# The Capital Purchase Program's Effect on Firm Dynamics over the Business Cycle

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March 2020

*Please see* `http://kdduncan.github.io/papers.html` *for the most current draft.*

**Abstract**

Following the 2008 Financial Crisis the Capital Purchase Program dispersed over $200 billion dollars to banks hoping to prevent foreclosure and ease tightened lending conditions. Using census data on county level business dynamics this paper estimates the impacts of the Treasury Department's Capital Purchase Program on establishment entry, establishment exit, employment expansion, and employment contraction following the 2008 Financial Crisis. We estimate the direct effects of a county having a bank receive Capital Purchase Program funds on local business dynamics in the seven years following treatment, as well as spillover effects as entrepreneurs and business in neighboring regions travel to gain access to credit. Estimates show the CPP had no effect on establishment entry and exit, nor employment expansion and contraction. This paper establishes that the business-lending aims of the CPP were not realized in the communities and regions that received funds, and casts further doubt on meaningful pass through of CPP funds to desirable local economic activity.

[*]I would like to thank Otavio Bartalloti and Helle Bunzel for feedback on this paper.

*"The breakdown of key markets for new securities has constrained the ability of even credit worthy small businesses and families to get the loans they need.... It is essential that we get these markets working again so that families and businesses can have access to credit on reasonable terms."*
*- Tim Geithner, Treasury Secretary 4/21/09*

# 1 Introduction

This paper estimates the impact of the Treasury Department's Capital Purchase Program (CPP) on establishment entry, establishment exit, employment expansion, and employment contraction in the 7 years after the 2008 financial crisis. The CPP provided $205 billion dollars to more than 700 banks in over 400 US counties in order to prevent foreclosure and stimulate loan supply as part of the broader Troubled Asset Relief Program (TARP). The CPP was one of the largest fiscal responses of the US government to the great financial crisis. The Treasury Department explicitly stated that benefits of the CPP ideally would be passed along to individual households and non-financial firms to bolster beliefs about the government's willingness to loosen credit markets. The aim would be that this would lead to otherwise improved economic activity throughout the worst part of the crisis, where households could gain loans for mortgages, and entrepreneurs or existing businesses could keep loans to start or stay in business at existing levels of employment.

We answer the question on whether or not the Capital Purchase Program impacted local establishment dynamics. If the CPP cased eased lending standards and actual pass through to local households and businesses, prospective entrepreneurs and business owners could have either opened new stablishments or expanded employment more than otherwise in communities with consumer demand for new goods and services. Alternatively, entrepreneuers and business owners might have been granted bridge loans to avoid excess or layoffs if they expected consumer demand to return soon, helping mitigate establishment exit or employment contraction. Previous work by Sheng [2015] shows that large firms that borrowed from banks that received CPP funds did not increase investment or R&D spending, and instead altered firms liquidity and financial decisions. Improved local establishment dynamics in comparison provides clear measures of positive economic value in comparison.

Positive impacts of the CPP would have lead to (1) increased establishment entry, (2) decreased establishment exit, (3) increased the number of establishment expansions, and (4) restricted the number of establishment contractions. Positive firm dynamics are a main contributor to TFP growth (Lee and Mukoyama [2015], Clementi and Palazzo [2016]) and lead to lower unemployment and stronger economic growth out of economic depressions. In practice the CPP can be viewed as a loan guarantee scheme, programs where the government takes up a guarantor of loans that financial institutes pass along to enterprises, where now the government precommits to back loan creation. Previous work on loan guarantee schemes has found they can provide an efficient means of job creation, but guaranteed projects are marginally more likely to fail, that they do induce funds from banks that otherwise would not be lent, and widening to larger firms and loans may hurt program benefits [Parker, 2005, Riding and Haines, 2001].

This paper estimates the direct impacts of a county receiving CPP funds utilizing census data on aggregate county level establishment dynamics. I further estimate the spillover effect of the CPP on neighboring counties that did not receive funds directly but were within 50 miles of a county that received treatment. These results extend previous work by Berger and Roman [2014] showing commercial real estate lending and off-balance-sheet real estate guarantees increased net job creation and net hiring establishments while decreasing business and personal bankruptcies. This paper further provides evidence on how young firm activity is tied to location financial health and credit supply [Davis and Haltiwanger, 2019]. A major concern is that previous work estimating whether or not the CPP induced increased commercial and industrial lending from banks. Many studies have come to inconclusive and often contradictory results [Bassett et al., 2017, Berger et al., 2019a, Black and Hazelwood, 2013, Blau et al., 2013, Cole, 2012, Contessi and Francis, 2011, Li, 2013]. Importantly, Jang [2017] shows that TARP money provided to distressed areas had spillover effects into neighboring, better performing, counties.

Complementary research has further explored other bank level responses to the Capital Purchase Program. Carow and Salotti [2014] show the Treasury Department gave CPP funds to weaker banks only if they had better performing loan portfolios. Operating efficiency of TARP banks generally decreased relative to non-TARP banks (Harris et al. [2013]). TARP receiving banks gained a competitive advantage by increasing market shares and power due to perceived safety of consumers (Berger and Roman [2016]), and were able to buy up other failed banks for substantial positive abnormal stock returns (Cowan and Salotti [2015]). Banks that received TARP money contributed less to economy wide systemic risk (Berger et al. [2019b]). That CPP funds provided only short term relief to participating commercial banks (Calabrese et al. [2017]). Broad overviews of research in this area have also been generated in Calomiris and Khan [2015] and Berger [2018].

Analysis of the CPP benefits from several stylized facts; the CPP had statutory requirements where the Treasury could only purchase stock valued between 1%-3% of a banks troubled assets, up to $25 billion and that among counties that received money only a few banks received CPP funds. Combined, these facts allows me to view a county as treated as long as at least one bank received CPP funds. We provide estimates of models with just direct and indirect effects, and then differentiate by timing differences on when counties had banks receive CPP funds to define potential outcomes of both own-treatment in either 2008 or 2009, and whether or not a county was adjacent- defined as being within 50 miles of a neighbor counties center- to a treated counties. Treatment effects might be differentiated across time due to both differences in when banks where mandates to apply by, and the type of banks and communities that might have received treatment in each period.

Treatment effects are estimated using a panel data method similar to Hsiao et al. [2012]. Since the number of treated and untreated counties is much larger than the number of pre-treatment time periods, we augment the procedure with a LASSO penalty term such as in Doudchenko and Imbens [2016]. This is different than the synthetic-control style estimators as it removes the convex hull assumptions such as in Abadie and Gardeazabal [2003], Abadie et al. [2010, 2015], Ferman and Pinto [2016]. We show that sample splitting techniques across different treated groups allow for easy estimation of the Average Treatment on the Treated even with spillover effects, and many treated counties that neighbor each other. This relaxes the shared spillover effects as in Cao and Dowd [2018], and allowing for two treated units to be adjacent to each other as ommitted from Di Stefano and Mellace [2020] as comparisons within the synthetic control literature. The downside is for counties with both individual specific direct and indirect treatment effects our estimates can only recover the mean effect for the group instead of individual specific effects.

The results indicate that both direct and spillover effect of a county having a bank receive CPP funds on establishment entry, establishment exit, employment expansion, and employment contraction where non-existant. Establishment entry among treated counties decreased around 10 fewer entrants a year, exits increased 40 additional exits a year, but showed long run improvement. The number of establishments increasing employment decreased by 50 directly following receiving CPP funds, and about 45 additional esetablishments contracted employment. However, five to six years after receiving treatment, firm entry returned to its previous levels, about 40 fewer firms exited treated counties starting in 2011, and there were about 50 more employment expanding- and 50 fewer employment contracting- firms.

Even as average causal effects showed generally no to undesirable outcomes among treated counties, county level heterogeneity shows many counties saw marked improvement. All treatment effects are highly correlated with each other, with a major driver being the large number of firms that enter and exit in a single year. This is not surprisingly since trying to provide funds directly to banks is similar to the pass through of monetary policy changes to credit markets which have previously been shown to have considerable heterogeneity [Blau et al., 2013]. One of the most striking results is that immediately following treatment employment expansions (contractions) are almost strictly negative (positive), indicating that few small and medium firms got access to bridge loans to stop them from having to lay off works in the face of contracting consumer demand. The lack of pass through to small and medium establishments is important as most small businesses do not have access to equity markets, and rely on local or regional banks for credit. Relationship lending has been recently established as a major way in which banks recover underlying firm specific behavior [Berger and Udell, 2002].

Motivation for synthetic control methods are provided in robustness checks, where tests for pre-trends are rejected across a variety of multiple difference-in-differences estimators and instrumental difference-in-differences specifications. Instead direct estimations of interactive fixed effects difference-in-difference models are carried out using a number of specifications that confirm with earlier synthetic control methods. Overall, this paper provides clear evidence that the CPP did not generate pass through to improved local establishment dynamics. Banks might have preferred providing pass through to households seeking home mortgages, or alternatively might have parked the money as a risk free loan from the banks to pay off other existing balance sheet effects.

The paper proceeds as follows. Section 2 describes the Capital Purchase Program in greater detail. Section 3 describes the data, providing preliminary data analysis and provides summary statistics. Section 4 formalizes the empirical design and estimation processes. Section 5 provides our preferred LASSO-synthetic control estimation results. Section 6 provides robustness checks. Section 7 concludes.

# 2 The Capital Purchase Program

The Capital Purchase Program provided extra capital to banks by buying non-voting senior preferred shares on standardized terms to offset now-high risk assets remaining on bank's balance sheets. The CPP provided $205 billion to more than 700 banks. The first 10 banks received just over $125 billion. These banks include Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan Chase, Morgan Stanley, State Street Corporation, Wells Fargo, 1st Financial Services Corporation, and Bank of Commerce Holdings. The public perception was that these banks were almost forced to take CPP funds as part of the government's bailout of the financial sector.

Individual banks applied for CPP funds through their federal regulator- the Federal Reserve, FDIC, Office of the Comptroller of the Currency, or the Office of Thrift Supervision.[1] Banks indicated a preferred level of stock purchase between one and three percent of the total risk-weighted Assets of the applicant up to $25 billion.

Federal regulatory agencies chose which banks received money and sent preferred set of applicants to the Treasury Department for final clearance. Duchin and Sosyura [2014] show that of roughly 600 public firms, 416 firms (79.8%) applied, 329 (79.1%) were accepted, and that 278 (84.5%) accepted the funds but 51 (15.5%) declined. Among private banks that applied, applications that were rejected or withdrawn were not announced or publicly disclosed. All initial payments to participating banks were made before January 1st, 2010. However, there are clear spikes in lending. A large number of funds were dispersed in 2008, a slow down through the holidays, and another large group of funds were dispersed at the start of 2009 (Figure 2). Many counties had only a few banks receive funds, and even a smaller share of banks received multiple injections. Between 2008 and the end of 2010, the average county had 2.06 injections in total, often in separate banks (Figure 3).

The non-voting senior preferred shares required a 5% dividend for the first 5 years and 9% afterwards.[2] However previous research has indicated that these purchases were preferential for the banks. The Congressional Oversight Panel estimated that the Treasury gave out $254 billion in 2008 across all TARP programs, for which it received assets worth approximately $176 billion, a difference of $78 billion. Equi-

[1] The application period lasted between October 3rd, 2008 to November 14th, 2008 for publicly held companies, December 8th for Privately held companies, and February 13th, 2008 for S Corporations. On May 20th, 2009, Timothy Geithner announced that for banks with assets less than $500 million would have a second window to apply for CPP funds for the following 6 months. `https://www.treasury.gov/press-center/press-releases/Pages/tg139.aspx`

[2] Participating banks would also be able to receive future Treasury purchases of common stock up to 15% of the initial CPP investment for the following 10 years- allowing for additional buy in if the Treasury judged their initial purchase was not high enough.

valently, Veronesi and Zingales [2010] estimate during the first 10 transactions of the CPP, the Treasury overpaid between $6-13 billion for financial claims.

Overall the CPP provided standardized amounts of capital to participating banks in one of two main treatment branches, at the very end of 2008 or the very beginning of 2009. We see bunching in funds per worker in Figure 4. Many banks applied, and few turned down funds after being accepted.[3] Since most counties only had a small number of banks receive CPP funds, treatment can be viewed through the lens of did a specific county have at least one bank receive CPP funds in either 2008 or 2009. This allows reduction of an otherwise complex problem with both continuous treatment assigning and treatment intensity as a more tractible problem with discrete treatment assignment and singular treatment intensities.

# 3 Data and Summary Statistics

The primary dependent variables of interest are county level establishment entry, establishment exit, employment expansion, and employment contraction from 1999 to 2015 provided by the Census Statistics of US Businesses & Business Information Tracking Series (SUSB).[4] Establishments are classified a single physical location in which business is conducted, where individual companies or enterprises can be spread across multiple establishments. Most importantly, each establishment has non-zero levels of employment, ruling out non-employee firms from the sample. Estimation of the average treatment on the treated, the average change caused by the CPP on entry, exit, employment expansion, or employment, covers a wide span of pass through activities from increased rates of lending.

Entrants are establishments with zero employment in the first quarter of the initial year, and positive employment in the subsequent year. Exiters have positive employment in the initial year and zero employment in the subsequent year. Expansions are establishments with positive first quarter employment in both the initial and subsequent years and increased employment during the time period between the first quarter of the initial year and the first quarter of the subsequent year. Contractions are establishments that have positive first quarter employment in both the initial and subsequent years and decrease employment during the time period between the first quarter of the initial year and the first quarter of the subsequent year. We exclude any county that had zero firm entry or exit, removing 161 counties (see Appendix A.3).[5]

Figures 5-6 plot mean establishment entry and establishment exit by observed treated status in 2008 and 2009. When plotted at levels, there exist large differences in firm dynamics. Counties that received treatment in both 2008 and 2009 average more than 1500 new entrants/exits a year. Counties that received only one treatment tend to average around 500-700 new entrants and exists a year, and non-treated counties have barely any entry. However rescaling each time series subject to within-group means and standard

[3] Official documentation guaranteed banks that applied and got turned down for funds did not get publicly announced. This makes extrapolation from the Duchin and Sosyura [2014] results difficult.

[4] The underlying files can be downloaded as `https://www2.census.gov/programs-surveys/susb/`.

[5] Moreover, as discussed later, our estimation strategy never picks up these counties when looking to create synthetic counties using either the level or rates of firm dynamics.

deviations prior to 2007 show considerable similarities in each group, and show that appropriate Difference-in-Differences techniques might be useful in creating valid counterfactuals for each treated sub-group.

The majority of firms are small. From the Census' County Business Patterns data, which tracks the total number of establishments in a given county, roughly 55% of firms have between one and four employees, 20% have between five and nine employees, and 12% have between 10 and nineteen employees. These numbers are very stable across all years in the sample. The majority of firms are small mom-and-pop set ups. The SUSB data does not disentangle firm size, but using this sample I assume that the majority of new entry is small. This is further supported by other studies, for example Bartelsman et al. [2005], Kaniovski and Peneder [2008], Mata and Portugal [1994]. Most firms enter and fail within the first year or two.

There is strong evidence that in good times credit constraints do not impact the decision to enter into entrepreneurial activity given a lack of a relationship between wealth and entry into entrepreneurship Hurst and Lusardi [2004]. Data from the 2003 National Survey of Small Business Finances show that among firms that had only opened after 2002, 25% of firms had 0 outstanding loans, and 50% had less than \$7000 in loans. Among those firms that had taken out capital leases, 25% of them owed less than \$4000 in principal, and 75% owed less than \$45,000.[6] The Federal Reserve's Small Business Credit Survey, in 2018, across the life cycle of firms, 25% to 35% of firms with employers had no outstanding debt. For debt, 46% of new firms did use a loan or line of credit as a regular source of external financing, while only 9% of new firms had outside equity financing. Over the life cycle the share of firms taking equity fell, while the share taking on loan increased. Almost half of firms between 0 and 15 years in business applied for financing in the previous year, most seeking between \$25,000 and \$100,000. Shane [2010] points roughly 48.4% start in residence-such as home or garage, and an additional 40.64% in a rented or leased space. And that the typical median start-up in the US requires \$24,000-30,000 in start up capital.

Most importantly there is large stability in the change in the number of establishments at different firm sizes. Figure 1 graphs the change in the share of establishments with different sets of employees, 1-4, 5-9, 10-19, 20-49, 50-99, 100-249. Both the number of new firm entrants in each county and the share of firms at different levels of employment are very stable. Firms with 1-4 employees consistently make up almost 55% of the change in establishments, firms with 5-9 employees has fallen slightly from being 20% of the change in establishments to 17%, and firms with 20-49 employees have increased from 0.8% to 1% of new entrants. These shifts are small, but follow general concerns about firm concentration, and a need for perceived higher capital constraints relative to the late 1990's.

Treatment status is defined as a county receiving CPP funds in a particular year. The Treasury Department updates the TARP Transaction Report that includes bank name, state name, and city name data. I directly attach Federal Reserve Replication Server System Database ID's (RSSD ID) using the 2008

[6] Of new firms that do not take out loans, most are in categories highly likely to fill consulting jobs, special trade contractors, miscellaneous manufacturing industries, personal services, and engineering and management services. Comparably among new entrants that did take out loans, they were more concentrated in restaurants, retail, business services, trucking, or durable storage.

and 2009 FFIEC Call Reports and Summary of Deposits.[7] From this we are able to calculate both a head quarty specific county treatment effect, and a bank network county treatment effect.

For concreteness, let $(i, j) \in \{1, \ldots, N_c\}$ index the number of counties, and $k, l \in \{1, \ldots, N_b\}$ index bank headquarters, and for each headquaker $k$ we have $b_k \in \{1, \ldots, N_{b_k}\}$ as an index for the number of branch locations, and each bank $k$ exists in some county $i$. Now there are two treatments, HQ treatment location, and bank wide (BW) treatment. Own treatment is defined as an indicator value on whether or not a county received any CPP funds during a given time period. I separate own-treatment status into two groups, the first being receiving CPP funds in 2008, and the second being receiving CPP funds in 2009. The own-treatment variable takes the form,

$$Own_{i,t}^{HQ} = 1\{\exists k \in i \text{ s.t. } CPP_{k,t} > 0\}$$

$$Own_{i,t}^{BW} = 1\{\exists b_k \in i \text{ s.t. } CPP_{k,t} > 0\}$$

The term $CPP_{i,t}$ is the dollar amount of CPP funds given to banks in county $i$ in period $t$. Using this definition, 63 counties received CPP funds only in 2008, 243 received CPP funds only in 2009, and 81 counties received CPP funds in both periods.

Similarly, credit markets may extend beyond county borders, implying that treating a county $i$ may impact nearby counties. A neighbor is defined to be any county with centroid distance within 50 miles of a subject county $i$.[8] This metric is used as entrepreneurs have empirically traveled moderate distances trying to find beneficial loan deals, such that in Belgian banks the maximum loan distance is 50 miles [Degryse and Ongena, 2005], while in the US average bank applications come from 10 miles away [Agarwal and Hauswald, 2011], with a standard deviation of 21 miles, while accepted applications come from even closer to the bank (2.62 miles), with a smaller standard deviation (10.67 miles). Thus while most bank applications are local, applicants are willing to drive moderate distances in search of favorable loan contracts. Under this setting we define the neighbor treatment variable as

$$Neigh_{i,t}^{HQ} = 1\{\exists j \text{ adjacent to } i \; s.t. \; CPP_{j,t} > 0\}$$

$$Neigh_{i,t}^{BW} = 1\{\exists b_k \text{ adjacent to } i \text{ s.t. } CPP_{k,t} > 0\}$$

A major source of possible bias is that the largest banks in the US were perceived to be highly illiquid at the start of the Great Financial Crisis. These banks were effectively told to take CPP funds,

[7] The TARP Transaction Report can be found here: `https://www.treasury.gov/initiatives/financial-stability/reports/Pages/TARP-Investment-Program-Transaction-Reports.aspx`. Similarly, the 2008 and 2009 FFIEC Call Reports can be found here: `https://www.fdic.gov/regulations/resources/call/index.html`. To match banks in the TARP Transaction Report to RSSDID's we first pick a bank-state-city group from the TARP Transaction Report, then condition the Call Report data on city, state, and only banks that contain the entirety of the bank from the Transaction Report (after removing REGEX and making both names lower case). This matches on roughly 630 of the 707 banks. The remaining share are added directly.

[8] Based on NBER County Distance Database restricted to county centroids within 50 miles of each other. `http://www.nber.org/data/county-distance-database.html`

and thus did not opt in to the program. Moreover, most of these banks paid back CPP loans quickly in order to remove requirements on executive pay and other conditions for the funds. The concern is that these banks sat on the funds rather than using them as part of regular bank operations, and adding in their responses might spoil results (see for example Li [2013]). The counties with the top 20 largest banks, and the communities immediately adjacent to them are removed from our sample. Moreover, we treat locations with branch locations as non-treated by the status of the headquarters.[9] The major reason for this assumption is that most of these banks had been caught with high credit risk due to investment activities, and not underlying weakness in branch location financial conditions.

Mean bank characteristics at the county level are calculated from FDIC call sheet data, Following Li [2013] we calculate troubled assets ratio, annualized Return on Assets, and loan-to-deposits ratio.[10] These proxy for local community bank health that the Federal Regulators may have observed when deciding which banks to accept into the CPP program.

Local labor market characters are provided through the BLS's Local Area Unemployment statistics on county level unemployment rates.[11] Previous work into firm entry has found small effects of taxes, where even along borders firms place rarely show a preference for side based on relative tax rate [Duncan, 2015]. Instead a major driver of firm entry appear to be unobserved demand for products and agglomeration economies. Measures of upstream and downstream agglomeration economies are calculated from input-output tables. These take three forms, the first is industry cluster, measured as each industry's share of total employment in a county/year pair relative to the industry share in the nation as a whole. Upstream and downstream measures of connectedness are calculated from the Bureau of Economic Analysis' 1997 Standard Use Table. The share of workers providing inputs to each 2 digit NAICS code is calculated for in each county and year. Using this again the upstream and downstream measures is calculated by taking the share of workers providing inputs into each 2 digit NAICS code divided by total employment in each period. This is again normalized by the average across the United States. Measures of household financial health are provided by the FDIC experimental county level home price index, however The FDIC data exclude counties without enough mortgages to draw a consistent enough estimate of household financial wealth, this using only counties where the home price index exists excludes many rural counties.

Summary statistics for each of these variables is provided in Table 1. The first column, "PrGFC"

[9] Most of these counties are bank holding companies. The FDIC call sheet data lists all downstream assets held by branches at the bank holding company's headquarters. The list of banks include, Goldman Sachs, J.P.Morgan Chase Bank, Keybank (Keycorp), PNC Bank, Fifth Third Bank, Bank of America, BB&T Bank (BB&T Corp), State Street, U.S. Bank (U.S Bancorp), Wells Fargo Bank, Suntrust Bank, Citibank, Capital One, Regions Bank, Bank of New York Mellon, Northern Trust Company, Comerica Bank, M&T Bank, Marshall&Ilsley Bank, and Morgan Stanley. In practice this excludes New York, NY; Charlotte, NC; Boston, MA; Minneapolis, MN; Cleveland, OH; Pittsburgh, PA; Cincinnati, OH; Atlanta, GA; McLean, VA; Birminham, AL; Chicago, IL; Dallas, TX; Buffalo, NY; and Milwaukee, WI.

[10] Values are calculated from call sheet data from 2008Q3. Tier 1 Ratio is calculated directly in the Call Sheets as RCON7206. Troubled Asset Ratio is loans 90 days past due/total capital. Troubled Assets are calculated as 90 Days Past Due C&I Loans (RCON5460) and All Other Loans Past Due 90 Days or More (RCON5460). For Total Capital are calculated as Total Assets (RCON2170) and subtracted Total Liabilities (RCON2948). Return on Assets was Net Income (RIAD4340) divided by Total Assets. Cash to Assets was Cash and Due From Depositors (RCON0010) divided by Total Assets. Loan to Deposits Ratio was Loans, Leases, Net Unearned Income (RCONB528) divided by Total Deposits (RCON2200).

[11] https://www.bls.gov/lau/

is Pre-Great Financial Crisis, provides the mean across all counties and year from 1999 to 2007. The second column, "PoGFC" is Post-Great Financial Crisis, and reports the mean across all counties and years from 2008 to 2015. The third column, "Diff" reports the difference-in-means between the first and second column. As expected, firm entry and employment expansion went down, first exit and employment contractions went up. Unemployment rates went up, banks deleveraged and Troubled Asset Ratio's decreased, and return on assets increased. The average change in the Home Price Index (HPI) was negative over the Post-GFC time period. Columns four and five report the standard deviation of the pre and post financial crisis period, and the sixth reports the difference. Entry, exit, and employment expansion all feature less variation in the post-financial crisis era, while contractions variation increased.

Finally, a number of other policy drivers have studied determinants of firm entry, such as right to work laws [Holmes, 1998] or lower taxes [Duncan, 2015, Rohlin et al., 2014]. Often specific research designs are used to estimate these effects and remove endogeneity of pro-business practices such as I exclude these variables due to fear of inducing larger biases in my estimates, especially given that they do not explain a large share of the overall variation in firm entry dynamics. In many of these cases the proposed models both explain a small share of the overall variation in firm entry, or show that the treatment effects have economically small coefficients.

## 4 Empirical Design

We are interested in recovering the direct and indirect Average Treatment on the Treated for a county having a bank receive CPP funds on future business dynamics. A major concern is that there is large heterogeneity in how communities were impacted by the 2008 financial crisis, and how local bank financial characteristics created pass through to local businesses and entrepreneurs. This creates ambiguity in what the appropriate counterfactual is to non-treated counties, and motivates the use of synthetic control methods.

A major source of confounding in my research design exists in credit market spillovers. Entrepreneurs are likely to travel moderate distances in order to acquire credit to start, expand, or stop foreclosure on a business. As a result counties are not independent of each other, and instead rely on a both their own sources of productivity and access to credit, as well as those around them. We follow Huber and Steinmayr [2019] to utilize a potential outcome framework with own and neighbor treated status. The core assumptions being that changes in which neighbor is treated does not impact your potential outcome outside of either a neighbor being treated or the number of neighbors being treated, and no complementarities between own treatment and neighbor treatment status.

More formally, there are $T$ time periods. From periods $0, \dots, T_0 < T - 2$ all counties are untreated. In periods $T_1 = T_0 + 1$ each county can receive CPP treatment. In periods $T_2, \dots, T$ no more treatment is assigned. Under this framework we have two treatments, own treatment $Own_{i,t} \in \{0, 1\}$ or neighbor treatment $Neigh_{it} \in \{0, 1\}$. Therefore individuals treatment status can be characterized in the set $S_{iT_1} =$

$(Own_{iT_1}, Neigh_{iT_1}) \in \{(0,0),(1,0),(0,1),(1,1)\}$. Assume the simple structural model for untreated counties as,

$$y_{it}(0,0) = x_{it}\beta + \lambda_t'\mu_i + \epsilon_{it} \tag{1}$$

Then for treated counties we get the following series of equations,

$$y_{it}(1,0) = y_{it}(0,0) + \alpha_{it} \tag{2}$$
$$y_{it}(0,1) = y_{it}(0,0) + \gamma it \tag{3}$$
$$y_{it}(1,1) = y_{it}(0,0) + \alpha_{it} + \gamma_{it} \tag{4}$$

Under this factor structure $\lambda_t$ is a $(1 \times F)$ vector of unobserved common factors, $\mu_i$ is an $(F \times 1)$ vector of unknown factor loadings, and the error terms $\epsilon_{it}$ are unobserved transitory shocks at the region level with zero mean. This structure is general and nests a number of common data generating processes.[12] Implicitly in this structure we assume no complementarities or substitution effects between treatment and neighbor treatment status. This allows estimation of average treatment effects through estimation of synthetic control on sample splitting. That is,

$$\alpha_{it}^{(1,0),(0,0)} = y_{it}(1,0) - E(y_{it}(0,0) \mid (1,0)) \tag{5}$$
$$\alpha_{it}^{(1,1),(0,1)} = y_{it}(1,1) - E(y_{it}(0,1) \mid (1,1)) \tag{6}$$
$$\gamma it^{(0,1),(0,0)} = y_{it}(0,1) - E(y_{it}(0,0) \mid (1,0)) \tag{7}$$
$$\gamma it^{(1,1),(1,0)} = y_{it}(1,1) - E(y_{it}(1,0) \mid (1,1)) \tag{8}$$

The aim is to construct a synthetic county out of linear combinations of counties with a different treatment status. Traditionally this was done through a convex hull assumption such as in Abadie and Gardeazabal [2003], Abadie et al. [2010, 2015], Ferman and Pinto [2016], where all weights are strictly positive and sum to one. This assumption was removed in Hsiao et al. [2012], Li and Bell [2017]. The main difference between the two is that the "panel data approach" is an unconstrained regression, and the synthetic control method is a constrained regression. Similar approaches without constraints have started to implement LASSO and other regularization methods [Amjad et al., 2018, Carvalho et al., 2018, Chernozhukov et al., 2017, Doudchenko and Imbens, 2016]. A comparison of these methods was conducted by Gardeazabal and Vega-Bayo [2017], Wan et al. [2018]. With only a single treatment, synthetic control estimates county specific

[12] It is common in the "synthetic control" literature to assume a shared time varying intercept for all counties in the sample, equivalently, the "panel data approach" assumes an county specific intercept. Both are special cases of the unconstrained fixed effects model. For example, while the model with the shared time varying intercept nests the differences-in-differences model when $\lambda_t = 1$, both models are nested when $\lambda_t' = [1\ \eta_t]',\ \mu_i = [\theta_i\ 1]$.

ATT's, but with two different treatment effects these estimates become a county specific total treatment effects, and parsing out average direct and spillover effects requires modifications.

Estimation of the LASSO-synthetic control estimator is carried out through minimizing the follow penalized regression.

$$\begin{bmatrix} \hat{w}_i \\ \hat{\beta}_{i,0} \end{bmatrix} = \underset{B_{i,0}, w_i}{\arg\min} \frac{1}{T_0} \sum_{t=1}^{T_0} \left( y_{it} - \beta_{i,0} - \sum_{j=1}^{N_0} w_{ij} y_{jt} \right)^2 + \phi \mid\mid w_i \mid\mid_2 \tag{9}$$

The first part of this equation is regular OLS as carried out in Hsiao et al. [2012], where we match a set of donor counties to a specific treated counties for all the pre-treatment time periods. However, since $N_B >> T_0$, we force the procedure to select only a subset of counties. Therefore the second term, $\phi \mid\mid w_i \mid\mid_2$ is LASSO a penalty term, where $\phi > 0$ determines the severity of the penalty for picking an additional county and is determined by cross validation, and $\mid\mid w_i \mid\mid_2 = \sum_j w_{ij}^2$. This structure is close to [Doudchenko and Imbens, 2016, Li and Bell, 2017, Wan et al., 2018]. Without loss of generality, assume we are estimating $\alpha_{it}^{A,B}$, where $A$ is a treated set, and $B$ is a donor set. Then Equation 5 can be reformulated

$$\begin{aligned} \alpha_{it}^{A,B} &= (y_{it} - w_i Y_{jt}) \\ &= \left( \alpha_{it} + (\gamma_{it} - \sum_{j \in B} w_{ij} \gamma_{jt}) + \lambda_t (\mu_i - \sum_{j \in B} w_{ij} \mu_j) + (\epsilon_{it} - \sum_{j \in B} \epsilon_{jt}) \right) \end{aligned}$$

This estimator becomes unbiased under the following assumption

**Assumption 4.1.**

$$E[\epsilon_{it} \mid Own_i \ Neigh_i] = E[\epsilon_{it}] = 0$$

$$\exists w^* \in \mathbb{R}^{N_B} \mid (\mu_i - \sum_{j \in B} w_{ij} \mu_j)) = 0, E[\gamma_{it} - \sum_{j \in B} w_{ij} \gamma_{jt}] = 0$$

The first part of this assumption states that treatment can be correlated with the factor loading term, $\lambda_t' \mu_i$, but are uncorrelated with idiosyncratic shocks to a given county. The second requires that our pre-treatment fit provides a close approximation for the unobserved time-invariant county specific factor loadings, and that in the post treatment time period provide a mean zero approximation for the second treatment effect. This implies that the shared treatment effects $\gamma_{it}$ all share common support across the target and donor pools.

A major concern is that the term $(\gamma_{it} - \sum_{j \in B} w_{ij} \gamma_{jt})$ varies meaningfully. Assumption 4.1 claims in each period the treatments are random effects, such that $y_{it} = y_t + \upsilon_{it}$ and $upsilon_{it}$ is white noise.[13] In turn we primarily focus on estimates of the effect mean,

[13] An implicit implication of this is that individual counties should have no meaningful heterogeneity, and instead should be jumping around the mean. But this is often violated in practice.

$$\alpha_t^{A,B} = \frac{1}{N_A}\sum_{i\in A}(y_{it} - \hat{y}_{it}) \tag{10}$$

However, variation in treatment assignment can further be leveraged in estimation of effects. As discussed previously, there was an initial wave of payouts at the end of 2008, a slow down, followed by a second wave of dispersed funds at the start of 2009. Under this setup there are now more effects, and estimation assuming single effects leads to plausibly biased samples. Extending the previous treatment assignment description to include two periods of own treatment and neighbor treatment is fairly routine. As above, in periods $0, \ldots, T_0 < T - 3$ all counties are untreated. In periods $T_1 = T_0 + 1$ and $T_2 = T_0 + 2$ each county can receive CPP treatment. In periods $T_3, \ldots, T$ no more treatment is assigned. Under this framework we now have two possible time periods where in each period one of two possible treatments can be received. In period $T_1$ individuals treatment status can be characterized as above. In period $T_2$ the nested outcomes generate sixteen potential outcomes. We index counties by their second period potential outcomes

$$(Own_{i,T_1}, Neigh_{i,T_1}, Own_{i,T_2}, Neigh_{i,T_2})$$

As above, assume the simple structural model for untreated counties,

$$y_{it}(0,0,0,0) = x_{it}\beta + \lambda_t'\mu_i + \epsilon_{it}$$

In period $T_1$ this generates the four possible outcomes in Equations 2. In period $T_2$ the potential framework becomes nested, where the four potential outcomes are repeated, conditional on treatment status from $T_1$. This leads to many cases similar to treated and neighbor treated in $T_1$, where there are many plausible individual specific parameters, but estimation of a single marginal effect (for example impact of first period treatment), now generates a large vector of nuissance parameters. First, under this framework, we can recharacterize the estimated treatment effect without loss of generality as,

$$\begin{aligned}
(\alpha_{it}^{T_1} &+ (\gamma_{it}^{T_1} - \sum_{j\in B} w_{ij}\gamma_{jt}^{T_1}) + (\alpha_{it}^{T_2} I\{Own_{jT_2} = 1\} + \gamma_{it}^{T_2} I\{Neigh_{jT_2} = 1\}) \\
&- \sum_{j\in B} w_{ij}(\alpha_{jt}^{T_2} I\{Own_{jT_2} = 1\} + \gamma_{jt}^{T_2} I\{Neigh_{jT_2} = 1\}) \\
&+ \lambda_t(\mu_i - \sum_{j\in B} w_{ij}\mu_j) + (\epsilon_{it} - \sum_{j\in B} \epsilon_{jt}))
\end{aligned}$$

The leading term $\alpha_{it}^{T_1}$ is assumed to be the estimated effect of interest. The second term is the difference between the spillover effect in the first time period. The third term represents omitted second treatment effects on the first period treated unit of interest. The fourth term reflects unaccounted for second period treatment effects within the donor pool. Without additional assumptions it is not possible to sign the difference between the third and fourth terms.

As above, the case of $Own_t = 1, Neigh_t = 1$ is hard to identify with the synthetic control method as the difference in secondary treatment effects not of interest creates a moving nuissance parameter. For example, consider the following set of possible potential outcomes in the two period, two treatments, framework.

$$y_{it} = \begin{cases} y_{iT_2}(0,0,0,0) + \alpha_{it}^{T_1} + \alpha_{it}^{T_2} & \text{if } Own_{T_1} = 1,\ Neigh_{T_1} = 0,\ Own_{T_2} = 1,\ Neigh_{T_2} = 0 \\ y_{iT_2}(0,0,0,0) + \gamma_{it}^{T_1} + \gamma_{it}^{T_2} & \text{if } Own_{T_1} = 0,\ Neigh_{T_1} = 1,\ Own_{T_2} = 0,\ Neigh_{T_2} = 1 \\ y_{iT_2}(0,0,0,0) + \alpha_{it} + \gamma_{it} & \text{if } Own_{T_1} = 1,\ Neigh_{T_1} = 0,\ Own_{T_2} = 0,\ Neigh_{T_2} = 0 \end{cases}$$

Without additional assumptions it is impossible to jointly identify $(\alpha_{iT_2}^{T_1}, \alpha_{iT_2}^{T_2})$, $(\gamma_{iT_2}^{T_1}, \gamma_{iT_2}^{T_2})$, nor $\{(\alpha_{iT_2}^{j}, \gamma_{iT_2}^{j})\}_{j \in \{T_1, T_2\}}$. As above I remedy this issue by conditioning on a given positive treatment regime, and targeting the specific average effect of interest. For example, if I am interested in $\alpha_t^{T_0}$, the donor pool becomes $A = (1,0,0,0)$, and the donor pool is $B = (0,0,0,0)$. Similarly, the target pool $A = (1,1,0,0)$ is paired with the donor pool $B = (0,1,0,0)$. The estimator is still unbiased under Assumption 4.1. This means all treatment effects- own treatment 2008, neighbor treatment 2008, own treatment 2009, neighbor treatment 2009, share common supports across all treated counties.

The advantages of this approach is reducing each equation down to the canon causal effects structure, with downside being the loss of data within each equation. In each case we construct an new synthetic control based around the donor pool, and the fit across the donor pools differs greatly. Counties that would be picked by selecting weights across the entire sample are often excluded due to treatment statuses outside of the comparison at hand. Cao and Dowd [2018] offer an alternative way to estimate this equation under an imposed symmetry for indirect effects of receiving treatment. Their method allows for using the full sample to estimate the set of weights for every county in the sample, but imposes a stronger structural assumption on the underlying causal framework.

Inference for synthetic control methods is carried out using a permutation test (Abadie et al. [2015]). For each group assume the null hypothesis of no-treatment effect. Then re-sample without replacement a new treated group of size $N_A$ and estimate the mean LASSO-synthetic control estimator. This procedure is repeated 1000 times. This approximates the exact null distribution under the sharp null of no-treatment effect. Therefore, there exists a treatment effect when the point estimates for the observed treated group lies outside the 95% permutation test confidence interval.

# 5 Results

Results for levels and rates are plotted in Figures 7-14. The results are similar both for levels and rates. In both cases the synthetic control estimator described in Section 4 fail to reject the null hypothesis of no treatment effect. The pre-treatment fit is well within the 95% permutation confidence interval, and never crosses the permutation confidence interval in the post-treatment time period.

Point estimates are notable, in levels the direct impact on establishment entry is almost 50 fewer firms a year in the period immediately after treatment, or about a 1% lower entry rate, with both returning towards zero in the long run. The major difference between these two terms is that the average treated county has moderately higher average firm entry rates as discussed in Section 3. Indirect effects are much smaller on entry, with about 20 fewer firms, or about 0.1% lower firm entry rate. Establishment Exit shows a long run decline in both levels and longs, with 50 fewer exits a year, or about -.1% lower exit rate. The spillover here is of the same magnitude as the direct effect. A curious part here is that the entire 95% permutation confidence interval is declining over the post-treatment time period even among the untreated pool.

Establishment employment expansion shows almost zero mass in the 95% permutation interval above zero in the years immediately following treatment. In the long run this rises to about 100 more establishment expansions per year for both the direct and indirect effects, however in rates this the point estimates are approximinately close to zero. Equivalently, there is almost zero permutation distribution below zero immediately following the 2008 financial crisis. In the long run levels return to zero, while contraction rates show moderate decreases in the rate of firm contractions in the entire sample.

These results indicate no-effect from counties receiving CPP both directly and indirectly on the counties around them. However, these graphs are misleading in two ways. Each mean effect pools the average of 12 different estimates. This generates variation in treatment timing, particularly as the majority of counties did not receive CPP funds until 2009. As a result, I plot both the mean response along with 90% quantiles for each effect in Figures 15 and 16.[14] However, each of these are subject to possible estimation error from residual components of their potential outcomes. As before, both direct and indirect effects of entry are centered around zero.

The most useful conclusion from these graphs is a sign of clear bridge loan pass through in establishment employment expansion and contraction. Estimated effects are almost uniformly negative (positive) in the case of employment expansion (contraction), implying that few firms were able to forgo impacts of cratering consumer demand on their own employment status.

Overall, results indicate no effect on local establishment dynamics after a counties bank received CPP funds. For both direct and indirect effects entry and expansions decreased, exit and contractions increased, however considerable heterogeneity exists in these effects on individual counties. Many counties

[14]Some of the models fit particularly poorly, and given the relatively few treated individuals in some groups, the 90% quantile is a good first-pass approximation to the distribution among treated units. Later I carry out permutation tests under the null of no effect, and can compare these quantiles.

saw considerable positive gains to firm entry for both direct and indirect effects. Direct effects on entry saw some counties saw large falls in firm exit, while spillover effects saw a large tail of counties that saw excess exit for years following treatment. Both direct and spillover effects on expansion were generally negative immediately following treatment, followed by either strong positive expansion starting in 2011 for the directly treated counties, and generally no effect for spillover counties. The direct and spillover effects of contractions saw a strong center directly on zero, with a large upper tail of excess contractions.

# 6 Robustness Checks

This section provides a variety of robustness checks on our primary results. We provide Difference-in-Differences and Instrumental Variables Difference-in-Differences results, specifically talking about how pre-trend violations occur and providing further evidence of the need of data driven methods of constructing counterfactuals. Generally we find violations of the pre-trend for Own treatment status across both model specifications, but Neighbor pretrends generally hold. However as above, there is still no discernable spillover effect.

Continued concerns about differing pretrends among treated and untreated individuals leads to estimation of interactive fixed effects difference-in-differences models Gobillon and Magnac [2016], Xu [2017]. These explicitly estimate a factor loading model such as Equation 10 to constructing counterfactuals, implying a stronger structure than mainline synthetic control estimates require.

Finally, as noted in Section 3 the Tarp Transaction Report is tied to branch location that received funds. We construct a full network of counties with a treated bank's branch locations and reestimate interactive fixed effects difference-in-differences models.

## 6.1 Difference-in-Differences

The four and sixteen potential outcome framework discussed in Section 4 enables canonical difference-in-differences estimation now including a combination of own and neighbor treatment statuses. Recent work have helped decompose multiple time period or spillover effect difference-in-differences, most notably Imai and Kim [2014], and dealing with treatments in multiple time periods [Callaway and Sant'Anna, 2018, Goodman-Bacon, 2018].

We proceed by first estimating models with a single treatment period, and then two treatment periods with a full set of heterogeneous treatment effects.[15] For each model joint significance tests over the pre-trend are conducted using clustered standard errors are the state level. Finally, a stepdown method is utilized to test whether or not there was an active policy duration. This provides a conservative test for exactly how long there was a policy effect from the CPP on local establishment dyanmics.

[15] Models are estimated using the plm package in R carrying out a within (individual FE) transformation with two way fixed effects. Heteroskedastic robust variance-covariance matrices are calculated with Arellano [1987] style standard errors with with county level clusters.

Recent work in difference-in-differences and event study methods have increasingly utilized policies that exhibit variation in treatment timing. Under these conditions it is common to generate pre and post treatment effects from time of initial treatment. Two concerns arise out of this. For counties treated in the second period treatment, the period prior to treatment is now subject to the Great Financial Crisis, something the treated in the first period group is not, therefore tests for differing pre-trend are carried out just on pre-Financial Crisis periods. The two-way fixed effects model, a saturated model with own and neighbor treatment effects, own and neighbor events for all years outside of $t = 2006$ to exclude the start of the financial crisis, county specific fixed effects, and time fixed effects, generates the estimated equation,

$$\begin{aligned} y_{it} = \beta_1 Own_i + \beta_2 Neigh_i + \beta_3 I\{t > T_0\} + \sum_{s=-9}^{7} \gamma_s Own_i I\{s = t - \min_k \{Own_{i,k+1} - Own_{i,k} = 1\}\} \\ + \sum_{s=-9}^{7} \alpha_s Neigh_i \{s = t - \min_k \{Neigh_{i,k+1} - Neigh_{i,k} = 1\}\} + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it} \end{aligned} \tag{11}$$

The term $I\{s = t - \min_k \{Own_{i,k+1} - Own_{i,k} = 1\}\}$ denotes the difference between the current time period and the first year a given county received treatment. This specification generates three different tests for pretrends of interest. The first is that all pretrends differ from zero, the second that only own pretrends differ from zero, and the third that only neighbor pretrends differ from zero.[16] Recent research has pointed out that by doing this, standard errors of post-treatment coefficients are often conservative [Kahn-Lang and Lang, 2019, Roth, 2018]- but generally this paper prefers a more conservative approach to estimating effects and does not carry out further corrections.

$$\begin{aligned} H_0^{ALL} &= \gamma_{-9} \ \dots \ \gamma_{-1} \ \alpha_{-9} \ \dots \alpha_{-1} = 0 \\ H_0^{OWN} &= \alpha_{-9} \ \dots \alpha_{-1} = 0 \\ H_0^{NEIGH} &= \gamma_{-9} \ \dots \ \gamma_{-1} = 0 \end{aligned}$$

Even study style graphs of results are presented in Figure reffig:DIDPooled, and the resulting joint hypothesis tests on pretrend are presented in Table 2. Among own treatment effect, firm entry, firm exit, and employment contractions all grow leading up to the initial period of treatment. This visible difference in pre-trends (and levels) between treated and untreated counties in different treatment groups invalidates the use of the (mean) non-treated counties as a viable counter factual. Explicit discussion of the resulting effects generated by this estimation procedure might create poor policy conclusions. Comparably, the neighbor treated effect seems to more likely to follow a shared pretrend, even though it is still rejected in the joint

[16] One concern is that since there is time-variation in treatment that occurs after the onset of the Great Financial Crisis that pre-trend tests might fail due to the large number of firms treated in period two and the pre-trend coefficient for $\gamma_{-1}$ being strongly negative due to the GFC. To remedy this I actually report F-tests for a model which estimates $OwnTreated \times Year$ factors, and then imposes that there is no differing pre-trend from 2000 to 2007.

test, but the resulting coefficients are close to zero.

As discussed in Section 4, there might have been meaningful choices in when Federal regulators and the Treasury decided to disperse funds to different banks or regions. As a result, the pooled estimator presented in Equation 11 does not capture the full heterogeneity in responses. Thus we estimated a fully differentiated model with differing pretrends and post treatment effects by each treatment group. This allows for heterogeneous responses within each own-treatment and neighbor-treatment couplet, and the resulting estimated equation then becomes.

$$
\begin{aligned}
y_{it} = {} & \beta_1 Own_i + \beta_2 Neigh_i + \beta_3 I\{t > T_0\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{10} I\{s = t - \min_k \{W_{k+1}^{10} - W_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{01} I\{s = t - \min_k \{W_{k+1}^{01} - W_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{11} I\{s = t - \min_k \{W_{k+1}^{11} - W_k^{11} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{10} I\{s = t - \min_k \{G_{k+1}^{10} - G_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{01} I\{s = t - \min_k \{G_{k+1}^{01} - G_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{11} I\{s = t - \min_k \{G_{k+1}^{11} - G_k^{11} = 1\}\} \\
& + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it}
\end{aligned}
\tag{12}
$$

Event study figures for results from Equation 12 are presented in Figures 19-20 in the Appendix A.1. As above, joint tests on pretrends are carred out, where now this extends to all pretrends for each treatment subgroup. While visually the estimates appear to be much more centered around 0, most models still reject the hypothesis that there are no differing pre-trends among the different treatment groups. Allowing for additional heterogeneity shows that estimates for neighbor spillover effects tend to satisfy the shared pre-trend assumption. This implies that DID estimates for spillover effects are not invalidated, and that post-treatment estimates are supported by a valid counter factual.

As the financial crisis becomes less severe, capital is likely to ease nationally, and renormalization between treated and untreated counties may occur. Thus we develop explicit tests for policy effectiveness duration by conducting a multiple hypothesis tests using a step-down multiple hypothesis test outlined in section 6.1, based on a test for nested hypotheses proposed by Bauer and Hackl [1987]. This test controls for family-wise error in trying to evaluate multiple p-values simultaneously. To motivate this problem imagine the set of hypotheses,

$$H_0^k: \ \gamma_s = 0 \quad \forall s \in [1, \ldots, k] \tag{13}$$

Then a level $\alpha$-test for any null hypothesis $H_0^k$ is given by the critical region $\min_{i \leq j \leq k} p_j \leq \alpha/(2(k - i + 1))$, as under the null,

$$P(rejectH_0^k) \leq \sum_{i=1}^{k} P(p_j \leq \alpha/(2(k-i+1))) \leq \alpha$$

By use of Bonferroni's inequality. This test then jointly controls for family wise error of multiple tests being conducted for the no treatment effect. This test is a worst-case bound for the existence of positive policy effective duration, and basically selects and carries out the appropriate joint hypothesis in an iterative fashion.[17] Tables for Stepdown tests of Equation 12 are presented in Tables 4-5 in Section A.2.

Consistent with the results from the synthetic control methods there is no policy duration effect for spillover effects. There exist moderate effect durations for Own Treatment, but without accepting the shared pre-trend it is hard to argue what exactly the Difference-in-Differences estimator recovers.

## 6.2 Instrumental Variables Estimation

A concern about identification is that treatment is correlated with still unobserved shocks, even after conditioning on the interactive fixed effects. As noted in Li and Bell [2017], if Federal regulators and the Treasury picked areas for CPP funds with high latent demand for loans, then these estimates would overstate the CPP effectiveness, while comparably if they picked areas with low latent demand for loans, this might understate CPP effects. In turn we instrument own and neighbor treatment using counties own or neighbors political ties, whether or not any bank in a given county had a board member serving as a branch Federal Reserve chair, whether or not the counties local House representative was serving on the banking and finance committee, the share of donations to the local representative coming from Financial, Investment, and Real Estate groups, and whether or not the local House representative was a democrat.

Following Ruonan Xu [2019] we estimate a bivariate Probit for for each year instrumenting using political connections of counties, where the outcomes are own and neighbor treated status. For treatment status in 2009 we further condition on whether or not a county or a neighbor received treatment in the previous time period. This generates six instruments, being the relative probabilities of own, neighbor, and both treatment status in both 2008 and 2009 from the two Probit models.

Sanderson and Windmeijer [2016]'s augmented F-test for multiple endogenous variables is carried out, where our instruments are strong using the Stock and Yogo [2005] tables. The generated conditional F-values are 27.9, 78.13, 13.6, and 28.6 for own treatment in 2008, neighbor treatment in 2008, own treatment in 2009, and neighbor treatment in 2009- respectively. Taking the norm bias of 10%, the relevant comparative

[17] There exist step down methods that use bootstrap methods to estimate dependence in the underlying tests to generate a less conservative tests. This test can also be augmented to explicitly test one sides hypothesis by using the appropriate t-values.

critical value is 11.12. We then instrument each of our treatment statuses as a function of each of our instruments

$$Treat_{i,t} = \beta_0 + \beta_1\hat{p}_{10}^{2008} + \beta_2\hat{p}_{01}^{2008} + \beta_3\hat{p}_{11}^{2008} + \beta_4\hat{p}_{10}^{2009} + \beta_5\hat{p}_{01}^{2009} + \beta_6\hat{p}_{11}^{2009} + X_{it}'\Gamma + \epsilon_{it}$$

$Treat_{i,t}$ here includes $Own_{i,t}, Neigh_{i,t}$, and the timing-variants discussed for estimating Equation 12. Moreover, $X_{it}$ includes county mean bank financial health, and local unemployment characteristics. Using these instrumented measured, we re-estimate Equations 11 and 12. As above, these models continue to reject the assumption of shared pre-trends presented in Table 6.

This is likely due to selection by central banks into which banks have members serve on the board, such that banks situated in larger areas where likely to be serving at the local Fed chair, and these regions were more likely to feature different trends in the build up to the Great Financial Crisis. Thus, even if the IV solves the issue of possible poaching by the Treasury into providing CPP funds to areas with disproportionately high or low latent credit demand, the IV might exacerbate issues underlying differing pre-trends among different counties in the US. As a result, I omit point estimates for treatment effects derived from the IV model.

## 6.3 Interactive Fixed Effects Differences in Differences

Instead of relying on a specific form of additively seperable individual and time specific fixed effects, the simple structural model presented in Equation 10 is built on interactive fixed effects, where $r$ unknown time loading factors $\lambda_t$ are interacted by county specific effects $\mu_i$ that determine how impactful certain shared shocks are on a given county.

This forces a more explicit structural model to be estimated than presented for the synthetic control model in Section 4, but enables a broader set of time-varying covariates. We follow the estimation processes outlined in Gobillon and Magnac [2016], Xu [2017], and results are presented in Figures 22-25. As before, results are often indistinguishable from zero for establishment entry and establishment exit. Comparably the long term the Interactive Fixed Effects DID models show long run improvements in employment expansion, and fewer employment contractions. However, these improvements become distinct from zero well after the counties received CPP funds. Therefore it is hard to know whether or not these results are coming from CPP treatment, or supplemental responses or policy changes happening in the long run.

### 6.3.1 Accounting for Downstream Counties

Results presented so far have relied on where the TARP Transaction Report said the receiving bank was located. Often this is tied to bank headquarters, where many of the banks that received TARP funds were publicly traded bank holding companies, or small regional branches. A concern about our earlier identification strategy is that banks might have passed CPP funds from the receiving headquarters location down to branches.

Identification of treatment effects here is difficult. By including all branch locations of the 10 largest banks, there is no identification to be had, and all remaining counties on our sample- almost 2500- become treated. However most of these banks were all but forced to take the money, and paid it back quickly to get out of requirements the CPP imposed on banks normal operation. As a result, we assume that these banks did not pass funds to downstream banks in their network. Instead this leaves about 1500 counties that received treatment as presented in Figures 26-28.

Previous robustness checks have cast consistent doubt on the presence of spillover effects, so rather than splitting the sample, we estimate only direct pooled treatment effects using the Interactive Fixed Effects Difference-in-Differences method. These results are presented in Figures 29-32, and generally confirm with out prior results, that is, an absence of treatment effects across the board. Results differ slightly though, there is a noticable drop off in the number of establishments expanding employment that is statistically different from zero. In the long run, this returns to zero. Secondly, long run trends in exits and employment contracting firms decrease in the long run- similar as they did for our non-downstream accounting for estimators earlier.

Since these trends are long after counties received CPP funds, it is hard to tie these improvements to the CPP. In all cases estimated effects are close to zero around the treatment window, or reflect negative policy outcomes.

# 7 Conclusion

The Capital Purchase Program provided over $200 billion to banks to shore up bank finances and ease credit constraints faced by credit worthy households and small businesses. In this paper we estimated how possible pass through of the CPP might have impacted county level establishment dynamics, including entry, exit, employment expansion, and employment contraction. This paper builds on the back of a borader corporate finance literature that found mixed evidence of whether or not banks generated more commercial or industrial loans, and that companies that borrowed from banks that received CPP funds generally did not put it towards R& D, employment, or new capital expenditures and instead changed around their underlying balance sheets.

Examining firm entry has several benefits over direct bank level responses. Relationship lending is a major driver of extending loans to new or existing entrepreneurs, and formally modeling the method by which banks extend these loans is difficult, leading to biased estimation by improper understanding of this mechanism. However, higher firm entry was still a preferred outcome of policy makers at this time as a way of encouraging new job growth and aiding recovery efforts.

The main estimation technique uses augments Hsiao et al. [2012] to include a LASSO penalty term, and leverages identification of marginal direct and spillover treatment effects for the treated using sample splitting techniques. This specification shows no evidence that counties receiving TPP funds having higher establishment entry, exit, employment expansion, or employment contraction both as a direct or indirect effect of the CPP. Most notably here is that bunching of both mean and county specific effects show the mass of treatment is above (below) zero for employment contractions (expansions), indicating that firms were unlikely to receive branch loans during periods directly following the CPP when counties and regional communities were most at risk for harsh contractions in consumer demand and business dynamics.

Robustness checks include a slew of more traditional Difference-in-Difference estimators that validate concerns about pre-trend violations among different treated groups after controlling for county specific and time fixed effects, as well as level of urbanization by time, and Federal Reserve branch area by time fixed effects. Instrumenting treatment status using political connections actively makes pretrend tests perform worse. Using a fully saturated model with different treatment effects based around two periods of treatment with two treatment statuses in each period (own and neighbor), Difference-in-Differences results satisfy pretrend assumptions but confirm with prior no spillover effect from our mainline specifications.

Two final robustness checks confirm our preferred LASSO-synthetic control estimates. We explicitly estimate Equation 1 and develop an interactive fixed effects difference-in-differences estimator that satisfies pretrends for all models, but continues to show no effects across entry, exit, employment expansion, and employment contraction. The last check incorporates all branch locations of treated banks outside the largest 20 banks in the country. These results mirror previous interactive fixed effects difference-in-differences estimates, where treated counties showed long term improvement in firm exit, employment expansion, and employment contraction, but generally occurred well after the program started, and are hard to tie explicitly to just county's CPP treatment.

These results closely mirror previous results showing no effect on bank level lending behavior following receiving CPP funds. If banks did not actively ease credit constraints to local firms, then new entrepreneurs and existing businesses would have continued to face the brunt of negative credit and consumer demand shocks unassisted. Given the large outlay of government funds to promote business lending, and poaching by Federal regulators and the Treasury to give money to predominately healthier banks, casts doubt on the use of such programs in the future.

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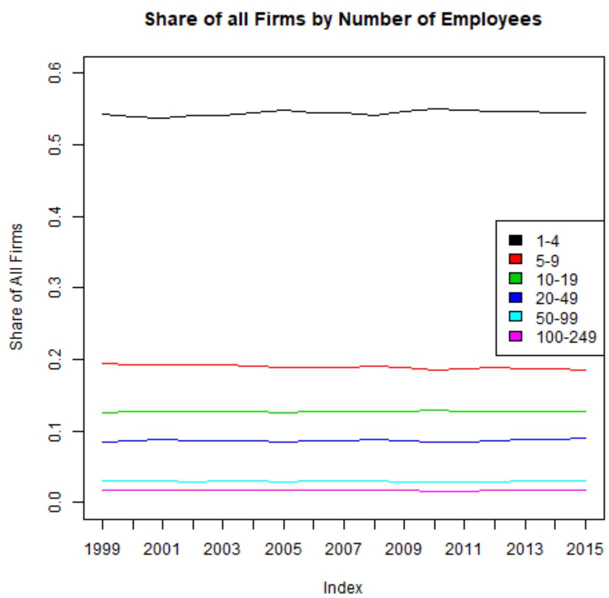
# A Appendix

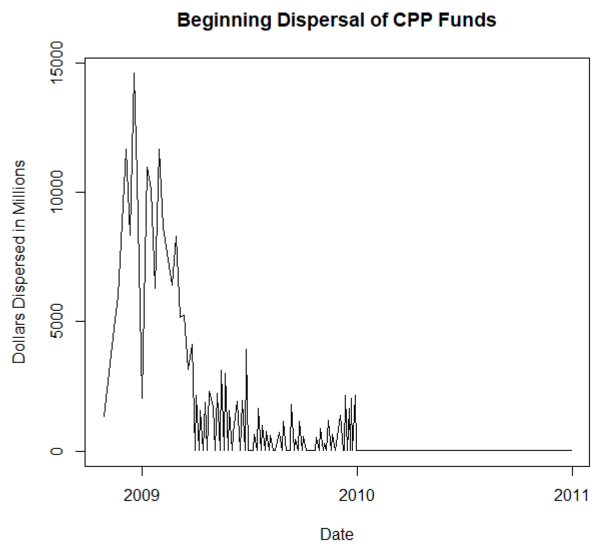
## A.1 Figures

Figure 1



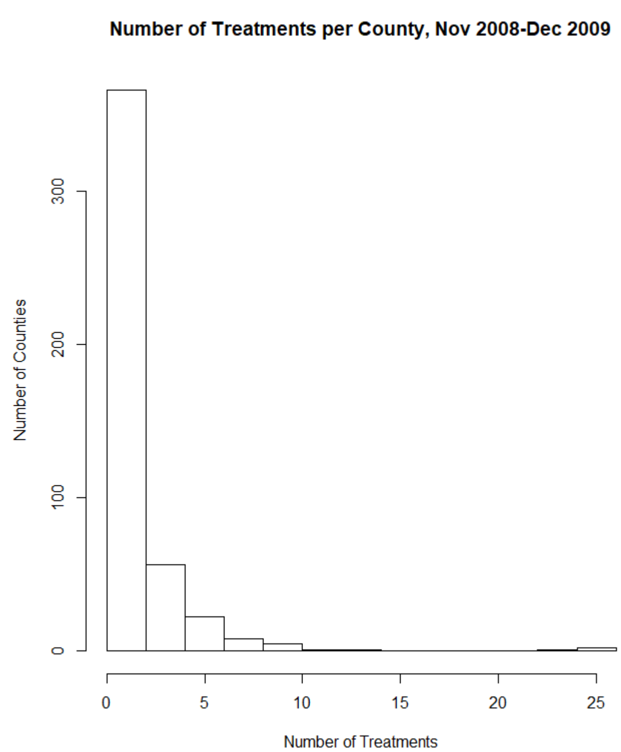

Data compiled from Census's County Business Patterns. Data shows share of establishments at different sizes from 1999 to 2015.

Figure 2: Dispersal of CPP Funds 2008-2009



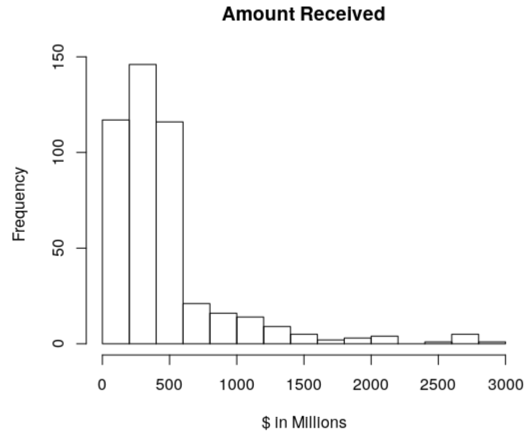

This figure shows the dispersal of CPP funds to banks across the US by date of Treasury to bank transaction listed in the TARP Transaction Report.

Figure 3: Number of Banks that Received CPP Funds Among Counties that Received CPP Funds



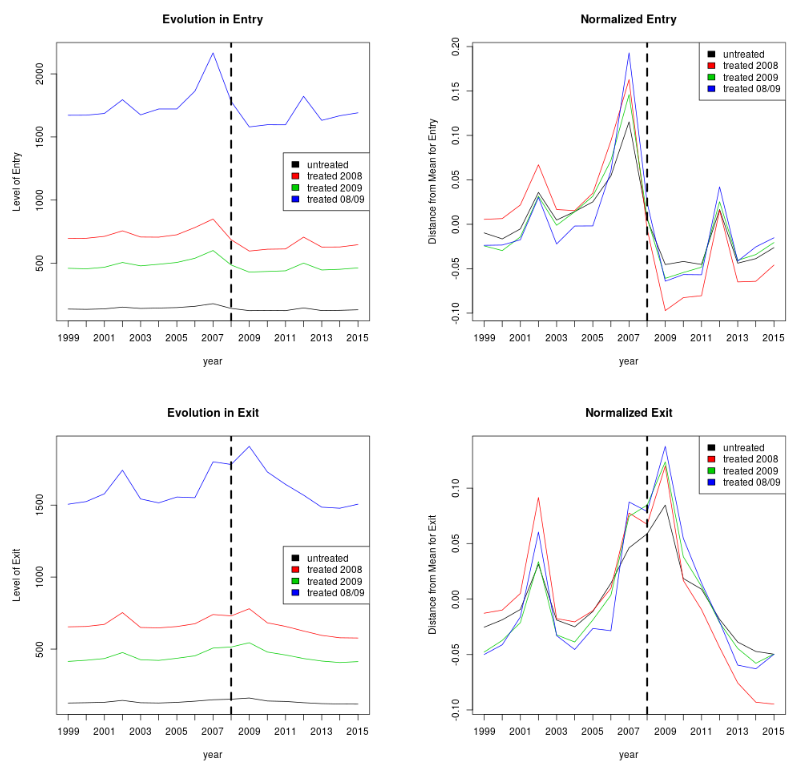

Data compiled from Treasury CPP Transaction Reports. Shows among counties how many banks in a given county received treatment. The presence of New York City, New York, is a clear outlier, from otherwise highly bunched few-treatments-per-county among the reamining sample.

Figure 4: Amount Received Per Worker



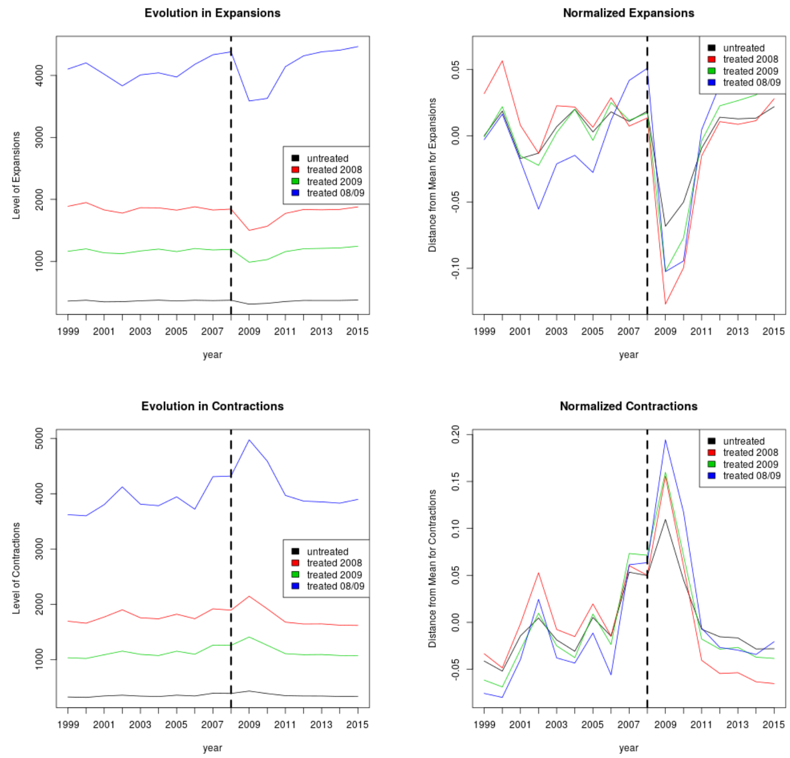

Total CPP funds per county divided by 2008 labor force compiled from Treasury CPP Transaction Reports and BLS local area unemployment statistics. Does not exclude counties that had a Bank Holding Company headquarters.

Figure 5: Subgroup Pre-Trends: Entry and Exit



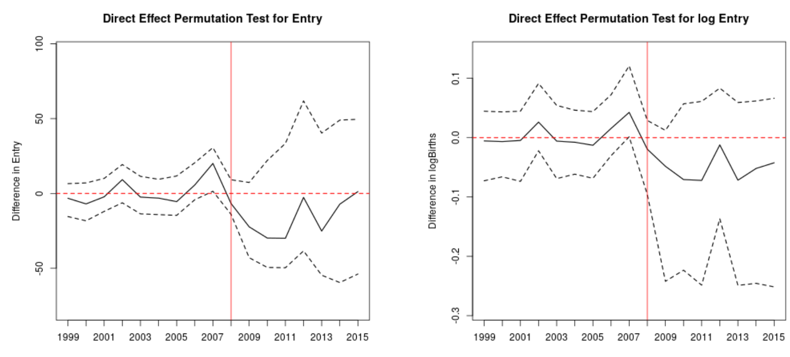

The left column charts trends in establishment entry levels by treatment group- receiving treatment in both 2008 and 2009, receiving treatment in only 2008 or 2009, and not receiving treatment. The right hand column normalizes the series by pre-treatment group means and variances.

Figure 6: Subgroup Pre-Trends: Employment Expansion and Contraction



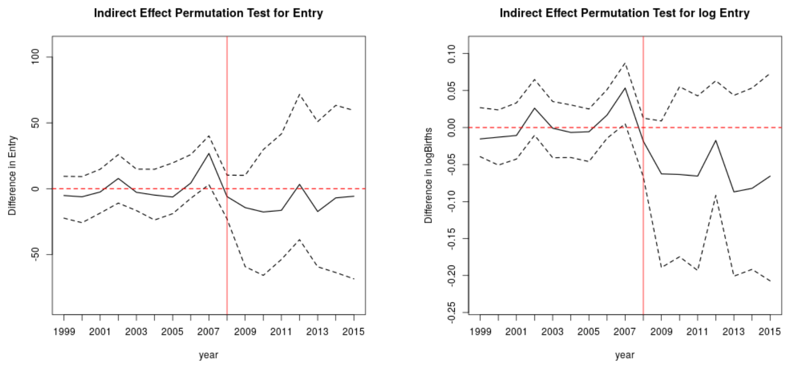

The left column charts trends in establishment entry levels by treatment group- receiving treatment in both 2008 and 2009, receiving treatment in only 2008 or 2009, and not receiving treatment. The right hand column normalizes the series by pre-treatment group means and variances.

Figure 7: Direct Effect Establishment Entry



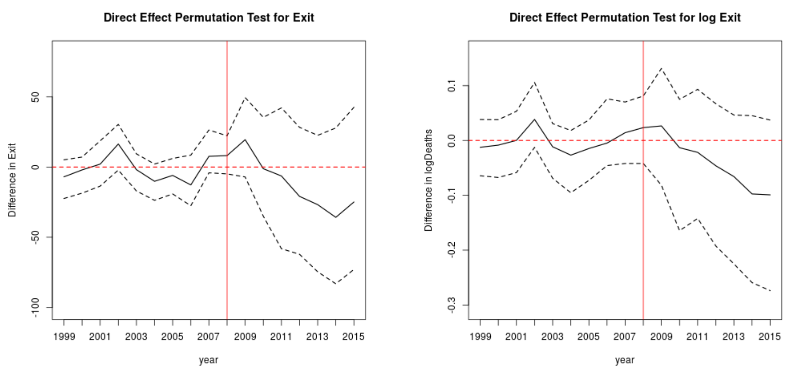

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 8: Indirect Effect Establishment Entry



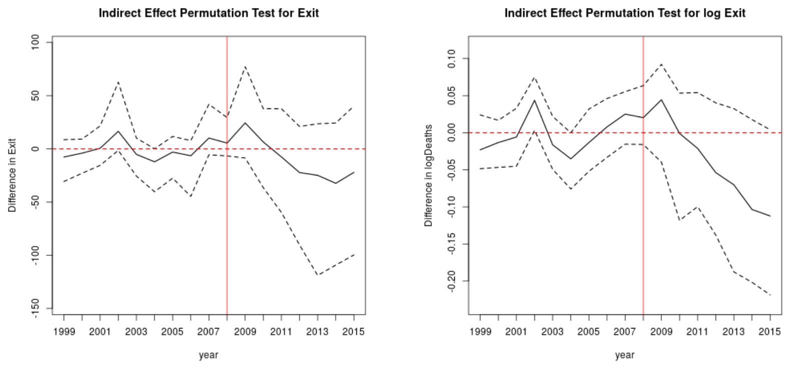

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 9: Direct Effect Establishment Exit



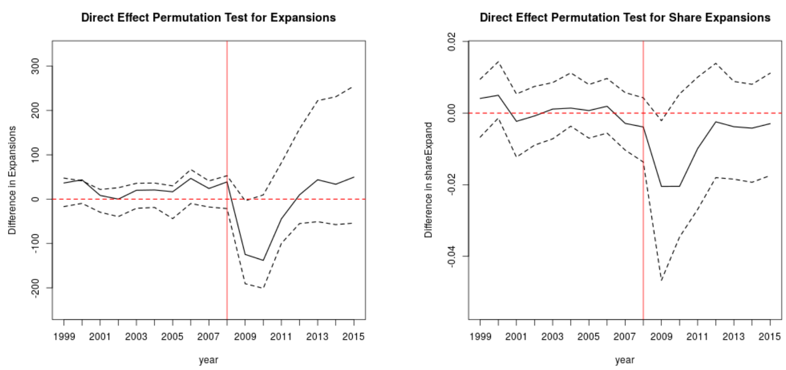

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 10: Indirect Effect Establishment Exit



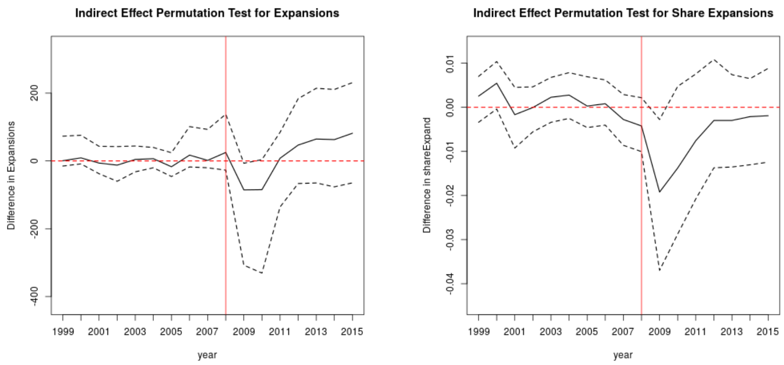

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 11: Direct Effect Employment Expansion



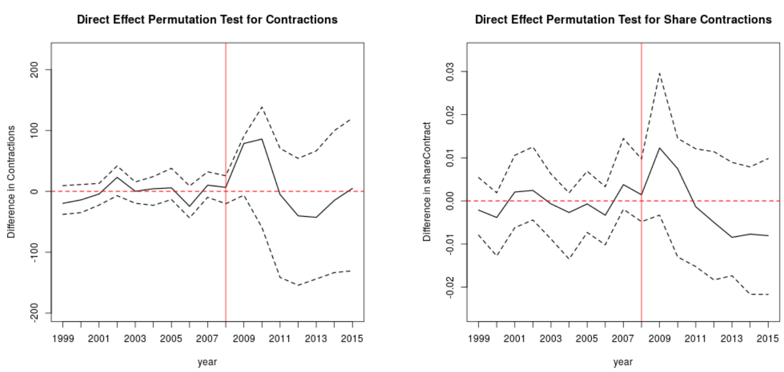

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 12: Indirect Effect Employment Expansion



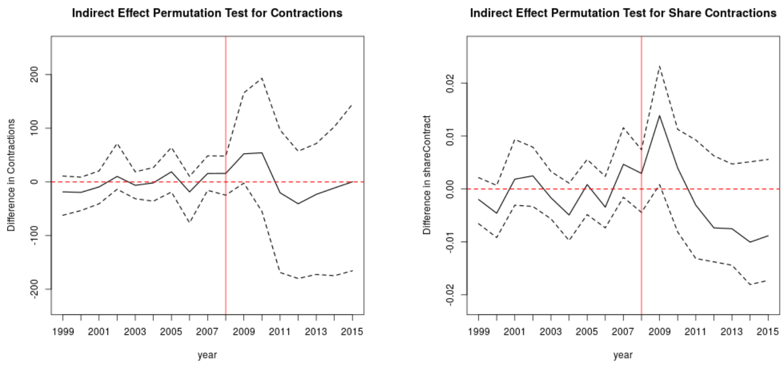

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 13: Direct Effect Employment Contraction



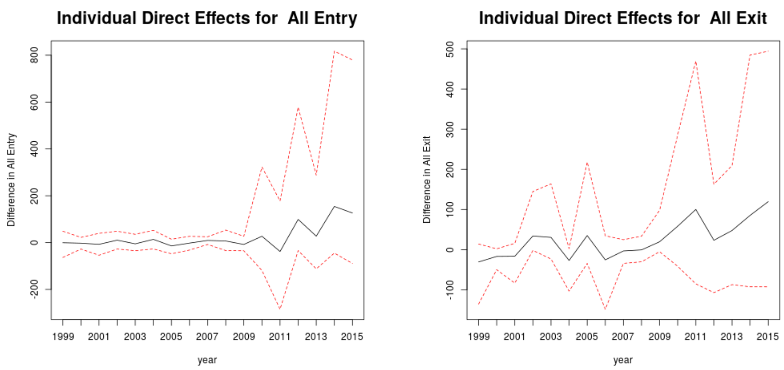

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 14: Indirect Effect Employment Contraction



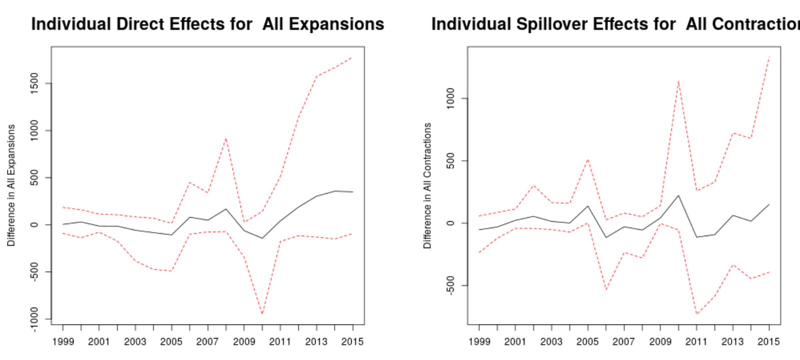

LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Black line is the estimate for the the empirically observed set of treated counties, and the dashed black lines represent the 90% permutation test confidence intervals under the null hypothesis of no treatment.

Figure 15: Heterogeneous Impacts: Entry & Exit



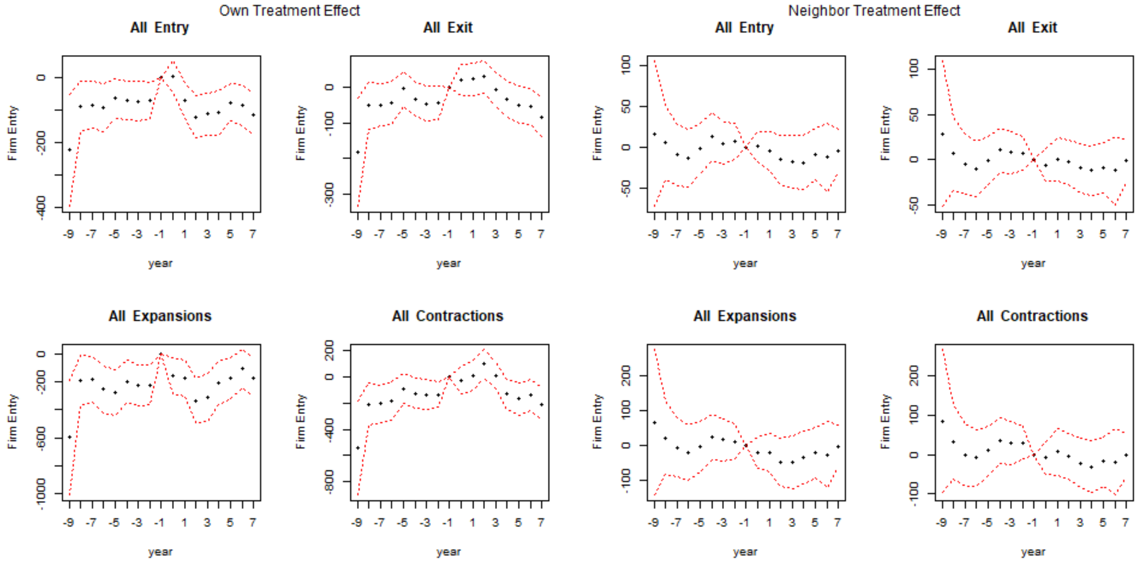

Black line is the mean effect among the empirically observed treatment group. Dashed black lines represent the 95% confidence interval among treated responses.

Figure 16: Heterogeneous Impacts: Expansions & Contractions



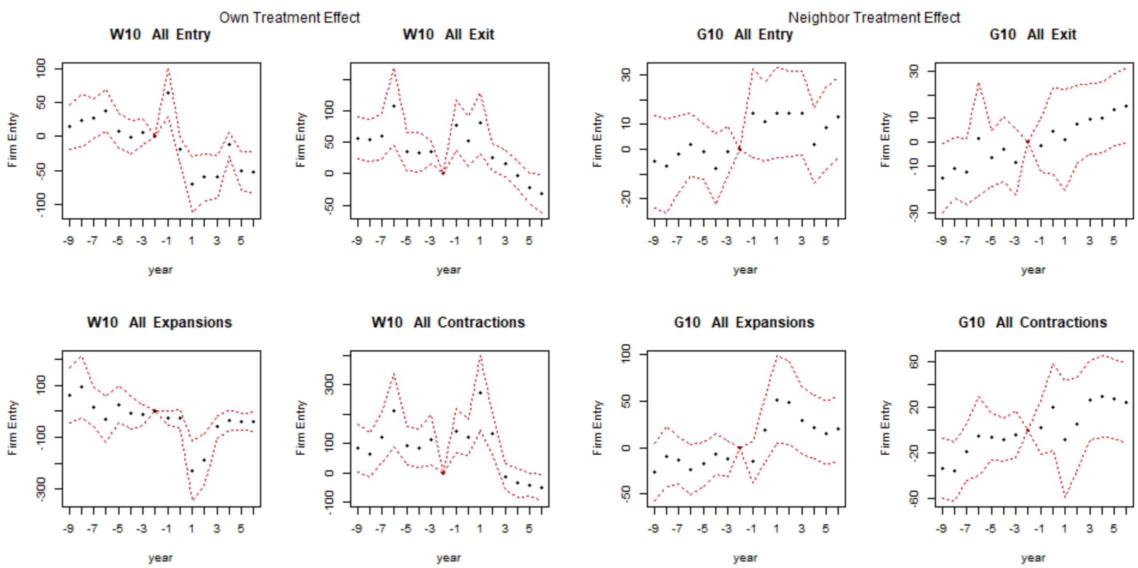

Black line is the mean effect among the empirically observed treatment group. Dashed black lines represent the 95% confidence interval among treated responses.

Figure 17: DID Own & Neighbor Treatment Status



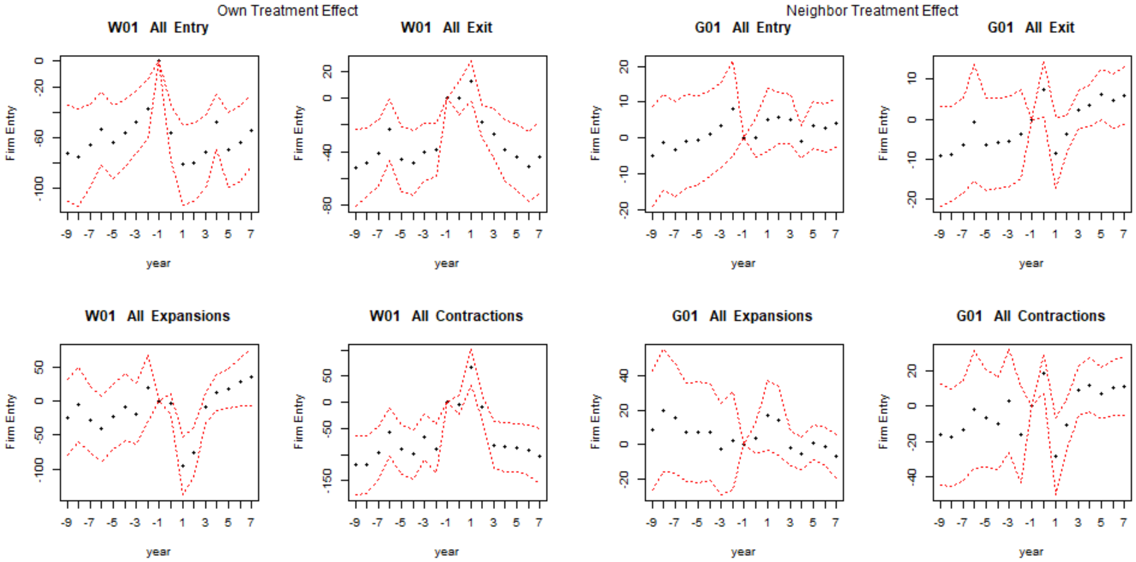

Event study plot of pre-trends and post-treatment effects for a Difference-in-Differences two-way fixed effects regression with level of urbanization by time and Federal Reserve branch by time effects and shared treatment effect across time-of-treat subgroups.

Figure 18: Own(1,0) & Neigh(1,0) Treatment Status



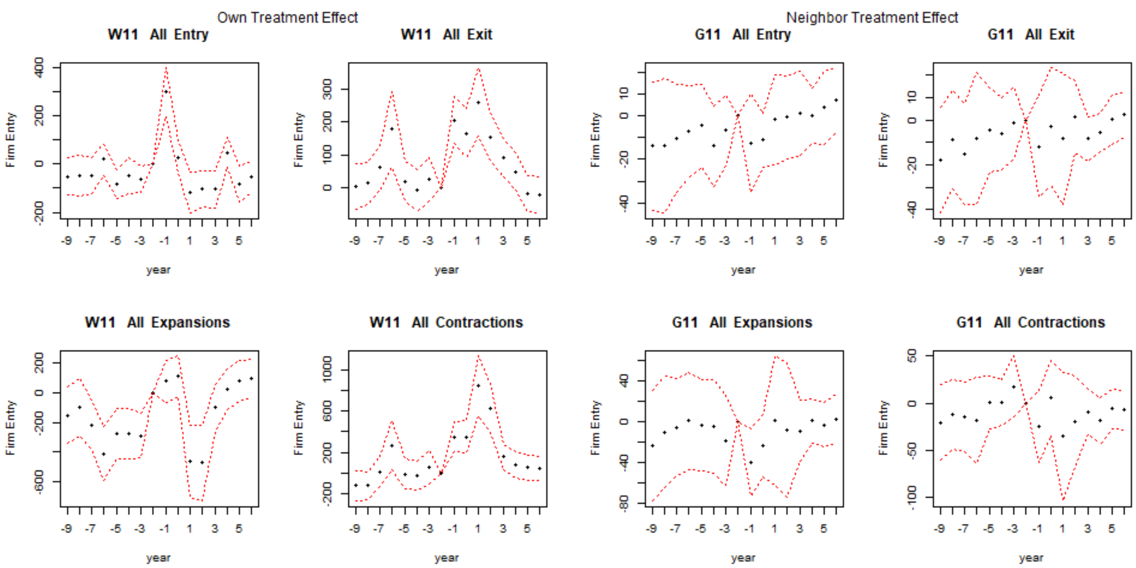

Event study plot of pre-trends and post-treatment effects for a Difference-in-Differences two-way fixed effects regression with level of urbanization by time and Federal Reserve branch by time effects and shared treatment effect for individuals who only received treatment, or have a neighbor receive treatment in 2008

Figure 19: DID Own(0,1) & Neigh(0,1) Treatment Status



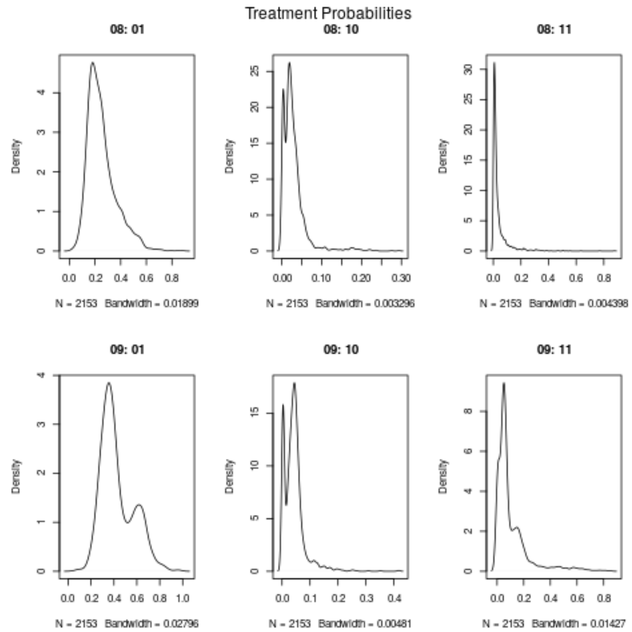

Event study plot of pre-trends and post-treatment effects for a Difference-in-Differences two-way fixed effects regression with level of urbanization by time and Federal Reserve branch by time effects and shared treatment effect for individuals who only received treatment, or have a neighbor receive treatment in 2009

Figure 20: DID Own(1,1) & Neigh(1,1) Treatment Status



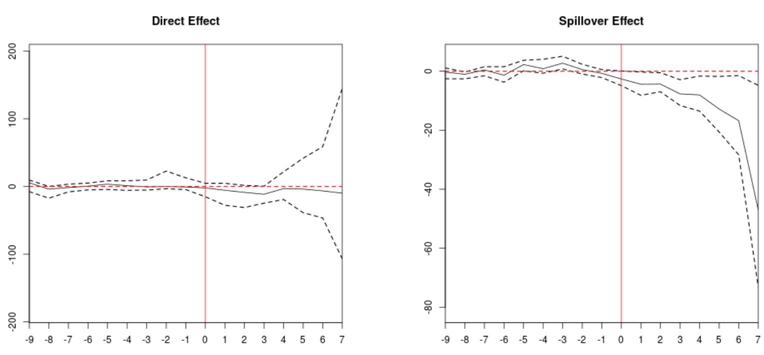

Event study plot of pre-trends and post-treatment effects for a Difference-in-Differences two-way fixed effects regression with level of urbanization by time and Federal Reserve branch by time effects and shared treatment effect for individuals who only received treatment, or have a neighbor receive treatment in both 2008 and 2009

Figure 21: Bivariate Probit Propensity Scores



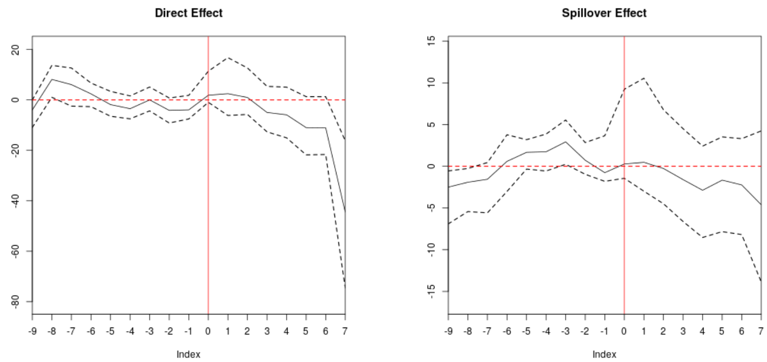

Each row from left to right is the probability of only Own Treatment, only Neighbor Treatment, or Both Treatment in either 2008 (top row) or 2009 (bottom row) based on estimating bivariate probits in 2008 and 2009 on a set of 4 instruments of county level political connections plus additional exogenous variables.

Figure 22: IEDID Entry



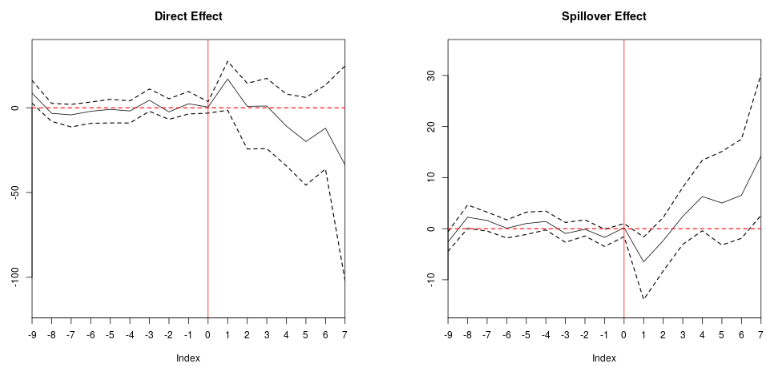

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009.

Figure 23: IEDID Exit



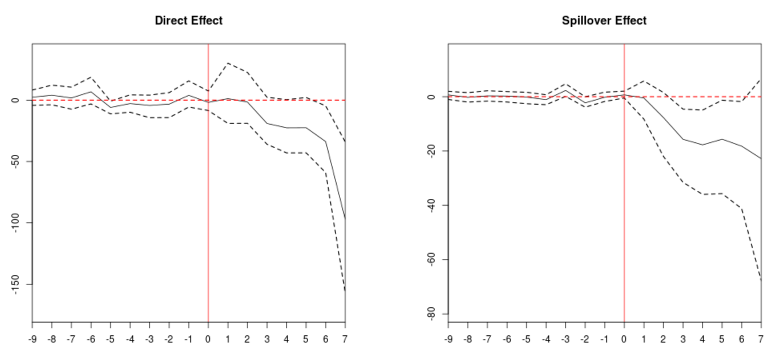

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009.

Figure 24: IEDID Expansions



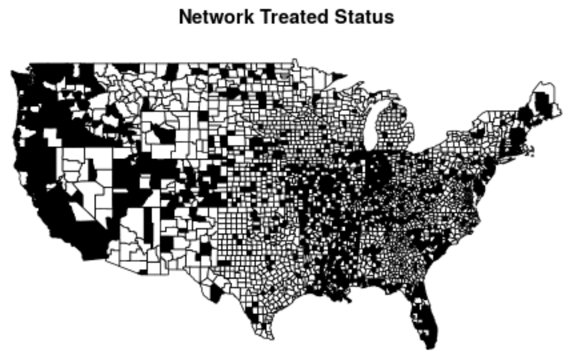

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009.

Figure 25: IEDID Contractions



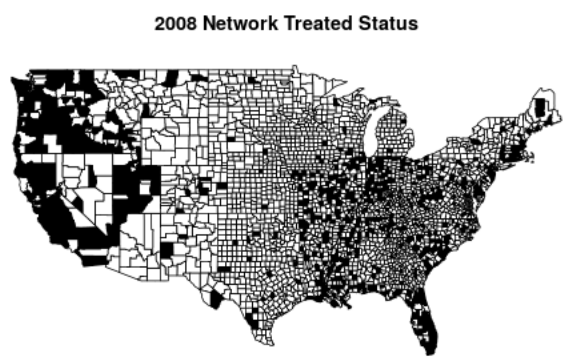

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009.

Figure 26: Treated Downstream Counties



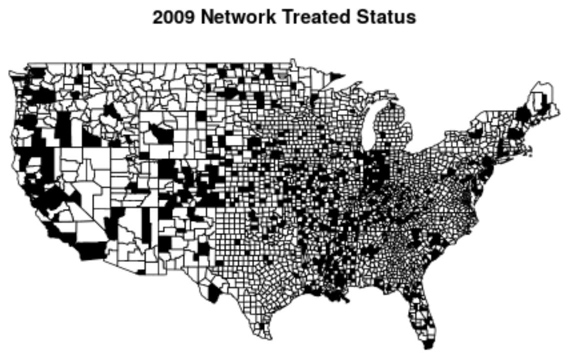

Map of all counties with a branch location of a bank that received CPP funds in either 2008 or 2009.

Figure 27: Treated Downstream Counties 2008



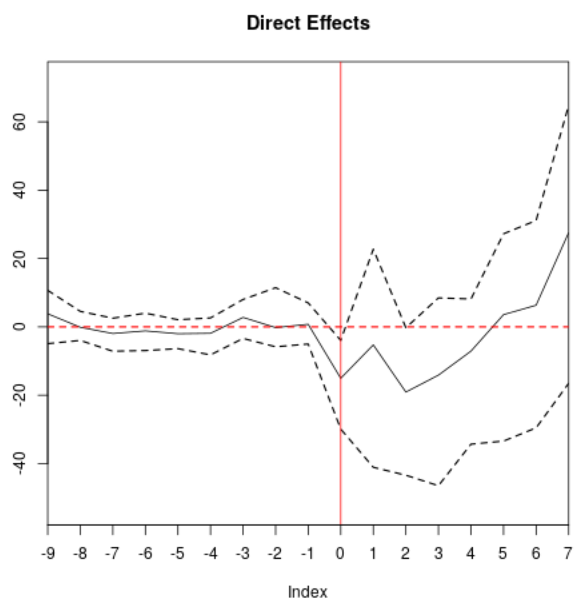

Map of all counties with a branch location of a bank that received CPP funds in 2008

Figure 28: Treated Downstream Counties 2009



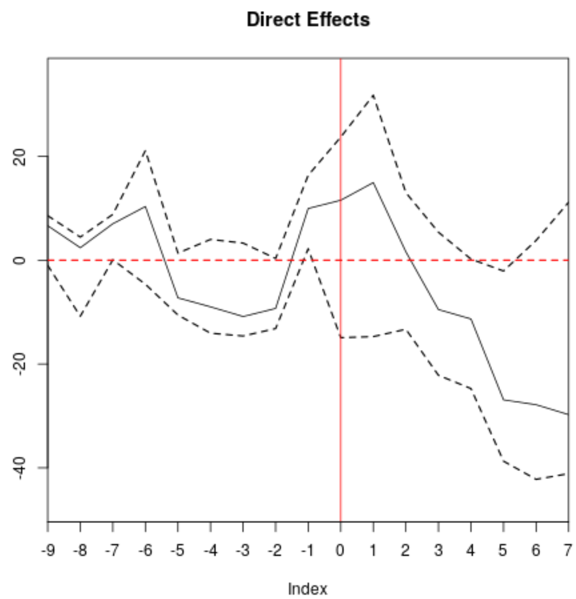

Map of all counties with a branch location of a bank that received CPP funds in 2009.

Figure 29: IEDID Network Entry ATT



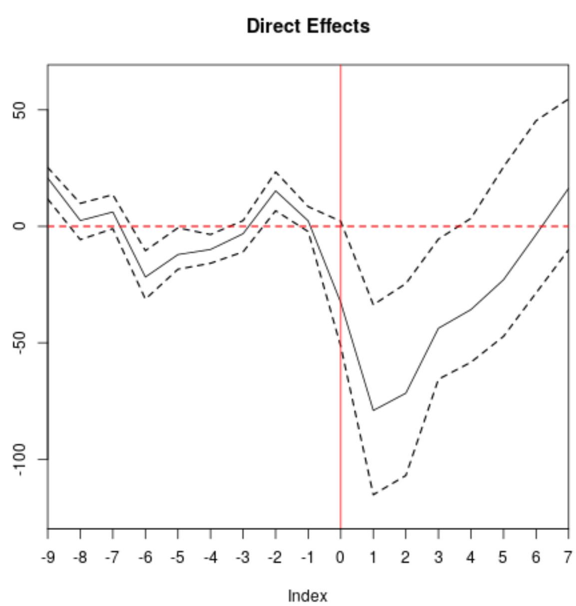

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009. Identifies all counties that had a bank from all branch locations of treated bank treated.

Figure 30: IEDID Network Exit ATT



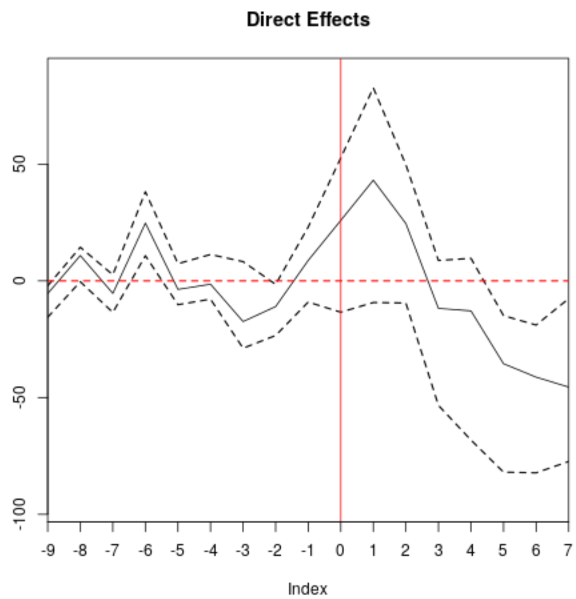

Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009. Identifies all counties that had a bank from all branch locations of treated bank treated.

Figure 31: IEDID Network Employment Expansions ATT




Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009. Identifies all counties that had a bank from all branch locations of treated bank treated.

Figure 32: IEDID Network Employment Contractions ATT




Treatment effect for time-from-treated. Estimating using Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009. Identifies all counties that had a bank from all branch locations of treated bank treated.

## A.2 Tables

Table 1: Summary Statistics of Data

| | PrGFC | PoGFC | Diff | prGFC SD | PoGFC SD | SD Diff |
|---|---|---|---|---|---|---|
| Firm Entry | 266.356 | 240.067 | -26.289 | 730.369 | 681.613 | -48.756 |
| Firm Exit | 238.954 | 242.725 | 3.771 | 659.948 | 659.921 | -0.027 |
| Emp. Expansion | 638.102 | 630.029 | -8.072 | 1,614.693 | 1,575.101 | -39.592 |
| Emp. Contraction | 604.360 | 637.556 | 33.196 | 1,539.482 | 1,565.769 | 26.287 |
| Unemp. Rate | 5.088 | 7.483 | 2.394 | 1.769 | 2.754 | 0.985 |
| Neighbor Unemp. Rate | 5.157 | 7.549 | 2.392 | 1.470 | 2.487 | 1.016 |
| Troubled Asset Ratio | 0.028 | 0.018 | -0.009 | 0.076 | 0.056 | -0.020 |
| Neigh. Troubled Asset Ratio | 0.029 | 0.019 | -0.010 | 0.046 | 0.028 | -0.017 |
| Return on Assets | 0.457 | 0.554 | 0.097 | 0.523 | 4.689 | 4.166 |
| Neigh. Return on Assets | 0.444 | 0.561 | 0.117 | 0.330 | 2.180 | 1.850 |
| Loans to Deposits | 52.320 | 60.262 | 7.942 | 49.362 | 38.405 | -10.957 |
| Neigh. Loans to Deposits | 50.671 | 58.118 | 7.447 | 34.230 | 20.979 | -13.252 |
| HPI Change | 4.820 | -0.496 | -5.317 | 4.465 | 4.623 | 0.159 |
| HPI | 228.467 | 255.897 | 27.430 | 125.742 | 132.322 | 6.579 |

Table 2: Wald Tests for Model 1 and NAICS code ..

| Pretrend | Significant |
|---|---|
| Entry All Treated | No Shared Pretrend |
| Entry Own Treated | No Shared Pretrend |
| Entry Neigh Treated | No Shared Pretrend |
| Exits All Treated | No Shared Pretrend |
| Exits Own Treated | No Shared Pretrend |
| Exits Neigh Treated | No Shared Pretrend |
| Expansions All Treated | No Shared Pretrend |
| Expansions Own Treated | No Shared Pretrend |
| Expansions Neigh Treated | No Shared Pretrend |
| Contractions All Treated | No Shared Pretrend |
| Contractions Own Treated | No Shared Pretrend |
| Contractions Neigh Treated | No Shared Pretrend |

No shared pretrend implies a p-value less than 0.005

Table 3: Step Down Tests for Non-Zero ATT Following 10 Treatment

| stepDownNames | own.diff.sig | neigh.diff.sig |
|---|---|---|
| Entry | Effect for 5 Time periods | No Effect |
| Exit | Effect for 2 Time periods | No Effect |
| Expansions | Effect for 2 Time periods | No Effect |
| Contractions | Effect for 7 Time periods | No Effect |

Table 4: Step Down Tests for Non-Zero ATT Following 01 Treatment

| stepDownNames | own.diff.sig | neigh.diff.sig |
|---|---|---|
| Entry | No Effect | No Effect |
| Exit | Effect for 2 Time periods | No Effect |
| Expansions | Effect for 2 Time periods | No Effect |
| Contractions | Effect for 2 Time periods | No Effect |

Table 5: Step Down Tests for Non-Zero ATT Following 11 Treatment

| stepDownNames | own.diff.sig | neigh.diff.sig |
|---|---|---|
| Entry | No Effect | No Effect |
| Exit | No Effect | No Effect |
| Expansions | No Effect | No Effect |
| Contractions | Effect for 7 Time periods | Effect for 3 Time periods |

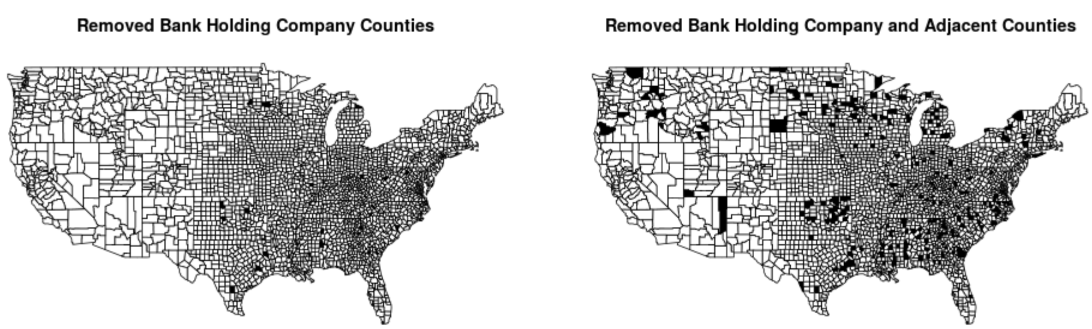
Table 6: Wald Tests for IV Pretrend

| Pretrend | Significant |
|---|---|
| Entry Own Treated 2008 | No Shared Pretrend |
| Entry Neigh Treated 2008 | No Shared Pretrend |
| Entry Own Treated 2009 | No Shared Pretrend |
| Entry Neigh Treated 2009 | No Shared Pretrend |
| Entry All | No Shared Pretrend |
| Exit Own Treated 2008 | No Shared Pretrend |
| Exit Neigh Treated 2008 | No Shared Pretrend |
| Exit Own Treated 2009 | No Shared Pretrend |
| Exit Neigh Treated 2009 | No Shared Pretrend |
| Exit All | No Shared Pretrend |
| Expansions Own Treated 2008 | No Shared Pretrend |
| Expansions Neigh Treated 2008 | No Shared Pretrend |
| Expansions Own Treated 2009 | No Shared Pretrend |
| Expansions Neigh Treated 2009 | No Shared Pretrend |
| Expansions All | No Shared Pretrend |
| Contractions Own Treated 2008 | No Shared Pretrend |
| Contractions Neigh Treated 2008 | No Shared Pretrend |
| Contractions Own Treated 2009 | No Shared Pretrend |
| Contractions Neigh Treated 2009 | No Shared Pretrend |
| Contractions All | No Shared Pretrend |

No shared pretrend implies a p-value less than 0.005

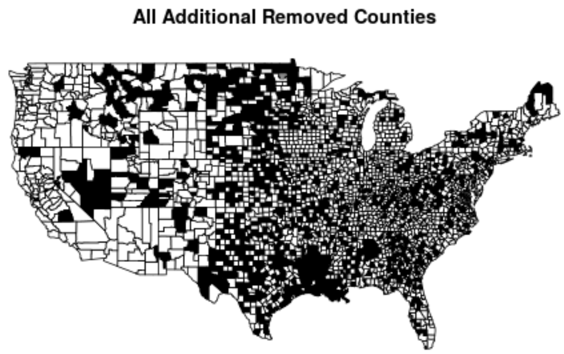
## A.3 Dropped Counties

Figure 33: Removed Bank Holding Company Counties




Left map are counties that had the top 20 largest banks or bank holding companies in them. The right map are all counties that had a county centroid within 50 miles of a county that had one of the largest banks or bank holding companies. All these counties are dropped from our sample.

Figure 34: Additional Removed Counties




All counties that are dropped for a variety of reasons. This includes being an unbalanced panel in our data set, not having enough loans to register in the FHFA's county level home price index, or having zero new establishment entrants or establishment exits for at least one period from 1999 to 2015. These counties are only dropped in our robustness checks that require additional covariates.