates for firms in the Y14 data that can be matched to Compustat in columns (1) to (4), and estimates for firms that cannot be matched to Compustat in columns (5) to (8). Panel C uses only firms that can be matched to Compustat because employment is not available in the Y14 data. Firm controls include the firms's leverage, z-Score, current income, and size measured as log of total assets, see eq. (11) for details. Robust standard errors in parentheses. Stars indicate significance at the 10%, 5%, and 1% level, respectively.

Table 10: *QE and county-bank level mortgage lending*

Panel A: Mortgage refinancing								
Dependent variable	ΔRefinancing (conforming)				ΔRefinancing (non-conforming)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times \left(\frac{MBS}{Assets}\right)_b^{(1)}$	0.740***	0.182**			0.863***	0.659**		
	(0.060)	(0.071)			(0.243)	(0.304)		
$QE_t^{(3)} \times \left(\frac{MBS}{Assets}\right)_b^{(3)}$			0.091	0.087			0.099	0.141
			(0.087)	(0.110)			(0.186)	(0.199)
R^2	.14	.26	.037	.14	.05	.28	.016	.18
N	173910	172138	217239	212450	25565	21781	46110	42604
No Banks	3078	3075	2757	2738	1923	1638	1776	1595
No Counties	3078	2910	3070	2910	1852	1009	1932	1172
Panel B: Mortgage origination								
Dependent variable	ΔOrigination (conforming)				ΔOrigination (non-conforming)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times \left(\frac{MBS}{Assets}\right)_b^{(1)}$	0.108	0.104			0.561	0.212		
	(0.126)	(0.140)			(0.485)	(0.553)		
$QE_t^{(3)} \times \left(\frac{MBS}{Assets}\right)_b^{(3)}$			0.100	0.113			0.486***	0.627***
			(0.068)	(0.077)			(0.169)	(0.199)
R^2	.055	.18	.034	.16	.18	.36	.042	.23
N	190282	187973	224625	216700	26843	23828	38568	35318
No Banks	3330	3330	3004	3000	1698	1502	1504	1315
No Counties	3083	2909	3070	2867	1501	881	1515	872
Bank-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	No	Yes	No	Yes	No	Yes	No
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interacted Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Time FE	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on county-bank level mortgage lending. The outcome variable is county-bank level mortgage refinancing growth for conforming and non-conforming mortgages in panel A, and county-bank level mortgage origination growth for conforming and non-conforming mortgages in panel B. All specifications include bank-county fixed effects and bank-level controls. Specifications include either time fixed effects or county-time fixed effects, see eq. (12) for details. We use the computationally efficient estimator of linear models with multiple high-dimensional fixed effects proposed by Correia (2017). Standard errors are two-way clustered at the county and time levels. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table 11: *Robustness: QE2, Tapering and mortgage lending*

Panel A: Mortgage refinancing

Dependent variable:	ΔRefinancing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(2)} \times Exposure_c^{(2)HMDA}$	0.045	0.082						
	(0.156)	(0.155)						
$QE_t^{(2)} \times Treat_c^{(2)HMDA}$			-0.006	-0.000				
			(0.011)	(0.011)				
$Taper_t \times Exposure_c^{(3)HMDA}$					-0.228	-0.229		
					(0.174)	(0.175)		
$Taper_t \times Treat_c^{(3)HMDA}$							-0.029***	-0.029***
							(0.011)	(0.011)
R^2	0.382	0.383	0.348	0.349	0.219	0.219	0.198	0.198
No. Counties	2869	2869	1907	1907	2860	2860	1906	1906
N	19762	19762	13081	13081	19679	19679	13054	13054

Panel B: Mortgage origination

Dependent variable	ΔOrigination							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(2)} \times Exposure_c^{(2)HMDA}$	0.068	0.045						
	(0.174)	(0.184)						
$QE_t^{(2)} \times Treat_c^{(2)HMDA}$			-0.014	-0.018				
			(0.013)	(0.014)				
$Taper_t \times Exposure_c^{(3)HMDA}$					-0.243	-0.317		
					(0.209)	(0.211)		
$Taper_t \times Treat_c^{(3)HMDA}$							-0.020	-0.030*
							(0.016)	(0.017)
R^2	0.257	0.257	0.246	0.246	0.333	0.334	0.300	0.301
No. Counties	2847	2847	1893	1893	2856	2856	1927	1927
N	19311	19311	12794	12794	19502	19502	13138	13138
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of MBS exposure on mortgage lending. The outcome variable is county-level quarterly growth of mortgage refinancing volume in panel A, and county-level quarterly growth in mortgage origination volume in panel B. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average mortgage origination volume in a county in the four quarters prior to either QE2 or Tapering. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 12: *Robustness: QE2, Tapering and employment*

Panel A: Exposure measured with bank-county small business lending activity								
Dependent variable	ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(2)} \times Exposure_c^{(2)SBL}$	0.006	0.000						
	(0.012)	(0.012)						
$QE_t^{(2)} \times Treat_c^{(2)SBL}$			0.000	-0.000				
			(0.001)	(0.001)				
$Taper_t \times Exposure_c^{(3)SBL}$					-0.028**	-0.023*		
					(0.013)	(0.013)		
$Taper_t \times Treat_c^{(3)SBL}$							-0.002**	-0.002*
							(0.001)	(0.001)
R^2	0.547	0.549	0.527	0.529	0.527	0.527	0.527	0.527
No. Counties	2947	2947	1967	1967	2950	2950	1967	1967
N	20278	20278	13494	13494	20335	20335	13549	13549

Panel B: Exposure measured with bank-county deposit activity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(2)} \times Exposure_c^{(2)DEP}$	0.012	0.013						
	(0.009)	(0.009)						
$QE_t^{(2)} \times Treat_c^{(2)DEP}$			0.001	0.001				
			(0.001)	(0.001)				
$Taper_t \times Exposure_c^{(3)DEP}$					-0.031	-0.032		
					(0.026)	(0.026)		
$Taper_t \times Treat_c^{(3)DEP}$							-0.005	-0.005
							(0.004)	(0.004)
R^2	0.521	0.521	0.512	0.512	0.370	0.370	0.345	0.345
No. Counties	2929	2929	1949	1949	2946	2946	1966	1966
N	20225	20225	13422	13422	20608	20608	13743	13743

Continued on next page

Table 12: *(continued)*

Panel C: Exposure measured with bank-county mortgage lending activity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(2)} \times Exposure_c^{(2)HMDA}$	0.043	0.010						
	(0.035)	(0.035)						
$QE_t^{(2)} \times Treat_c^{(2)HMDA}$			0.004*	0.002				
			(0.002)	(0.003)				
$Taper_t \times Exposure_c^{(3)HMDA}$					-0.023	-0.020		
					(0.018)	(0.018)		
$Taper_t \times Treat_c^{(3)HMDA}$							-0.003	-0.003
							(0.002)	(0.003)
R^2	0.539	0.540	0.529	0.530	0.516	0.517	0.478	0.478
No. Counties	2900	2900	1974	1974	2945	2945	1965	1965
N	20279	20279	13797	13797	20493	20493	13661	13661
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
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of MBS exposure on employment. The outcome variable is county-level quarterly growth of employment. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a county (panel A), weighted by banks' average deposit volume in a county (panel B) and weighted by banks' average mortgage origination volume (panel C) prior to each QE event. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients 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 13: *Robustness: Tapering and firm-level lending*

Panel A: Continous treatment						
Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in Compustat		Compustat	
	(1)	(2)	(3)	(4)	(5)	(6)
Taper × $\left(\frac{\text{MBS}}{\text{TotAssets}}\right)_b^{(3)}$	-0.051***	-0.120***	-0.050	-0.141***	-0.071***	-0.070***
	(0.009)	(0.025)	(0.039)	(0.036)	(0.025)	(0.026)
R_a^2	0.148	0.473	0.153	0.512	0.122	0.419
No Banks	26	26	26	26	26	26
No Firms	145122	11396	142480	9532	2718	1890
No Bank-Firm-Relationships	175129	39483	163053	28231	12188	11318
No obs	997172	226112	921785	156069	75102	69785
Panel B: Binary treatment						
Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in Compustat		Compustat	
	(1)	(2)	(3)	(4)	(5)	(6)
Taper × $\text{Treat}_b^{(3)}$	-0.007***	-0.013***	-0.008**	-0.016***	-0.011***	-0.008***
	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
R_a^2	0.147	0.473	0.153	0.512	0.127	0.419
No Banks	25	25	25	25	25	25
No Firms	144847	11360	142220	9506	2704	1880
No Bank-Firm-Relationships	174682	39280	162683	28099	12112	11247
No obs	996278	225698	921045	155800	74950	69640
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	No	Yes	No	Yes	No
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interacted Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	No	Yes	No	Yes	No	Yes

This table reports estimates of banks' MBS exposure on bank-firm-level lending. The outcome variable is growth of total lending from each bank to each firm. MBS exposure is a bank's average MBS-to-assets ratio in the four quarters prior to QE3. All specifications include bank-firm fixed effects and bank-level controls. Specifications include either time fixed effects or firm-time fixed effects, see eq. (7) for details. We use the computationally efficient estimator of linear models with multiple high-dimensional fixed effects proposed by Correia (2017). Exposure is continuous in panel A and binary in panel B, i.e. in panel B, treatment is equal to 1 if a bank's MBS exposure is above the median of the cross-bank MBS exposure distribution. In columns (1) and (2), the data consist of all bank-firm relationships identified in the Y14 data. Columns (3) and (4) report estimates for firms within the Y14 data that cannot be matched to Compustat, and columns (5) and (6) report estimates that can be matched to Compustat. Standard errors in parentheses are clustered at the firm-time level and stars indicate significance at the 10%, 5%, and 1% level, respectively.

Description of appendices

- Appendix A: Bank-level analysis of QE and mortgage and small business lending
- Appendix B: Supplementary figures
- Appendix C: Supplementary tables
- Appendix D: Variable construction

Appendix A Bank-level analysis

A.1 Empirical Strategy

This section provides detailed bank-level results complementary to those of previous studies. We construct our bank-level exposure measure as a bank's MBS scaled by total assets. Figure B.1 shows the distribution of the exposure measure averaged over the four quarters prior to QE1. More than a quarter of all commercial banks held no MBS at all, and the average MBS-to-asset ratio was around 12% in the upper quartile of the cross-sectional distribution across banks.

[FIGURE B.1 ABOUT HERE]

While banks with higher MBS shares tend to be larger and tend to operate with higher leverage than banks with relatively lower MBS shares, banks sorted by MBS shares are otherwise very similar in other observable characteristics. Table C.5 gives a sense of those differences, splitting the sample of all commercial banks in the United States by the median of the average MBS share in the 4 quarters prior to QE1.

[TABLES C.5 and C.6 ABOUT HERE]

In order to assess the effect of the Fed's actions at the bank level, 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:

$$\ln(y_{bt}) = \alpha + \beta \left(\frac{\text{MBS}}{\text{Total Assets}}\right)_b^{(j)} \text{QE}_t^{(j)} + \theta X_{bt} + \gamma_b + \tau_t + \epsilon_{bt} \tag{13}$$

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.[11] $\text{QE}_t^{(j)}$ is an indicator variable equal to 1 after the introduction of the j-th round of quantitative easing.[12] We measure bank b's

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

[12] Given that the data on the commercial bank level is quarterly, we choose 2009Q1 as the event date for QE1, and 2010Q4

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 C.6.

A.2 Results

Table C.8 estimates Equation (13) 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 C.8 ABOUT HERE]

We make three observations about the results depicted in Table C.8. 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. [13] 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 (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

and 2012Q4 as the event dates for QE2 and QE3, respectively.

[13]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)).

sense of the timing of effects. Table C.9 shows the estimates from Equation (13), 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 C.8. 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 C.9 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 C.10 shows results. Consistent with the results in Table C.8, small business lending does not respond in any category after QE1 and estimated coefficients fluctuate widely (columns (1), (3), (5), and (7) of Table C.10). 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.

[TABLE C.10 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 is comparable to the magnitude of the effect on total C&I lending in Table C.8.

Appendix B Supplementary Figures

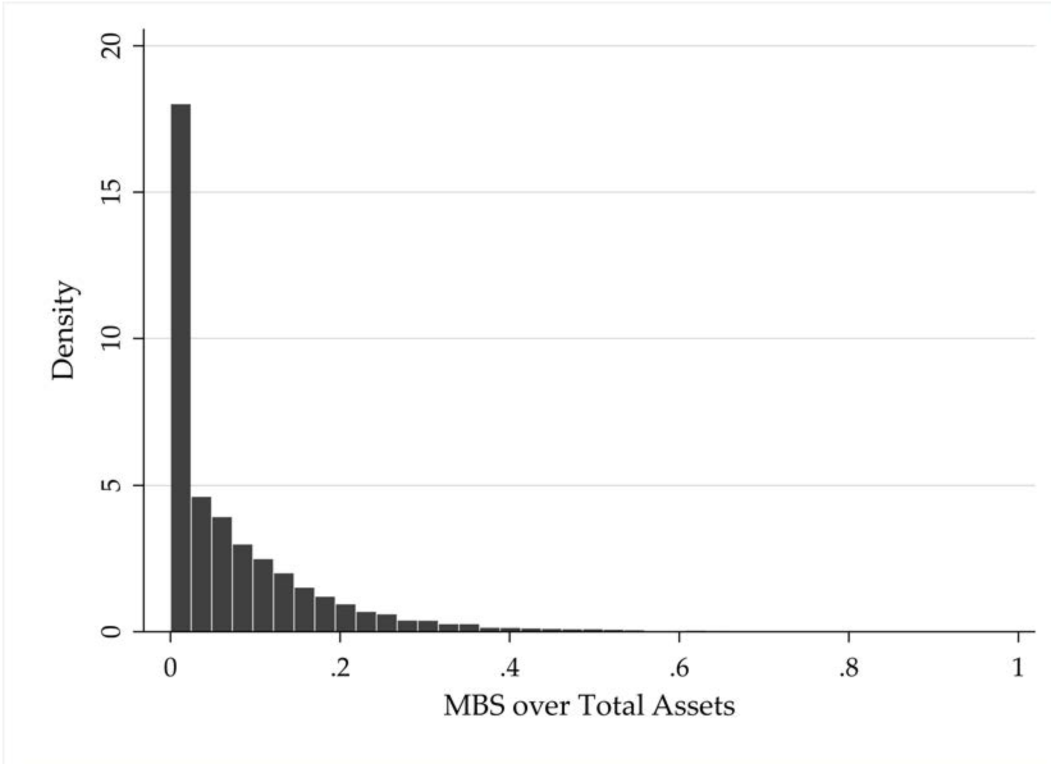
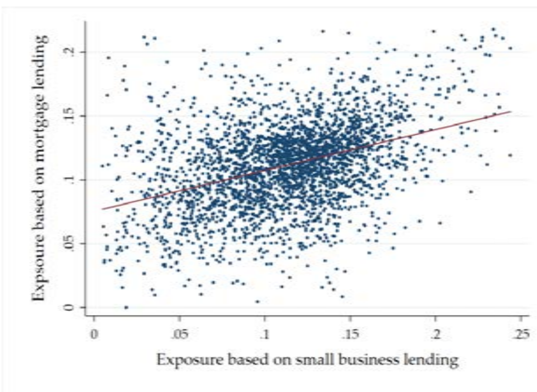
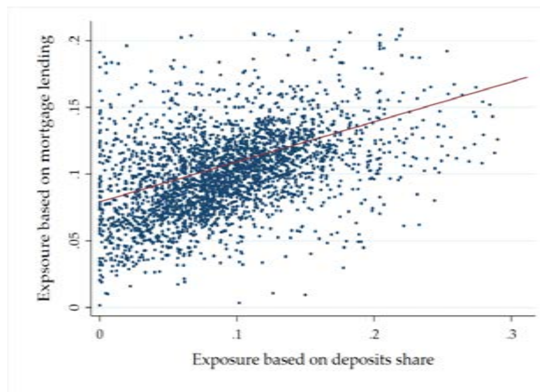


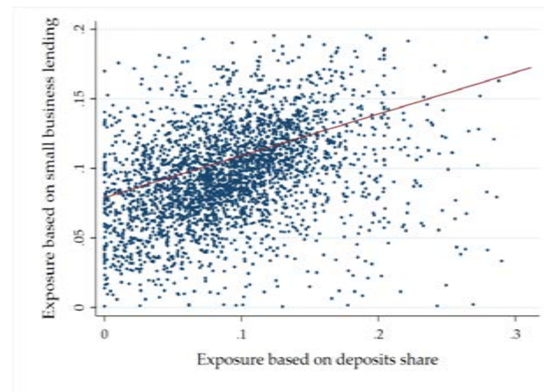
Figure B.1: *Distribution of banks' average MBS shares prior to QE1*



(a) *Mortgage and small business lending,* $\rho = 0.49$



(b) *Mortgage lending and deposits,* $\rho = 0.56$



(c) *Small business lending and deposits,* $\rho = 0.54$

Figure B.2: *Scatterplot of different exposure measures prior to QE3. A county's MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a county, weighted by banks' average deposit volume in a county (panel B) and weighted by banks' average mortgage origination volume (panel C) prior to each QE event.*

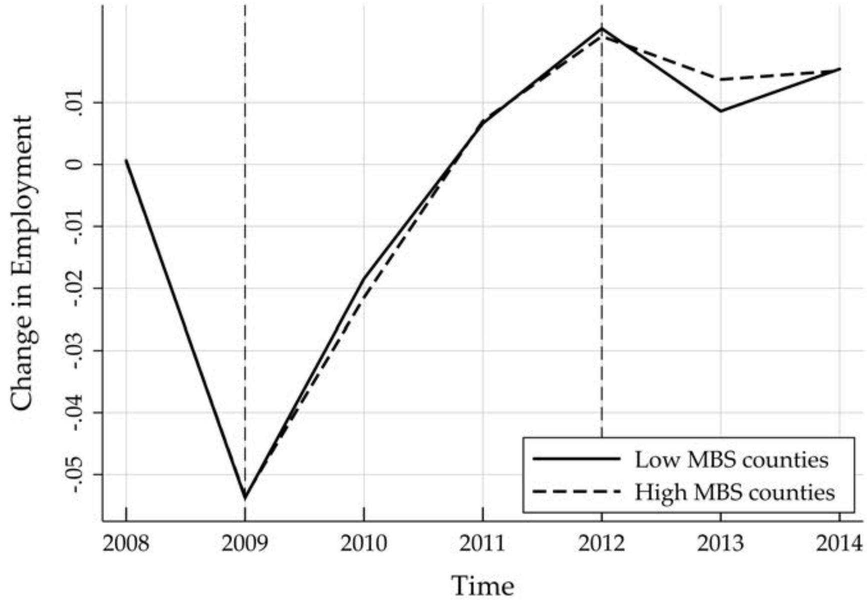


Figure B.3: *Employment growth between 2008 and 2014 in the annual CBP data. Annual employment growth rate,* ΔEmp_{ct}*, averaged separately over counties in the upper and in the lower terciles of the cross-county MBS exposure distribution prior to QE3.*

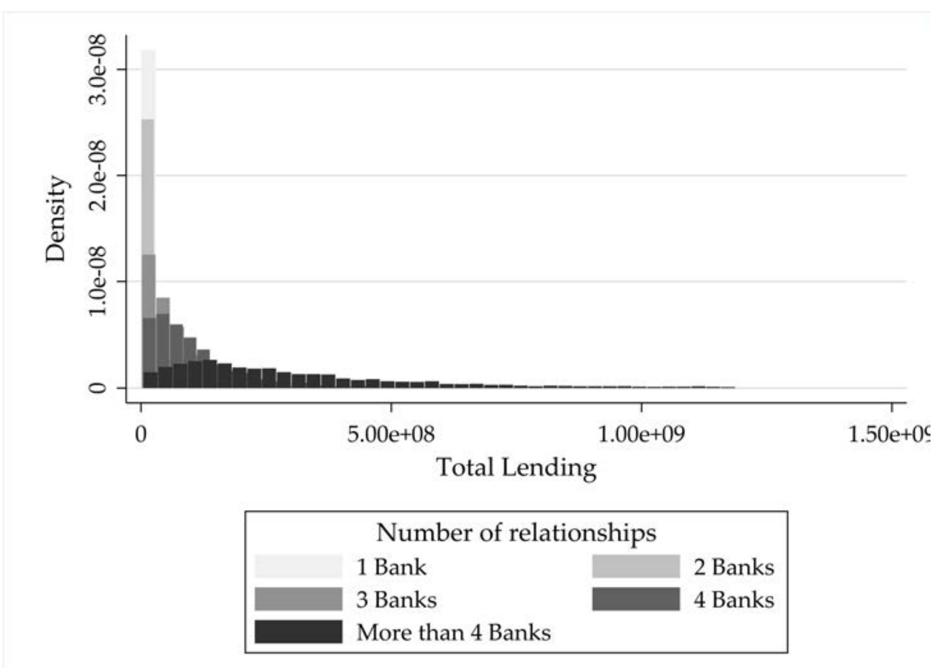


Figure B.4: *Distribution of firms' average total lending in 2012 in the Y-14 data by the number of bank-firm relationships. A firm and a bank have a relationship if the bank lends to the firm in at least three quarters between 2011Q3 and 2016Q2.*

Appendix C Supplementary Tables

Table C.1: *Banks with high and low MBS shares: Bank in Y14 data*

	Low MBS Share		High MBS Share		Difference	
	Mean	Std	Mean	Std	Diff	t-stat
log(assets)	19.428	1.418	19.042	0.948	-0.339	-0.696
Equity/TotalAssets	0.106	0.029	0.116	0.015	0.009	1.125
Tier 1 Ratio	0.129	0.024	0.123	0.021	-0.005	-0.694
RoA	0.009	0.012	0.005	0.009	-0.003	-1.165
Real Estate Ratio	0.175	0.182	0.279	0.145	0.096	1.452
Loans to Assets	0.469	0.262	0.560	0.214	0.083	0.862
C&I Loan Ratio	0.317	0.321	0.231	0.091	-0.086	-0.929
Non-performing Loans Ratio	0.010	0.010	0.012	0.009	0.001	0.505

This table reports bank characteristics prior to QE3 averaged separately for banks above and below the median of the cross-bank MBS-to-assets distribution, for banks in the Y14 data only. MBS-to-assets are averaged over the four quarters prior to QE3 for each bank.

Table C.2: *Descriptive statistics for bank-level control variables: Banks in Y14 data*

	Mean	Std	10th Perc	25th Perc	Median	75th Perc	90th Perc	N
MBS/TotalAssets	0.095	0.058	0.009	0.031	0.103	0.154	0.162	502
MBS/TotalSecurities	0.553	0.293	0.195	0.236	0.578	0.806	0.907	502
log(assets)	19.237	1.223	17.826	18.208	18.961	20.434	21.365	502
Equity/TotalAssets	0.111	0.024	0.080	0.096	0.111	0.123	0.135	502
Tier 1 Ratio	0.126	0.023	0.101	0.111	0.122	0.137	0.158	502
RoA	0.007	0.012	-0.001	0.003	0.006	0.011	0.017	502
Real Estate Ratio	0.227	0.173	0.002	0.029	0.225	0.369	0.468	502
Loans to Assets	0.514	0.243	0.075	0.340	0.630	0.699	0.746	502
C&I Loan Ratio	0.282	0.279	0.053	0.148	0.221	0.303	0.603	502
Non-performing Loans Ratio	0.011	0.010	0.001	0.004	0.009	0.016	0.024	502

This table reports means, standard deviations and various percentiles of variables used as controls in bank-level regressions. The dataset runs from 2011Q3 to 2016Q3 and includes 25 banks.

Table C.3: *Descriptive statistics for firm-level variables*

	Mean	Std	10th Perc	25th Perc	Median	75th Perc	90th Perc	N
Sample				**Compustat**				
Total Assets (in million)	15005.26	108343.67	192.69	611.90	2019.86	6728.73	21728.00	1831
Total Bank Credit	3823.87	7029.06	162.74	500.00	1819.39	4747.69	9231.01	1831
Capital Expenditures	414.85	1449.08	1.13	10.18	51.92	234.46	904.52	1831
No of Employees	16927.64	67383.27	238.00	999.00	3400.00	11800.00	35283.00	1831
Sample				**Not in Compustat**				
Total Assets (in million)	1381.05	27442.46	1.57	4.94	16.05	71.55	421.42	40319
Total Bank Credit	211.18	765.16	12.07	18.59	40.20	129.14	402.39	40319
Capital Expenditures	30.29	1013.57	-0.17	0.00	0.01	1.17	11.94	40319

This table reports means, standard deviations and various percentiles of firm characteristics for firms in the Y14 data collection. Panel A includes firms that can be matched to Compustat, and also includes employment which is not available in the Y14 data. Panel B includes firms that cannot be matched to Compustat. Data are as reported in 2012Q4.

Table C.4: *Robustness: Main employment effect*

Dependent variable	ΔEmp							
	Population		Exclude Northeast		QE1 exposure		MBS Exposure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(3)} \times Exposure_c^{(3)}$	0.044**		0.045**					
	(0.022)		(0.022)					
$QE_t^{(3)} \times Treat_c^{(3)}$		0.004**		0.004**				
		(0.002)		(0.002)				
$QE_t^{(3)} \times Exposure_c^{(1)}$					0.037*			
					(0.022)			
$QE_t^{(3)} \times Treat_c^{(1)}$						0.004*		
						(0.002)		
$QE_t^{(3)} \times Exposure_c^{(3)MBS}$							0.012**	
							(0.005)	
$QE_t^{(3)} \times Treat_c^{(3)MBS}$								0.004*
								(0.002)
R_a^2	0.457	0.452	0.436	0.433	0.433	0.428	0.435	0.420
No. Counties	1750	1161	2590	1734	2888	1922	2784	1862
N	12069	8079	17628	11756	19718	13076	18978	12675
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
Interacted Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports estimates of the effect of MBS exposure on employment in a number of robustness checks. The outcome variable is county-level quarterly growth of employment. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average deposit volume in a county prior to QE3. All specifications include county and time fixed effects and county-level controls, see Equation (3) and Equation (4) for details. In columns (1) and (2), the sample is restricted to counties with population of more than 15000 and no more than 250000. In columns (3) and (4), the sample excludes the following states: Connecticut, Delaware, Massachusetts, Maine, New York, New Jersey, New Hampshire, Pennsylvania, Rhode Island, Vermont. In columns (5) and (6), exposure is based on the MBS holdings of banks in the 4 quarters prior to QE1. In columns (7) and (8), exposure is based on MBS over total securities instead of MBS over total assets. Standard errors in parentheses are clustered by county and quarter. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table C.5: *Banks with high and low MBS shares*

	Low MBS Share		High MBS Share		Difference	
	Mean	Std	Mean	Std	Diff	t-stat
log(assets)	11.622	1.262	12.171	1.396	0.549	17.660
Equity/TotalAssets	0.133	0.119	0.115	0.075	-0.018	-8.263
Deposit Ratio	0.799	0.152	0.798	0.101	-0.001	-0.365
Trading Book Ratio	0.001	0.013	0.001	0.006	-0.000	-0.245
Profitability	0.006	0.183	0.003	0.015	-0.003	-1.598
Overhead Ratio	0.820	1.225	0.807	1.129	-0.012	-0.612
Net interest margin	0.020	0.685	0.023	0.011	0.003	0.451
Non-performing Loans Ratio	0.010	0.018	0.010	0.017	-0.001	-1.480
Delinquency Ratio	1.950	1.104	1.916	1.066	-0.034	-1.476
Real Estate Ratio	0.676	0.180	0.693	0.163	0.016	4.080
C&I Loan Ratio	0.154	0.114	0.155	0.099	0.002	0.698
Tier 1 Ratio	0.233	1.116	0.173	0.350	-0.061	-3.360

This table reports bank characteristics prior to QE1 averaged separately for banks above and below the median of the cross-bank MBS-to-assets distribution. MBS-to-assets are averaged over the four quarters prior to QE1 for each bank.

Table C.6: *Descriptive statistics for bank-level control variables*

	Mean	Std	10th Perc	25th Perc	Median	75th Perc	90th Perc	N
Agency MBS/TotalAssets	0.08	0.10	0.00	0.00	0.05	0.12	0.21	166563
Agency MBS/TotalSecurities	0.36	0.33	0.00	0.02	0.31	0.60	0.84	166563
Treasuries/TotalAssets	0.01	0.03	0.00	0.00	0.00	0.00	0.01	166563
Treasuries/TotalSecurities	0.03	0.13	0.00	0.00	0.00	0.00	0.04	166563
log(Assets)	12.07	1.31	10.62	11.23	11.93	12.71	13.61	166563
Equity/TotalAssets	0.11	0.04	0.08	0.09	0.10	0.12	0.15	166563
Deposit Ratio	0.84	0.07	0.75	0.81	0.85	0.88	0.90	166563
Trading Book Ratio	0.00	0.01	0.00	0.00	0.00	0.00	0.00	166563
Profitability	0.00	0.01	-0.00	0.00	0.00	0.01	0.01	166563
Overhead Ratio	0.77	2.59	0.53	0.61	0.71	0.82	0.97	166560
Net interest margin	0.02	0.01	0.01	0.01	0.02	0.03	0.04	166563
Non-performing Loans Ratio	0.01	0.02	0.00	0.00	0.01	0.02	0.03	166563
Delinquency Ratio	2.11	1.73	0.31	0.85	1.71	2.93	4.42	166563
Real Estate Ratio	0.69	0.19	0.43	0.59	0.73	0.83	0.89	166563
C&I Loan Ratio	0.15	0.10	0.05	0.08	0.12	0.19	0.26	166563
Tier 1 Ratio	0.16	0.24	0.10	0.12	0.14	0.18	0.24	166563

This table reports means, standard deviations and various percentiles of variables used as controls in bank-level regressions. The dataset runs from 2007Q1 to 2015Q2, and includes up to around 7000 banks.

Table C.7: *QE and MSA-level employment*

Dependent variable	%ΔEmp							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times Exposure_c^{(1)SBL}$	-0.003	0.001						
	(0.014)	(0.014)						
$QE_t^{(1)} \times Treat_c^{(1)SBL}$			-0.000	0.000				
			(0.001)	(0.001)				
$QE_t^{(3)} \times Exposure_c^{(3)SBL}$					0.044***	0.048***		
					(0.013)	(0.013)		
$QE_t^{(3)} \times Treat_c^{(3)SBL}$							0.004***	0.004***
							(0.001)	(0.001)
R^2	0.723	0.724	0.704	0.704	0.541	0.541	0.560	0.560
No. Counties	950	950	633	633	928	928	617	617
N	6383	6383	4236	4236	6419	6419	4266	4266
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interacted Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table reports estimates of the effect of MBS exposure on employment. The outcome variable is MSA-level quarterly growth of employment. MBS exposure is measured as banks' MBS-to-asset ratios, weighted by banks' average small business lending volume in a MSA prior to each QE event. All specifications include MSA and time fixed effects and MSA-level controls, see Equation (3) and Equation (4) for details. Column (1)-(2) and (5)-(6) report coefficients for the continuous treatment variable and column (3)-(4) and (7)-(8) report coefficients for the indicator treatment variable. Standard errors in parentheses are clustered by MSA and quarter. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table C.8: *QE and bank-level lending*

Dependent variable	log(Lending)			log(RE lending)			log(C&I)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	0.073***			0.102***			0.005		
	(0.026)			(0.033)			(0.035)		
$QE_t^{(2)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(2)}$		0.135			0.015			0.026	
		(0.117)			(0.056)			(0.106)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$			0.073***			0.067***			0.078**
			(0.024)			(0.026)			(0.035)
R_a^2	0.198	0.053	0.073	0.174	0.044	0.031	0.046	0.009	0.008
No. Banks	6947	6448	6014	6919	6423	5995	6910	6414	5984
N	60931	56718	53081	60666	56479	52903	60520	56366	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
Interacted Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports estimates of the effect of banks' MBS-to-asset ratios on lending using Call report data. The outcome variable is the log of a bank's quarterly total lending in columns (1) to (3), the log of quarterly real estate lending in columns (4) to (6), and the log of C&I lending in columns (7) to (9). MBS-to-assets is averaged over the four quarters prior to each QE event. All specifications include bank and time fixed effects and bank-level controls, see eq. (13) for details. Standard errors in parentheses are two-way clustered at the bank and quarter levels. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table C.9: *QE and bank-level mortgage lending*

Dependent variable	log(Total)		log(Refinance)		log(Origination)	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	0.452***		0.712***		0.212	
	(0.161)		(0.206)		(0.148)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$		0.229**		0.220*		0.214*
		(0.113)		(0.129)		(0.127)
R_a^2	0.057	0.032	0.119	0.064	0.065	0.108
No. Banks	4178	3581	4039	3495	4102	3493
N	24958	25449	22811	23206	23381	23780
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Interacted Controls	Yes	Yes	Yes	Yes	Yes	Yes

This table reports estimates of the effect of banks' MBS-to-asset ratios on mortgage lending using Home Mortgage Disclosure Act (HMDA) data. The outcome variable is the log of a bank's quarterly total mortgage lending in columns (1) and (2), the log of mortgage refinancing volume in columns (3) and (4), and the log of mortgage origination volume in columns (5) to (6). MBS-to-assets is averaged over the four quarters prior to each QE event. All specifications include bank and time fixed effects and bank-level controls, see eq. (13) for details. Standard errors in parentheses are two-way clustered at the bank and quarter levels. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table C.10: *QE and bank-level small business lending*

Dependent variable	log(C&I lending)							
Loan size	[0, 100k]		[100k, 250k]		[250k, 1m]		Rev<1mil	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$QE_t^{(1)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(1)}$	-0.126		0.012		0.107		0.248*	
	(0.124)		(0.120)		(0.122)		(0.150)	
$QE_t^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$		0.137		0.183**		0.395***		0.299**
		(0.114)		(0.089)		(0.125)		(0.134)
R_a^2	0.316	0.072	0.322	0.136	0.319	0.150	0.350	0.119
No. Banks	743	652	742	653	744	654	743	650
N	1826	1674	1821	1674	1818	1681	1826	1665
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
Interacted Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports estimates of the effect of banks' MBS-to-asset ratios on mortgage lending using Community Reinvestment Act (CRA) data. The outcome variable is the log of a bank's total C&I loans with loan volume less than $100k in columns (1) and (2), the log of total C&I loans with loan volume between $100k and $250k in columns (3) and (4), the log of total C&I loans with loan volume between $250k and $1m in columns (5) and (6), and the log of total C&I loans to businesses with revenue less than $1m in columns (7) and (8). MBS-to-assets is averaged over the four quarters prior to each QE event. All specifications include bank and time fixed effects and bank-level controls, see eq. (13) for details. Standard errors in parentheses are two-way clustered at the bank and quarter levels. Stars indicate significance at the 10%, 5% and 1% levels, respectively.

Table C.11: *Firm-level regression: Investment and employment effects for firms in Y14 and for firms matched to Compustat*

Panel A: Continous treatment

Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in SNC		SNC sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE^{(3)} \times \left(\frac{MBS}{TotAssets}\right)_b^{(3)}$	0.344***	0.402***	0.301**	0.501***	0.206***	0.226***
	(0.023)	(0.054)	(0.120)	(0.145)	(0.041)	(0.046)
R^2	0.206	0.524	0.217	0.560	0.189	0.529
No Banks	25	25	25	25	25	25
No Firms	127305	9768	117713	4101	9592	5667
No Bank-Firm-Relationships	152803	33642	124696	9726	28107	23916
No obs	641048	145669	511503	37675	129545	107994

Panel B: Binary treatment

Dependent variable	ΔC&I lending					
Sample	Entire sample		Not in SNC		SNC sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$QE^{(3)} \times Treat_b^{(3)}$	0.022***	0.026***	0.021***	0.030***	0.024***	0.022***
	(0.001)	(0.004)	(0.007)	(0.011)	(0.004)	(0.004)
R^2	0.208	0.533	0.217	0.560	0.189	0.529
No Banks	25	25	25	25	25	25
No Firms	127305	9768	117713	4101	9592	5667
No Bank-Firm-Relationships	152803	33642	124696	9726	28107	23916
No obs	641048	145669	511503	37675	129545	107994

Bank-firm level analysis with firm-time fixed effect. Observation are restricted to 7 quarters, 3 quarters before and 3 quarters after QE3. Standard errors in parentheses are clustered at the firm-time 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).

Appendix D Variable construction

Bank variables are from the merger-adjusted Consolidated Reports of Condition and Income (FFIEC031 and FFIEC041). Banks are indexed by b, time is indexed by t (quarters).

- Total securities (available for sale, fair value)$_{b,t}$: RCFD1773
- Total securities (held to maturity, amortized cost)$_{b,t}$: RCFD1754
- MBS (held to maturity, amortized cost)$_{b,t}$: The sum of all item in Schedule RC-B, item 4, column A, excluding items a.(3), b.(3), and c(1b) and c(2b).
- MBS (available for sale, fair value)$_{b,t}$: The sum of all item in Schedule RC-B, item 4, column C, excluding items a.(3), b.(3), and c(1b) and c(2b).
- Bank size$_{b,t}$: the log of total assets: Log(RCFD2170)
- Return on Assets$_{b,t}$: Income (loss) before discontinued operations over assets: RIAD4300/RCFD2170
- Overhead ratio$_{b,t}$: The ratio of Noninterest expense (RIAD4093) divided by revenue. Revenue is the sum of net interest income (RIAD4074) and noninterest income (RIAD4079)).
- Net-interest margin$_{b,t}$: The ratio of Annualized net interest income (RIAD4074) divided by (30-day average) interest-earning assets (RCFD3381+ RCFDB558 + RCFDB559 + RCFDB560 + RCFD3365 + RCFD3360 + RCFD3484 + RCFD3401)
- (Delinquencies/Loan Loss Reserves)$_{b,t}$: The ratio of Delinquencies on all loans and leases (RC-N) divided by reserves for loan losses (RCFD3123)
- Ratio of non-performing loans$_{b,t}$: The sum of all loans that are past due 90 days or more and still accruing (Schedule RC-N, Items 1 – 9 Column B) divided by total loans (RCFD2112)
- Equity ratio$_{b,t}$: Total equity capital over assets: RCFDG105/RCFD2170
- Real estate loan ratio$_{b,t}$: Loans secured by real estate over total loans and leases held for investment and held for sale: RCFD1410/RCFD2122
- Deposit ratio$_{b,t}$: Deposits in foreign and domestic offices over assets:
(RCON2200 + RCFN2200)/RCFD2170

- Loans $\text{Loans}_{b,t}$: Total loans and leases held for investment and held for sale over assets: RCFD2122/RCFD2170
- C&I $\text{Lending}_{b,t}/\text{Loans}_{b,t}$: Commercial and industrial loans over total loans: (RCFD1763+RCFD1764)/RCFD2122
- Tier 1 capital $\text{ratio}_{b,t}$: RCFA7206