The Impact of Federal Policy Implementation Frictions on Commercial Real Estate Market Dynamics

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ABSTRACT (200 words)
During periods of financial stress, policymakers may take actions to stabilize credit conditions, but it is not always clear whether these interventions will transmit successfully into real asset markets. This study examines the U.S. Treasury's Capital Purchase Program (CPP), which provided capital injections to banks during the Great Financial Crisis, to evaluate whether this public support transmitted to commercial real estate (CRE) markets. Using county-level panel data, we define treatment as local exposure to CPP through bank participation. Outcome measures track establishment and employment dynamics. Existing studies typically evaluate CPP through lending activity or aggregate employment outcomes. Non-random program participation and differential pre-treatment trends limit the validity of standard difference-in-differences approaches. To address these concerns, we estimate causal effects using a synthetic control framework that accounts for staggered treatment timing and spatial exposure to nearby credit markets. There are limited effects from CPP exposure. Short-run impacts dissipate over time and there is little evidence of spillovers. The findings indicate that CPP funds supported balance sheet activities but did not expand credit, which constrained transmission to CRE markets. This highlights how financial stabilization priorities can, even when effective in preventing systemic distress, mute spillovers to local property markets.

KEYWORDS
commercial real estate, capital flows, stabilization policies, market frictions, market dynamics

JEL CLASSIFICATIONS
R14, R33, E44, E63, G21

Acknowledgments
We appreciate feedback provided by Otavio Bartalloti and Helle Bunzel. Helpful comments were also received from conference and seminar participants at the Federal Housing Finance Agency (FHFA), Midwest Economics Association, and the U.S. Census Bureau.

Disclosure statement
This paper was written in an independent capacity. The analysis and conclusions are those of the authors alone and should not be represented or interpreted as conveying an official position, policy, analysis, opinion, or endorsement of the United States government or any other employer. Any errors or omissions are the sole responsibility of the authors. No potential conflict of interest was reported by the author(s).

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1. Introduction

The Great Financial Crisis (GFC) fundamentally disrupted the flow of credit to both financial and real asset markets (Sun et al., 2015).[1] As capital markets seized up in 2008, the sharp contraction in bank lending cascaded into the commercial real estate sector by impairing refinancing, halting development, and triggering widespread valuation declines. To stem a systemic collapse, the U.S. Department of the Treasury introduced the Capital Purchase Program (CPP) under the Troubled Asset Relief Program (TARP).[2] The CPP injected $205 billion into more than 700 banks across 400 counties, thereby providing regulatory capital to stabilize balance sheets and restore the flow of credit to the broader economy. Although the program was designed as a financial-sector intervention, its transmission to local commercial real estate markets through credit channel effects remains unclear. Because commercial real estate investment and refinancing depend critically on local credit availability, the CPP offers a setting to test whether improved bank capitalization translates into changes in local space demand, leasing activity, and property market conditions.

In the standard urban model, firm entry, expansion, and exit collectively shape spatial patterns of land use and the demand for commercial property types such as office, retail, industrial, and mixed-use space. Within this context, examining local establishment dynamics provides a window into how credit stabilization affected (or failed to do so) commercial real estate fundamentals through tenant-side demand conditions rather than price or return measures.[3] New business formation stimulates demand for office and retail space, while firm closures and employment contractions release built environment inventory and suppress local rents. Similarly, when credit constraints tighten, development projects stall, leasing activity slows, and market vacancy rates rise. Hence, establishment dynamics are not only indicators of general economic health but also key determinants of equilibrium outcomes in commercial real estate markets (An et al., 2016). Understanding how public crisis response programs, like the CPP, influence these dynamics provides valuable insight into how financial stabilization policies propagate through commercial property markets (Ling et al., 2025). In the context of commercial property cycles (Wheaton & Torto, 1998; Fisher et al., 2017), we interpret establishment turnover as an indicator of space demand and rent pressures that link firm dynamics to the evolution of local commercial real estate (CRE) markets.

The CPP can therefore be viewed as a natural experiment in commercial real estate capital supply. By providing preferred equity to banks, the program altered the cost of capital and the risk tolerance of financial intermediaries that supply most debt financing for local commercial real estate projects. Banks with stronger post-CPP capital positions could, in principle, extend new credit for commercial development, restructure distressed property loans, or support tenant businesses occupying retail or office space. Conversely, if the injected capital remained on balance sheets to repair liquidity (or to sit untouched) rather than being recycled into new lending, the pass-through to local property markets may have been minimal. This distinction motivates our focus on implementation frictions that limit the transmission from bank recapitalization to real estate markets. Moreover, spatial credit spillovers, where firms or developers seek financing from nearby counties, imply that the CPP's effects may extend beyond directly treated regions through cross-county credit flows, although such transmission may be limited in practice (Ling et al., 2023).

This study investigates these linkages by examining how the CPP affected local establishment dynamics that proxy for commercial property demand and space utilization.[4] Using county-level panel data, we estimate both the direct effects of having a bank in a county receive CPP funds and the indirect effects on adjacent counties within the region. These responses—changes in firm entry, exit, employment expansion, and contraction—capture shifts in the underlying demand for and utilization of commercial real estate space rather than direct movements in prices or rents. Our empirical framework utilizes LASSO-augmented synthetic control estimators to isolate the causal effect of capital injections on local real estate-linked activity.

These results extend previous work by Berger & Roman (2017) 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-specific financial health and credit supply (Davis & Haltiwanger, 2024). Previous work examines if the CPP increased lending or aggregate business activity whereas we focus on whether those changes translated into CRE demand conditions. Results in this literature are often mixed, inconclusive, or even contradictory (Black & Hazelwood, 2013; Blau et al., 2013; Cole & Damm, 2020; Contessi & Francis, 2011; Egly & Mollick, 2013; Li, 2013; Berger et al., 2019; Bassett et al., 2020).

This study contributes to the CRE literature in three key ways. First, it investigates the transmission from bank recapitalization to CRE fundamentals instead of focusing on lending or employment outcomes in isolation. Second, it links the urban economics perspective on firm dynamics and space demand where establishment turnover can be a critical driver of leasing activity, occupancy, and utilization of commercial property. Third, it evaluates whether improved bank capitalization spreads spatially through credit markets by identifying how liquidity shocks to local banks can alter investment patterns and property usage beyond county borders. This paper's approach differs from prior CPP studies by investigating the transmission of bank recapitalization to CRE demand conditions rather than firm activity or lending outcomes alone. Our findings suggest that while the CPP achieved its objective of stabilizing bank balance sheets, its pass-through to local establishment performance, and by extension, to commercial real estate activity was limited. The muted response underscores a disconnect between financial capital injections and real asset market outcomes, which highlights the importance of understanding whether crisis-era financial interventions transmit to the built environment.

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 summary statistics. Section 4 evaluates how business establishment measures relate to commercial real estate market conditions. Section 5 formalizes the empirical design and estimation processes, and then Section 6 provides our preferred LASSO-synthetic control estimation results. Section 7 reports robustness checks, while Section 8 concludes.

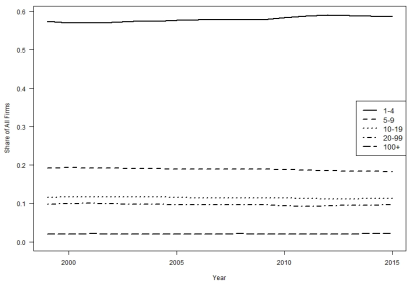
2. The Capital Purchase Program

The CPP disbursed $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, JPMorgan 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 effectively required to accept CPP funds as part of the government's broader financial sector stabilization efforts.

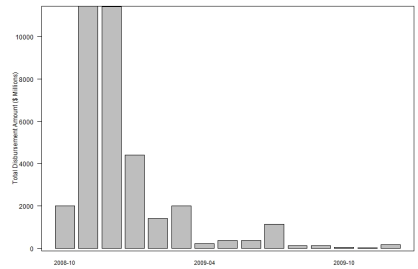
Individual banks applied for CPP funds through their federal regulator, including the Federal Reserve, FDIC, Office of the Comptroller of the Currency, or the Office of Thrift Supervision.[5] Banks indicated a preferred level of stock purchase between one and three percent of the total risk-weighted assets of the applicant up to $25 billion.

It may not be appropriate to consider CPP participation as being randomly assigned if it disproportionately targeted institutions under financial stress during the crisis. Federal regulatory agencies selected which banks would receive money and forwarded approved applicants to the Treasury Department for final clearance. Duchin & Sosyura (2014) show that of roughly 600 public firms, 416 firms (79.8%) applied and 329 (79.1%) were accepted. Of the 329, 278 (84.5%) accepted the funds, while 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 1, 2010.

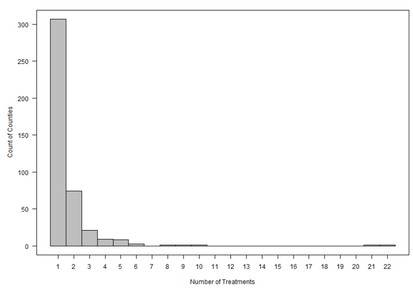
(a) Share of firms by number of employees



(b) Beginning disbursement of CPP funds



(c) Number of treatments per county



(d) Amount received

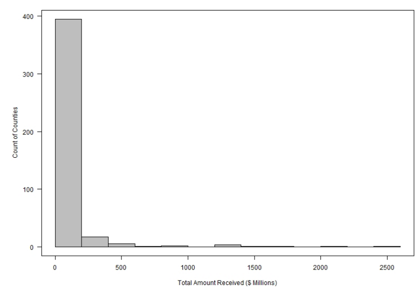


Figure 1. Setting the background about CPP funding for banks.
Notes: This figure illustrates several ways that Capital Purchase Program (CPP) funding could affect financial institutions. Panel (a) uses data compiled from Census's County Business Patterns to show the share of establishments at different sizes from 1999 to 2015. Panel (b) shows the disbursement of CPP funds across the United States by the Department of the Treasury to banks via transactions listed in the TARP Transaction Report. Panel (c) shows among counties how many banks in a given county received treatment as compiled across CPP Transaction Reports. The presence of New York City is a clear outlier, from otherwise highly bunched few-treatments-per-county among the remaining sample. Panel (d) illustrates total CPP funds per county divided by 2008 labor force compiled from Treasury CPP Transaction Reports and BLS local area unemployment statistics. The data do not exclude counties that have a Bank Holding Company headquarters. Source: Author calculations.

Figure 1 presents several aspects of the CPP funding provided to banks. Panel (a) illustrates that over 90% of the public aid was sent to firms with under 100 employees. Both the number of new firm entrants in each county and the share of firms at different levels of employment are quite stable. Panel (b) shows clear spikes in disbursements, with a large volume of funds distributed in the latter quarter of 2008, a slowdown during the holidays, and then additional disbursements finalizing at the end of the first quarter of 2009. Panel (c) denotes how many banks received funds in each location. Most counties had only a few banks receive funds, and an even 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. Panel (d) demonstrates considerable bunching in funds per worker across counties. These observations reflect general concerns about firm concentration and a need for perceived higher capital constraints relative to the late 1990s.

The non-voting senior preferred shares required a 5% dividend for the first 5 years and 9% afterwards.[6] However, previous research indicates these purchases were favorable to participating banks. The Congressional Oversight Panel estimates 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. Equivalently, Veronesi & Zingales (2010) estimate during the first 10 CPP transactions, the Treasury overpaid by between $6-13 billion for financial claims.

Overall, the CPP provided standardized capital injections to participating banks in two primary waves, at the end of 2008 or the beginning of 2009. Many banks applied, and some declined funds after being approved.[7] Since most counties only had a small number of banks receive CPP funds, a *treatment* is defined as whether a county had at least one bank receive CPP funds in either 2008 or 2009. This approach simplifies an otherwise complex setting with heterogeneous funding amounts by focusing on exposure to CPP rather than treatment intensity. We adopt this specification because bank operations often span multiple market areas, which complicates our ability to attribute funding amounts to specific locations. This setup also helps us isolate whether changes in bank capitalization translated into local credit conditions that affect commercial real estate demand.

3. Data and Summary Statistics

The primary dependent variables of interest are county-level establishment entry, establishment exit, employment expansion, and employment contraction provided by the Census in its Statistics of U.S. Businesses (SUSB) and Business Information Tracking Series (BITS).[8] We interpret these measures as proxies for CRE fundamentals like tenant formation, space utilization, and building occupancy rather than direct measures of prices or rents. Establishments are classified as 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 sole proprietorships 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. The treatment effects capture how CPP-induced changes in credit conditions translate into firm dynamics that correspond with underlying CRE demand.

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 with positive first quarter employment in both the initial and subsequent years and decreased 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 has zero firm entries or exits, removing 161 counties (illustrative maps are in the Appendix in Figure A1 and Figure A2).[9] Dropped counties are predominantly small or in rural markets with limited establishment turnover and exclusion does not affect the identification strategy that relies on variation among economically active locations. Robustness checks confirm that later results still hold when alternative sample restrictions are applied.

Figure 2 plots mean establishment and employment measures by observed treated status. The top row is split between establishment entry (panels a and c) and exit (panels b and d) while the bottom row has employment expansion (panels e and g) and contraction (panels f and h). When plotted in levels (panels a, b, e, and f on the left half), there are large differences across treatment groups in all four outcomes for firm and labor dynamics. Counties that received treatment in both 2008 and 2009 average more than 1,500 new establishment entrants/exits a year and correspondingly large employment adjustments. Counties that received treatment in only one period tend to average around 500–700 new entrants and exits a year and smaller labor market changes, while non-treated counties consistently have the lowest levels across all measures. However, re-scaling each time series by its pre-2007 within-group mean and standard deviation reveals similar pre-treatment dynamics across groups for both establishment and employment measures. The normalized series (panels c, d, g, and h on the right) indicate parallel trends prior to treatment across entry, exit, expansion, and contraction outcomes. These visual diagnostics support the use of difference-in-differences techniques to construct valid counterfactuals for each treated subgroup.

The majority of firms are small with roughly 75% having fewer than 10 employees. These numbers are very stable across all years in the sample. The Statistics of U.S. Businesses data do not disentangle firm size but, using this sample, we can assume the majority of new entrants are small, which is supported by Mata & Portugal (1994), Bartelsman et al. (2005),

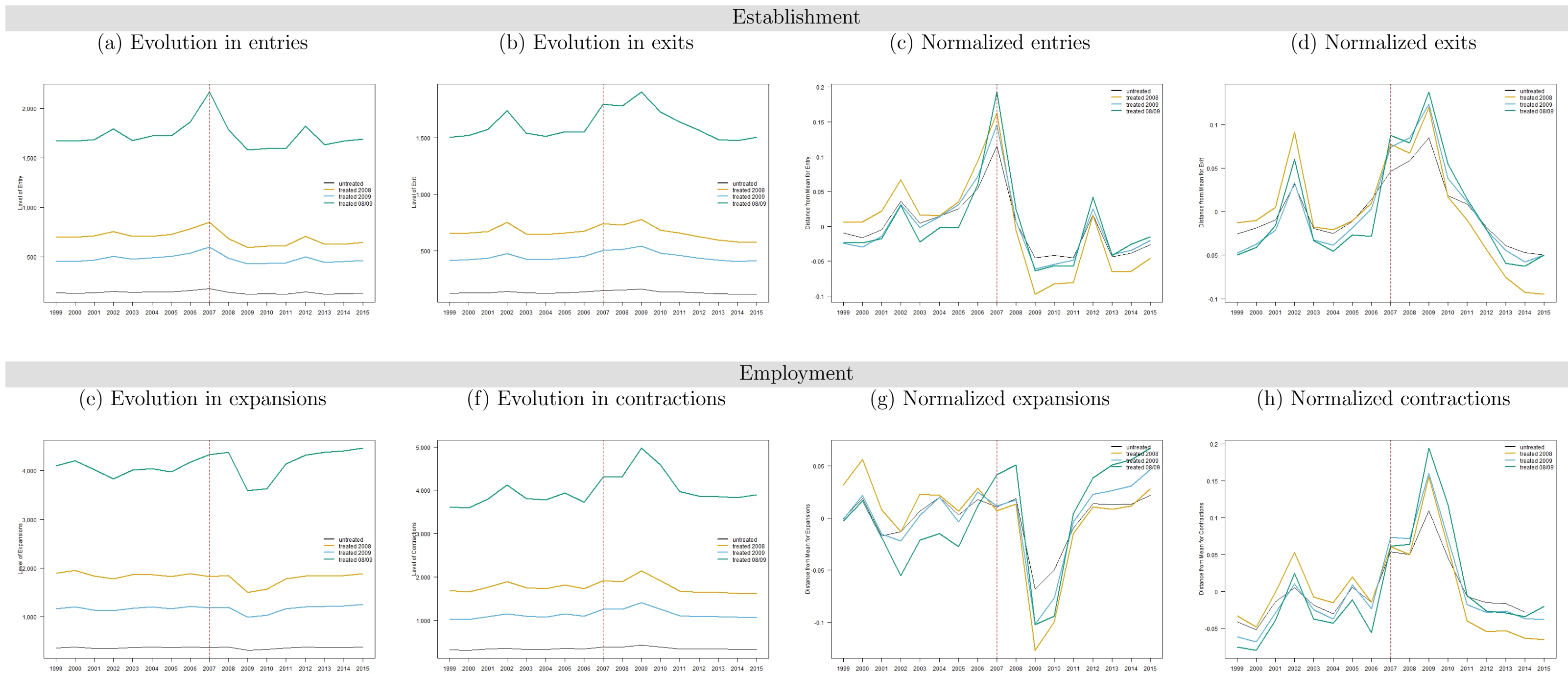


Figure 2. Subgroup pre-trend diagnostics for establishment and employment measures.

Notes: This figure shows pre-trend series split between panels for establishment and employment measures. Within each row, the first two figures show trends in levels by treatment group (panels a, b, e, and f) and the second two figures display normalized series by pre-treatment group means and variances (panels c, d, g, and h). Across all figures, the legends note treatment is received in either 2008 or 2009, only 2008, only 2009, or none at all. Source: Author calculations.

and Kaniovski & Peneder (2008). Because such establishments disproportionately occupy smaller and flexible commercial spaces, their entry and exit decisions can directly affect local occupancy and leasing. Unfortunately, most firms do not survive beyond a couple of years.

There is strong evidence that in good times credit constraints do not impact the decision to enter into entrepreneurial activity given the lack of a relation between wealth and entry into entrepreneurship (Hurst & Lusardi, 2004). Data from the 2003 National Survey of Small Business Finances show that among firms that had only opened after 2002, 25% had no outstanding loans, and 50% had less than $7,000 in loans. Among those firms that had taken out capital leases, 25% of them owed less than $4,000 in principal, and 75% owed less than $45,000.[10] Shane (2010) documents roughly 48.4% of new businesses start in a residence, while an additional 40.64% begin in a leased space. Finally, the median start-up in the U.S. requires $24,000-30,000 in capital.

Treatment status is defined as whether a county is exposed to CPP funding through local bank participation in a particular year. The Treasury Department updates the TARP Transaction Report that includes bank name, state name, and city name data. We merge in Federal Reserve Replication Server System Database IDs (RSSD ID) using the 2008 and 2009 FFIEC Call Reports and Summary of Deposits.[11] We are then able to calculate both a headquarter specific county treatment effect, and a bank network county treatment effect.

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 headquarter 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 a 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. We 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

$$\begin{aligned} Own_{i,t}^{HQ} &= 1\{\exists k \in i \text{ s.t. } CPP_{k,t} > 0\} \\ Own_{i,t}^{BW} &= 1\{\exists b_k \in i \text{ s.t. } CPP_{k,t} > 0\} \end{aligned} \tag{1}$$

where 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. Although CPP funding magnitude varied across banks, our baseline model explores treatment exposure rather than its intensity to assess if local credit conditions changed. Robustness checks using alternative exposure definitions produce qualitatively similar results.

Credit markets may extend beyond county borders, which implies that treating county i may impact nearby counties. A neighbor is defined as any county with a centroid distance within 50 miles of subject county i.[12] This metric is motivated by evidence that borrowers, such as entrepreneurs, will search beyond local markets for credit assistance. For example, in Belgian banks, the maximum loan distance is 50 miles (Degryse & Ongena, 2005), while in the U.S. average bank applications come from 10 miles away (Agarwal & Hauswald, 2010), 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 borrowing remains local, credit access may extend across nearby counties. Under this setting we define the neighbor treatment variable as:

$$\begin{aligned} Neigh_{i,t}^{HQ} &= 1\{\exists j \text{ adjacent to } i \; s.t. \; CPP_{j,t} > 0\} \\ Neigh_{i,t}^{BW} &= 1\{\exists b_k \text{ adjacent to } i \text{ s.t. } CPP_{k,t} > 0\} \end{aligned} \tag{2}$$

This definition measures potential exposure to nearby credit supply rather than a structural aspect of lending relationships, which may be affected by bank specialization or long-standing borrower networks. A major source of possible bias is that the largest banks in the U.S. were perceived to be highly illiquid at the start of the Great Financial Crisis. These banks were effectively told to take CPP funds, and thus did not opt into 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 financial institutions used CPP funds more for balance sheet operations rather than new lending, which could attenuate effects (see 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.[13] 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 the loan-to-deposits ratio.[14] 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 characteristics are provided through the BLS's Local Area Unemployment statistics on county-level unemployment rates.[15] While taxes do impact firm entry decisions, often these impacts are economically small, and explain a very small fraction of

Table 1. Summary statistics of outcome measures before and after treatment.

	Mean			Standard deviation		
	Pre-GFC	Post-GFC	Diff.	Pre-GFC	Post-GFC	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

Notes: This table presents mean and standard deviation statistics for before ("Pre-") and after ("Post-") the Great Financial Crisis (GFC) as well as the difference ("Diff.") between the values. Computations show whether there is a level shock (but not slope) to either statistic for subsequent difference-in-difference tests. Other abbreviations are as follows: "Emp." is employment, "Unemp." is unemployment, and "Neigh" is neighborhood. Source: Author calculations using Federal Financial Institutions Examination Council (FFIEC) Call Reports and Summary of Deposits, Replication Server System Database (RSSD) IDs from the Federal Reserve, Statistics of U.S. Businesses (SUSB) and County Business Patterns from the U.S. Census Bureau, and Troubled Asset Relief Program (TARP) Transaction Report from the U.S. Department of the Treasury. Data span 1999-2015.

the variation in firm entry decisions. Instead, a major driver of firm entry appears 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 two-digit NAICS code is calculated for each county and year. The upstream and downstream measures are calculated by taking the share of workers providing inputs into each two-digit NAICS code divided by total employment in each period. This measure is normalized by the average across the United States. Measures of household financial health are provided by the FDIC experimental county-level house price index, however, the FDIC data exclude counties without enough mortgages to draw a consistent enough estimate of household financial wealth; thus, using only counties where the house price index (HPI) exists excludes many rural counties.

Summary statistics for each of these variables are provided in Table 1. The first column, Pre-GFC, is Pre-Great Financial Crisis, and it provides the mean across all counties and year from 1999 to 2007. The second column, Post-GFC, is Post-Great Financial Crisis, and it reports the mean across all counties and years from 2008 to 2015, keeping the pre-and post-windows symmetrical. The third column, Diff, reports the difference-in-means between the first and second columns. As expected, firm entry and employment expansion decreased, while firm exit and employment contractions increased. Unemployment rates went up, banks deleveraged and Troubled Asset Ratios decreased, and the return on assets increased. The average change in the HPI was negative over the Post-GFC time period. Columns four and five report the standard deviation of the pre- and post-financial crisis periods, while the sixth column reports the difference. Entry, exit, and employment expansion all feature less variation in the post-financial crisis era, while contraction variation increased.

Finally, a number of other policy drivers have been examined as possible determinants of firm entry, such as right to work laws (Holmes, 1998) and lower taxes (Rohlin et al., 2014). Specific research designs are often used to estimate these effects and remove endogeneity of pro-business practices, and as such, we exclude these variables due to fear of inducing larger biases in the 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 either explain a small share of the overall variation in firm entry or show that the treatment effects have economically small coefficients.

4. Validating Business Dynamics as Measures of Commercial Real Estate Demand

The main results in this paper use Business Dynamics Statistics (BDS) outcomes to study CPP effects on commercial real estate (CRE) demand fundamentals.[16] To provide empirical validation for this interpretation, we estimate separate individual panel regressions and jointly specify panel estimations between BDS and CRE indicators that include rents, price per square foot, and vacancy rates. CRE data have important limitations that prevent them from being used for the main results, including the lack of consistent county-level coverage, potential geographic misalignment for MSAs that we use in our treatment design, and missing data prior to the Great Financial Crisis. Still, we are able to investigate whether BDS variables react similarly to standard CRE market outcomes to determine whether they capture meaningful variation in CRE fundamentals rather than general business cycle fluctuations.

To do this, we relate the BDS outcomes to CRE measures across different property classes (office, retail, and multifamily) by tracking changes in rent growth, rental price per sq ft, vacancy rates, and transaction volumes. We use CRE data from CoStar, which aggregates information by cities or metropolitan statistical areas (MSAs), on a quarterly basis from the end of 2000 through 2023.[17] As a result, we estimate a series of fixed effects models to assess whether BDS measures match CRE market outcomes in the period where reliable data are available. We expect BDS measures to align with CRE outcomes that reflect real estate forces like tenant demand and space utilization. Specifically, business establishment entry and job creation should be positively associated with rent and price growth, but negatively associated with vacancy rates. Establishment exit and job destruction should lead to the opposite outcomes.

Table 2 shows that coefficient signs are consistent with economic expectations across property types. The table presents two sets of models with the left side (panel A) showing separate estimations and the right side (panel B) listing results from joint specifications. We regress each BDS variable on CRE measures and use a two-way fixed effects structure of MSA and year controls while clustering standard errors at the MSA-level.

The left side of the table shows stepwise panel estimations for each BDS outcome regressed separately on quarterly CRE measures using a two-way fixed effects structure (with MSA and year controls) and cluster standard errors (at the MSA-level). Establishment entry and job creation increase with rent and price growth, but generally decline as vacancies rise. These relations are consistent in sign and economically meaningful in magnitude. For instance, in the retail sector, a 10 percent increase in rent growth leads to a 1.24% increase in establishment entry rates and 3.72% decline in exits, which represents a modestly inelastic change in firm dynamics. The multifamily sector has a similar pattern but the estimated effects are about one-sixth the magnitude. A 10 percentage point increase in the vacancy rate is associated with a 0.6% lower creation rate and 1.3% destruction rate for retail properties and about half those estimated magnitudes in the office sector. These results reinforce that BDS measures reflect variations in market conditions that are related to CRE performance.

The right side of the table further confirms the results when multiple CRE measures are estimated jointly in a similar panel design, although collinearity across CRE measures reduces precision in some specifications.[18] This specification style helps us determine if BDS measures can simultaneously reflect multiple aspects of CRE market conditions. Across the columns, the joint specifications reflect that rent growth and price appreciation are positively associated with establishment entry and job creation but negatively associated with firm exit

Table 2. Business dynamics and commercial real estate market conditions.

	Panel A: Individual estimations				Panel B: Joint estimations			
	Establishment		Employment		Establishment		Employment	
	Entry	Exit	Creation	Destruction	Entry	Exit	Creation	Destruction
	Office				Office			
P.C. Rent Growth	0.023**	-0.116***	0.073***	-0.184***	0.000	-0.064***	0.017	-0.123***
	(0.010)	(0.012)	(0.021)	(0.025)	(0.011)	(0.013)	(0.023)	(0.027)
P.C. Price per Foot^2	0.015***	-0.062***	0.042***	-0.082***	0.015***	-0.052***	0.037***	-0.061***
	(0.004)	(0.004)	(0.007)	(0.009)	(0.004)	(0.004)	(0.008)	(0.009)
P.P.C. Vacancy Rate	-0.001	0.038***	-0.032***	0.098***	0.002	0.032***	-0.024*	0.076***
	(0.006)	(0.007)	(0.011)	(0.014)	(0.006)	(0.007)	(0.012)	(0.014)
	Retail				Retail			
P.C. Rent Growth	0.124***	-0.372***	0.258***	-0.466***	0.068*	-0.417***	0.223***	-0.549***
	(0.023)	(0.027)	(0.047)	(0.054)	(0.028)	(0.034)	(0.060)	(0.069)
P.C. Price per Foot^2	0.011***	-0.020***	0.009**	-0.021***	0.009***	-0.013***	0.006	-0.013*
	(0.002)	(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.005)	(0.005)
P.P.C. Vacancy Rate	-0.031***	0.110***	-0.060***	0.134***	-0.021*	0.089***	-0.046*	0.107***
	(0.009)	(0.011)	(0.019)	(0.022)	(0.009)	(0.011)	(0.019)	(0.022)
	Multifamily				Multifamily			
P.C. Rent Growth	0.024***	-0.067***	0.060***	-0.134***	0.044*	-0.084***	0.007	-0.138***
	(0.004)	(0.005)	(0.009)	(0.010)	(0.017)	(0.019)	(0.036)	(0.040)
P.C. Price per Foot^2	0.021***	-0.061***	0.056***	-0.126***	-0.018	0.014	0.052	-0.002
	(0.004)	(0.005)	(0.009)	(0.010)	(0.016)	(0.018)	(0.034)	(0.038)
P.P.C. Occupancy Rate	-0.016***	0.010	-0.012	0.003	-0.020***	0.022***	-0.022$^+$	0.028*
	(0.006)	(0.006)	(0.012)	(0.014)	(0.006)	(0.006)	(0.012)	(0.014)

Notes: This table presents stepwise regressions of each Business Dynamics Statistics (BDS) outcome on Commercial Real Estate (CRE) property class measures. The left side of the table shows individual panel estimations that regress BDS outcomes on each separate CRE measure using a two-way fixed effects specification with controls for metro area and year. The right side of the table lists results from joint estimations that include all CRE measures together. The column headings capture business establishment dynamics with entry and exit rates as well as employment dynamics with job creation and destruction rates. Coefficients are reported down columns for regressions of BDS outcomes on CRE measures. The coefficients listed below in parentheses are the standard errors, which are clustered at the metro level. A value of zero reflects a magnitude less than 5×10^{-4}. Abbreviations are "P.C." for percentage change and "P.P.C." for percentage point change. Statistical significance is denoted as *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, and + $p < 0.10$. Source: CoStar quarterly data from 2008Q4–2023Q4.

and job destruction. Vacancy rates continue to reflect the opposite patterns. Although the coefficients are attenuated when compared to the individual estimations on the left side of the table, the direction and relative magnitudes are reassuring because they are stable as we move down the row groups across real estate sectors. This panel demonstrates that BDS measures encompass multiple dimensions of CRE market conditions simultaneously.

Overall, the consistency of the independent and jointly estimated results across property sectors reinforces the interpretation that BDS outcomes capture tenant-side demand channels through which credit conditions affect CRE markets. This validation motivates the use of establishment dynamics as the primary outcomes in the main empirical analysis. These measures can be viewed as proxies for commercial real estate demand conditions by capturing changes in space demand and occupancy rather than broad business cycle fluctuations. We now turn to estimating how CPP-induced changes in bank capital affect these outcomes.

5. Empirical Design

We estimate the direct and indirect Average Treatment on the Treated (ATT) effects of a county having a bank that receives CPP funds on future business dynamics. An important concern is heterogeneity in how communities are affected by the 2008 financial crisis and how local bank financial characteristics create credit pass-throughs to businesses and entrepreneurs. This is particularly relevant given potentially non-random CPP assignment because that introduces differential pre-trends across treated and untreated counties. These concerns create ambiguity in defining the appropriate counterfactuals and motivate the use of synthetic control methods.

A source of potentially confounding effects is credit market spillovers. Borrowers may access credit beyond their home county when starting, expanding, or sustaining a business. As a result, counties are not independent of each other and may be affected by both local credit conditions and their access to nearby credit markets. We follow Huber & Steinmayr (2021) to utilize a potential outcome framework with *own* and *neighbor* treated status. A core assumption is that variation in which specific neighbor is treated does not impact outcomes beyond overall neighbor treatment effects, and that there are no complementarities between *own* and *neighbor* treatment status. These assumptions help simplify the spatial structure and imply that estimated neighbor effects should be interpreted as reduced-form evidence of credit transmission rather than structural spillovers.

More formally, there are T time periods. From periods $0, \ldots, T_0 < T - 2$, all counties are untreated. In periods $T_1 = T_0 + 1$, each county can receive CPP treatment. In periods

$T_2, \ldots, T$, no additional treatment is assigned. Under this framework, we have two treatments, *own* treatment $Own_{i,t} \in \{0,1\}$ and *neighbor* treatment $Neigh_{it} \in \{0,1\}$. Therefore, individual 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:

$$y_{it}(0,0) = x_{it}\beta + \lambda_t' \mu_i + \epsilon_{it} \tag{3}$$

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

$$y_{it}(1,0) = y_{it}(0,0) + \alpha_{it} \tag{4}$$
$$y_{it}(0,1) = y_{it}(0,0) + \gamma_{it} \tag{5}$$
$$y_{it}(1,1) = y_{it}(0,0) + \alpha_{it} + \gamma_{it} \tag{6}$$

Under this factor structure, λ_t is a $(1 \times F)$ vector of unobserved common factors, μ_i is an $(F \times 1)$ vector of unknown factor loadings, and the error terms ϵ_{it} are unobserved transitory shocks at the region level with zero mean. This structure is general and nests several common data generating processes.[19] Implicitly, we assume no complementarities or substitution effects between own and neighbor treatment status. This allows for the estimation of average treatment effects to happen through synthetic controls on sample splitting and be written:

$$\alpha_{it}^{(1,0),(0,0)} = y_{it}(1,0) - E(y_{it}(0,0) \mid (1,0)) \tag{7}$$
$$\alpha_{it}^{(1,1),(0,1)} = y_{it}(1,1) - E(y_{it}(0,1) \mid (1,1)) \tag{8}$$
$$\gamma it^{(0,1),(0,0)} = y_{it}(0,1) - E(y_{it}(0,0) \mid (1,0)) \tag{9}$$
$$\gamma it^{(1,1),(1,0)} = y_{it}(1,1) - E(y_{it}(1,0) \mid (1,1)) \tag{10}$$

The aim is to construct a synthetic county as a weighted linear combination of counties with different treatment status. Traditionally, this has been done through a convex hull assumption such as in Abadie & Gardeazabal (2003); Abadie et al. (2010, 2015); Ferman & Pinto (2016), where all weights are strictly positive and sum to one. This assumption was removed in Hsiao et al. (2012); Li & Bell (2017). The main difference between the two is that the panel data approach is an unconstrained regression, whereas the synthetic control method is a constrained regression. Similar approaches without constraints have started to implement LASSO and other regularization methods (Doudchenko & Imbens, 2016; Amjad et al., 2018; Carvalho et al., 2018; Chernozhukov et al., 2021). A comparison of these methods was conducted by Gardeazabal & Vega-Bayo (2017) and Wan et al. (2018). With only a single treatment, the synthetic control estimates a county specific Average effect of Treatment on the Treated (ATT). But with two different treatment effects, these estimates become a county specific total treatment effect, and parsing out average direct and spillover effects requires modifications.

LASSO-synthetic control estimation is carried out through minimizing penalized regression,

$$\begin{bmatrix} \hat{w}_i \\ \hat{\beta}_{i,0} \end{bmatrix} = \underset{B_{i,0},w_i}{\arg\min} \frac{1}{T_0} \sum_{t=1}^{T_0} \left(y_{it} - \beta_{i,0} - \sum_{j=1}^{N_0} w_{ij} y_{jt} \right)^2 + \phi \mid\mid w_i \mid\mid_2 \tag{11}$$

The first part of this equation is traditional ordinary least squares (OLS) as carried out in Hsiao et al. (2012), where we match a set of donor counties to a specific treated county 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, $\phi \mid\mid w_i \mid\mid_2$ penalizes the inclusion of additional donor counties. The coefficient $\phi > 0$ tunes the severity of the penalty for including additional donor counties and is determined by cross validation as well as $\mid\mid w_i \mid\mid_2 = \sum_j w_{ij}^2$. This structure is close to Wan et al. (2018), Doudchenko & Imbens (2016), and Li & Bell (2017). 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 7 can be reformulated as:

$$\begin{aligned} \alpha_{it}^{A,B} &= (y_{it} - w_i Y_{jt}) \\ &= \left(\alpha_{it} + (\gamma_{it} - \sum_{j \in B} w_{ij}\gamma_{jt}) + \lambda_t(\mu_i - \sum_{j \in B} w_{ij}\mu_j) + (\epsilon_{it} - \sum_{j \in B} \epsilon_{jt}) \right) \end{aligned} \tag{12}$$

This estimator becomes unbiased under the following joint assumption:

$$\begin{aligned} E[\epsilon_{it} \mid Own_i \ Neigh_i] = E[\epsilon_{it}] = 0 \\ \exists w^* \in \mathbb{R}^{N_B} \mid (\mu_i - \sum_{j \in B} w_{ij}\mu_j)) = 0, E[\gamma_{it} - \sum_{j \in B} w_{ij}\gamma_{jt}] = 0 \end{aligned} \tag{13}$$

The first condition allows treatment to correlate with the factor loading term, $\lambda_t' \mu_i$, but it must be uncorrelated with idiosyncratic shocks to a given county. The second requires that the 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 the shared treatment effects γ_{it} all share common support across the target and donor pools.

A concern is that the term $(\gamma_{it} - \sum_{j \in B} w_{ij}\gamma_{jt})$ varies over time. The equation 13 assumption is that the treatments are random effects in each period, such that $y_{it} = y_t + \upsilon_{it}$ and $upsilon_{it}$ are white noise.[20] In turn, we primarily focus on estimates of the mean effect,

$$\alpha_t^{A,B} = \frac{1}{N_A} \sum_{i \in A} (y_{it} - \hat{y}_{it}) \tag{14}$$

However, variation in the treatment assignment can further be leveraged in the estimation of effects. As previously discussed, there was an initial wave of payouts at the end of 2008, then a slowdown, followed by a second wave of disbursed 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* 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, individual treatment status can be characterized as above. In period T_2, the nested outcomes generate 16 potential outcomes. We index counties by their second period potential outcomes of $(Own_{i,T_1}, Neigh_{i,T_1}, Own_{i,T_2}, Neigh_{i,T_2})$ and assume the simple structural model for untreated counties such that $y_{it}(0, 0, 0, 0) = x_{it}\beta + \lambda_t' \mu_i + \epsilon_{it}$.

In period T_1, this generates the four possible outcomes in Equations 4. 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 unit-specific parameters, but estimation of a single marginal effect (for example, impact of the first period treatment), now generates a large vector of nuisance parameters. First, under this framework, we can recharacterize the estimated treatment effect without loss of generality as:

$$\begin{aligned}
&(\alpha_{it}^{T_1} + (\gamma_{it}^{T_1} - \sum_{j \in B} w_{ij} \gamma_{jt}^{T_1}) + (\alpha_{it}^{T_2} I\{Own_{jT_2} = 1\} + \gamma_{it}^{T_2} I\{Neigh_{jT_2} = 1\}) \\
&\quad - \sum_{j \in B} w_{ij} (\alpha_{jt}^{T_2} I\{Own_{jT_2} = 1\} + \gamma_{jt}^{T_2} I\{Neigh_{jT_2} = 1\}) \\
&\quad + \lambda_t (\mu_i - \sum_{j \in B} w_{ij} \mu_j) + (\epsilon_{it} - \sum_{j \in B} \epsilon_{jt}))
\end{aligned} \tag{15}$$

The leading term $\alpha_{it}^{T_1}$ is the primary effect of interest. The second term is the difference between the spillover effect in the first time period, while the third term represents omitted second treatment effects in 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.

The case of $Own_t = 1, Neigh_t = 1$ is difficult to identify with the synthetic control method as the difference in secondary treatment effects (not of interest) creates a moving nuisance parameter. For example, consider the following set of potential outcomes in the two-period, two-treatments framework.

$$y_{it} = \begin{cases} y_{iT_2}(0,0,0,0) + \alpha_{it}^{T_1} + \alpha_{it}^{T_2} & \text{if } Own_{T_1} = 1,\ Neigh_{T_1} = 0,\ Own_{T_2} = 1,\ Neigh_{T_2} = 0 \\ y_{iT_2}(0,0,0,0) + \gamma_{it}^{T_1} + \gamma_{it}^{T_2} & \text{if } Own_{T_1} = 0,\ Neigh_{T_1} = 1,\ Own_{T_2} = 0,\ Neigh_{T_2} = 1 \\ y_{iT_2}(0,0,0,0) + \alpha_{it} + \gamma_{it} & \text{if } Own_{T_1} = 1,\ Neigh_{T_1} = 0,\ Own_{T_2} = 0,\ Neigh_{T_2} = 0 \end{cases} \tag{16}$$

Without additional assumptions, it is impossible to jointly identify $(\alpha_{iT_2}^{T_1}, \alpha_{iT_2}^{T_2})$, $(\gamma_{iT_2}^{T_1}, \gamma_{iT_2}^{T_2})$, and $\{(\alpha_{iT_2}^{j}, \gamma_{iT_2}^{j})\}_{j\in\{T_1,T_2\}}$. As above, we remedy this issue by conditioning on a given positive treatment regime, and targeting the specific average effect of interest. For example, if we are 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 equation 13. This means all treatment effects, *own* treatment 2008, *neighbor* treatment 2008, *own* treatment 2009, *neighbor* treatment 2009, share common support across all treated counties.

The advantage of this approach is that it reduces each estimation to a canonical causal effects structure, with the downside being the loss of data within each equation. For each observation in the treated branch, we construct a synthetic control county using the donor pool, and the fit across the donor pools greatly differs. 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 & Dowd (2019) 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 the 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, re-sample without replacement a new treated group of size N_A, and estimate the mean LASSO-synthetic control estimator. This procedure is repeated 1,000 times to approximate the null distribution under the sharp null of no treatment effect. Therefore, a treatment effect is inferred when the observed point estimates fall outside the 95% permutation confidence interval.

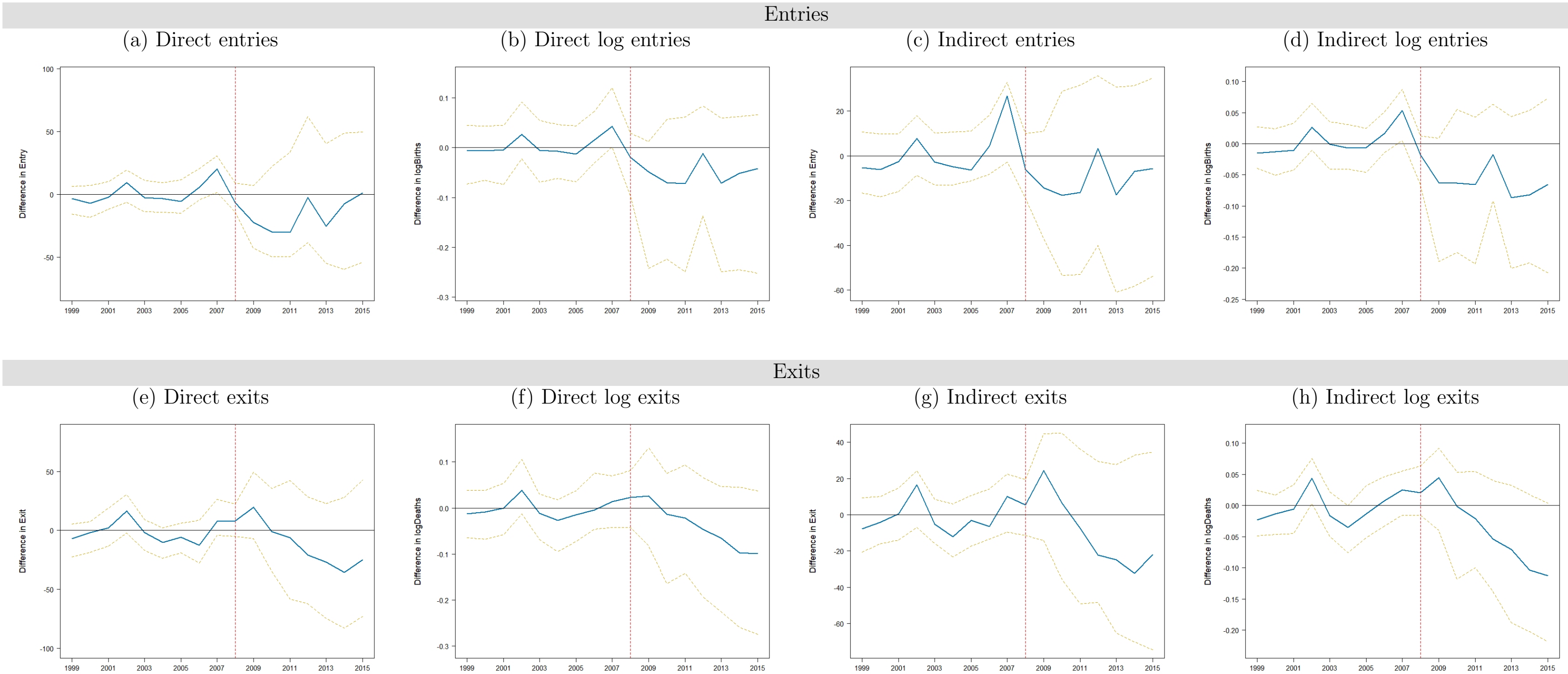


Figure 3. Different effects of establishment entries and exits.

Notes: This figure presents LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Establishment entries are on the top row and exits on the bottom row. Within each row are direct and indirect results. The solid blue lines are the estimate for the empirically observed set of treated counties, and the dashed gold lines represent the 95% permutation test confidence intervals under the null hypothesis of no treatment. Source: Author calculations.

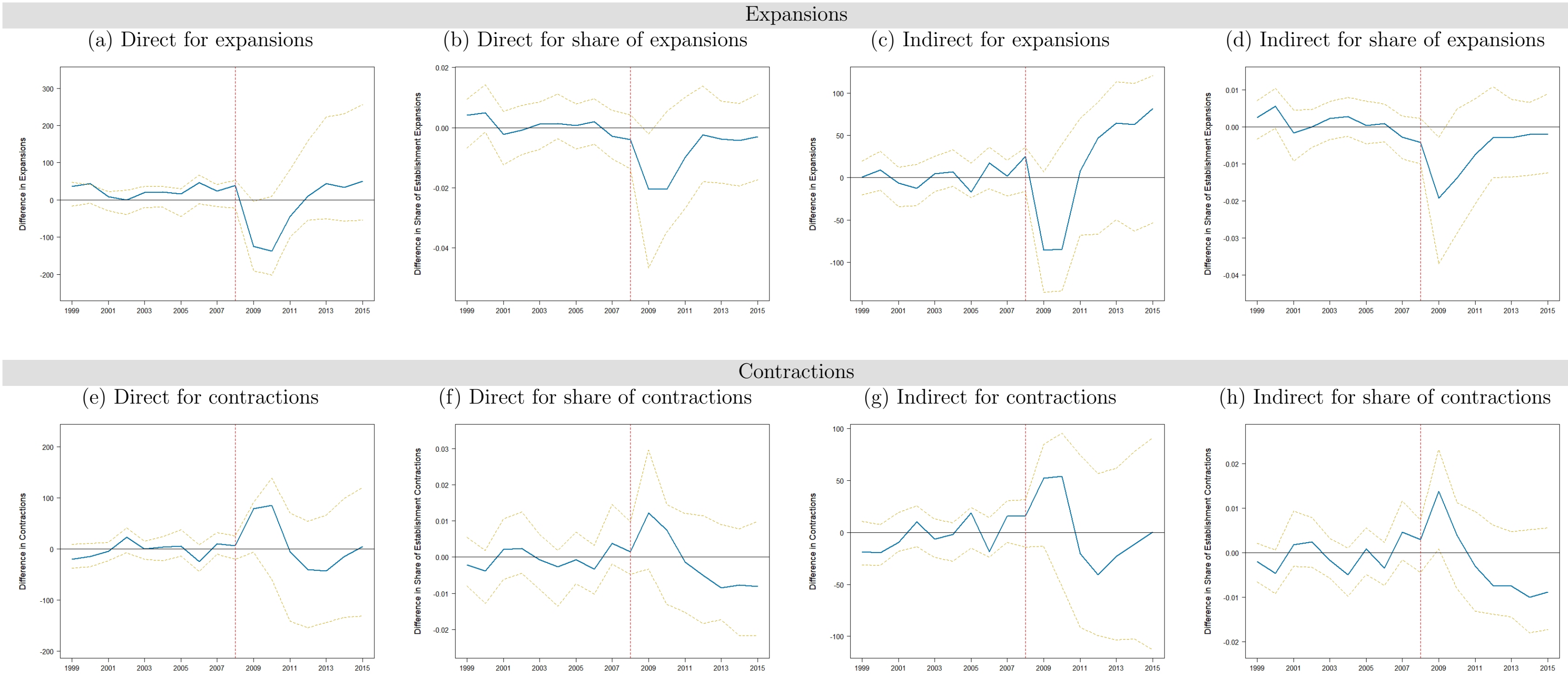


Figure 4. Different effects of employment expansions and contractions.

Notes: This figure presents LASSO-synthetic control estimates for the pooled effect of receiving treatment in either 2008 or 2009 in both levels and logs. Employment expansions are on the top row and contractions on the bottom row. Within each row are direct and indirect results. The solid blue line is the estimate for the empirically observed set of treated counties and the dashed gold lines represent the 95% permutation test confidence intervals under the null hypothesis of no treatment. Source: Author calculations.

6. Results

The results are similar both for levels and rates, and are interpreted relative to a synthetic counterfactual instead of raw outcomes changes, as seen in Figure 3 for establishment (entries and exits) and Figure 4 for employment (expansions and contractions) for direct and indirect effects. In both cases, the synthetic control estimator described in Section 5 fails to reject the null hypothesis of having no treatment effect and indicates no sustained divergence relative to the synthetic counterfactual. The pre-treatment fit is well within the 95% permutation confidence interval, and does not cross the confidence interval in the post-treatment period. Inference is based on permutation tests using never-treated counties so statistical significance is evaluated via the counterfactual distribution rather than point estimates alone.

Point estimates show modest short-run establishment declines in Figure 3. However, these results may reflect crisis-era conditions and selection into CPP participation rather than the causal effect of the program. In levels and rates (panels a and b), the direct effect on establishment entry is approximately 50 fewer firms per year immediately after treatment or about a 1% reduction in entry rates, with both measures reverting back toward zero over time. The indirect effects (panels c and d) are much smaller. There are roughly 20 fewer firms that enter or about a 0.1% reduction in entry rates. These results convey that treated counties have moderately higher firm entry rates, as discussed in Section 3. Establishment exits show a gradual decline in both direct levels and rates (panels e and f), with around 50 fewer exits per year or about a -0.1% lower exit rate. The estimated spillover effect is close in magnitude to the direct effect. Notably, the 95% permutation confidence intervals widen over the post-treatment time period, even within the untreated pool, which suggests broader post-crisis dynamics.

Similarly, employment expansion shows a limited immediate response in Figure 4, with point estimates falling immediately but returning close to zero in the years following treatment. In the longer run, point estimates are slightly positive for direct effects (panel a) and rise to roughly 100 additional establishment expansions per year for indirect effects (panel c), however, the corresponding rate-based effects remain negative and close to zero (panels b and d). In the bottom row, the contraction measures show inverse effects. The direct effects (panels e and f) have an immediate increase following the 2008 financial crisis but, a few years later, exhibit limited deviation with almost zero permutation distribution around zero in levels with direct and indirect effects (panels e and g). There are, though, modest declines in contraction rates over time (panels f and h). An important short-term implication from these figures is a sign of clear bridge loan pass-through in establishment employment

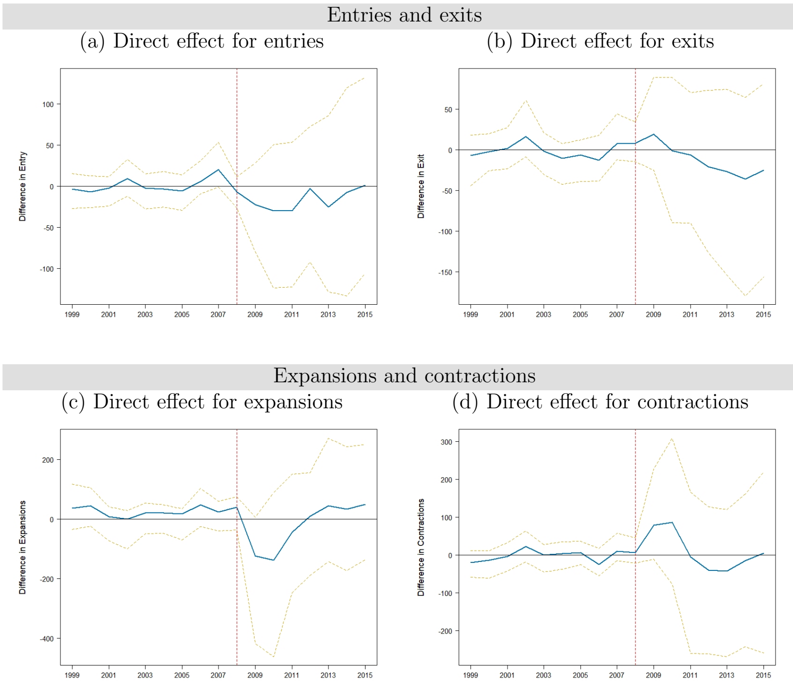


Figure 5. Heterogeneous impacts across outcome measures.

Notes: This figure plots heterogeneous impacts for the direct effects for entries, exits, expansions, and contractions. The solid blue line is the mean effect among the empirically observed treatment group and the dashed gold lines represent the 95% quantiles for each effect. Source: Author calculations.

expansion and contraction. During the first several years after treatment, the short-term estimated effects are almost uniformly negative (positive) in the case of employment expansion (contraction), which implies that few firms are able to forgo impacts of cratering consumer demand on their own employment status. In the longer term, when expansions return to zero after a few years, the employment contraction rates indicate moderate offsetting decreases. The findings suggest limited effects on employment adjustment margins.

Across both figures, the results do not show a statistically significant effect of CPP exposure on local establishment or employment dynamics, either directly or indirectly through neighboring counties. The confidence intervals never allow the point estimates to be unambiguously positive or negative. However, aggregated graphics may mask important heterogeneity. Each mean effect pools the average across 12 different treatment cohorts, which introduces variation in treatment timing because most counties did not receive CPP funds until 2009. As a result, we plot both the mean response along with 95% quantiles for each effect in Figure 5.[21] While even these graphics may have shortcomings (possible estimation error from residual components of their outcomes), the results comport: both the direct and indirect effects are centered around zero.

Overall, the empirical findings indicate limited or imprecise effects of CPP exposure on local establishment and employment dynamics. Point estimates suggest modest declines in entry and expansion but increases in exit and contraction immediately following treatment. As a reminder, these effects are not always statistically distinguishable from zero and they dissipate over time. At the same time, there is substantial heterogeneity across counties. Some counties experience increases in firm entry and reductions in exit while others exhibit the opposite pattern. Direct effects on expansion are generally weak in the short-run, with some recovery in later periods, while spillover effects remain muted except for a large tail of counties that experience excess exit for years following treatment. Contraction responses are centered near zero but display dispersion across counties, and similarly have a large upper tail of excess contractions. For timing, both direct and spillover effects are generally negative immediately after treatment, followed by strong positive expansion starting in 2011 for the directly treated counties, and generally no effect for spillover counties. The empirical effects suggest that improvements in bank capitalization did not translate into consistent or broad-based changes in local economic or commercial real estate demand conditions to any substantial degree when evaluated relative to counterfactual trends.

7. Robustness Checks

This section provides robustness checks for the primary results. We present Difference-in-Differences and Instrumental Variables Difference-in-Differences results to address pre-trend violations and provide further evidence for the need of data-driven counterfactual construction. We find violations of the pre-trend assumption for *own* treatment status across model specifications while *neighbor* pre-trends tend to hold. However, there is no consistent evidence of spillover effects. Continued concerns over differing pre-trends among treated and untreated counties lead to estimation using interactive fixed effects in difference-in-differences (DID) models (Gobillon & Magnac (2016); Xu (2017)). These models explicitly estimate a factor loading approach such as in Equation 14 to construct counterfactuals, which implies a stronger structure than mainline synthetic control estimates require. Finally, as noted in Section 3, the TARP Transaction Report is tied to branch locations that receive funds. We construct a full network of counties with a treated bank's branch locations and re-estimate interactive fixed effects using DID models.

7.1. Difference-in-Differences

The potential outcomes framework discussed in Section 5 enables canonical difference-in-differences estimation with both *own* and *neighbor* treatment statuses. Recent works have helped decompose multiple time periods and spillover effects in DID models, including Imai & Kim (2021), and extensions for staggered treatment timing across multiple time periods (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021).

We proceed by first estimating models with a single treatment period, and then two treatment periods with a full set of heterogeneous treatment effects.[22] For each model, joint tests of pre-trends are conducted using clustered standard errors at the state level. We also implement a step-down method to measure any policy duration. This provides a conservative test for exactly how long there was a policy effect from the CPP on local establishment dynamics.

Recent work in DID and event study methods have increasingly utilized policies that exhibit variation in treatment timing (Sun & Abraham, 2021). Under these conditions, it is common to generate pre- and post-treatment effects from the time of initial treatment. However, two concerns arise. For counties treated in the second period, the period prior to treatment is now subject to the Great Financial Crisis, something those treated in the first period are not. Therefore, tests for differing pre-trends 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:

$$\begin{aligned} y_{it} &= \beta_1 Own_i + \beta_2 Neigh_i + \beta_3 I\{t > T_0\} \\ &\quad + \sum_{s=-9}^{7} \gamma_s Own_i I\{s = t - \min_k\{Own_{i,k+1} - Own_{i,k} = 1\}\} \\ &\quad + \sum_{s=-9}^{7} \alpha_s Neigh_i\{s = t - \min_k\{Neigh_{i,k+1} - Neigh_{i,k} = 1\}\} \\ &\quad + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it} \end{aligned} \tag{17}$$

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 pre-trends of interest. The first is that all pre-trends differ from zero, the second that only *own* pre-trends differ from zero, and the third that only *neighbor* pre-

Table 3. Wald tests for Model 1 and NAICS code.

Pretrend	Significant
Entry all treated	No shared pre-trend
Entry own treated	No shared pre-trend
Entry neigh. treated	No shared pre-trend
Exits all treated	No shared pre-trend
Exits own treated	No shared pre-trend
Exits neigh. treated	No shared pre-trend
Expansions all treated	No shared pre-trend
Expansions own treated	No shared pre-trend
Expansions neigh treated	No shared pre-trend
Contractions all treated	No shared pre-trend
Contractions own treated	No shared pre-trend
Contractions neigh. treated	No shared pre-trend

Notes: No shared pre-trend implies a p-value less than 0.005. Source: Author calculations using FFIEC, RSSD, SUSB, and TARP data from 1999–2015.

trends differ from zero.[23] Recent research has found that by doing this, standard errors of post-treatment coefficients are often conservative (Roth, 2018; Kahn-Lang & Lang, 2020), but generally, this paper favors a more conservative approach to estimating effects and does not carry out further corrections.

$$\begin{aligned} H_0^{ALL} &= \gamma_{-9} \ \ldots \ \gamma_{-1} \ \alpha_{-9} \ \ldots \alpha_{-1} = 0 \\ H_0^{OWN} &= \alpha_{-9} \ \ldots \alpha_{-1} = 0 \\ H_0^{NEIGH} &= \gamma_{-9} \ \ldots \ \gamma_{-1} = 0 \end{aligned} \tag{18}$$

The resulting joint hypothesis tests on pre-trend are presented in Table 3 (see the Appendix for event study style graphs in Figure A3). Among the *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 undermines the validity of the (mean) non-treated counties as a counterfactual. As a result, treatment effect estimates from these specifications may be misleading. In contrast, *neighbor* treatment effects more closely follow a shared pre-trend (with the caveat that joint tests still reject it) but the resulting coefficients are close to zero.

As discussed in Section 5, there might have been meaningful choices at the time when federal regulators and the Treasury decided to disburse funds to different banks or regions.

As a result, the pooled estimator presented in Equation 17 does not capture the full heterogeneity in responses. Thus, we estimate a fully differentiated model with group-specific pre-trends and post-treatment effects. This allows for heterogeneous responses within each *own*-treatment and *neighbor*-treatment couplet, and the resulting estimated equation is:

$$
\begin{aligned}
y_{it} = {} & \beta_1 Own_i + \beta_2 Neigh_i + \beta_3 I\{t > T_0\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{10} I\{s = t - \min_k \{W_{k+1}^{10} - W_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{01} I\{s = t - \min_k \{W_{k+1}^{01} - W_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} Own_i^{11} I\{s = t - \min_k \{W_{k+1}^{11} - W_k^{11} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{10} I\{s = t - \min_k \{G_{k+1}^{10} - G_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{01} I\{s = t - \min_k \{G_{k+1}^{01} - G_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} Neigh_i^{11} I\{s = t - \min_k \{G_{k+1}^{11} - G_k^{11} = 1\}\} \\
& + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it}
\end{aligned} \tag{19}
$$

Event study figures for results from Equation 19 are presented in Figures A4, A5, and A6 in the Appendix. As above, joint tests on pre-trends are carried out, where now this extends to all pre-trends for each treatment subgroup. While visually the estimates are more centered around zero, most models still reject the hypothesis that there are no differing pre-trends among the different treatment groups. Allowing for additional heterogeneity demonstrates that estimates for neighbor spillover effects tend to satisfy the shared pre-trend assumption. This suggests that DID estimates for spillover effects are less affected by pre-trend violations and that post-treatment estimates are supported by a valid counterfactual.

As the financial conditions normalize, differences between treated and untreated counties may diminish. Thus, we develop explicit tests for policy effectiveness duration by employing step-down multiple hypothesis tests outlined in Section 7.1, based on a test for nested hypotheses proposed by Bauer & Hackl (1987). This test controls for family-wise error in trying to evaluate multiple p-values simultaneously. To motivate this problem, imagine the

Table 4. Step-down tests for non-zero ATT following different treatments.

StepDownNames	OwnDiffSig	NeighDiffSig
	10 Treatment	
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
	01 Treatment	
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
	11 Treatment	
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

Notes: This table presents statistical results for a step-down method that tests whether or not there is an active policy duration for several outcomes in an iterative fashion over a combination of treatment statuses (own and neighbor). The variations of 10, 01, and 11 denote the treatment couplets. Abbreviations are as follows: "StepDownNames" is a particular outcome, "OwnDiffSig" is own treated status, "NeighDiffSig" is neighborhood treated status, and "ATT" is the average treatment on the treated effect. Source: Author calculations using FFIEC, RSSD, SUSB, and TARP data from 1999–2015.

set of hypotheses:

$$H_0^k:\ \gamma_s = 0 \quad \forall s \in [1, \dots, k] \tag{20}$$

Then a level α-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,

$$P(rejectH_0^k) \leq \sum_{i=1}^{k} P(p_j \leq \alpha/(2(k-i+1))) \leq \alpha \tag{21}$$

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 provides a conservative bound on positive policy duration by iteratively testing nested joint hypotheses.[24] Results for step-down tests of Equation 19 are presented in Table 4. The gray rows distinguish treatments while outcomes are reported across columns for own and neighbor treated status.

Consistent with the synthetic control exercises, these results do not indicate sustained policy effects or spillovers. There is some evidence supporting short-lived effects for *own* treatment but, without accepting the shared pre-trend, the interpretation of the DID estimator remains unclear.

7.2. Instrumental Variables Estimation

A concern about identification is that the treatment is correlated with still unobserved shocks, even after conditioning on the interactive fixed effects. As noted in Li & Bell (2017), if federal regulators and the Treasury chose areas for CPP funds with high latent demand for loans, these estimates would overstate CPP effectiveness, whereas comparably, if they selected areas with low latent demand for loans, this might understate CPP effects. To investigate, we instrument *own* and *neighbor* treatments using county-level political connections, whether or not any bank in a given county has a board member serving as a branch Federal Reserve chair, whether or not the county's local House representative is serving on the banking and finance committee, the share of donations to the local representative coming from Financial, Investment, and Real Estate groups, and if the local House representative is a Democrat.

Following Xu (2021), we estimate a bivariate probit for each year instrumenting for 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, the relative probabilities of *own*, *neighbor*, and both treatment status in both 2008 and 2009 from the two Probit models.

The Sanderson & Windmeijer (2016) augmented F-test for multiple endogenous variables is carried out, where our instruments are strong using the Stock & Yogo (2002) 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 critical value is 11.12. We then instrument each of our treatment statuses as a function of each of our instruments.

$$Treat_{i,t} = \beta_0 + \beta_1 \hat{p}_{10}^{2008} + \beta_2 \hat{p}_{01}^{2008} + \beta_3 \hat{p}_{11}^{2008} + \beta_4 \hat{p}_{10}^{2009} + \beta_5 \hat{p}_{01}^{2009} + \beta_6 \hat{p}_{11}^{2009} + X_{it}' \Gamma + \epsilon_{it} \quad (22)$$

$Treat_{i,t}$ includes $Own_{i,t}, Neigh_{i,t}$, and the timing-variants discussed for estimating Equation 19. Moreover, X_{it} includes county mean bank financial health and local unemployment characteristics. Using these instrumented measures, we re-estimate Equations 17 and 19. As above, these models continue to reject the assumption of shared pre-trends in Table 5.

Table 5. Wald tests for IV pre-trend.

Pre-trend	Significant
Entry own treated 2008	No shared pre-trend
Entry neigh. treated 2008	No shared pre-trend
Entry own treated 2009	No shared pre-trend
Entry neigh. treated 2009	No shared pre-trend
Entry all	No shared pre-trend
Exit own treated 2008	No shared pre-trend
Exit neigh. treated 2008	No shared pre-trend
Exit own treated 2009	No shared pre-trend
Exit neigh. treated 2009	No shared pre-trend
Exit all	No shared pre-trend
Expansions own treated 2008	No shared pre-trend
Expansions neigh. treated 2008	No shared pre-trend
Expansions own treated 2009	No shared pre-trend
Expansions neigh. treated 2009	No shared pre-trend
Expansions all	No shared pre-trend
Contractions own treated 2008	No shared pre-trend
Contractions neigh. treated 2008	No shared pre-trend
Contractions own treated 2009	No shared pre-trend
Contractions neigh treated 2009	No shared pre-trend
Contractions all	No shared pre-trend

Notes: No shared pre-trend implies a p-value less than 0.005. Source: Author calculations using FFIEC, RSSD, SUSB, and TARP data from 1999–2015.

This likely reflects selection into political connections, which are correlated with larger areas (more likely to serve at the local Fed chair) and economically distinct regions (more likely to feature different trends in the buildup to the Great Financial Crisis). Thus, even if the IV solves the issue of possible strategically targeted funding by the Treasury where CPP funds may be provided to areas with disproportionately high or low latent credit demand, the IV may exacerbate pre-trend differences across counties. As a result, we do not report IV-based treatment effect estimates.

7.3. Interactive Fixed Effects in Differences-in-Differences

Instead of relying on a specific form of additively separable individual and time-specific fixed effects, the simple structural model presented in Equation 14 is built on interactive fixed effects, where r unknown time loading factors λ_t are interacted by county specific effects μ_i

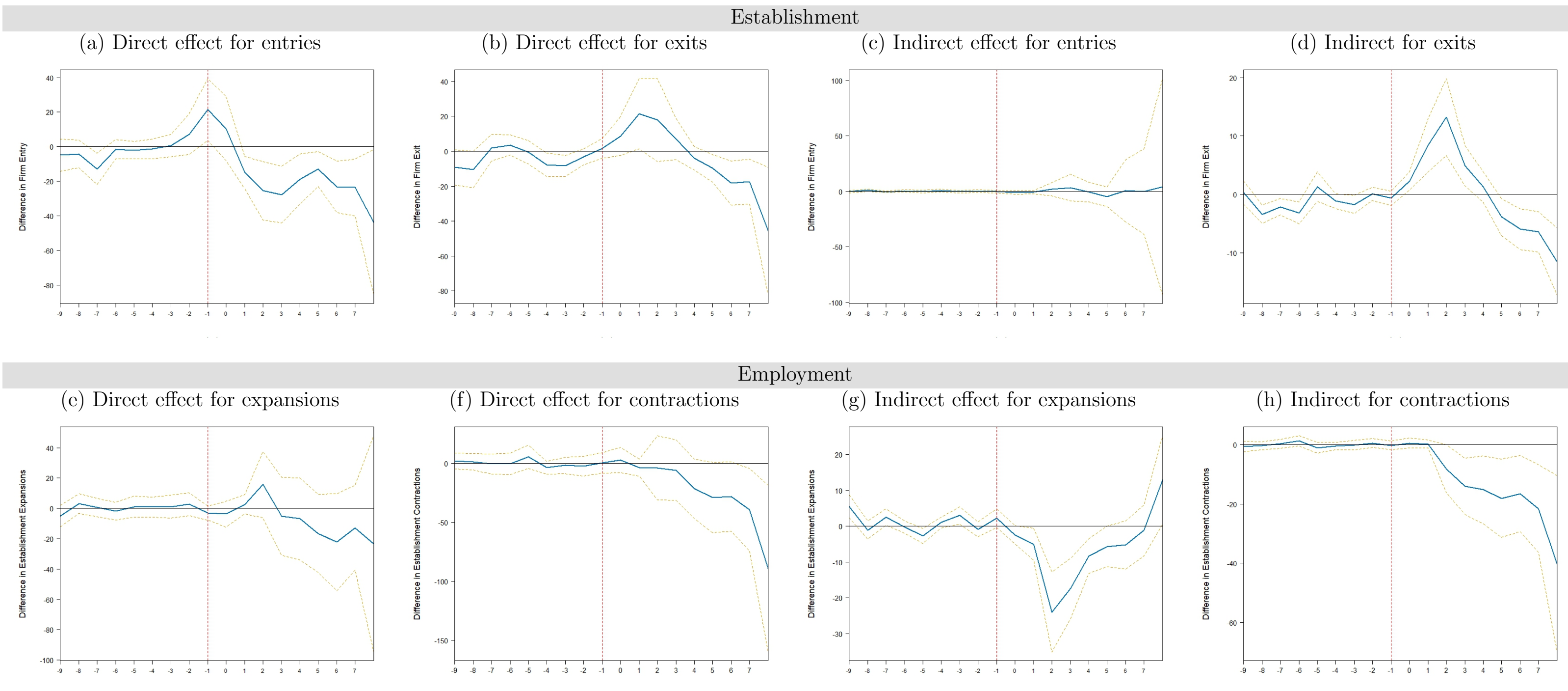


Figure 6. Interactive fixed effects difference-in-differences of establishment and employment.

Notes: This figure shows the treatment effect for time-from-treated for establishment and employment measures. Estimations use an Interactive Fixed Effects Difference-in-Differences model based on pooled treatment effect across counties that received treatment in 2008 or 2009. Within each row, the first two figures are direct effects (panels a, b, e, and f) and the last two figures are indirect effects (panels c, d, g, and h). The solid blue lines are the estimate for the empirically observed set of treated counties and the dashed gold lines represent the 95% permutation test confidence intervals under the null hypothesis of no treatment. Source: Author calculations.

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 5, while enabling a broader set of time-varying covariates to control for unobserved heterogeneity. We follow the estimation processes outlined in Gobillon & Magnac (2016), Xu (2017), and results are presented in Figure 6. As before, results are often statistically indistinguishable from zero for establishment entry and exit in the short-run but the sign is less neutral in the medium- and long-run. The interactive fixed effect specifications show direct effects of long-run declines in establishment entries but there are fewer exits and more limited employment contractions. The indirect effects suggest fewer establishment exits coupled with more employment expansions and fewer contractions. However, these effects seldom become distinct from zero until several years after the counties receive CPP funds. Therefore, it is difficult to attribute the results to CPP treatment versus supplemental responses or policy changes happening in the long-run.

7.4. Accounting for Downstream Bank Networks

We also consider whether CPP funds may have been transmitted through bank branch networks rather than headquarter locations. Results presented so far have relied on where the TARP Transaction Report has said the receiving bank is located. This is often tied to bank headquarters. Many of the banks that received TARP funds were either 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 is difficult to establish with empirical certainty. By including all branch locations of the 10 largest banks, there is no identification to be had, and all remaining counties in our sample, almost 2,500 of them, become treated. However, most of the largest 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 operations. As a result, we suppose these banks did not pass funds to downstream institutions in their network. Instead, this leaves about 1,500 counties that received treatment (see the Appendix for treatments mapped out in Figure A8).

Previous robustness checks have cast consistent doubt on the presence of spillover effects. 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 Figure 7, and broadly convey limited treatment effects in the short-run for entries,

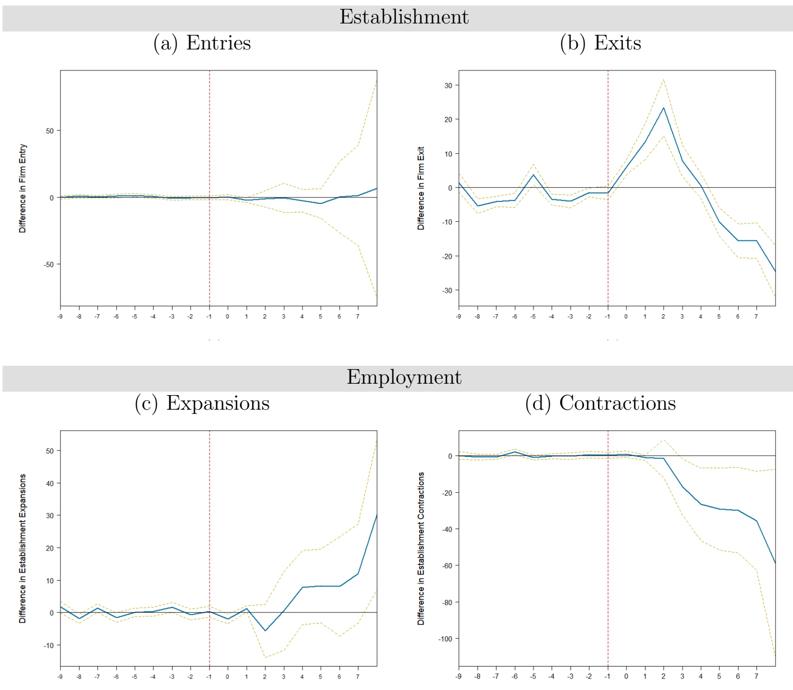


Figure 7. Interactive fixed effects difference-in-differences network ATT.

Notes: This figure shows the treatment effect for time-from-treated. Estimations use an 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. The solid blue lines are the estimate for the empirically observed set of treated counties and the dashed gold lines represent the 95% permutation test confidence intervals under the null hypothesis of no treatment. Source: Author calculations.

expansions, and contractions. Over time, establishment entries suggest a null effect that does not deviate from zero but there are mild long-run implications for gains in employment expansions and fewer contractions. Results differ for establishment exits where there is a noticeable and consistent increase several years after treatment; then a decline begins, which eventually turns into a negative long-run outcome. Only the establishment entry measure is consistent with earlier results that do not indicate downstream exposure effects, and a shorter post-treatment window of a couple of years could also capture both employment measures. In all three cases, estimated effects are close to zero around the treatment window or reflect negative policy outcomes. However, since notable trends do not manifest until well past treatment when the counties receive funds, they are unlikely to be attributable to CPP.

7.5. Bank Volume Lending Channel

In the prior sections, we have assessed whether counties exposed to CPP through local banks exhibit stronger business dynamics as proxies for CRE demand conditions. Across synthetic control, difference-in-differences, and interactive fixed effects specifications, the estimates have usually remained centered near zero even after accounting for spillovers from neighboring counties. Here, we consider whether these muted responses reflect a limited pass-through from bank recapitalization to lending activity.

To do so, we construct bank-level measures of CRE lending by matching the TARP transaction file to Call Report data at the bank holding company level from 2005 to 2011.[25] This period captures the disaggregated reporting of CRE lending and precedes regulatory changes associated with Basel III. Relative to prior work that focuses on persistent level shifts in lending, we estimate event study specifications that account for heterogeneous treatment timing and cohort-specific effects.

Figure 8 illustrates event study estimates of bank-level CRE lending around CPP receipt using three alternative estimators: two-way fixed effects, the Sun & Abraham (2021) interaction-weighted approach, and a dynamic specification that allows for heterogeneous treatment timing. Each row offers results for lending levels and as a share of gross tangible assets (GTA). As previously reported, even the fully heterogeneous estimator does not show signs of bias from pooling treatment cohorts, and both pre- and post-treatment periods are nearly identical. Both in levels and shares, there are violations of pre-trend assumptions in the two years prior to treatment where estimated coefficients deviate from zero. As a result, the treatment effects should not be interpreted as providing causal evidence.

In all six plots, near-to-treatment changes in CRE levels and share of GTA follow before-treatment trends in either increasing or decreasing directions. However, in levels, the trend results show a minimal near-to-treatment increase, a small medium-run effect (only consistently positive in the first two rows), and no long-term effect after two years (shown as eight quarters in the graphics). The confidence band is not usually significant beyond zero. In shares, the trends are consistently negative, statistically significant, and do not revert back to zero which suggests a long-term effect. The responses, then, should be viewed as being muted and that is also consistent with earlier evidence that increased bank capitalization did not translate into substantially expanded credit supply for CRE markets.

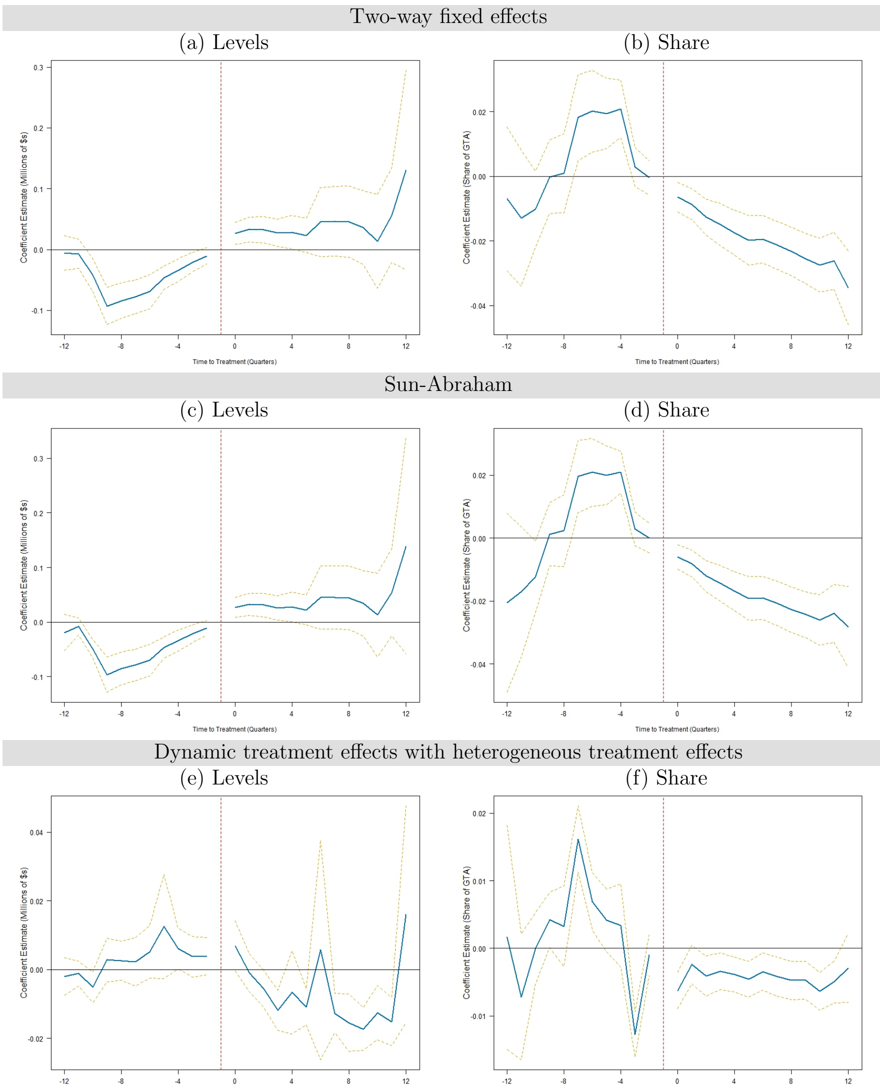


Figure 8. Event studies of the bank lending channel using alternative estimators.

Notes: Event study estimates of bank-level CRE lending around CPP receipt. Rows compare three different econometric methods: two-way fixed effects, Sun-Abraham interaction weighted estimator, and Sun-Abraham interaction weighted estimator with a lagged dependent variable (see Sun & Abraham, 2021). Left-side figures show levels and right-side figures display rates as a share of gross tangible assets. Coefficients are relative to the omitted pre-treatment period noted with the dashed vertical line. Standard errors are clustered at the bank holding company level. As before, we remove the twenty largest TARP recipient institutions. Source: Author calculations.

8. Conclusion

This study evaluates how the U.S. Treasury's Capital Purchase Program influenced commercial real estate markets through local credit supply and banking stability. By providing equity injections to financial institutions, the CPP was intended to strengthen lender balance sheets and promote the flow of credit to businesses and property markets. We investigate whether this stabilization translated into observable improvements in local establishment dynamics, which proxy for investment, space utilization, and employment linked to CRE performance. The absence of widespread gains in establishment dynamics is not a null finding in the usual sense but, instead, reflects the CPP achieved one of its principal objectives of stabilizing the financial system rather than expanding credit.

Across a range of empirical specifications, including LASSO-augmented synthetic control methods and interactive fixed effects difference-in-differences estimators, we find limited evidence that bank participation in the CPP generated measurable gains in local economic or property-related activity. Counties whose banks received CPP funds did not experience long-run statistically significant increases in firm formation, employment expansion, or reductions in business contraction. Nearby counties exposed to potential credit spillovers exhibited similarly muted responses. These results suggest liquidity infusions stabilized lenders but did not meaningfully expand credit availability within commercial real asset markets. Large institutions lacked incentives to deploy the capital they received, while smaller and regional lenders used the funds to restore balance sheets and absorb losses. These adjustments limited new credit creation but likely helped avert deeper contractions in lending and employment.

These results highlight an asymmetry in the transmission of financial stabilization policy to commercial real estate outcomes. While bank recapitalization improved solvency and reduced systemic risk, the pass-through to property investment and space demand was limited. This suggests policy interventions that target capital adequacy may not translate into greater credit availability for CRE projects or tenants, particularly when banks prioritize balance sheet repair over new lending. For investors and developers, this implies that policy measures aimed at financial solvency may do little to ease constraints on refinancing, construction lending, or tenant credit during downturns.

More broadly, the findings underscore spatial frictions and institutional constraints that influence whether financial policy shocks permeate local real estate markets. The CPP strengthened the banking sector but the funds offered negligible spillover benefits for the built environment. For commercial real estate markets, the lesson is that federal stabilization can steady lenders without reactivating debt flows to property transactions or development. Future analyses could extend this framework to other real estate measures of property investment, capitalization rates, or vacancy adjustments to test whether related stabilization programs, like during the recent pandemic, influence tangible market outcomes. Policy interventions seeking to support commercial real estate may require more explicit links between financial stabilization and property-level liquidity to ensure that capital infusions reach markets where investment, occupancy, and development occur.

Notes

1. Pence (2022) and Wang & Zhou (2023) examine a corresponding real estate market disruption after the pandemic.
2. A program overview along with documents on auctions, agreements, and results can be found at https://home.treasury.gov/data/troubled-assets-relief-program/bank-investment-programs/cap/overview.
3. Interpreting muted aggregate responses requires care. The CPP was structured to prevent insolvency and restore confidence rather than to expand credit supply immediately. Large institutions that received funds were already sufficiently capitalized and thus had little need to deploy them, while smaller and regional banks used the support primarily to strengthen balance sheets and avert failures. The limited pass-through to credit and establishment activity could be viewed as a stabilization success rather than a shortfall in policy transmission.
4. Detailed CRE data at the sub-metropolitan level are generally proprietary, expensive to access, and unavailable for public usage. We rely on county-level establishment dynamics and employment distributions as reasonable proxies for CRE presence and activity. This approach follows the broader literature in using business formation and labor measures to infer shifts in space demand and investment patterns when property-level data are unavailable.
5. The application period lasted between October 3, 2008, to November 14, 2008, for publicly held companies, December 8 for privately held companies, and February 13, 2009, for S corporations. On May 20, 2009, Timothy Geithner announced that banks with assets less than $500 million would have a second window to apply for CPP funds for another six months. https://www.treasury.gov/press-center/press-releases/Pages/tg139.aspx
6. 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.
7. Official documentation guaranteed banks that applied and got turned down for funds were not publicly announced. This makes extrapolation from the Degryse & Ongena (2005) results difficult.
8. Data span from 1999 through 2015. We intentionally select the number of years preceding the treatment to symmetrically equal the number of years subsequent to the treatment. The underlying files can be downloaded as https://www2.census.gov/programs-surveys/susb/.
9. 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.
10. 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. Compared to new entrants that did take out loans, they were more concentrated in restaurants, retail, business services, trucking, or durable storage.
11. TARP Transaction Report. Similarly, the 2008 and 2009 FFIEC Call Reports can be found at https://www.fdic.gov/regulations/resources/call/index.html. To match banks in the TARP Transaction Report to RSSDIDs 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 630 of the 707 banks. The remaining share are added directly.

12. 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.
13. 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 includes 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; Birmingham, AL; Chicago, IL; Dallas, TX; Buffalo, NY; and Milwaukee, WI.
14. 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). Total Capital is calculated as Total Assets (RCON2170) minus Total Liabilities (RCON2948). Return on Assets is Net Income (RIAD4340) divided by Total Assets. Cash to Assets is Cash and Due From Depositors (RCON0010) divided by Total Assets. Loan to Deposits Ratio is Loans, Leases, Net Unearned Income (RCONB528) divided by Total Deposits (RCON2200).
15. See https://www.bls.gov/lau/.
16. Business Dynamics Statistics are available for the economy overall and aggregated by establishment and firm characteristics from the United States Census at https://www.census.gov/programs-surveys/bds.html.
17. More than 75% of observations are missing prior to 2008, which limits the feasibility of using these data in the main analysis to study outcomes for communities that received CPP funds. Even so, we are able to show in the post-treatment period there is systematically high correlation between CRE outcomes and establishment entry and exit rates, and to a lesser extent job creation and destruction rates.
18. The number of observations and goodness-of-fit are omitted for ease of interpreting how the table is organized. On the left side, every cell with a coefficient and standard error represents a separate panel estimation. Listing every N or R^2 value is a typesetting challenge and the information would have limited value. On the right side, each column (i.e., entry, exit, creation, and destruction) represents a joint estimation across all the CRE controls within each property sector. For the joint estimations, sample sizes are 6,545 for office properties, 6,113 for retail properties, and 8,533 for multifamily properties. The goodness-of-fit is at least 0.88 for entry rates, 0.73 for exit rates, 0.65 for creation rates, and 0.63 for destruction rates.
19. 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 a 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 $\lambda_t' = [1\ \eta_t]',\ \mu_i = [\theta_i\ 1]$.
20. An implicit implication is that individual counties should have no meaningful heterogeneity, and instead should be jumping around the mean. But this is often violated in practice.
21. Some of the models fit particularly poorly, and given the relatively few treated individuals in some groups, the 95% quantile is a good first-pass approximation of the distribution among treated units. We later carry out permutation tests under the null of no effect, and compare these quantiles.
22. 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 county-level clusters.
23. One concern is that since there is time-variation in treatments 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 γ_{-1} being strongly negative due to the GFC. To remedy this, we 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.

24. There exist step-down methods that use bootstrap methods to estimate dependence in the underlying tests to generate a less conservative test. This test can also be augmented to explicitly test a one-sided hypothesis by using the appropriate t-values.
25. We first did an exact match by name, state, and city between the TARP transaction file and the FR Y-9c file, followed by directly against the call report bulk file both using as of 2007 Q4 information, the remaining matches were manually verified using FFIEC's National Information Center. We then merge treatment status back onto call reports, and then normalize all financial variables using the PCE deflator to put them in real terms.

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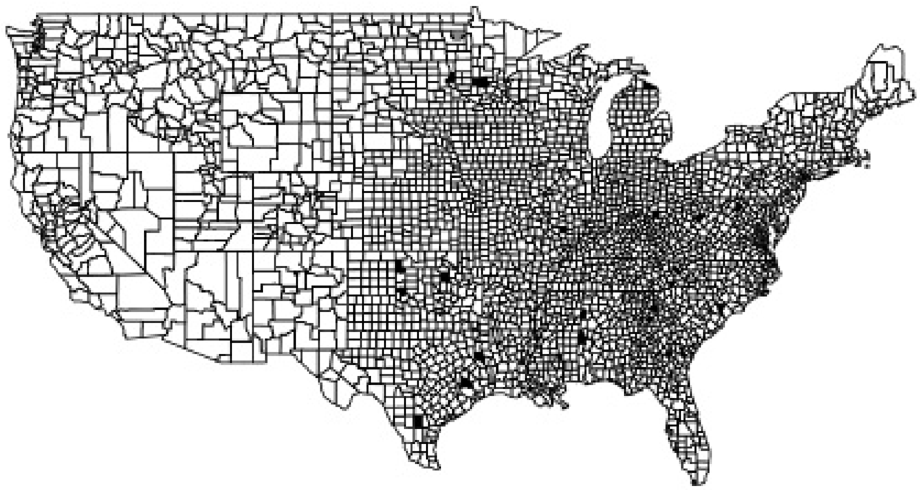
Appendix

(a) Removed bank holding company counties



(b) Removed bank holding company and adjacent counties

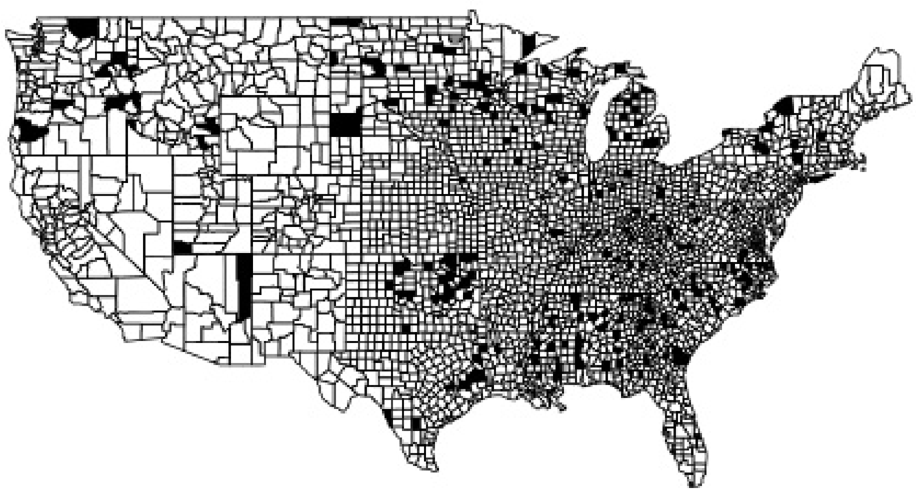


Figure A1. Removed bank holding company counties.

Notes: The left map shows counties that had the top 20 largest banks or bank holding companies in them. The right map shows 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.

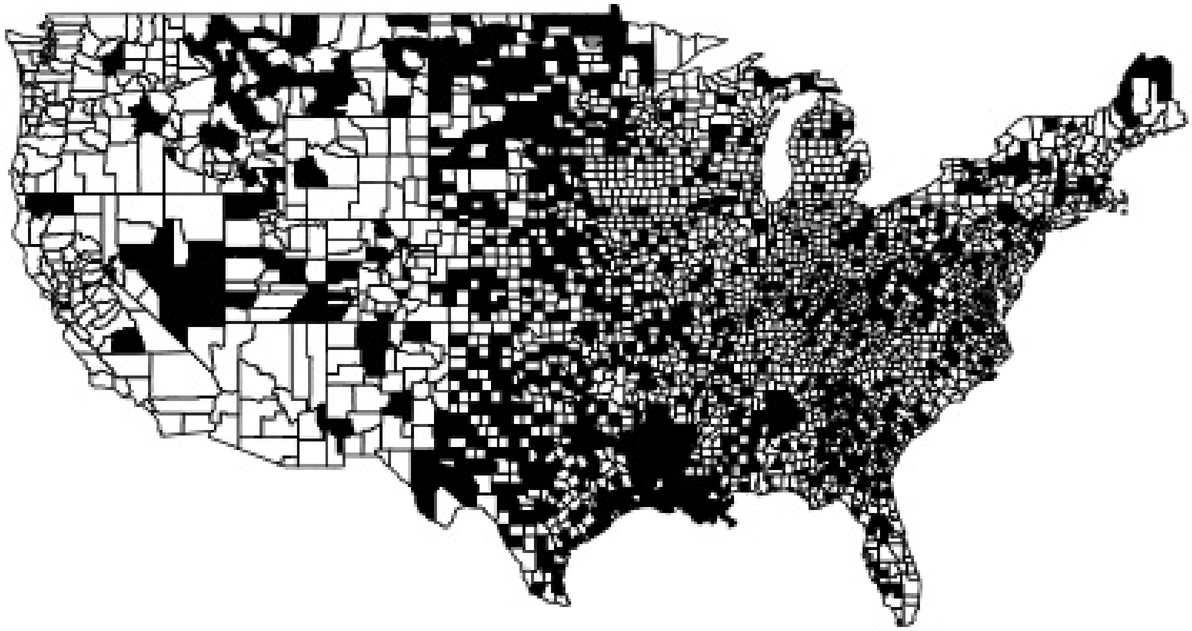


Figure A2. Additional removed counties.

Notes: 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.

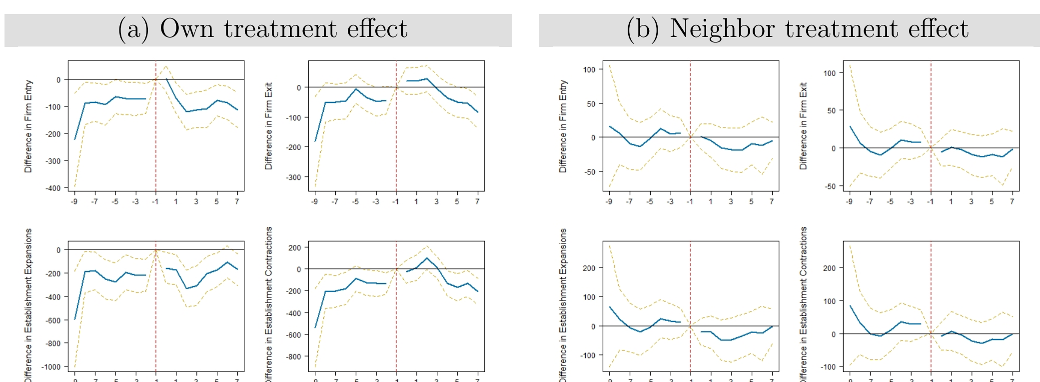


Figure A3. DID own and neighbor treatment status.

Notes: 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.

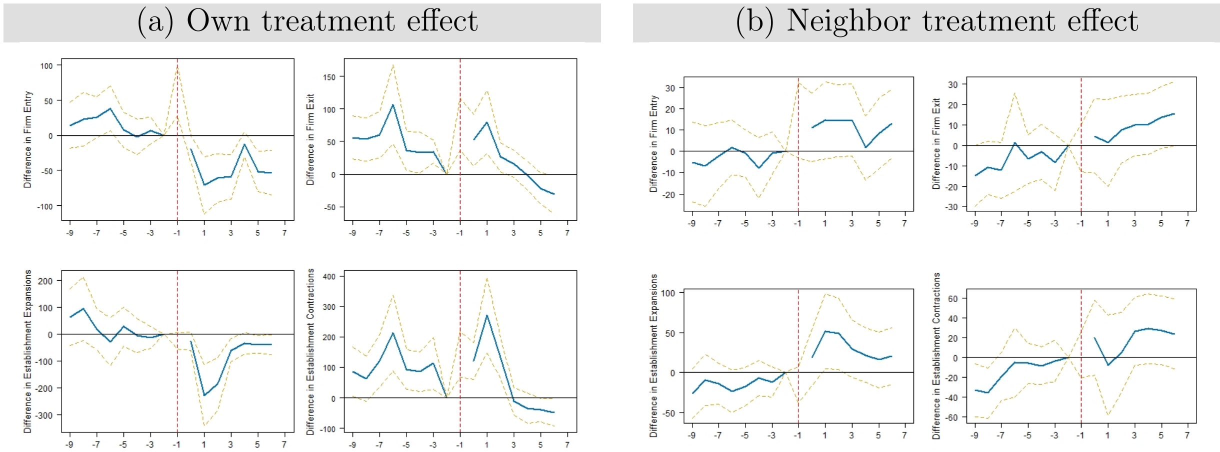


Figure A4. Own(1,0) and Neigh(1,0) treatment status.

Notes: 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 had a neighbor receive treatment in 2008.

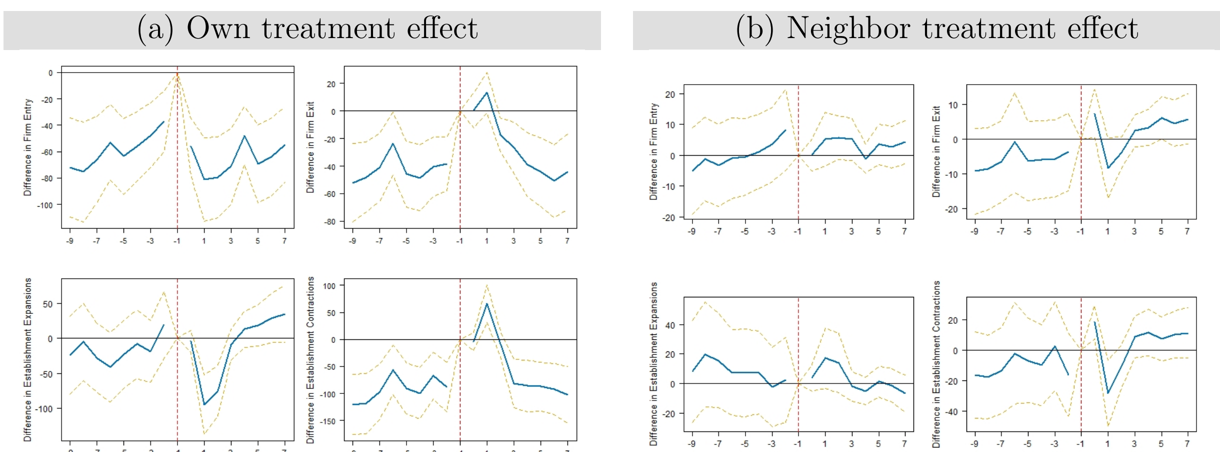


Figure A5. DID Own(0,1) and Neigh(0,1) treatment status.

Notes: 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.

(a) Own treatment effect

(b) Neighbor treatment effect

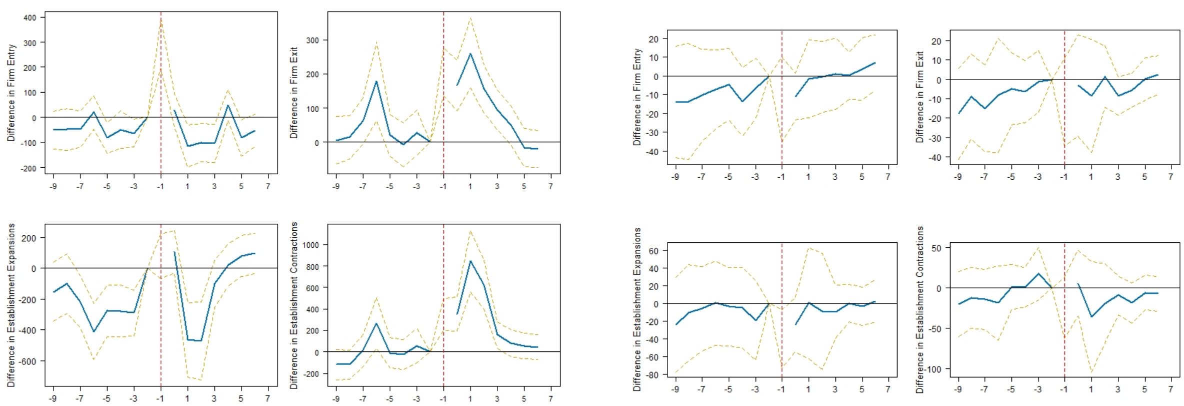


Figure A6. DID Own(1,1) and Neigh(1,1) treatment status.

Notes: 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.

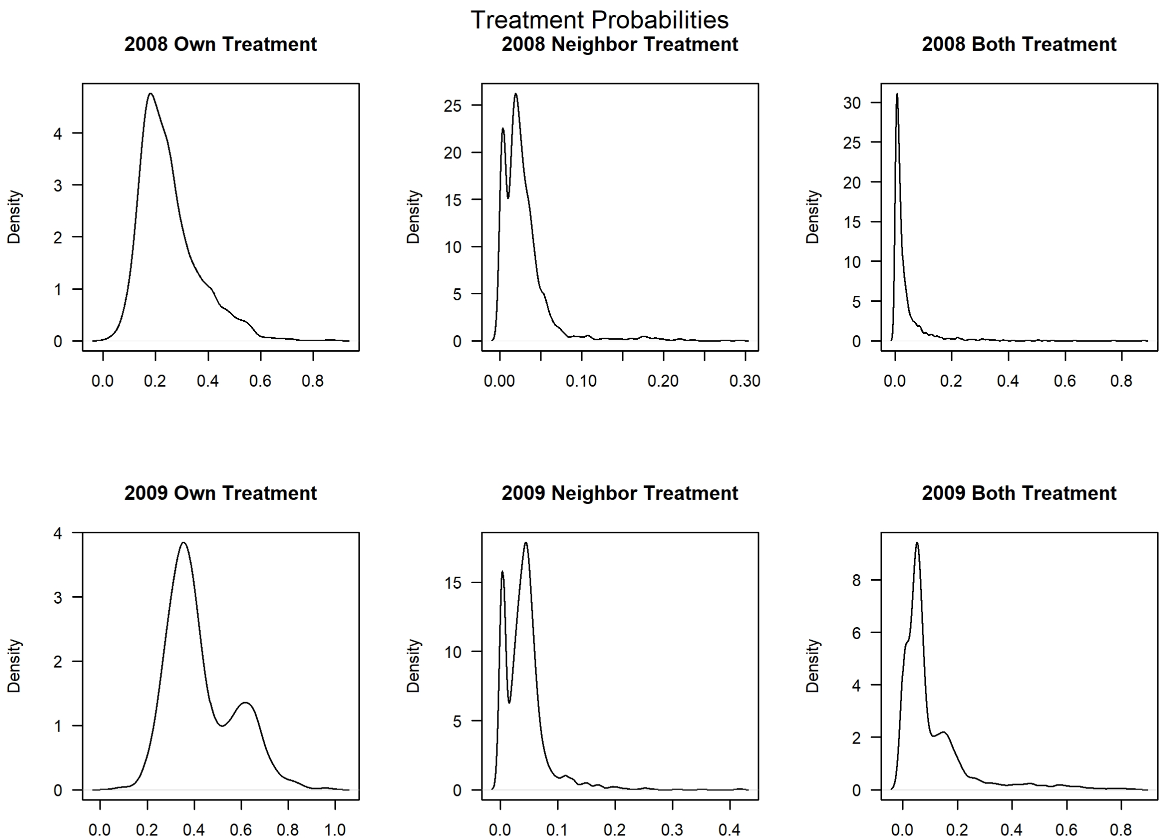


Figure A7. Bivariate probit propensity scores.

Notes: 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.

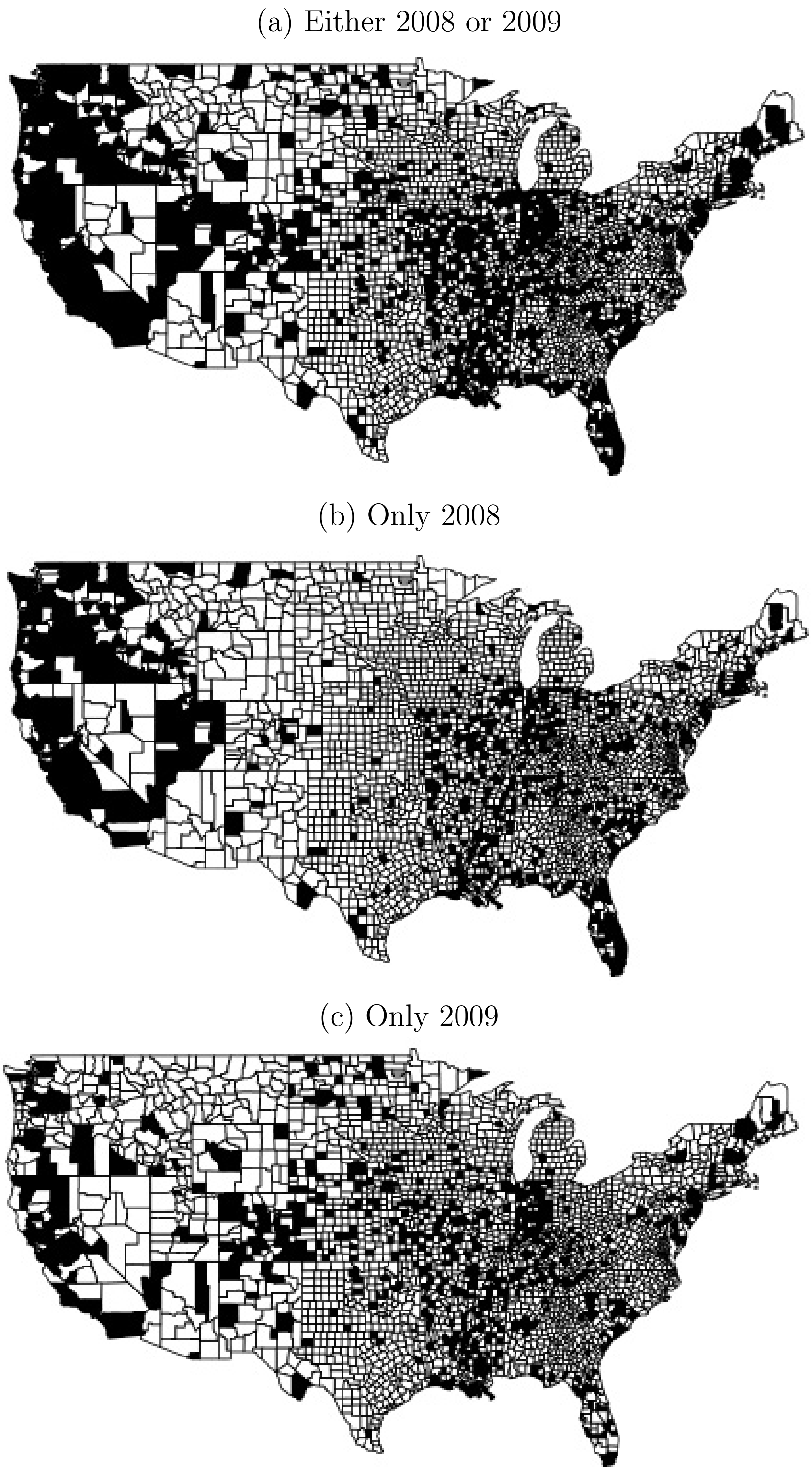


Figure A8. Network treated downstream counties.

Notes: Maps show all counties with a branch location of a bank that received CPP funds. Panel (a) shows data for either 2008 or 2009, panel (b) limits to 2008, and panel (c) limits to 2009. Source: Author calculations.