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

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

Abstract

Using census data on county level business dynamics I estimate the impacts of the Treasury Department's Capital Purchase Program on firm entry, firm exit, employment expansion, and employment contraction following the 2008 Financial Crisis. The Capital Purchase Program bought stock in banks to shore up risky assets and ideally induce banks to extend new loans to credit worthy households and small businesses. If the Capital Purchase Program made banks more likely to provide credit to local firms and entrepreneurs, counties should have seen improved firm entry and employment expansion and decreased firm exit and employment contraction relative to untreated counties. Using synthetic control methods I estimate the direct and spillover effects of a county having a bank receive Capital Purchase Program funds on local business dynamics in the seven years following treatment. The estimates show the CPP had small impacts on firm entry, but caused moderate improvements in long run firm exit, expansion, and contraction behavior.

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

1 Introduction

This paper estimates the impact of the Treasury Department's Capital Purchase Program (CPP) on firm entry, firm exit, employment expansion, and employment contraction following the 2008 financial crisis. The CPP provided $205 billion dollars to more than 700 banks in over 400 US counties in order to prevent foreclosure and stimulate loan supply as part of the broader Troubled Asset Relief Program (TARP). The CPP was one of the largest fiscal commitments of the US government as a response to the great financial crisis. The Treasury Department explicitly stated that benefits of the CPP ideally would be passed along to individual households and non-financial firms to bolster beliefs about the government's willingness to loosen credit markets .

My paper answers the question on whether or not the Capital Purchase Program impacted local firm dynamics. Firm entry, firm exit, employment expansion, and employment contraction cover a wide range of economic activity that would have been supported by a viable credit supply shock to the availability of loans to existing businesses and prospective entrepreneurs. Eased access to credit would have helped prospective entrepreneurs looking to enter or expand employment, or made banks more willing to extend credit to business otherwise having to shut down or lay off employees. Ideally the CPP would have (1) increased firm entry, (2) decreased firm exit, (3) increased the number of firm expansions, and (4) restricted the number of firm contractions. Positive firm dynamics are a main contributor to TFP growth (Lee and Mukoyama [2015], Clementi and Palazzo [2016]) and lead to lower unemployment and stronger economic growth out of economic depressions. The CPP ideally played two roles, first would be to insure against bank closure. However, work by Carow and Salotti [2014] show the Treasury Department gave CPP funds to weaker banks only if they had better performing loan portfolios. The second effect would be in extending loans to credit worthy households and businesses. Thus studying the real impacts of the CPP allow policy makers to evaluate the effectiveness and efficacy of the program.

Credit constraints might appear during financial crisis if banks become unwilling to lend. My estimates for employment expansion and contraction further access to role of access to credit for established firms, where younger firms often perceive to have a worse access to bank loans Canton et al. [2013]. In many ways I can view the CPP as an additional Loan Guarantee Scheme, where the government takes up a guarantor of loans that financial institutes pass along to enterprises. Previous work here has found that such Loan Guarantee Schemes can provide an efficient means of job creation, but guaranteed projects are marginally more likely to fail, that they do induce funds from banks that otherwise would not be lent, and widening to larger firms and loans may hurt program benefits Parker [2005], Riding and Haines [2001].

I estimate the direct impacts of a county receiving CPP funds utilizing census data on aggregate county level firm dynamics. I further estimate the impact of the CPP on neighboring counties that did not receive funds directly, the spillover effect. These results extend previous work by Berger and Roman [2014] showing commercial real estate lending and off-balance-sheet real estate guarantees increased net job creation and net hiring establishments while decreasing business and personal bankruptcies. It further provides evidence on how young firm activity is tied to location financial health and credit supply [Davis and Haltiwanger, 2019]. This contribution further extends to second order effects of bank level responses to receiving CPP funds. A primary question is how bank commercial and industrial lending increased or decreased among firms that received CPP funds. Many studies have come to inconclusive and often contradictory results [Bassett et al., 2017, Berger et al., 2018, Black and Hazelwood, 2013, Cole, 2011, Contessi and Francis, 2011, Li, 2013, Thomas et al., 2013]. Related to my interest in the indirect effects of CPP injections into neighboring counties, Jang [2017] shows that TARP money provided to distressed areas had spillover effects into neighboring, better performing, counties.

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

My paper further shows that the impacts of the CPP on individual counties had a high degree of outcome heterogeneity across firm dynamics. While average results are small and close to negative, considerable heterogeneity exists between type of banks and lending behavior and similar outcomes indicated in resulting county business dynamics. This is not surprisingly since trying to provide funds directly to banks is similar to the pass through of monetary policy changes to credit markets which have previously been shown to have considerable heterogeneity [Thomas et al., 2013]. Secondly, most small businesses do not have access to equity markets, and rely on local or regional banks for credit. Relationship lending has been recently established as a major way in which banks recover underlying firm specific behavior [Berger et al., 2010].

I estimate my effects using a synthetic control estimator [Abadie and Diamond, 2010, Abadie and Gardeazabal, 2003, Abadie et al., 2015, Ferman and Pinto, 2017]. While typically difference-in-difference methods have been used, a primary concern, backed up in robustness checks, is there exist considerable heterogeneity in unobserved characteristics that contributed to the chance of a severe local shock from the Great Financial Crisis. As a result the common pre-trend assumption in Difference-in-Difference methods will be violated, and the untreated counties become a poor representative as a set of counterfactuals. Comparably synthetic control assigns weights to counties in a 'donor' pool, such as untreated counties, to mirror counties in a 'target' pool, such as treated counties. This method aims to create a synthetic county that closely mirrors the behavior of a target county in the pre-treatment period such that it acts as a valid counter factual if the county did not receive treatment in post-treatment time periods.

My estimation strategy includes timing differences on when counties had banks receive CPP funds to define potential outcomes of both own-treatment in either 2008 or 2009, and whether or not a county was adjacent- defined as being within 50 miles of a neighbor counties center- to a treated counties. Under this setup I am able to estimate both the average direct effects and spillover effect of a county having a bank receive CPP funds. Analysis of the CPP benefits from several stylized facts; the CPP had statutory requirements where the Treasury could only purchase stock valued between 1%-3% of a banks troubled assets, up to $25 billion and that among counties that received money only a few banks received CPP funds. Combined, these facts allows me to view a county as treated as long as at least one bank received CPP funds. The second stylized fact is that funds were dispersed in two waves, reducing treatment dynamics to a binary variable based on receiving treatment in either 2008 or 2009.

The results indicate that both direct and spillover effects where generally negative and close to zero. Firm entry only decreased by about 10 fewer entrants a year, exit spikes to about 40 additional exits a year, but showed long run improvement, about 50 fewer firms expanded employment directly following receiving CPP funds, and about 45 additional firms contracted employment. However, five to six years after receiving treatment, firm entry returned to its previous levels, about 40 fewer firms exited treated counties starting in 2011, and there were about 50 more employment expanding- and 50 fewer employment contracting- firms.

These results further show even as average causal effects were negative and close to zero many counties saw marked improvement, and large heterogeneity existed in county firm dynamics following treatment. All treatment effects are highly correlated with each other, with a major driver being the large number of firms that enter and exit in a single year. One of the most striking results is that immediately following treatment employment expansions (contractions) are either entirely centered at zero and negative (positive).

The paper proceeds as follows. Section 2 describes the Capital Purchase Program in greater detail, including application process, funds approval process, and costs to receiving banks. Section 3 describes the data, providing preliminary data analysis and provides summary statistics. Section 4 formalizes the empirical design and estimation processes. Section 5 provides estimation results and robustness checks. Section 6 provides robustness checks, including difference-in-differences and IV difference-in-differences specifications. Section 7 concludes.

2 The Capital Purchase Program

The Capital Purchase Program provided extra capital to banks by buying up non-voting senior preferred shares on standardized terms to offset now-high risk assets remaining on bank's balance sheets. The CPP provided \$205 billion to more than 700 banks. The first 10 banks received just over \$125 billion. These banks include Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan Chase, Morgan Stanley, State Street Corporation, Wells Fargo, 1st Financial Services Corporation, and Bank of Commerce Holdings.

The application period lasted between October 3rd, 2008 to November 14th, 2008 for publicly held companies, December 8th for Privately held companies, and February 13th, 2008 for S Corporations. On May 20th, 2009, Timothy Geithner announced that for banks with assets less than \$500 million would have a second window to apply for CPP funds for the following 6 months.[1]

Individual banks applied for CPP funds through their federal regulator- the Federal Reserve, FDIC, Office of the Comptroller of the Currency, or the Office of Thrift Supervision. Bank applications included the number of preferred shares they wanted bought, amount of authorized but not issued preferred stock available, authorized but not issued common stock, amount of total risk-weighted assets as reported on their most recent call report.

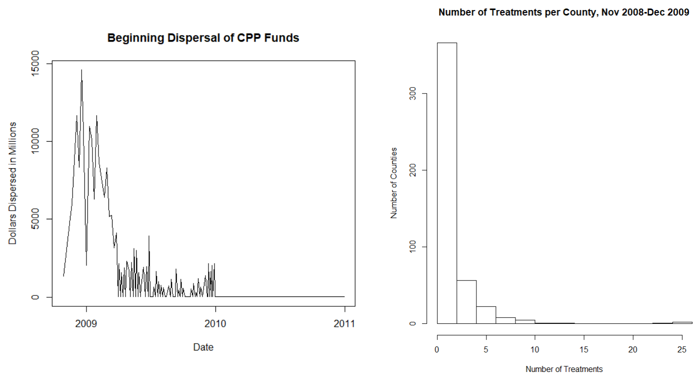
Banks could indicate a preferred level of stock purchase between one and three percent of the total risk-weighted Assets of the applicant up to \$25 billion. Many counties had only a few banks receive funds, and even a smaller share of banks received multiple injections. Between 2008 and the end of 2010, the average county had 2.06 injections in total, often in separate banks.

The program was not cost-free, and the non-voting senior preferred shares required a 5% dividend for the first 5 years and 9% afterwards. 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. Research has indicated that these purchases were preferential for the banks. The Congressional Oversight Panel estimated that across all TARP programs, the Treasury gave out \$254 billion in 2008, for which it received assets worth approximately \$176 billion, a difference of \$78 billion. Equivalently, Veronesi and Zingales [2010] estimate during the first 10 transactions of the CPP, the Treasury overpaid between \$6-13 billion for financial claims.

Federal regulatory agencies chose which banks received money and sent their preferred set of applicants to the Treasury Department for final clearance. Applications that were rejected or withdrawn were not announced or publicly disclosed. Sosyura [2012] show that of roughly 600 public firms, 416 firms (79.8%) applied, 329 (79.1%) were accepted, and that 278 (84.5%) accepted the funds but 51 (15.5%) declined. Comparably the share of private banks that applied for CPP, how many were accepted to receive funds, and of those, what share ultimately received or turned down CPP funds is unknown, and I act as if every bank that was offered funds received them. All payments to participating banks were made before January 1st, 2010. However, there are clear spikes in lending. Between November 14th and Dec 31st 2008, there is a clear peak in CPP payments to banks. There was then a second peak at the beginning of 2009. Both the dispersal of CPP funds by date, and the average number of treatments per treated county, are depicted in Figure 1.

[1] https://www.treasury.gov/press-center/press-releases/Pages/tg139.aspx

Figure 1



Data compiled from Treasury CPP Transaction Reports

Overall the CPP provided standardized amounts of capital to participating banks in one of two main treatment branches, at the very end of 2008 or the very beginning of 2009. Since most counties only had a small number of banks receive CPP funds, I can view own treatment status as a binary event in each time period as long as the bank in a county received CPP funds.[2]

3 Data and Summary Statistics

My primary dependent variables of interest are county level firm entry, firm exit, expansion, and contraction from 1999 to 2015 provided by the Census Statistics of US Businesses & Business Information Tracking Series (SUSB).[3] Own treatment is defined as an indicator value on whether or not a county received any CPP funds during a given time period. I separate own-treatment status into two groups, the first being receiving CPP funds in 2008, and the second being receiving CPP funds in 2009. The own-treatment variable takes the form,

$$W_{i,t} = 1\{CPP_{i,t} > 0\}$$

Where W is a proxy for "Own Treatment", i indexes counties, t is the year. Similarly $CPP_{i,t}$ is the dollar amount of CPP funds given to banks in county i in period t. Using this definition, 63 counties received CPP funds only in 2008, 243 received CPP funds only in 2009, and 81 counties received CPP funds in both periods. Similarly, credit markets may extend beyond county borders, implying that treating a county i may impact nearby counties. A neighbor is defined to be any county with centroid distance within 50 miles of a subject county i.[4] This metric is used as entrepreneurs have empirically traveled moderate distances trying

[2]The TARP Investment Program Transaction Reports provide Bank Name, City, and State as identifiers, and not FFHIC RSSD identifiers associated with Call Reports. Matching on Bank Name, City, State, generates only about 700 matches out of the 2100 total purchases made.

[3]`https://www2.census.gov/programs-surveys/susb/`

[4]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`

to find beneficial loan deals, such that in Belgian banks the maximum loan distance is 50 miles [Degryse and Ongena, 2005], while in the US average bank applications come from 10 miles away [Agarwal and Hauswald, 2008], with a standard deviation of 21 miles, while accepted applications come from even closer to the bank (2.62 miles), with a smaller standard deviation (10.67 miles). Thus while most bank applications are local, applicants seem to be willing to drive moderate distances in search of favorable loan contracts. Under this setting we define the neighbor treatment variable as

$$G_{i,t} = 1\{\exists j \text{ adjacent to } i \ s.t. \ CPP_{j,t} > 0\}$$

As above, $G_{i,t}$ is a proxy for "Neighbor Treated" status for county i in year t. Adjacency is defined by the NBER 50 mile centroid file, and $CPP_{i,t}$ is defined as above. A major source of possible bias is that the largest banks in the US were perceived to be highly illiquid at the start of the Great Financial Crisis, and thus almost guaranteed to receive CPP funds. Almost half of the total outlay went to a handful of the largest banks. Other papers have removed the 20 largest banks from the analysis (see Li [2013]). In turn, I remove the counties with the 20 largest banks, as well as any neighboring county.[5]

Graphing mean firm entry, firm exit, employment expansion, and employment contraction grouped by own-treatment status in each time period shows large differences in the differences in firm dynamic levels, however rescaling each time series subject to within-group means and standard deviations show considerable similarities in each group (see Figures 10-11 in the Appendix).

Mean bank characteristics at the county level are calculated from FDIC call sheet data,[6] Following Li [2013] I include troubled assets ratio, annualized Return on Assets, and loan-to-deposits ratio.[7] These proxy for local community bank health that the Federal Regulators may have observed when deciding which banks to accept into the CPP program.

BLS's Local Area Unemployment statistics on county level unemployment rates.[8] Previous work into firm entry has found small effects of taxes, where even along borders firms place rarely show a preference for side based on relative tax rate [Duncan, 2016]. Instead a major driver of firm entry appear to be unobserved demand for products and agglomeration economies. Measures of upstream and downstream agglomeration economies are calculated from input-output tables. These take three forms, the first is industry

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

[6] `https://www.fdic.gov/regulations/resources/call/index.html`

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

[8] `https://www.bls.gov/lau/`

cluster, measured as each industry's share of total employment in a county/year pair relative to the industry share in the nation as a whole. Upstream and downstream measures of connectedness are calculated from the Bureau of Economic Analysis' 1997 Standard Use Table. The share of workers providing inputs to each 2 digit NAICS code is calculated for in each county and year. Using this again the upstream and downstream measures is calculated by taking the share of workers providing inputs into each 2 digit NAICS code divided by total employment in each period. This is again normalized by the average across the United States. Measures of household financial health are provided by the FDIC experimental county level home price index, however The FDIC data exclude counties without enough mortgages to draw a consistent enough estimate of household financial wealth, this using only counties where the home price index exists excludes many rural counties.

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

Table 1: Summary Statistics of Data

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

Some additional comments need to be made about the nature of firm dynamics in the data. The majority of firms are small. From the Census' County Business Patterns data, which tracks the total number of establishments in a given county, roughly 55% of firms have between one and four employees, 20% have between five and nine employees, and 12% have between 10 and nineteen employees. These numbers are very stable across all years in the sample. The majority of firms are small mom-and-pop set ups. The SUSB data does not disentangle firm size, but using this sample I assume that the majority of new entry is small. This is further supported by other studies. See for example Bartelsman et al. [2005], Kaniovski and Peneder [2008], Mata and Portugal [1994]. Across many countries most firms enter and fail within the first year or two. This pattern is also experienced throughout counties in the United States. Thus most firms enter and exit in a single year.

Most prospective entrepreneurs do not face capital constraints. There is strong evidence that in contemporary good times credit constraints does not impact the decision to enter into entrepreneurial activity given a lack of a relationship between wealth and entry into entrepreneurship Hurst and Lusardi [2004]. Data from the 2003 National Survey of Small Business Finances show that among firms that had only opened after 2002, 25% of firms had 0 outstanding loans, and 50% had less than $7000 in loans. Among those firms that had taken out capital leases, 25% of them owed less than $4000 in principal, and 75% owed less than $45,000.[9] Using the Federal Reserves Small Business Credit Survey, in 2018 33% of new firms had no outstanding debt, and 22% owed between $1 and $25,000. For debt, 46% of new firms did use a loan or line of credit as a regular source of external financing, while only 9% of new firms had outside equity financing. Almost half of young firms applied for financing in the previous year, most seeking between $25,000 and $100,000. These figures point to credit constraints not necessarily existing during good periods of growth and economic activity.

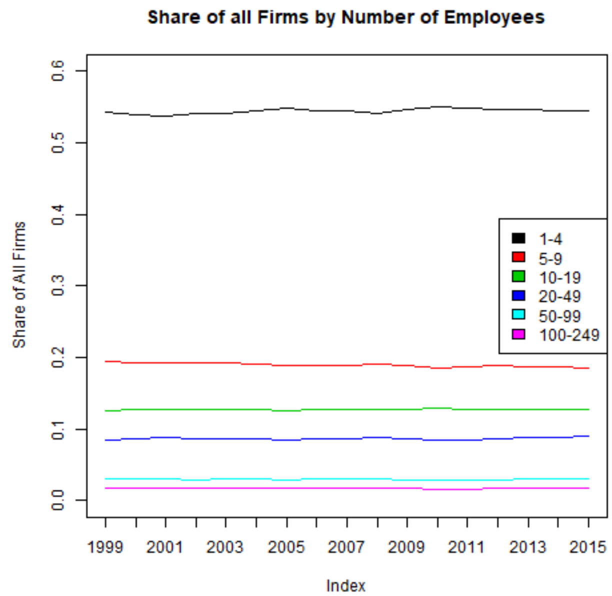
Shane [2010] points out "The typical new business is extremely ordinary... the typical start-up isn't innovative, doesn't intend to challenge existing companies, lacks a single competitive advantage, and is not intended to grow." This is reflected in capital needs of small firms, roughly 48.4% start in residence- such as home or garage, and an additional 40.64% in a rented or leased space. And that the typical median start-up in the US requires $24,000-30,000 in start up capital. Shane further shows that banks are the leading source of external debt financing for new businesses, and account for 16% of the total financing of businesses that are less than two years old, making up a larger source of financing than friends and family, government agencies, and strategic investors.

Most importantly there is large stability in the change in the number of establishments at different firm sizes. Figure 2 graphs the change in the share of establishments with different sets of employees, 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, that makes up more than 95% of all entrants in a given year. Overall both the number of new firm entrants in each county and the share of firms at different levels of employment are very stable. Firms with 1-4 employees consistently make up almost 55% of the change in establishments, firms with 5-9 employees has fallen slightly from being 20% of the change in establishments to 17%, and firms with 20-49 employees have increased from 0.8% to 1% of new entrants. These shifts so a small change in

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

the composition of the change of establishments is slowly being dominated by moderate number of employee firms. This might be indicative of higher capital constraints among contemporary entrepreneurs relative to the late 1990's.

Figure 2



Data compiled from Census County Business Patterns

Finally, a number of other policy drivers have studied determinants of firm entry, such as right to work laws [Holmes, 1998] or lower taxes [Duncan, 2016, Rohlin et al., 2014]. Often specific research designs are used to estimate these effects and remove endogeneity of pro-business practices such as I exclude these variables due to fear of inducing larger biases in my estimates, especially given that they do not explain a large share of the overall variation in firm entry dynamics. In many of these cases the proposed models both explain a small share of the overall variation in firm entry, or show that the treatment effects have economically small coefficients. In the following section I explicitly detail the empirical design and estimation strategy.

4 Empirical Design

This paper estimates the direct and indirect Average Treatment for the Treated under the presence of dynamic treatment assignment and local credit markets. Due to the uniformity of TARP payments as a share of bank Troubled Assets Ratio and the low number of interventions per county allows me to reduce own treatment status to treatment in one of two periods. A major source of confounding in my research design exists in credit market spillovers. Entrepreneurs are likely to travel moderate distances in order to acquire credit to start, expand, or stop foreclosure on a business. As a result counties are not independent of each other, and instead rely on a both their own sources of productivity and access to credit, as well as those around them.

To account for this I extend the potential outcome framework to include own and neighbor treated status in each of two possible time periods. Neighbor treated status is defined by the NBER 50 mile Adjacency metric, which treats any county whose centroid is within 50 miles of a particular subject county as a neighbor. To motivate a simple example of how neighbor treatment status impacts the potential outcomes of a particular subject county, Figure 3 plots the five counties of Rhode Island and each county's centroids. When adjacency is defined by border contiguity the middle plot represents which counties classify as a neighbor, I equivalently provide a matrix representation. If adjacency is determined by a county-centroid distance metric, I pick a distance metric and replace each non-zero term with either a zero or one depending on if the distance between the two points is great or less than that distance respectively.

Figure 3: Rhode Island County Borders

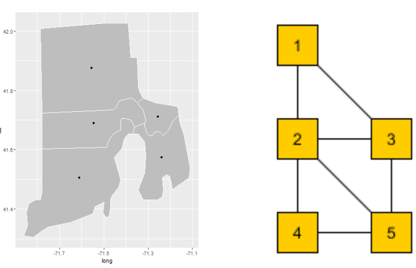


Table: Border Adjacency Matrix

	1	2	3	4	5
1	0	1	1	0	0
2	1	0	1	1	0
3	1	1	0	1	1
4	0	1	0	0	1
5	0	1	1	1	0

For example, in the case of Rhode Island, a distance metric of adjacency being county centroids no more than 30 miles apart renders all off diagonal elements 1, and a distance of less then 9 miles renders the whole matrix 0's. Preference for the distance metric over border adjacency are beliefs in local economic similarities being determined by distance between counties, versus simple boarder adjacency. Downsides are that even counties that are border adjacent may not be distance adjacent in the Midwest due to large county distances.

There are T time periods. From 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 $T_4, \ldots, T$ 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, own treatment $W_{it} \in \{0, 1\}$ or neighbor treatment $G_{it} \in \{0, 1\}$, for $t \in \{T_1, T_2\}$. Therefore in period T_1 individuals treatment status can be characterized in the set $S_{iT_1} = (W_{iT_1}, G_{iT_1}) \in \{(0, 0), (1, 0), (0, 1), (1, 1)\}$, with sixteen potential outcomes in periods $t = T_2, \ldots, T$, $S_{iT_2} \in \{(S_{iT_1}, 0, 0), (S_{iT_1}, 1, 0), (S_{iT_1}, 0, 1), (S_{iT_1}, 1, 1)\}$. In general I refer to counties by their second period potential outcomes, where I generally index by $(W_{iT_1}, G_{iT_1}, W_{iT_2}, G_{iT_2})$.

The aim of the empirical design is to estimate the average treatment for the treated (ATT), i.e. for counties that had banks receive CPP funds. This differs from the average treatment effect (ATE) in that I do not care about estimating the implied effect among counties that could have, but did not, receive treatment. Under this condition negative-sorting, banks and counties in the overall worst financial condition, applying to and receiving CPP funds, doesn't impact the validity of estimates. One can think of this as the ATE would include the impact of giving money to many healthy counties, while the ATT focuses just on the value of the treatment to counties with banks in the worst financial condition. Assume the simple structural model for untreated counties,

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

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 a number of common data generating processes.[10]

Synthetic control offers a direct data-driven way to proxy the unobserved factor loading structure by creating a combination of counties from a donor class, usually untreated counties or counties that share a different treatment regime, on the pre-treated time period. These methods vary quite a bit. Abadie and Diamond [2010], Abadie and Gardeazabal [2003], Abadie et al. [2015], Ferman and Pinto [2017] construct weights similar to matching estimators, requiring that all weights be strictly positive with sum equal to one. This assumption was removed in Hsiao et al. [2012], Li and Bell [2017]. The main difference between the two is that the "panel data approach" is an unconstrained regression, and the synthetic control method is a constrained regression. Similar approaches without constraints have started to implement LASSO and other regularization methods [Amjad et al., 2018, Carvalho et al., 2018, Chernozhukov et al., 2018, Doudchenko and Imbens, 2016]. A comparison of these methods was conducted by Gardeazabal and Vega-Bayo [2017], Wan et al. [2018]. With only a single treatment, synthetic control estimates county specific ATT's, but with two different treatment effects these estimates become a county specific total treatment effects, and parsing out average direct and spillover effects requires modifications.

Let r_{it} denote own-treatment effect in period t and m_{it} to be the neighbor effect. In period T_1 this generates the four [possible outcomes,

$$y_{iT_1} = \begin{cases} y_{iT_1}(0,0) + r_{iT_1} & \text{if } W_{T_1} = 1,\ G_{T_1} = 0 \\ y_{iT_1}(0,0) + m_{iT_1} & \text{if } W_{T_1} = 0,\ G_{T_1} = 1 \\ y_{iT_1}(0,0) + r_{iT_1} + m_{iT_1} & \text{if } W_{T_1} = 1,\ G_{T_1} = 0 \\ y_{iT_1}(0,0) & \text{if } W_{T_1} = 0,\ G_{T_1} = 0 \end{cases}$$

Creating a linear combination of pre-treatment untreated counties will only estimate $\hat{y}_{iT_1}(0,0)$ in

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

period T_1, thus the naive synthetic control will only recover the total effect of treatment. This problem becomes more pronounces in period T_1, where now there are up to four individual specific effects that existing methods estimate in aggregate. For brevity, I share a subsample of relevant treatment statuses.

$$y_{iT_2} = \begin{cases} y_{iT_2}(0,0,0,0) + r_{iT_2}^{T_1} + r_{iT_2}^{T_2} & \text{if } W_{T_1} = 1,\ G_{T_1} = 0,\ W_{T_2} = 1,\ G_{T_2} = 0 \\ y_{iT_2}(0,0,0,0) + m_{iT_2}^{T_1} + m_{iT_2}^{T_2} & \text{if } W_{T_1} = 0,\ G_{T_1} = 1,\ W_{T_2} = 0,\ G_{T_2} = 1 \\ y_{iT_2}(0,0,0,0) + r_{iT_2} + m_{iT_2} & \text{if } W_{T_1} = 1,\ G_{T_1} = 0,\ W_{T_2} = 0,\ G_{T_2} = 0 \\ y_{iT_2}(0,0,0,0) & \text{if } W_{T_1} = 0,\ G_{T_1} = 0,\ W_{T_2} = 0,\ G_{T_2} = 0 \end{cases}$$

Before imposing any additional assumptions, it is impossible to jointly identify $r_{iT_2}^{T_1}$ and $r_{iT_2}^{T_2}$, $(m_{iT_2}^{T_1}, m_{iT_2}^{T_2})$, $\{(r_{iT_2}^{j}, m_{iT_2}^{j})\}_{j\in\{T_1,T_2\}}$. In practice all these effects are absorbed into a time & individual varying intercept. The first way I attempt to remedy this issue is by explicitly conditioning on a given positive treatment regime, and targeting the specific average effect of interest. For example, if I am interested in $r_t^{W_{T_0}}$, I can estimate this parameter by taking $A = (1,0,0,0)$ as a treated class and $B = (0,0,0,0)$ as a donor class, alternatively $A = (1,1,0,0)$ as a treated class and $B = (0,1,0,0)$ is a viable pair to estimate the effect. Under this framework a specific effect, denoted $\alpha_t^{A,B}$ becomes

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

Without loss of generality, let us assume α_{it} is the individual treatment effect target of interest, and γ_{it} is a composite of individual-time varying nuisance parameters. The point of this approach is to directly condition on neighbor treated status. This removes shared confounders that might be unobserved to the researcher. In all cases we observe the mean among treated counties, the leading term on the left hand side of equation 1. What remains is to estimate the counterfactual synthetic county.

In each case, imagine we have a univariate series y_{it}, where $y_{it}(1)$ is the "treated" side of the potential outcome, and $y_{it}(0)$ is the untreated side. Estimation is carried out for each treated county i by regressing their outcome variable- firm entry, firm exit, employment expansion, employment contraction- on the outcome variable for all donor counties. This returns weights w_{ij} for each donor county j that show how the pair co-move. This estimation procedure if subject to a penalty term $\lambda \mid\mid w_i \mid\mid_2$, where $\mid\mid \cdot \mid\mid$ is the Euclidean norm and $\lambda > 0$, such that restricts the number of donor county weights that receive positive mass $(w_{ij} > 0)$. The concern is that in all cases the number of donor counties drastically outweighs the number of time periods, and normal processes would return a perfect pre-treatment fit. This penalization forces the pre-treatment fit to be based on donor counties being the most similar to a specific treated county. This estimation procedure can be written as,

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

This structure is close to [Doudchenko and Imbens, 2016, Li and Bell, 2017, Wan et al., 2018]. We do away with the non-negativity constraints of earlier work, and instead focus on the fact that $N >> T_0$, and use regularization methods to pick weights.

Under these conditions Equation 1 can be reformulated

$$\begin{aligned} \alpha_t^{A,B} &= \frac{1}{N_A} \sum_{i \in A} (y_{it} - W_i Y_{jt}) \\ &= \frac{1}{N_A} \sum_{i \in A} \left(\alpha_{it} + (\gamma_{it} - \sum_{j \in B} w_{ij} \gamma_{jt}) + \lambda_t (\mu_i - \sum_{j \in B} w_{ij} \mu_j) + (\epsilon_{it} - \sum_{j \in B} \epsilon_{jt}) \right) \end{aligned}$$

This estimator becomes unbiased under the following assumption

Assumption 4.1.

$$E[\epsilon_{it} \mid W_i G_i] = E[\epsilon_{it}] = 0$$

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

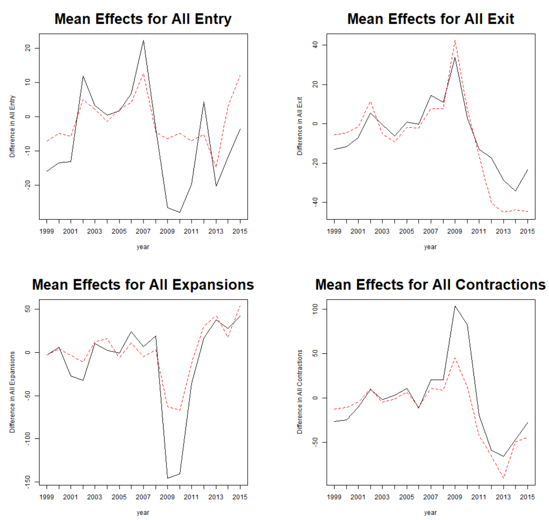
The first part of this assumption states that treatment can be correlated with the factor loading term, $\lambda_t' \mu_i$, but are uncorrelated with idiosyncratic shocks to a given county. The second requires that our pre-treatment fit provides a close approximation for the unobserved time-invariant county specific factor loadings, and that in the post treatment time period provide a mean zero approximation for the second treatment effect. The second part further assumes that the shared treatment effects γ_{it} all share common support across the target and donor pools. Since in practice I use all relevant pairs of target and donor pools, this means all treatment effects- own treatment 2008, neighbor treatment 2008, own treatment 2009, neighbor treatment 2009, share common supports across all treated counties.

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

5 Results

The first set of results I share include both the direct and indirect effects. My results indicate that post-2008, entry saw very little change. For firm exit, there is an initial large spike, where treated counties on average saw 40 extra firms exit a county a year, followed by a long decline into having a lower exit rate between 2011 and 2015. Firm level employment expansions similarly show an initial decrease after treatment is first provided, followed by improvement and in the long run shows continued improvement in relative number of firm expansions. Contractions show a similar trend to firm exit, at first there is a small to moderate increase in the number of firms contracting employment, but starting in 2010-2011 these counties have a lower number of firms decreasing employment than untreated counties.

Figure 4: Mean Effects



These results indicate a general no-effect from counties receiving CPP. Firm exit is generally fairly flat, with at most a loss of about 10 missing firms a year. Comparably excess exit reached as high as 40 firms a year for the first year following the CPP, followed by prolonged improvement in these counties. This is the most striking result as the relative increases in firm employment contractions and it's prolonged improvement seem to be equivalent to losses in employment expansion.

However, these graphs are misleading in two ways. For each effect I generate 12 different estimates, and then average across each estimate. This generates variation in treatment timing, particularly as the majority of counties did not receive CPP funds until 2009. As a result, I plot both the mean response along with 90% quantiles for each effect.[11] However, each of these are subject to possible estimation error from residual components of their potential outcomes.

As before, both direct and indirect effects of entry are mostly centered around zero. For the direct

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

effects, a single observation drives much of the jumpiness seen in the average estimates, and the synthetic control method does not match well over the pre-treatment time period, enough that inferring a treatment effect for this group is unlikely.

Figure 5: Heterogeneous Impacts: Entry & Exit

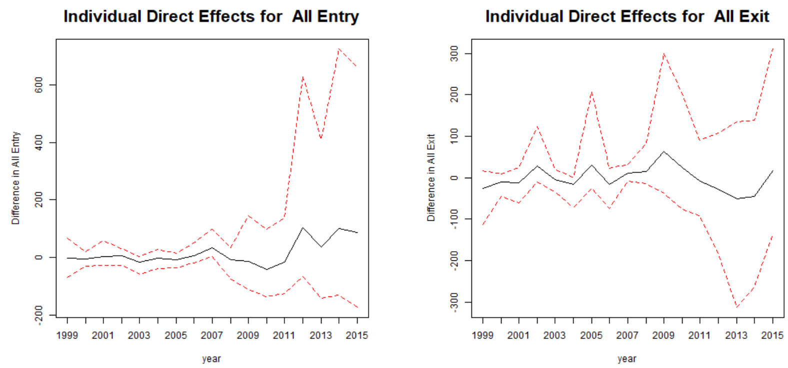
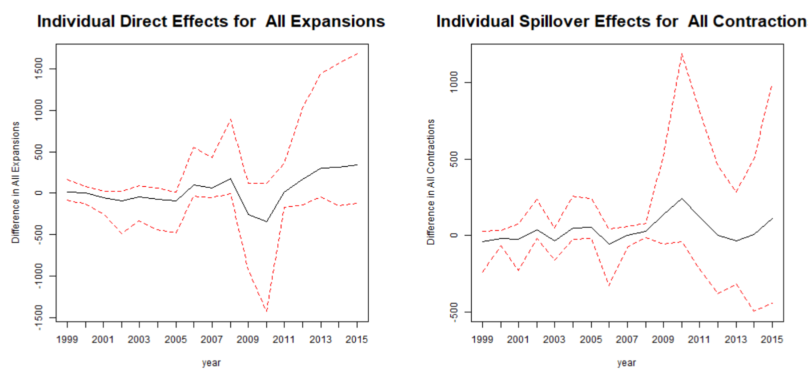


Figure 6: Heterogeneous Impacts: Expansions & Contractions



Treatment effects vary significantly, even if average causal effects of the CPP were generally negative. Most effects are centered around zero. Both direct and indirect spillover effects saw a significant share of treated counties have large gains to entry. Individual effects on exit are fairly symmetric around zero. Most striking is that contractions feature results that are almost uniformly non-zero immediately following treatment.

Overall, results indicate a small to modest negative effect from receiving CPP funds. For both direct and indirect effects entry and expansions decreased, exit and contractions increased, however considerable heterogeneity exists in these effects on individual counties. Many counties saw considerable positive gains to firm entry for both direct and indirect effects. Direct effects on entry saw some counties saw large falls in firm exit, while spillover effects saw a large tail of individuals who saw excess exit for years following treatment. Both direct and spillover effects on expansion were generally negative immediately following treatment, followed by either strong positive expansion starting in 2011 for the directly treated counties, and generally no effect for spillover counties. The direct and spillover effects of contractions saw a strong center directly on zero, with a large upper tail of excess contractions.

6 Robustness Checks

In this section I provide a variety of robustness checks on my primary results. I first provide permutation, or randomization, tests for the robustness of my entry and exit results. I then provide a measure of policy effectiveness duration, which provides a method of evaluating in a single method both the existence and duration of a non-zero policy effect. I then check my results against two benchmark difference-in-difference estimates, and finally carry out some tests for instrument strength.

6.1 Permutation Tests

Inference for synthetic control methods is carried out using a permutation test (Abadie et al. [2015]). For each group I assume no-treatment effect of interest, and re-sample without replacement a new treated group N_A and repeat my estimator. I then repeat this 500 times. This classifies as an exact test under the assumption of no-treatment effect I am able to estimate the exact distribution I am then able to test whether or not my treatment effects lie outside the 95% empirical distribution confidence intervals both in a given time period, and over the duration of my post-treated sample.

500 permutation draws are drawn from the untreated counties to construct an exact test for the existence of a true treatment effect. These draws are represented by the gray lines, while the red lines represent the path for the estimates for each periods Average Treatment for the Treated represented by net firm entry. Across all three estimated main effects the data rejects the existence of a treatment effect at the 10% level.

Figure 7: Permutation Tests for Entry & Exit

Several striking features appear here. First, for both direct results prior to treatment the difference between the observed and synthetic control counties are often well outside the 90% permutation confidence interval. Secondly, both direct effects reach or exceed the 90% permutation confidence interval immediately following treatment, exits are abnormally large, and entry very low. However, by 2011, the difference treatment effect on firm entry is seemingly no longer meaningful. However, by 2013, firm exits have decreased so much in treated counties as to again exceed the 90% permutation confidence interval.

The permutation confidence intervals for spill over effects make it appear spurious. The pre-treatment synthetic control estimates appear to be behaving appropriately in being inside the 90% permutation confidence interval, but following treatment there is much larger variation in permutation-neighbor responses.

Overall, there is significant evidence that the synthetic control might not be behaving appropriately in generating matches for treated counties. Moreover, most results appear well within the randomization test confidence intervals, such that only significant responses come immediately following injection of CPP funds, with some residual long run response on improved firm exit levels in treated counties.

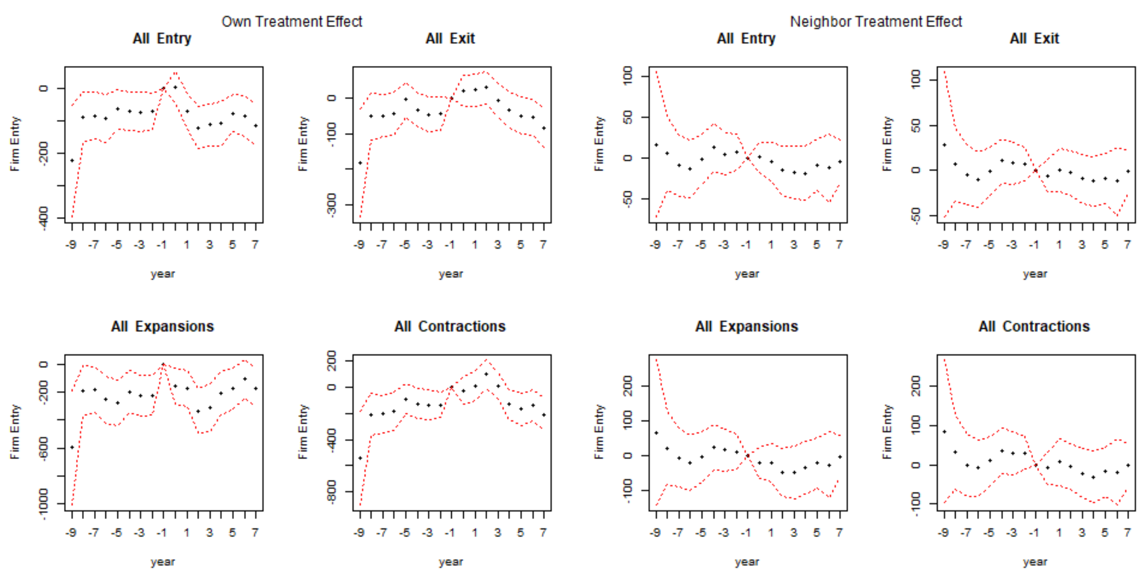
6.2 Difference-in-Differences

Now with a potential outcomes framework in mind I augment the canonical differences-in-differences extended that pools all counties that had positive own treatment or neighbor treatment in either of possible treatment time periods. Several working papers exist in this area, explicitly dealing with decomposing the two-way fixed effects estimator Imai and Kim [2019], and dealing with treatments in multiple time periods [Callaway and Sant'Anna, 2018, Goodman-Bacon, 2018]. This assumes no heterogeneity across either of the treatment statuses. I provide estimates for the pre and post treatment time periods.[12] This generates the estimated equation,

$$\begin{aligned} y_{it} = \beta_1 W_i + \beta_2 G_i + \beta_3 I\{t > T_0\} + \sum_{s=-9}^{7} \gamma_s W_i I\{s = t - \min_k\{W_{i,k+1} - W_{i,k} = 1\}\} \\ + \sum_{s=-9}^{7} \alpha_s G_i\{s = t - \min_k\{G_{i,k+1} - G_{i,k} = 1\}\} + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it} \end{aligned} \tag{3}$$

Since there exists differences in timing between counties that receiving either own-treatment or neighbor-treatment, the term $I\{s = t - \min_k\{W_{i,k+1} - W_{i,k} = 1\}\}$ denotes the difference between the current time period and the time period in which a given county received treatment.

Figure 8: Own & Neighbor Treatment Status



The most striking fact is that the coefficients in the pre-trend differ greatly from zero, and moreover many of them are not-consistent. In particular firm entry, firm exit, and employment contractions all grow leading up to the initial period of treatment. This visible difference in pre-trends (and levels) between treated and untreated counties in different treatment groups invalidates the use of the (mean) non-treated counties as a viable counter factual. Explicit discussion of the resulting effects generated by this estimation procedure might create poor policy conclusions.

Recent work in difference-in-differences and event study methods have increasingly utilized policies that exhibit variation in treatment timing. Under these conditions it is common to generate pre and post

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

treatment effects from time of initial treatment. Two concerns arise out of this. For counties treated in the second period treatment, γ_{-1} and α_1 are now also subject to the shared macroeconomic shock λ_{10}. Therefore I carry out tests for pre-trend just on pre-Financial Crisis periods, as well as pre-Treatment periods. Secondly, it is not a priori clear that CPP effectiveness will provide a permanent shift in relative entry rates between treated and untreated counties.

I perform joint hypothesis tests on $\{\gamma_s, \alpha_s\}_{s,g \leq T_0} = 0$, as well as $\{\gamma_s\}_{s,g \leq T_0} = 0$, $\{\alpha_s\}_{s,g \leq T_0} = 0$ as explicit pre-trend tests. Recent research has pointed out that by doing this, standard errors of post-treatment coefficients are often conservative [Kahn-Lang and Lang, 2019, Roth, 2018]- but since all these tests are rejected we do not try to carry out further corrections.

Analysis is carried out in two ways. First I do carry out Wald tests on pre-trends. For each model this includes three joint tests, (1) both Own and Neighbor Effects have no differing pre-trends, (2) Own treated counties had no differing pre-trends, (3) Neighbor treated counties had no differing pre-trends.[13] I then carry out the step-down multiple hypothesis test outlined in section 6.2. However, since models I reject all hypothesis relating to no differing pre-trend, I do not report the results.

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

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

No shared pretrend implies a p-value less than 0.005

Given the large n, large T nature of my panel unobserved heterogeneity might be highly correlated with intent to treat and observed treatment. As a result I also estimate a model where I allow for heterogeneous responses within each own-treatment and neighbor-treatment couplet. This ignores the multiplicative interaction terms. The resulting estimated equation then becomes.

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

$$
\begin{aligned}
y_{it} = {} & \beta_1 W_i + \beta_2 G_i + \beta_3 I\{t > T_0\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} W_i^{10} I\{s = t - \min_k \{W_{k+1}^{10} - W_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} W_i^{01} I\{s = t - \min_k \{W_{k+1}^{01} - W_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \gamma_s^{10} W_i^{11} I\{s = t - \min_k \{W_{k+1}^{11} - W_k^{11} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} G_i^{10} I\{s = t - \min_k \{G_{k+1}^{10} - G_k^{10} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} G_i^{01} I\{s = t - \min_k \{G_{k+1}^{01} - G_k^{01} = 1\}\} \\
& + \sum_{s=-9}^{7} \alpha_s^{10} G_i^{11} I\{s = t - \min_k \{G_{k+1}^{11} - G_k^{11} = 1\}\} \\
& + \Gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it}
\end{aligned} \tag{4}
$$

I similarly present results from Equation 4 in Figures 13-14 in the Appendix. Again, I carry out joint tests for no difference in pre-trends. While visually the estimates appear to be much more centered around 0, most models still reject the hypothesis that there are no differing pre-trends among the different treatment groups.

Table 3: Wald Tests for Model 2

Joint Test	(1,0)	(0,1)	(1,1)
Entry All Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Entry Own Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Entry Neigh Treated	0.348	0.001	0.623
Exits All Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Exits Own Treated	0.017	No Shared Pretrend	No Shared Pretrend
Exits Neigh Treated	0.036	0.001	0.071
Expansions All Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Expansions Own Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Expansions Neigh Treated	0.166	No Shared Pretrend	0.008
Contractions All Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Contractions Own Treated	No Shared Pretrend	No Shared Pretrend	No Shared Pretrend
Contractions Neigh Treated	0.003	No Shared Pretrend	0.003

I generally find that estimates for neighbor spillover effects satisfy the shared pre-trend assumption. This implies that DID estimates for spillover effects are not invalidates, and that our post-treatment estimates are supported by a valid counter factual. Under this framework, I now wish to understand if there was a non-zero policy effect, and how long this duration existed.

As the financial crisis becomes less severe, capital is likely to ease nationally, and renormalization between treated and untreated counties may occur. As a result I try to evaluate policy effectiveness duration by conducting a mix of joint and multiple hypothesis tests. I also carry out multiple hypothesis tests for non-zero effect duration. I utilize a test for nested hypotheses proposed by Bauer and Hackl [1987]. This test controls for family-wise error in trying to evaluate multiple p-values simultaneously. To motivate this problem imagine the set of hypotheses,

$$H_0^k: \ \gamma_s = 0 \quad \forall s \in [1, \ldots, k] \tag{5}$$

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,

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

By use of Bonferroni's inequality. This test then jointly controls for family wise error of multiple tests being conducted for the no treatment effect. This test is a worst-case bound for the existence of positive policy effective duration, and basically selects and carries out the appropriate joint hypothesis in an iterative fashion.[14]

Since I reject the pre-trend assumption for the jointly pooled estimator, I provide step down tests for each of the different treatment regimes present in Equation 4.

Table 4: Step Down Tests for Non-Zero ATT Following 10 Treatment

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

Table 5: Step Down Tests for Non-Zero ATT Following 01 Treatment

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

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

Table 6: Step Down Tests for Non-Zero ATT Following 11 Treatment

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

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

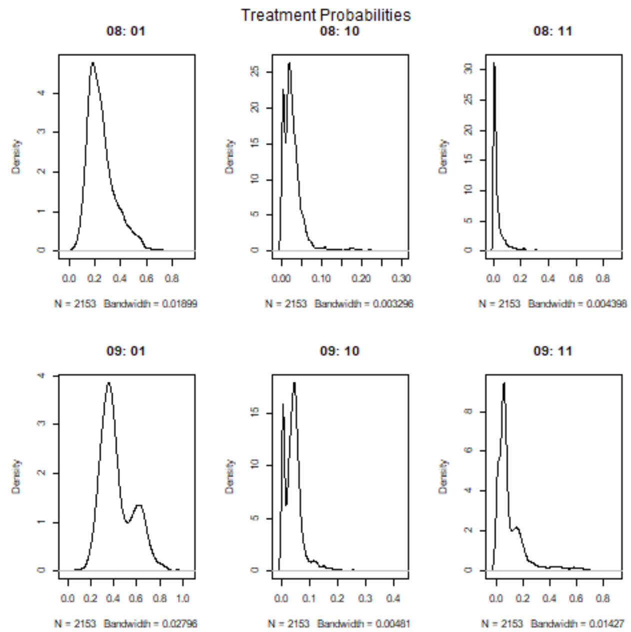
6.2.1 Instrumental Variables Estimation

A concern about identification is that treatment is correlated with still unobserved shocks, even after conditioning on the interactive fixed effects. Following Ruonan Xu [2019] I estimate a bivariate Probit for for each year instrumenting using political connections of counties. In the first period of treatment this includes whether or not any bank in a given county had a board member serving as a branch Federal Reserve chair, whether or not the counties local House representative was serving on the banking and finance committee, the share of donations to the local representative coming from Financial, Investment, and Real Estate groups, and whether or not the local House representative was a democrat. I further include the rest of my exogenous controls. In the second period I further condition on whether or not a county or a neighbor received treatment in the previous time period.

I then carry out the heteroskedastic robust variation of Sanderson and Windmeijer [2016]'s augmented F-test for multiple endogenous variables. Comparing to the Stock and Yogo [2002] tables these instruments are considered strong. Due to the first stage Probit I generate six instruments included in the first stage Bivariate Probit's. Sanderson and Windmeijer [2016] generate a conditional F test, so I look at critical values from Stock and Yogo [2002]'s single endogenous variable table. 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.

I first directly recreate my Difference-in-Difference estimates. However, similarly as above, I continue to reject pre-trend tests. As above, the main issue is that a large number of untreated, or low propensity score, counties are included in the analysis that have no value when compared to counties that have high IV values.

Figure 9: Bivariate Probit Propensity Scores



Each row represents the probability of only Own Treatment, only Neighbor Treatment, or Both Treatment, respectively.

Table 7: Wald Tests for IV Pretrend

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

No shared pretrend implies a p-value less than 0.005

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

7 Conclusion

The Capital Purchase Program provided over $200 billion to banks to shore up bank finances and ease credit constraints faced by credit worthy households and small businesses. In this paper I examine the role that counties having banks that received CPP funds had on subsequent firm entry, firm exit, employment expansion, and employment contraction. I utilize a LASSO augmented synthetic control to estimate both the direct and spillover effects of a county receiving CPP funds.

I find no evidence that counties receiving TPP funds having higher firm entry, confirm the absence of spillover effects of the program in both my preferred specification and robustness checks. I find evidence of a positive change in long run firm exit, employment expansion, and decreased employment contraction, in counties that received CPP funds. However these improvements begin long after treatment. I further showed there existed significant heterogeneity in county responses to receiving CPP funds. While the average county experienced no expansion in firm entry or employment (relative to untreated counties), nor lower firm exit or contraction, many counties saw drastically improved local business dynamics.

Following up checks show that a major source of of these benefits come from counties that share a high number of new firm entrants, followed by a large number of them failing in the same year. Removing and rerunning the model on just own-treatment status in either 2008 or 2009 produce similar results to the main specification, as does running the model on all firm entry besides for healthcare- removing countervailing effects from the Patient Protection and Affordable Care Act, manufacturing, or retail jobs. Conversely changing from levels to either log entry, log exit, or share of firms expanding employment or share of firms contracting employment removes much of the direct effects.

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

I further show that previous studies that relied on difference-in-differences techniques are likely poorly identified due to the large heterogeneity in the characteristics of counties and firms that received CPP funds relative to untreated counties. Both canonical multi-treatment and period Difference-in-Differences, both with and without an instrument, often reject shared pre-trend assumption. The data driven methods I utilize to create counter factual counties often have high mean squared error in both pre-treatment and post-treatment periods relative to permutation tests, a sign that even those methods might not be creating appropriate counter factual counties. Longer pre-treatment time series on firm dynamics and better normalization techniques might help remove some of this remaining bias.

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

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8 Appendix

8.1 Additional Tables and Figures

Figure 10: Subgroup Pre-Trends: Entry and Exit

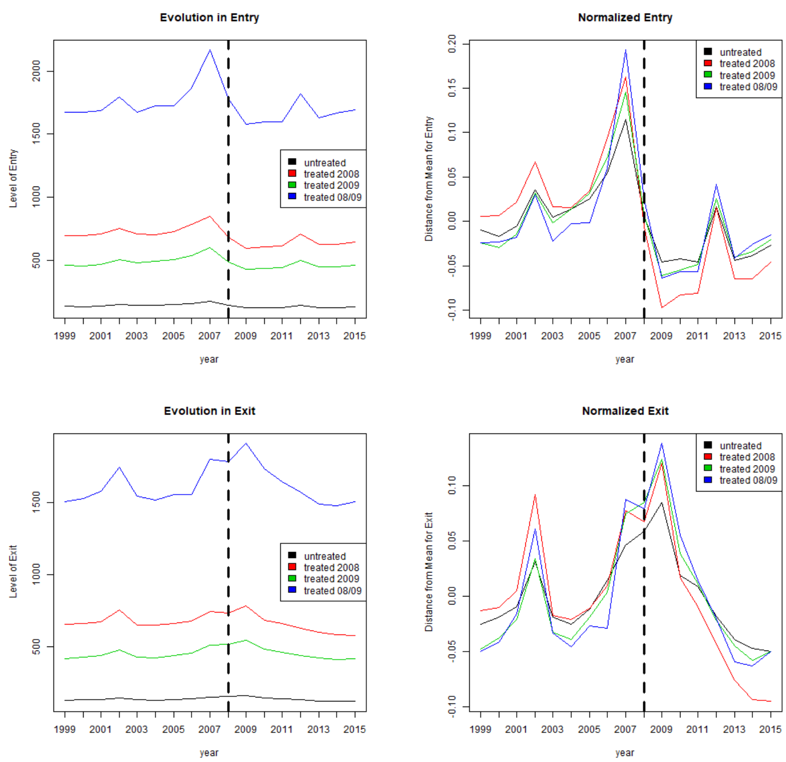

Figure 12: W(1,0) & G(1,0) Treatment Status

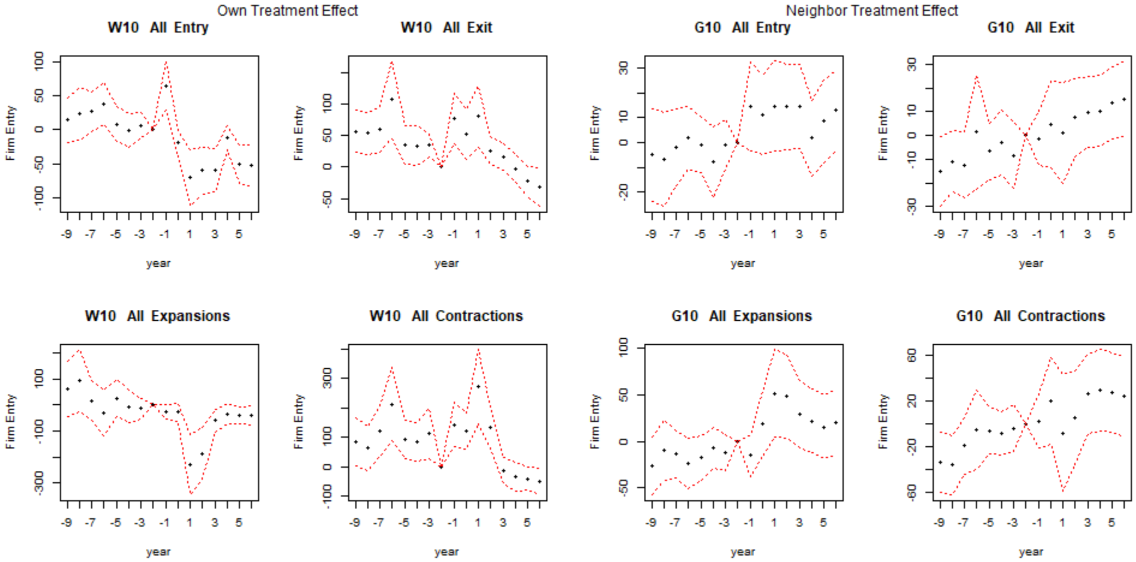

Figure 11: Subgroup Pre-Trends: Employment Expansion and Contraction

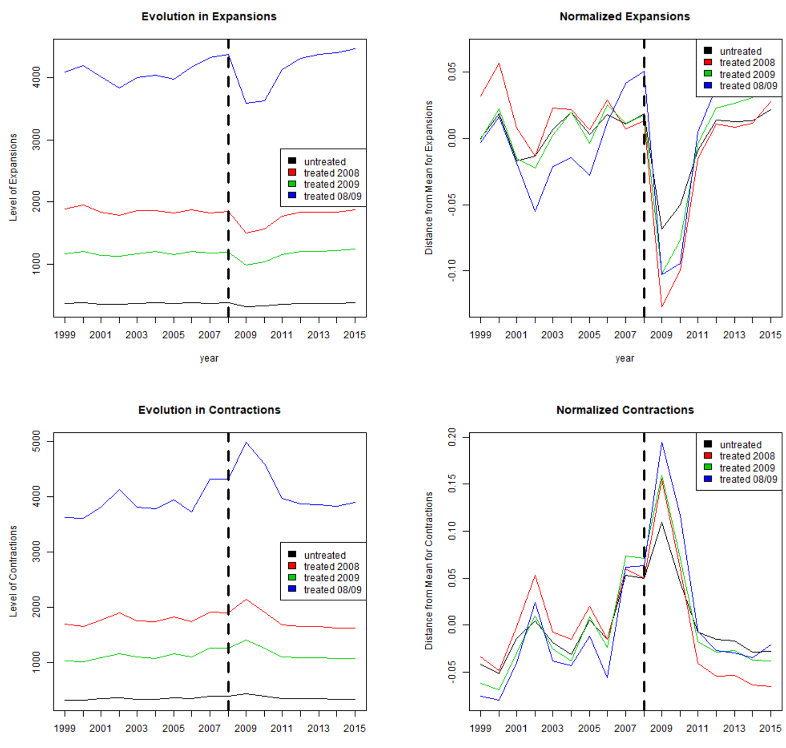

Figure 13: DID W(0,1) & G(0,1) Treatment Status

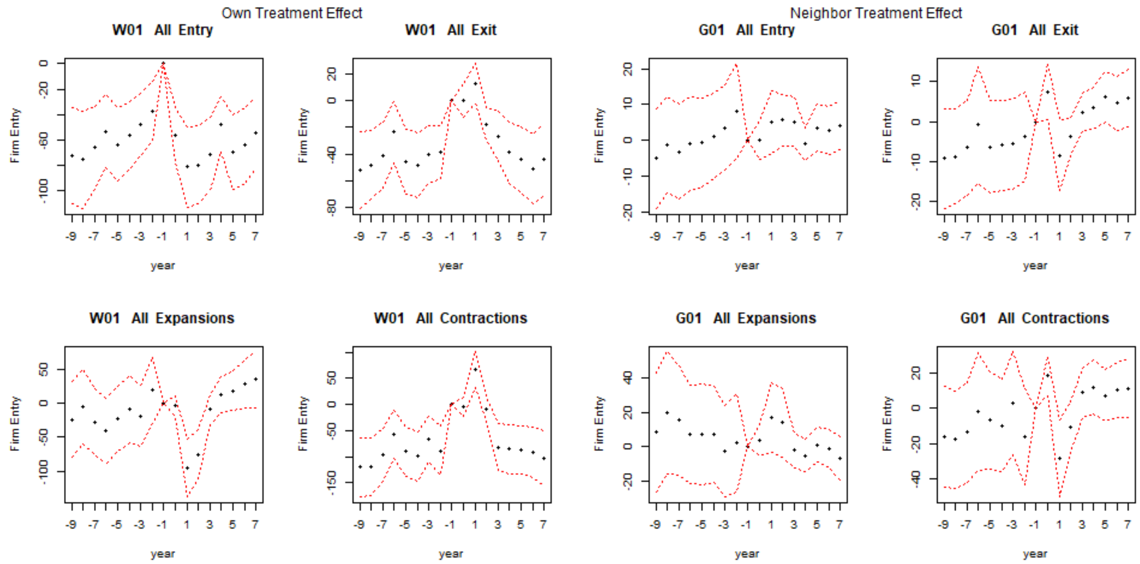

Figure 14: DID W(1,1) & G(1,1) Treatment Status