Local Effects of a Military Spending Shock: Evidence from Shipbuilding in the 1930s

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Abstract

I estimate the effect of government spending at the county level using a previously little studied 1934 naval spending program. This act aimed to build up the Navy to treaty allowances, stimulated by fears about Japanese military expansion. Using data from historical sources, I find that manufacturing output, employment, earnings, retail sales and household consumption all grow faster in the 1930s in counties hosting shipyards before 1934. An exercise to translate these results to an aggregate government spending multiplier suggest a multiplier of $2.64 during the Great Depression.

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1 Introduction

What are the effects of government purchases on local economies, especially when the aggregate economy is in a state of weakness? Normally, these effects are summarized in terms of a "multiplier," defined as the amount of extra output generated by an additional dollar of government purchases. One of the benefits of examining fiscal multipliers at the local level is that one can observe a larger number of regions, who are all subject to the same national monetary policy. This is helpful, because potentially endogenous monetary policy changes can confound the estimation of aggregate government spending multipliers. Knowledge of how purchases affect local economies can also provide insight into the transmission mechanisms of government spending on a broader scale. This is because one might expect purchases to impact the areas in which the funds are directly spent most quickly and powerfully. Spillover effects can also be important, because positive spillovers to neighboring areas are indicative of a large multiplier overall, while negative spillovers to neighboring areas suggest that the government's activity is merely inducing a reallocation of resources from one region to another. Another advantage of tackling this question at the local level is that identification of federal spending shocks might be easier. Especially in a time of war or the threat of war, it may be more plausible that the federal government is not increasing spending solely in response to local area conditions.

This paper fills a gap in the literature on the effects of federal government spending at the local level by exploiting a previously understudied spending episode, the Vinson-Trammell Act of 1934, which aimed to build the United States Navy up to treaty limitations imposed at the end of World War I, and was a response to Japanese naval expansion. Using historical sources, I am able to identify the counties receiving shipbuilding contracts, and I track the evolution of their economies throughout the 1930s. In particular, I examine the responses of manufacturing output, employment, and retail sales, among other outcomes. The timing of this act is fortuitous for me, as the 1930s were a period in which nominal interest rates were pinned to the zero lower bound throughout. This is important, because during this period monetary policy did not react to the fiscal shock with higher interest rates. Christiano, Eichenbaum, and Rebelo (2011) argue that fiscal multipliers are particularly large when there are no changes in interest rates to offset fiscal policy. In addition, Kuhn and George (2014) show that occasionally binding capacity constraints can also produce countercyclicality of the government spending multiplier. As a point of comparison, in the appendix, I also examine the local economies of areas building ships for the government during World War II, when economic capacity was more constrained.

My results show that counties hosting shipyards in 1934 (the year the Vinson-Trammell Act was passed) experienced relatively greater manufacturing output growth and relatively greater retail sales growth in the latter part of the decade, compared to counties not hosting shipyards. In particular, counties that hosted shipyards at the time of the act's passage saw an extra 12-13 percentage points of output growth over the two-year periods from 1935-1937 and 1937-1939, relative to otherwise identical counties that did not host shipyards. In addition, retail sales growth in these counties was 3-4 percentage points higher in the latter half of the 1930s. There is evidence that the naval spending spilled over into neighboring counties, boosting retail sales growth there as well. At the household level, consumers in shipyard counties spent more on consumption goods the more they were exposed to the naval spending. This result holds even when controlling for the household's income, which is consistent with the idea that labor supply and consumption were complements in the utility function. This is important, because Nakamura and Steinsson (2014) demonstrate that such complementarity could be key to generating an aggregate multiplier greater than one. What is more, these effects generally do not hold for the period during World War II, when shipbuilding activity really ramped up and when capacity constraints began to impede the economy. With regard to other economic variables, the spending did not alter education choices on the extensive margin, but seems to have had a negative effect on the resources devoted to schooling.

Of course, for the purposes of policy, what is of interest is the actual aggregate government spending multiplier. One of the drawbacks of conducting the analysis at the county level is the difficulty of "scaling up" the local results. The results of this paper are consistent with those of Nakamura and Steinsson (2014), who use a multi-region DSGE model to translate their local results to an aggregate multiplier. In the context of their model, the spending shock identified here may have had a multiplier of around 9. Although this figure is huge, it is also worth considering that in the depths of the Great Depression, with a vast amount of unused capacity and an economy relatively closed to trade, it may not be out of the question.

The rest of the paper proceeds as follows. Section 2 contains a brief literature review. In Section 3, I discuss my empirical methodology, including background information on the Vinson-Trammell Act of 1934, my method for identifying shipyard locations, the data, and my regression specifications. Results follow in Section 4, and this is followed by an attempt to interpret the baseline local results as an aggregate government spending multiplier in Section 5. Section 6 concludes.

2 Literature Review

The empirical literature on the output effects of government purchases has grown rapidly in recent years, as many national and local governments have pursued fiscal stimulus in an attempt to boost flagging economies. Still, there is little consensus on whether this spending has been a net positive or if it has rather crowded out private activity. Most papers have concentrated on aggregate government spending multipliers, with an offshoot of the literature focusing on whether these output effects depend on the condition of the economy in the period when the spending hits. See, for example, Ramey (2011), Barro and Redlick (2011), and Auerbach and Gorodnichenko (2012), among many others. Another substrand of this field has attempted to estimate government spending multipliers at the local level. In the United States, this has included multipliers on spending in the fifty states or at county level. In some studies, the source of the spending is the federal government, while in others, the source of the spending is the state government.

The literature estimating local government spending multipliers has exploited a variety of identification strategies in a handful of settings, and many have found strong positive effects on local economies, although that is not a uniform conclusion. Serrato and Wingender (2014) use population revisions after decennial Censuses to instrument for federal spending that is a function of county population, and they find a local income multiplier of 1.57. Nakamura and Steinsson (2014) utilize military procurement contracts, which indicate the particular U.S. state at the receiving end of funding, to find an output multiplier of around 1.5. Hooker (1996) undertakes a similar analysis to find that military spending cuts are particularly harmful to state economies. Chodorow-Reich et al. (2012) use expansions in Medicaid funding enacted in 2009 to estimate that for every extra $100,000 in transfers from the federal government to the states, 3.8 job-years are created, including 3.2 in the private sector. Shoag (2010) identifies state government spending shocks generated by windfalls in pension fund returns, and he finds an income multiplier of 2.12 for the years 1987 to 2008 and a multiplier of 1.43 for the period of the Great Recession. Fishback and Kachanovskaya (2010) and Fishback, Horrace, and Kantor (2005) estimate the effect of New Deal grants (instrumented by a number of political variables) on real per capita income at the state level and retail sales per capita at the state and county level. They find an income multiplier just above 1 and a positive impact on retail sales for many types of grants. Acconcia, Corsetti, and Simonelli (2014) use an Italian law which mandates the removal of local councils upon evidence of Mafia infiltration. Such council dismissals were often associated with dramatic declines in public investment, so they use dismissals as an instrument to find a local multiplier of 1.2 and a longer-term multiplier of 1.8 for provinces in Italy in the 1990s. Hausman (2013) takes a slightly different tack, examining the impacts of the unexpected early payment of the 1936 Veterans' Bonus to find that many bonus recipients quickly went out and spent their windfall.

Many papers have, however, produced dissenting views on the local effects of government spending. Fishback and Cullen (2013) demonstrate that

World War II spending at the county level did not influence many economic indicators. Of course, there are limitations to data availability in this period, and performing any analysis on the U.S. economy during World War II is necessarily dealing with an economic environment unlike any other in the nation's history. Bruckner and Tuladhar (2014) do not find a local government spending multiplier greater than one when looking at the effects of central government spending in Japanese prefectures during the 1990s, although they do find evidence that different kinds of spending produce different results, as well as stronger effects when local economies are relatively weak. Clemens and Miran (2012), taking advantage of heterogeneity in the stringency of balanced budget requirements of U.S. states, find that the multiplier on investment spending is likely less than one. Finally, Cohen, Coval, and Malloy (2011) use the ascendance of local representatives to powerful positions on influential congressional committees as an instrument for federal earmarks, and they find a significantly negative effect on corporate investment by firms headquartered in those districts. They attribute this negative impact to the government crowding out private activity.

Unlike the work of Chodorow-Reich et al. (2012) or Hausman (2013), I will be looking at the effects of government purchases, not transfers. Unlike Nakamura and Steinsson (2014), the purchases that I study take place solely during a period of severe economic weakness. Unlike Fishback and Kachanovskaya (2010), I will be able to delve to the county level, as opposed to the state level. Unlike Clemens and Miran (2012) and Acconcia, Corsetti, and Simonelli (2014), I deal with increases in government purchases, as opposed to cuts to government investment. Although my paper is not the first to exploit military shipbuilding in this era (see, for example, Thornton and Thompson (2001)), I am not aware of any others that explore its wider effects.

3 Empirical Methodology

This section will outline the process for identifying a military spending shock and estimating its local effects. First, I will describe the Vinson-Trammell Act of 1934. Then, I will demonstrate how I identify shipyard (and neighboring) counties. The third subsection will provide information on the various sets of data that I will employ, and the fourth section will detail the regression specifications.

3.1 The Vinson-Trammell Act of 1934

The Washington Naval Treaty was signed in 1922 by representatives of the United States, the United Kingdom, France, Italy, and Japan.[1] The aim of the treaty was to prevent the sort of arms race that was believed to have contributed to the outbreak of the First World War nearly a decade earlier. The treaty placed limits on the amount of tonnage that the signatories' navies could employ, as well as limits on the types of weapons that could be carried on naval vessels. In addition, many shipbuilding programs that were underway in these countries were to be halted and scrapped. The stipulations of the agreement were extended and reinforced in the London Naval Treaty signed by the same five powers in 1930.

Throughout the 1920s and the early part of the 1930s, the United States Navy did not build up to its treaty allowance. Cook (2004) reports that in the ten years after the initial Washington Naval Treaty, the United States had built more than a hundred fewer ships than any of the other signatories and a total of zero destroyers. This inactivity was due partly to greater isolationist and pacifist sentiment and partly to a lack of political will. President Hoover, for example, staunchly opposed naval expansion. This was not the case in Japan, which had built its fleet up quickly with more modern, capable ships. Some in the policy-making establishment, such as Carl Vinson, a U.S. Senator from Georgia, had begun to get nervous about Japanese intentions and started to agitate for increased naval spending. In late 1931, Japan invaded Manchuria, in clear violation of several treaties it had signed, and in 1933, it announced plans to increase spending on its navy by 25%. These concerns convinced President Roosevelt "that a longterm building program was essential if the

[1] The source for much of the information contained in this subsection is Cook (2004), Chapters 3 and 5.

navy were to keep pace with Japan." (Cook (2004), p. 87).

What would become the Vinson-Trammell Act of 1934[2] was introduced by Senator Vinson in January of 1934 and passed Congress on 20 March 1934, to be signed by President Roosevelt a week later. The bill authorized the government to build the Navy up to the country's treaty allowances. The passage of the Vinson-Trammell Act also raised expectations of future government spending, as it is listed as an exogenous spending news shock equal to about 1.5% of GDP in the series constructed by Owyang, Ramey, and Zubairy (2013) and Ramey and Zubairy (2014).[3] Although there had been some appropriations to naval shipbuilding made as part of the 1932 National Industrial Recovery Act (which appropriations were also pushed by Senator Vinson with eyes focused on the emerging Japanese threat), the spending that was anticipated as a result of this bill was much larger. Also, unlike the 1932 bill, the motivation behind the Vinson-Trammell Act was not economic revitalization.[4]

The first shipbuilding contracts awarded in conjunction with the bill were placed in August of 1934. According to *The New York Times*, "plans have nearly all been completed so that work can start, not only in private but in government yards, within a reasonable time."[5] The kinds of ships that Senator Vinson envisioned being constructed required about three years for completion

[2]Senator Park Trammell of Florida had authored a competing bill that he eventually dropped to support Senator Vinson's.

[3]The passage of the bill is not explicitly mentioned in either of these papers, but its inclusion is indicated in the narrative of the data series available on Valerie Ramey's website.

[4]Opposition to the passage of the bill came mainly from pacifists, who argued that the supposed Japanese threat was an illusion manufactured by shipbuilders so as to obtain government contracts. Cook (2004, Chapter 5) offers some specific examples. Indeed, if that was the case, it would threaten the exogeneity of this spending. Senator Vinson, the main proponent of the bill seems not to have believed in this notion. He had been involved in a special audit into aircraft manufacturers that examined whether they had made "excessive profits" from 1927 to 1933. No evidence was ultimately found, but the senator was concerned enough to push for a more formal investigation. On the possibility of the government being exploited by private firms, Senator Vinson said, "We are not going to stand by and let the Government be at the mercy of any private company; we are not going to be held up. If they're making too much, we'll put a stop to it," (Cook (2004), p. 96). In fact, the bill included a provision limiting profits on shipbuilding contracts to ten percent (Lane (1951), p.798). It seems, then, unlikely that this spending program was implemented so as to benefit shipbuilding firms.

[5]*The New York Times*, "Awards Contracts for 24 Warships," 23 August 1934.

(Cook (2004), p. 96), so one might expect that the spending beginning in 1935 and extending into 1936 would have effects until the end of the decade.

3.2 Shipyard Locations

Identifying the locations of shipyards active at the time of the bill's passage in 1934 will be key to understanding the effects of the spending. Although there is evidence of further yards opening in the latter half of the 1930s, I exclude these from my baseline analysis because of concerns that their opening may have been endogenous to the spending. The central assumption that I will make in my empirical analysis is that the counties that received the Vinson-Trammell spending did so because of pre-existing shipbuilding facilities and not because of any other local economic conditions.

My primary source for identifying shipbuilding locations around the country is the fifth part of the first chapter of Lane (1951). Further information on yard locations comes from contemporaneous newspaper sources, such as the article from *The New York Times* referenced in Footnote 9. Figure I displays the geographical county locations of the identified shipyards, and Table A.I gives a listing of the counties. The identified shipbuilding centers also include counties hosting major steel producing facilities owned by the Bethlehem Steel Company, which also owned several shipyards. The locations of these facilities are also obtained from Lane (1951). I include them on the presumption that any economic benefit as a result of this spending shock accruing to counties hosting Bethlehem shipyards would also be experienced by counties hosting the steel facilities supplying them. This is the reason for several inland counties in Pennsylvania being included in the list of shipyard counties.[6]

From examining the list of shipyard counties and eyeing the associated map, it is the case that the shipyard counties cluster around urban areas, particularly in the northeastern part of the country. Cities such as New York,

[6]Since I will also estimate whether the supposed economic benefits spilled over into neighboring counties, for each identified shipyard county, I gather a list of counties bordering it or that have strong economic links to it, as defined by the 1991 Contiguous County File, ICPSR Data Set 9835.

Boston, Philadelphia, Baltimore, Los Angeles, San Francisco, and Seattle are included,although other major cities such as Chicago, Detroit, and St. Louis are not. Admittedly, this poses a concern with regard to whether the effects that I will pick up are not rather due to, for example, relatively faster growth in urban areas. I will attempt to demonstrate that this is not the case by controlling for the percentage of each county that is urban, as well as state fixed effects. On the other hand, none of the shipyards that were open at the time of the bill's passage were located in Georgia or Florida, the home states of the senators for which it is named, which relieves any concern about spending being allocated for politically motivated reasons.

3.3 Data

The data used to conduct the analysis in this paper comes from a couple of different sources. The primary dataset is that of Fishback et al. (2011b). This dataset is an annual county level panel that covers the years from 1930 to 1940. It includes a large number of variables, of which I will make use of a smaller subset. The data set includes information on county population in 1930 and 1940 (as well as linearly interpolated figures for the intervening years). It also has information on the number of manufacturing establishments, along with the average number of employees at each establishment, manufacturing output and value added, and wage payments to manufacturing workers and average earnings. This manufacturing data is available for the years 1931, 1933, 1935, 1937, and 1939. It also includes retail sales data for the years 1933, 1935, and 1939. The manufacturing data I will use as a county level proxy for output, while the retail sales data will stand in for consumption. The data set has variables for retail and wholesale employment, wholesale net sales, and average retail and wholesale earnings for the years 1935 and 1939. Also included is the number of automobile registrations for the years 1930, 1931, and 1936. Finally, it has information on the number of tax returns filed in each county for every year in the sample.

In addition to these series, which will provide the bulk of the outcomes

I consider in the analysis, this data identifies the percentage of each county which is "urban," and has an indicator for whether each individual county is located on the Great Lakes or the Atlantic, Pacific, or Gulf coasts. These will help me to control for the urban character of each county as well as whether or not it depends greatly on maritime industries. It also has an interpolated series of New Deal spending for each county. That is, there is information on total New Deal spending over the course of 1933 to 1939 in each county, and this sum is interpolated into an annual time series using information on New Deal grants at the state level.

The second source of data to be employed is the Study of Consumer Purchases in the United States, 1935-1936,[7] which was also featured in Hausman (2013). That paper contains extensive details on this survey, but it is worth noting here that it has information on where households are located (which I use to map them to shipyard counties, counties bordering shipyard counties, or counties unrelated to shipyards), their income, their age, their race, and their expenditures on a large number of items. The survey was conducted over the course of 1935 and 1936 and is meant to capture expenditures in the preceding calendar year. There are problems with using this data, since it is certainly not nationally representative and limited to urban areas, as noted in Hausman (2013). The time span covered by the survey is at the very start of the period seeing spending associated with the Vinson-Trammell Act of 1934. Critically, I am able to identify the extent to which the household's survey year overlaps with spending on naval vessels by relying on newspaper articles reporting on the awarding of contracts. This provides crucial identification. Also, among the counties hosting active shipyards at the time of passage, only New York City and Mobile, Alabama are represented in this survey, although there are respondents living in a number of counties bordering shipyard counties. I follow Hausman (2013) in constructing my categories of consumption expenditure.

[7]ICPSR Data Set 8908.

3.4 Regression Specification

With a varied set of outcomes with differing time observations, it is necessary for me to estimate a number of different regressions. I will start by considering outcomes available in the Fishback et al. (2011b) data set. For a number of variables related to manufacturing, which have observations on the years 1931, 1933, 1935, 1937, and 1939, I estimate

$$\begin{aligned}\Delta Y_{it} = \alpha + \beta_1 Shipyard_{1934,i} + \beta_2 BordersShipyard_{1934,i} + \sum_{t=1935}^{1939} \delta_t I(Year = t) \\ + \sum_{t=1935}^{1939} \gamma_{1,t} Shipyard_{1934,i} * I(Year = t) + \sum_{t=1935}^{1939} \gamma_{2,t} BordersShipyard_{1934,i} * I(Year = t) \\ + X_i'\Omega + \eta_{it} \ ,\end{aligned} \tag{1}$$

where ΔY_{it} is the two-year growth rate in some manufacturing variable, such as real manufacturing output or the number of manufacturing establishments, $Shipyard_{1934,i}$ is a dummy indicator for whether or not county i hosted a shipyard at the time of passage of the Vinson-Trammell Act of 1934, $BordersShipyard_{1934,i}$ is a dummy variable indicating whether county i bordered a county with a shipyard in 1934,[8] I(Year=t) is a series of dummies that stand in for time fixed effects and proceed in two-year intervals, and X_i is a vector of control variables including state fixed effects, a dummy for whether the county is located on a coast, and whether the county is relatively industrialized or urban.[9] "Relatively industrialized" means that its percentage of the population employed in manufacturing before passage of the bill is greater than the national average in that year.[10] For a county's relative urban nature, I control for the percentage

[8] In cases where a county hosted a shipyard and bordered another county also hosting a shipyard, I coded the $BordersShipyard_{1934}$ variable to be 0.

[9] Given the heavy northeastern concentration of the shipyards, one might think that region fixed effects would be more appropriate than state fixed effects, but inclusion of Census Bureau Region or Division fixed effects did not impact the results.

[10] Effectively, this means the share of the population employed in manufacturing in 1933 must be above the national average.

of the county considered urban in 1930. With several of these variables being invariant over time, including the $Shipyard_{1934,i}$ and $BordersShipyard_{1934,i}$ variables, county fixed effects would lead to identification problems. At the same time, however, state fixed effects help control for the possibility that relative strength or weakness of balanced budget rules at the state level confound the results. When conducting the regression analysis, I first exclude the top and bottom percentiles of the dependent variable so as to remove outliers. Then, because I am interested in running this regression on a balanced panel, I drop any county's observations if it is missing data for any year in the sample.[11]

It is worthwhile to take a moment to consider how to interpret the coefficients from this regression. The coefficient on the term $Shipyard_{1934,i}$ is identified only by variation in the first two-year interval. Thus, one can read this coefficient as the difference between the growth rates for shipyard counties and non-shipyard counties for the years between 1931 and 1933, i.e. before the spending shock took place. The coefficients on the three interactions between $Shipyard_{1934,i}$ and the fixed effects for the intervals from 1933 to 1935, 1935 to 1937, and 1937 to 1939 are read as the difference between the growth rates for shipyard and non-shipyard counties for these respective time periods, holding everything else equal. An analogous interpretation holds for all terms with the $BordersShipyard_{1934,i}$ variable. In a sense, one can read this regression as a sort of disaggregated difference-in-difference specification.[12] Because I do not have reliable data on where exactly among the shipyard counties the spending was allocated, I use dummy variables in the regression. Thus, one can interpret the effects that I uncover as an "Intention to Treat" (ITT) effect.

One possible threat to identification is that the shipbuilding industry was well placed for a return to health after a particularly nasty few years at the beginning of the Great Depression. It is hard to rule this idea out entirely, due to the relative paucity of data before the act's passage. I can show that when

[11] The results are entirely robust to including large observations of the dependent variable and allowing the panel to be unbalanced.

[12] When a more conventional difference-in-differences specification is employed, the results are broadly similar, but I cannot observe the detailed changes over time.

I only look at the observations on manufacturing up to the year 1933, there is little evidence that counties with shipyards were performing statistically differently from other counties. As an attempt to refute this "mean-reversion" story, I also run regressions on pre-1934 data only for the outcomes for which it is available. I also pursue an alternative route using a "propensity score"-type of methodology.

A further robustness check includes a variable that captures the change in or the level of New Deal spending for each year for which I have manufacturing data. I define New Deal spending per county as the sum of grants and loans from a number of programs, for which information is available in the Fishback et al. (2011b) data set.[13] There is no annual data at the county level for New Deal spending. Fishback et al. (2011a) interpolate a county-level series for this type of aid by using the total amount of New Deal spending over the 1930s at the county level and state-level year-by-year fluctuations. New Deal spending is likely to be endogenous as the explicit purpose of the program was to help the economy emerge from the Depression. With this in mind, I follow Fishback, Horrace, and Kantor (2005) by employing a set of instruments in a Two-Stage Least Squares framework. As in that paper, my instruments for New Deal spending are the standard deviation of the share of the vote going to the Democratic Party in presidential elections from 1896 to 1928, voter turnout in the 1928 election, the log of the area (in square miles) of the county, the latitude and longitude of its county seat, and the share of the population that belonged to a church in 1926. It is not clear *ex ante* whether

[13]This includes Reconstruction Finance Corporation loans, Disaster Loan Corporation loans, Public Works Administration Nonfederal loans, United States Housing Authority loan contracts, Farm Credit Administration loans, Farm Security Administration Rural Rehabilitation loans, Farm Security Administration Tenant Purchase loans, Rural Electrification loans, Home Owners Loan Corporation loans, Federal Housing Administration Title 1 insured loans, Federal Housing Administration Title 2 insured loans, Agricultural Adjustment Administration grants, Farm Security Administration Rural Rehabilitation grants, Public Roads Administration completed grants, Public Works Administration Nonfederal grants, Public Works Administration federal grants, Public Building Administration grants, Works Progress Administration grants, other works program grants, Social Security Administration grants, United States Housing Authority Public House grants, Federal Emergency Relief Administration grants, and Civil Works Administration grants.

New Deal spending should enter the regression in log levels or in growth rates, so I try both specifications.

The next set of variables that I am interested in are those pertaining to consumption, such as retail sales data. These variables are only available in the years 1933, 1935, and 1939, necessitating a somewhat simpler specification. The associated regression equation that I estimate is

$$\begin{aligned} \Delta Y_{it} &= \alpha + \beta_1 Shipyard_{1934,i} + \beta_2 BordersShipyard_{1934,i} + \delta I(Year = 1939) \\ &\quad + \gamma_1 Shipyard_{1934,i} * I(Year = 1939) + \gamma_2 BordersShipyard_{1934,i} * I(Year = 1939) \\ &\quad + X_i'\Omega + \eta_{it} \ , \end{aligned} \tag{2}$$

where here, ΔY_{it} is the average annual growth rate since the last observation.[14] $I(Year = 1939)$ is an indicator variable for observations in 1939, and the coefficient on its interaction term with either the shipyard dummy variable or the shipyard border dummy variable is the coefficient of interest. These four years that follow 1935 are the only information I have on the possibly differential behavior of retail sales after the passage of the Vinson-Trammell Act. Like with the manufacturing outcomes, I drop the top and bottom percentiles of the distribution of the dependent variable and then also any counties missing data for one of the three years that in which I have observations.

Finally, I run regressions based on the 1935-1936 Consumer Survey. I exploit variation in the residence of the respondents (i.e., whether they live in a county hosting a shipyard or not), as well as in the extent to which the schedule year the household reports on overlaps with the initial burst of spending. Specifically, I estimate an equation of the form,

$$\begin{aligned} Y_i &= \alpha + \beta_1 Shipyard_{1934,i} + \beta_2 BordersShipyard_{1934,i} + \beta_3 Overlap_i \\ &\quad + \beta_4 Shipyard_{1934,i} * Overlap_i + \beta_5 BordersShipyard_{1934,i} * Overlap_i \\ &\quad + X_i'\Omega + \eta_i \ . \end{aligned} \tag{3}$$

[14] That is, for observations in 1935, $\Delta Y_{it} = \frac{ln(Y_{1935}) - ln(Y_{1933})}{2}$, and for observations in 1939, $\Delta Y_{it} = \frac{ln(Y_{1939}) - ln(Y_{1935})}{4}$.

Here, as before $Shipyard_{1934,i}$ and $BordersShipyard_{1934,i}$ refer to whether the respondent household lives in a county hosting a shipyard or one of its bordering counties, respectively. Y_i denotes dollars spent in the past twelve months on some consumption category. To construct the variable $Overlap_i$, I take the difference between the end of the survey year for household i and the date when the first set of contracts were awarded as part of the Vinson-Trammell Act of 1934, which, according to *The New York Times*[15], was 22 August 1934. This variable is measured in days. The assumption underlying this variable's construction is the following. The article referred to makes plain that this set of contracts awarded was the first associated with the new navy spending and that building would start "promptly." Therefore, if the household is reporting on consumption before this date, then that consumption occurred without knowing when or where the new spending would be taking place. Also, the article alludes to the fact that more contracts would be awarded later, so to the extent that the household's consumption year moves further from this date, the more one might expect it to be influenced by the government spending. By interacting this variable with whether or not the household lives in a county hosting a shipyard or near a shipyard, I can examine the differential effect experienced by households in shipyard counties exposed to greater amounts of spending relative to those who do not live in shipyard counties and those who live in shipyard counties but are exposed to smaller amounts of government spending. Following Ozer-Balli and Sørensen (2013), I demean the $Overlap_i$ variable in the interaction term. X_i is a vector of controls that include the age and age squared of the husband and wife in the household, a dummy for whether the head of household is not white, and the household's income.[16]

4 Results

Before discussing the results, I report summary statistics on a number of key outcomes that I will be examining in Table A.III. The table shows that there

[15] *The New York Times*, "Awards Contracts for 24 Warships," 23 August 1934.

[16] The regression results are robust to the exclusion of the income term.

was considerable variation in these dependent variables, with many of the standard deviations in growth rates far above the mean values. This is to be expected, given that the sample period that I examine is one of the more volatile economic episodes in the history of the modern United States. The table also illustrates the attractiveness of winsorizing the data, given the substantial outliers on both the high and low ends of the distributions. Next, I report the results of the regression analysis.

4.1 Results on Fishback et al. (2011b) Data

The data set constructed by Fishback et al. (2011b) contains a large number of variables that are of interest for this study. I will start by examining outcomes related to the manufacturing industry, for which the data has some of the best detail. Following that, I will consider retail sales outcomes, for which the analysis resembles a more conventional difference-in-differences framework, and conclude this subsection with a treatment of a number of miscellaneous outcomes.

I start by examining the results on growth in manufacturing output and manufacturing value added. Plots of the coefficients are found in Figure II. These regressions come from a specification of the regression with a full set of control variables, excluding New Deal grants and loans. Results from a regression without controls are very similar. From the plot, one can see that manufacturing output in counties with shipyards grew over thirteen faster in the two years to 1937 than they otherwise would have been expected to. This difference is significant at the one percent confidence level. This is followed by growth of nearly identical magnitude and significance in the following two year period to 1939. The figure for manufacturing value added tells a very similar story. For neither outcome do I see significant effects in bordering counties. The sum of the extra growth in shipyard counties between 1933 and 1939 is 32%, with an associated p-value of 0.053. If I only consider the extra growth from 1935 to 1939, the sum is 28%, significant at the one percent level. No significant effects are seen for border counties in Figure A.II.

The results of these regressions imply that the effects of the spending on local economies' manufacturing output and value added were extremely large. This is interesting in light of the fact that the entire economy was in a very dire state at the time the spending bill was passed. By including time fixed effects, I am able to disentangle the effects of spending on the treated counties from a more general tendency on the part of the entire United States to recover from the trough of the Depression. It is also interesting that there seems to have been no significant effect on the manufacturing output of nearby counties, although the signs of the coefficients are positive (in the latter part of the decade). At least with regard to areas in very close proximity, it is not apparent that the large output increases in shipyard counties drew resources away from their neighbors. Below, I will consider how these effects on output and value added may have also had an impact on employment, consumption, and other variables.

Table I contains estimates from a battery of robustness checks applied to the baseline regressions for manufacturing output. The first concern is that concurrent with this increased spending on warships was the New Deal spending program instituted by the Roosevelt Administration. Many of the programs associated with the New Deal were transfer payments, loans, and subsidies (not, as in the case of the Vinson-Trammell Act studied here, purchases of goods and services. In any event, it is beyond the scope of this paper to evaluate the effectiveness of the New Deal in stimulating economic activity. My only concern is that, for some reason, New Deal spending may have been systematically allocated to areas also likely to have shipyards. It is not clear, *ex ante*, whether when controlling for New Deal spending, the spending should be specified in log levels or in log differences, especially considering the biannual nature of my observations on manufacturing variables.[17] Therefore, I try both specifications, as well as one that controls for the sum of New Deal spending over the two year interval. What is clear is that New Deal spending

[17]Specifying the New Deal spending in log levels seems to be the more natural approach, given the temporary nature of the programs, but this risks throwing out information on spending that took place in the intervening year.

is endogenous, as it was allocated to areas suffering from weaker economic activity. I follow Fishback, Horrace, and Kantor (2005) then in using an instrumental variables approach. The instrument set for total New Deal grants and loans is that described in Section 3.4, as well as state fixed effects.

The first column of Table I contains the baseline specification already reported. The next three columns demonstrate that the inclusion of New Deal grants and loans do not qualitatively affect the results, and in the case where New Deal spending is specified in levels (over intervals of one or two years), the results do not change very much at all. Thus, I can conclude that the positive effects on manufacturing output that I am finding are due to the shipbuilding program and not to simultaneous New Deal payments.

In the baseline estimation, standard errors are clustered at the state level and I use state fixed effects. The next two columns of Table I consider whether or not the baseline findings are sensitive to these specification choices. The fifth column of the table demonstrates that clustering the standard errors at county, rather than state, level leaves the point estimates unaltered and the significance levels nearly so. The same outcome is the case when regional fixed effects are substituted for state fixed effects. One may be concerned that the heavy Northeastern concentration of the shipyards still operating in 1934 is partly driving the estimated effects, but it is clear that this is not an issue.

A reasonable question to ask is whether it is not the case that counties that are home to shipyards are not in some respect fundamentally different from other counties. That is, it may not be appropriate to pool these relatively urban, highly industrialized areas with more rural, sparsely populated local economies. In an effort to address this concern, I undertake the following exercise, which is similar to a propensity score-type analysis. I first run a cross-sectional regression, in which the dependent variable is the presence of a shipyard in 1934 and the independent variables are state fixed effects, location on a coast, the percentage of the county that is urban, and whether or not it is highly industrialized. I then sort the counties by the fitted values from this regression and limit the sample to only the top 25% by this "propensity score." I then rerun the baseline regression on this smaller, theoretically more homogeneous sample. The results are in the seventh column of Table I (the one labeled "Propensity Score 1"), where it is apparent that even among like counties, those hosting shipyards see significantly faster growth in the latter part of the 1930s.

I conduct a further robustness check by examining whether counties with heavy concentrations in other industries see a similar time path of output and retail sales over the 1930s. The results of this experiment can be found in the first three columns of Table IV. To make the regressions in this experiment comparable to those evaluating the outcomes of shipyard counties, I exploit the fact that the Fishback et al. (2011b) data set gives the number of employees in a variety of industries as of 1930. I divide the workforce in each industry by the population in the county and then rank each county by the industry's share of the population. Because I have 26 shipyard counties, I code the top 26 counties in each industry with a dummy variable indicating them as having a heavy concentration of that industry. Then, I repeat the regression of Equations 1 replacing the $Shipyard_{1934}$ dummy variables with the dummy variables for each of the industries that I consider. One can interpret these as a sort of placebo test. It is not obvious that any of these industries were explicitly subject to a government spending shock, so, on balance, there should not be any significant difference in output. At the least, they should have sequences different from those of shipyards.

Table IV gives the results for the output growth regressions. As can be seen in the table, for many industries, the effect is insignificant in all three years considered. No industry sees a pattern that matches the trajectory of shipyards exactly (with large significant increases in the last two biannual periods in the decades). Even those that do see significant increases tend to be those that would be related to shipbuilding, such as iron, lumber, and rubber.[18] Thus, it is clear that shipyard counties see a unique combination of

[18] One surprising result of this exercise is the really poor performance seen by counties for whom cotton was an important industry. This is likely due to policies associated with the Agricultural Adjustment Act of the New Deal, which incentivized farmers not to plant and may have had very negative effects on other industries in those counties as well. I thank Ray Mataloni at the Bureau of Economic Analysis for raising this possibility.

effects on output and retail sales that I conclude is due to the sizable shock to demand emanating from the government starting in 1934 and 1935.

The number of treated counties is relatively small, and this might produce worries that the results are driven by particularly large responses in one or two shipyard counties. In Table V, I attempt to address this concern by dropping individual shipyard counties, one by one, from the regression equation. Each column in the table reports the coefficient on $Shipyard_{1934,i}$ interacted with the fixed effect for the indicated year. From the table, it is clear that the results are robust to dropping any one individual shipyard county from the sample.

Before moving on to other outcomes, it may be important to demonstrate that the results found so far are not due to mean reversion. That is, I would like to argue against the notion that the positive effects on manufacturing and consumption reported above are due solely to the natural recovery of the shipbuilding industry and its environs. To be sure, this is an extremely difficult story to rule out, especially considering the relative paucity of data available to me before the passage of the Vinson-Trammell Act. I do attempt to argue against this explanation in a couple of different ways. Table III gives the results from regressions including only data before 1934.[19] For most of the outcomes under consideration, this reduces to the two-year period from 1931 to 1933, although for manufacturing employment and retail sales per capita, I can also include the two-year period from 1929 to 1931. The table shows that there is only weak evidence (seen in coefficients significant at the ten percent level for manufacturing value added and wage payments) that shipyard counties were doing especially badly before the passage of the bill. This is inconsistent with the idea that they were subsequently "due" for a stronger-than-average recovery.

In the last column of Table I, I conduct another propensity score-type analysis, in which I include, along with the variables mentioned above, the

[19] As can be very clearly seen from the table, I include all of the outcomes that I am considering in this section of the paper, although the main results on these outcomes will be discussed formally below.

manufacturing output growth rate from 1931 to 1933. In this way, I hope to limit the sample not only to counties similar to shipyard counties in terms of their demographic and structural characteristics, but also to those that had a similar experience economically in the last observed two-year period wholly previous to the passage of the bill. In this regression, it is clear that the signs and magnitudes of all of the coefficients are roughly the same as in the baseline estimation, but the significance is weakened somewhat, especially for the two years from 1935 to 1937. Still, manufacturing output in shipyard counties grew nearly 12% faster than in other counties (significant at a confidence level of five percent) from 1937 to 1939, even when limiting the sample to areas that had similar economic dynamics leading up to the authorization of the spending program.

I turn now to results on some other manufacturing outcomes. Looking at total manufacturing employment in Figure II, the point estimates on the three post-1934 interaction terms are all positive, but they are imprecisely estimated. The p-values on the interaction terms with the 1935 and 1937 fixed effects range from 0.11 to 0.15 for shipyard counties. There is no significant effect on bordering counties. It is interesting that output should be so positively effected, while the effect on employment is more muted. I will use the next series of graphs to try to untangle why this is so.

Figure II illustrates that total wage payments by manufacturers were positively affected by the spending in shipyard counties. Again, no significant effect is discernible in bordering counties (See Figure A.II). The magnitude of the effect on wage payments is similar to that on manufacturing output, and all three post-1934 interaction terms are significant at the fiver percent level. When I look at the results for average earnings per manufacturing employee (Figure III), I can see that the post-1934 interaction terms all have positive point estimates. In bordering counties, this positive estimate is statistically significant for 1935, and for shipyard counties, it is significant for 1939. Therefore, the significant effect on wage payments that I observe must be due to some combination of firms hiring more workers and paying their existing workers more.

Figure III provides an additional layer of detail. Here, one can see that the effect of the spending bill on manufacturing establishments was negative. This negative impact is significant at the ten percent level in the two year period to 1935 and has a p-value of about 0.11 in the two year period to 1937. This decline in the number of manufacturing firms is accompanied by a rise in the average number of employees per firm that is strongly significant (see Figure III). Additionally, one can see from Figure III that manufacturing output per worker also grew significantly faster in shipyard counties than elsewhere from 1937 to 1939 (again, with little significant impact on border counties).

Therefore, the data reveals a story in which the spending on ships has a negative impact on the number of firms, possibly through higher wages, while surviving firms are larger and more productive (at least with regards to labor productivity). The increased hiring of the existing firms is offset by a decline in the number of firms, muddying the effect on total employment. The result seems to be modestly higher employment with modestly higher earnings per worker, causing a rise in total wage payments and having a negative impact on the number of establishments. Although a detailed examination of the effect of this aggregate demand shock on the industrial organization of the affected counties is beyond the scope of this paper, these firm distribution dynamics are interesting and merit further research.[20]

My results suggest an increase in employment in shipyard counties. If I see an increase in consumption as well, this finding would be consistent with the assumption of nonseparable preferences, such as complementarity between consumption and labor. Although I do not have data on consumption at the county level for this period, I do have evidence on retail sales. Retail sales are by no means a perfect proxy for consumption, but they have been used for this purpose in previous studies, such as Ostergaard, Sørensen, and Yosha (2002), Fishback, Horrace, and Kantor (2005), Shoag (2010), and Romer and Romer

[20]Kehrig (2015), for example, builds a model intended to explain the observation that in recessions, dispersion in productivity among firms becomes greater as all firms, even productive ones, use resources less efficiently. As a result of the (positive) shock I study, the number of firms declines and the survivors use more labor more efficiently, so it appears, at first glance, that these results are consistent with the model of Kehrig (2015).

(2014). The results from estimating Equation 2 can be found in Table II.

The table shows a positive impact of a shipyard's presence on retail sales growth in a county.[21] The coefficient on the $Shipyard_{1934,i}$ dummy variable is 0.038, and is significant at the one percent confidence level. What is more, this positive effect on retail sales also spilled over into bordering counties, where the coefficient is 0.045 and is also significant at the one percent level. This is an interesting result in that an aggregate fiscal multiplier greater than unity should involve positive effects even outside the area that directly receives the spending. This is also found in the international context of Auerbach and Gorodnichenko (2013b). These results also hold up in an instrumental variables regression that includes New Deal spending (see the additional columns in Table II) and when shipyard counties are dropped from the regression on an individual basis (see the results reported in the last column of Table V. In addition, when I consider counties with high concentrations in other industries, for very few of them does the same pattern emerge (see the last column of Table IV). Therefore, the data reveals the complementarity between labor and consumption implied by nonseparable preferences and necessary for the large aggregate multiplier suggested by the Nakamura and Steinsson (2014) model. This is important not least because they are not able to explicitly test for this complementarity since they do not have reliable consumption data. In this sense, my results support those of Nakamura and Steinsson (2014) by estimating results for output that are similar to theirs and providing direct evidence for nonseparable preferences, an assumption critical to their model.

4.2 Results from Consumer Survey

For the last set of regressions with 1930s data, I consider the consumption habits of households living in counties home to shipyards in 1934. I follow Hausman (2013) in making use of the Study of Consumer Purchases in the United States, 1935-1936, an early attempt by the government to gain an understanding of individual consumption behavior. It is an imperfect measure

[21] The results for per-capita retail sales growth, data for which are present in Fishback et al. (2011b), are very similar.

of consumption in many ways,[22] but this survey ought to provide at least some insight into whether households living near shipyards were able to consume more as a result.

Table VI gives the first set of regressions of Equation 3. The first column of the table demonstrates that, on average, consumption in shipyard counties is significantly greater than in non-shipyard counties. Though this number is stark, it does not, in itself, carry much information, because it does not say anything about whether consumption increased as a result of the naval spending. Similarly, the coefficient on the variable measuring the number of days overlapping the household's survey year and the time since the announcement of the first contracts awarded suggests that overall consumption throughout the country began to rise later in the survey period, but it is not possible to attribute this to the Vinson-Trammell spending. On the other hand, the significant coefficient on the interaction between the shipyard indicator variable and the number of days overlapping is quite informative. It implies that for every day more than the national average that a particular household's survey year overlapped with the Vinson-Trammell spending when they lived in a shipyad county, they consumed an extra \$2.33 relative to households living in a non-shipyard county. This coefficient is significant at the five percent level. This is on top of the extra \$0.93 per day that they consumed relative to their neighbors whose survey year overlapped less with the spending. Although the signs of the coefficients when income is regressed on the same equation are the same, they are not significant.

It is worthwhile to put this result into context. A household living in a shipyard county spends \$2.33 per day (relative to the average) that they are exposed to the shipbuilding program. The median number of extra days of exposure (again, relative to the average) is 11, implying that the median household with a greater than average exposure to the program spends an extra \$25.63 (\$2.33 per day $\times$ 11 days) in their survey year. This translates to about an extra \$325 in 2009 dollars. Thus, the extra spending is large enough

[22]See the detailed description in Hausman (2013) or Section 3.4 above. Also, the spending categories discussed below follow directly from the definitions in Hausman (2013).

to be significant, but it is not an implausible jump in consumption.

The first column in the first panel in Table VII shows that this is not merely due to a relaxing of the household's budget constraint. The regressions in this table include income as a right hand side variable. For total consumption, the coefficient on the interaction term between living in a shipyard county and the overlap between the survey year and the spending barely changes. Consumption rises by an extra \$2.34 per day even holding income constant. There are two possible explanations for this. It could be that households know that further spending is on ships is on the way as made clear in the newspaper article already mentioned. Thus, their expectations for higher income in the future are driving higher consumption now. It is unclear how much weight to give this explanation given the depressed economic environment and the parlous state of the banking sector at this time. The other explanation could be, as argued above, that labor supply and consumption are complements in the utility function, and the increased employment in shipyard counties is causing an increase in consumption as well.[23]

The rest of Table VII gives a more detailed breakdown of the type of spending that consumers were increasing. The most significant effects are on housing operation, medical care, recreation, and food. Interestingly, spending on education declines significantly by \$0.35 per day of overlap. Although the census regressions in Appendix A.4 do not show any significant change in whether children were attending school, it does seem that, on the intensive margin, they were investing less in schooling. This would be consistent with a story in which the increased public spending raised the opportunity cost of education and made working a more viable alternative for younger agents.

[23]The coefficients on variables relating to households living in counties bordering shipyards were almost all insignificant, so I do not report them to conserve space.

5 Scaling the Local Multiplier to the Aggregate Level

In the literature, it is the aggregate government spending multiplier that is often of greatest interest. Local government spending multipliers may not adequately convey information about general equilibrium effects that could cause the aggregate multiplier to fall below unity even as a dollar of spending in a given county generates more than a dollar of output in that county. If output in counties that do not receive spending (or that have spending taken away) falls by more than the lost government purchases, these negative effects could, in the aggregate, outweigh the booms experienced by areas that receive government spending. For example, I show in Appendix A.4 that the spending program compelled a movement of individuals into bordering counties. In this section, I will attempt to take the results that I have presented thus far and interpret what they imply for the government spending multiplier that is often estimated in the literature on fiscal policy. The first exercise will be to see what the model of Nakamura and Steinsson (2014) implies for my results.

To do so, I must alter my baseline regression so that it looks a little more like that estimated in the empirical section of Nakamura and Steinsson (2014). I first observe that Ramey and Zubairy (2014) estimate the Vinson-Trammell Act spending at about 1.5% of 1933 nominal GDP, which was about 57.2 billion dollars. This implies a spending program of about 858 million dollars. It is implausible to assume that the spending was distributed evenly among all the shipyard counties, but, for the purposes of this exercise, I will do so, since I cannot well defend any other allocation assumption without more detailed data. In Section 3.2, I identify 27 shipyard counties, but I do not have manufacturing data for Newport News, Virginia, so I will assume that the other 26 counties split the spending equally among themselves. This obviously raises potential problems, as the regression will be understating the effects of spending in counties that received less than average, while overstating the effects of spending in the counties that received more than average. Add to this the fact that, if any funds were allocated to Newport News, then the regression is now distributing those funds elsewhere, thus potentially further understating the effects of spending overall. Again, however, I do not mean this to be a formal multiplier estimate, but rather to see what the model of Nakamura and Steinsson (2014) implies for this data.

I also would need to scale the amount of spending by overall output in order to match the regression of Nakamura and Steinsson (2014). Since I do not have overall output at the county level for this time period, I create a rough measure by scaling manufacturing output in 1933 by the percentage of the population employed in manufacturing in that year. While this likely introduces further possible measurement error into the hypothetical regression, it is the best option available to me. I rerun Equation 1, substituting the per-county amount of spending scaled by overall output in the county for the $Shipyard_{1934,i}$ dummy variable and the $BordersShipyard_{1934,i}$ dummy variable in the interaction terms. The results of this regression are found in Table VIII. This table shows that, if the ship purchases were distributed evenly across the shipyard counties, the additional manufacturing output over the course of 1933 to 1939 that could be attributed to them summed to 2.18 dollars for every dollar spent by the federal government on ships. This scales up to a multiplier of 2.64.[24] This is the "Open Economy Relative Multiplier" of Nakamura and Steinsson (2014).[25][26]

Nakamura and Steinsson (2014) consider develop a model in which re-

[24]That is, each coefficient $\beta_t \; for \; t = 1935, 1937, 1939$ is multiplied by the inferred growth rate of output over the period from 1933. For example, to interpret β_{1935} as a "multiplier," (β^M_{1935}) I calculate

$$\beta^M_{1935} = \beta_{1935} \times \frac{Y_{1935}}{Y_{1933}} = \frac{\Delta(Y_{1937} - Y_{1935})}{Y_{1935}} \times \frac{Y_{1933}}{\Delta Shock} \times \frac{Y_{1935}}{Y_{1933}}. \quad (4)$$

I follow a similar process for β_{1937} and β_{1939}, with Y_{1937} and Y_{1939}, respectively, substituting for the numerator in the final term of the expression.

[25]In their paper, the open economy relative multipliers on total output range from 1.4 to 1.9.

[26]Of course, this extra 2.64 dollars in manufacturing output may have crowded out some other kind of output, but the data is not capable of revealing this explicitly. For this exercise, I will assume that no crowding-out or crowding-in results from this extra manufacturing output. Results below on relative changes in the industrial composition in shipyard counties suggest no crowding in or out.

gions within a monetary union are subject to differential government spending shocks.[27] That is, they examine how the aggregate economy will respond when only one region in their model economy is subject to an increase in government spending. They consider several different specifications of their model. For my purposes, the one that is likely to be most relevant is that where there is nominal price rigidity, nominal interest rates are held constant by the monetary authority (because rates were at zero during the Great Depression), and there is complementarity between consumption and labor in the representative agent's utility function. This last point is supported by my empirical results that show that manufacturing output and retail sales rose simultaneously in shipyard counties in response to the Vinson-Trammell Act and that individual households exposed to the spending spent an extra \$2.33 per day that they were exposed in spite of the fact that their incomes had not yet risen. The results of this specification of their model can be found in the third and fourth rows of Table 7 in Nakamura and Steinsson (2014). When the government spending shock is relatively short-lived, the model implies a local government spending multiplier of \$2.04, which is not very different from my empirical finding of \$2.64. In this case, Nakamura and Steinsson (2014) find that the aggregate government spending multiplier implied by a local multiplier of this magnitude is 8.73. That would suggest that my empirical results would suggest a multiplier at least this large.

Of course, this figure is huge, and I am not aware of any aggregate multipliers estimated in postwar data that come very close to this. That said, as implausible as such a large multiplier might be in the context of the modern postwar United States economy, it may not be so incredible for the 1930s, when the economy was experiencing an extremely large degree of slack[28] and it was much less open to international trade (and likely even intra-national trade).

Some may even consider the \$2.64 figure as a decent approximation to the

[27] A brief summary of the model can be found in Appendix B.

[28] According to the data set accompanying the work of Ramey and Zubairy (2014), the unemployment rate was never below 12% between 1934 and 1940 and in some periods, it was higher than 20%.

aggregate multiplier. This number is also large according to modern theory and empirics, but it is much closer to the standard range than something between eight and nine. For this multiplier to approximate the actual amount, however, one would have to take very seriously the idea that there were no spillovers, positive or negative, in counties not hosting shipyards. Further, one would have to assume that, although the tax burden was increased in counties not playing host to shipyards, this did not alter the economic behavior of these counties, which does not seem like a palatable assumption to make.

6 Conclusion

In this paper, I have contributed to the study of government spending multipliers at the local level by considering in detail the effects of the Vinson-Trammell Act of 1934, a bill that facilitated the purchase of a substantial number of naval vessels in response to military expansion by Japan and in order to build the United States Navy up to treaty provisions. Using a combination of historical sources and contemporary news media, I am able to identify counties that hosted shipyards before the passage of the act. I combine this with county level data on various economic indicators in the 1930s to investigate the effect of this spending bill on local economic outcomes.

I find that counties that hosted shipyards in 1934 experience significantly faster growth in manufacturing output and value added. Total manufacturing wage payments are also significantly positively impacted, with this likely composed of higher employment and higher average wages per worker. This combination seems to have favored larger firms and negatively affected the number of manufacturing firms in each county. Retail sales growth grew significantly faster in these counties as well, lending support to the use of a model with complementarity between labor and consumption in the utility function. These results are not due to faster population growth and they are robust to the inclusion or exclusion of a number of control variables, including spending associated with the New Deal. By considering a consumer survey that was coincident with much of the spending, I find that households living in shipyard counties spent upwards of two dollars a day more for each day that they were exposed to the government spending.

Results reported in the appendix are suggestive of the notion that such positive effects may not have lasted through the Second World War. This supports the idea that government spending multipliers may be higher when nominal interest rates are pinned to the zero lower bound, as in Christiano, Eichenbaum, and Rebelo (2011) or Kuhn and George (2014), or when there is relatively more slack in the economy.

When attempting to scale these results into an aggregate government spending multiplier, guided by the general equilibrium model of Nakamura and Steinsson (2014), one obtains a local multiplier of about $2.64, which may even translate to a fmuch larger figure, depending on the specification of the model. Still, it seems that more research is needed for translating such local multiplier estimates into the aggregate government spending multiplier that most policymakers are interested in.

Although this study has caveats, not least the fact that I do not have hard data on an annual basis that describes the amount of spending in each county, and that I rely on imperfect proxies to identify where the money was likely to be spent, I believe that it is the first to examine local government spending multipliers on purchases (as opposed to transfers) before World War II (when aggregate data collection was harmonized to a lesser degree), comparing the effects in times when capacity was highly utilized and when there was a great deal of slack in the economy. Also, I am able to roughly translate my estimates to an aggregate government spending multiplier (which may be as high as between 7 and 9). Thus, this paper provides evidence consistent with the segment of the literature finding that federal spending can have stimulative effects in local economies.

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Table I: Effect of Shipbuilding Program on Manufacturing Output: Robustness Tests

	Baseline	New Deal (Levels) (2SLS)	New Deal (Changes) (2SLS)	New Deal (Two-Year Sum) (2SLS)	County Cluster	Region FE	Propensity Score 1	Propensity Score 2
$1935 \times Shipyard_{1934}$	0.068	0.066	0.259	0.060	0.068	0.068	0.062	0.014
	(0.049)	(0.050)	(0.190)	(0.056)	(0.049)	(0.049)	(0.058)	(0.059)
$1937 \times Shipyard_{1934}$	0.130^{***}	0.127^{***}	0.341^{**}	0.113^{**}	0.130^{***}	0.130^{***}	0.117^{**}	0.080
	(0.040)	(0.041)	(0.150)	(0.045)	(0.044)	(0.040)	(0.046)	(0.050)
$1939 \times Shipyard_{1934}$	0.128^{**}	0.125^{**}	0.368^{**}	0.121^{**}	0.128^{***}	0.128^{**}	0.157^{***}	0.119^{**}
	(0.053)	(0.055)	(0.169)	(0.061)	(0.045)	(0.053)	(0.056)	(0.056)
Num. obs.	6420	6420	6420	6399	6420	6420	2184	1608

Each column reports the coefficients on the $Shipyard_{1934}$ variable interacted with the time fixed effect indicated by the row heading for a given specification of the regression indicated by the column heading. More detailed descriptions of each robustness specification are given in the text. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table II: Effect on Retail Sales Growth of Hosting or Bordering a Shipyard

Independent Variable	Baseline	New Deal Changes (2SLS)	New Deal Levels (2SLS)
$Shipyard_{1934}$	-0.026^{***}	-0.025^{***}	-0.034^{***}
	(0.007)	(0.007)	(0.012)
$I(Year = 1939) * Shipyard_{1934}$	0.038^{***}	0.037^{***}	0.037^{***}
	(0.010)	(0.010)	(0.010)
$BordersShipyard_{1934}$	-0.026^{***}	-0.026^{***}	-0.034^{***}
	(0.007)	(0.007)	(0.009)
$I(Year = 1939) * BordersShipyard_{1934}$	0.045^{***}	0.045^{***}	0.042^{***}
	(0.012)	(0.012)	(0.013)
$I(Year = 1939)$	-0.074^{***}	-0.077^{**}	-0.073^{***}
	(0.007)	(0.037)	(0.006)
Observations	5282	5286	5286
R-Squared	0.342	0.341	0.318

The table gives coefficient estimates from regressions of the average annual change in real retail sales on dummy variables for $Shipyard_{1934}$ and $BordersShipyard_{1934}$ and the interaction of these dummy variables with year fixed effects. Standard errors clustered at state level are in parentheses. $^{***},^{**}, and^{*}$ indicate significance at the 1,5, and 10 percent levels, respectively.

Table III: Pre-Vinson-Trammell Act Outcomes

Outcome	$Shipyard_{1934}$	$BordersShipyard_{1934}$
Δ Number of Manufacturing Establishments	−0.024	−0.002
	(0.025)	(0.017)
Δ Manufacturing Employment	0.020	0.032
	(0.024)	(0.020)
Δ Average Employees per Manufacturing Firm	−0.040	−0.006
	(0.054)	(0.032)
Δ Manufacturing Output	−0.071	−0.000
	(0.051)	(0.045)
Δ Manufacturing Wage Payments	-0.073^{*}	−0.010
	(0.038)	(0.034)
Δ Average Earnings per Manufacturing Employee	−0.010	−0.008
	(0.026)	(0.013)
Δ Manufacturing Value Added	-0.095^{*}	0.004
	(0.048)	(0.045)
Δ Retail Sales per capita	0.001	0.002
	(0.005)	(0.005)

This table gives coefficients on $Shipyard_{1934}$ and $BordersShipyard_{1934}$ from regressions on each outcome including only data before 1934. Standard errors clustered at state level are in parentheses. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table IV: Effect on Output and Retail Sales of Being a County with a High Concentration in Other Industries

Industry	(Mfg)1935	(Mfg)1937	(Mfg)1939	Retail
Shipyards	0.068	0.130***	0.128**	0.038***
Coal	0.057	0.040	0.139	0.009
Oil and Gas	−0.020	0.089	−0.009	−0.008
Other Mining	0.181	0.104	0.049	−0.010
Mineral Extraction	0.146	0.137***	−0.170	0.004
Chemicals	−0.044	−0.008	−0.058	0.038**
Cigars	−0.049	−0.062	−0.048	0.007
Glass	0.046	0.163*	0.046	0.048**
Bread	0.075	−0.008	0.028	−0.002
Meat	0.224***	0.039	0.058	0.027*
Automobiles	0.198***	0.159***	−0.102*	−0.023**
Iron	0.225***	0.272***	0.021	0.001
Metals	0.103	0.199***	0.086	0.016
Planing Mills	−0.003	0.025	−0.194**	−0.004
Lumber	0.117**	0.197**	0.068	−0.016
Boots and Shoes	−0.032	0.031	0.049*	0.011
Printing, Publishing, and Engraving	0.093	0.026	0.064	0.028
Pulp and Paper	−0.054*	0.048	−0.073	−0.007
Cotton Textiles	−0.519***	−0.270***	−0.373***	0.056***
Rubber	0.088***	0.066***	0.091***	0.020

Each row in the table reports coefficients on a dummy variable indicating that the county has is in the top 26 for the whole country in employment per population in the given industry interacted with the year indicated by the column heading. All regressions include state fixed effects, a dummy for whether the county is situated on a coast, the proportion of the county considered "urban" in the 1930 census, and whether the county is "industrialized." Standard errors are clustered at the state level. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table V: Sensitivity of Output and Retail Sales Results to Exclusion of Individual Shipyards

Sample	(Mfg)1935	(Mfg)1937	(Mfg)1939	Retail
All Counties	0.068	0.130***	0.128**	0.038***
New London, CT	0.077	0.14***	0.138**	0.037***
Sagadahoc, ME	0.058	0.113***	0.104**	0.038***
Norfolk, MA	0.066	0.134***	0.123**	0.038***
Suffolk, MA	0.072	0.133***	0.128**	0.037***
Rockingham, NH	0.055	0.117***	0.112**	0.040***
New Castle, DE	0.077	0.128***	0.136**	0.038***
Camden, NJ	0.065	0.133***	0.118**	0.037***
Hudson, NJ	0.067	0.126***	0.124**	0.038***
Cambria, PA	0.075	0.113**	0.146***	0.033***
Dauphin, PA	0.059	0.124***	0.129**	0.038***
Delaware, PA	0.079	0.143***	0.142***	0.039***
Lackawanna, PA	0.081	0.143***	0.128**	0.041***
Lehigh/Northampton, PA	0.065	0.120***	0.126**	0.039***
Philadelphia, PA	0.068	0.134***	0.130**	0.039***
Norfolk, VA	0.034	0.127***	0.118**	0.038***
Newport News, VA				0.040***
Mobile, AL	0.080	0.140***	0.130**	0.036***
Charleston, SC	0.080	0.133***	0.135**	0.035***
Baltimore (county), MD	0.072	0.114***	0.132**	0.034***
Baltimore (city), MD	0.065	0.130***	0.134**	0.036***
Los Angeles, CA	0.065	0.129***	0.130**	0.039***
San Francisco, CA	0.070	0.134***	0.130**	0.036***
Solano, CA	0.070	0.143***	0.138**	0.041***
King, WA	0.069	0.131***	0.128**	0.038***
New York, NY	0.068	0.136***	0.123**	0.039***

Each row in the table reports coefficients on $Shipyard_{1934}$ interacted with the year indicated by the column heading when the row county is excluded from the regression. All regressions include state fixed effects, a dummy for whether the county is situated on a coast, the proportion of the county considered "urban" in the 1930 census, and whether the county is "industrialized." Standard errors are clustered at the state level. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table VI: Results from Regressions based on 1935-1936 Consumer Survey

Independent Variable	Total Consumption	Household Income
Shipyard	1244.50^{***}	1353.94
	(299.98)	(1336.67)
Borders Shipyard	-360.99	-867.56
	(312.36)	(1278.86)
Overlap	0.93^{***}	0.36
	(0.29)	(1.27)
Shipyard*Overlap	2.33^{**}	2.63
	(0.94)	(3.50)
Borders Shipyard*Overlap	0.42	-1.90
	(0.86)	(3.77)

The table gives coefficient estimates from regressions of Equation 3. Ordinary least squares regressions include controls for the age and age squared of the husband and wife of the household as well as a dummy for whether the household is not white and state dummies. *Overlap* and its interaction terms are described in the text. Standard errors are in parentheses. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table VII: Results from Regressions based on 1935-1936 Consumer Survey

Indep. Var.	Total Consump.	Housing	Housing Op.	Medical Care	Recreation	Tobacco	Reading	Education
Shipyard	1212.02***	369.22***	43.90	40.62	39.07	22.79**	15.19***	42.27
	(291.93)	(65.16)	(45.34)	(37.26)	(35.73)	(9.57)	(4.31)	(37.51)
Overlap	0.93***	0.17***	0.16***	0.00	0.06*	0.04***	0.02***	0.02
	(0.28)	(0.06)	(0.04)	(0.04)	(0.03)	(0.01)	(0.00)	(0.04)
Shipyard*Overlap	2.34**	0.26	0.47***	0.18*	0.31***	0.04*	0.01	−0.35***
	(0.91)	(0.17)	(0.12)	(0.10)	(0.09)	(0.03)	(0.01)	(0.10)

Indep. Var.	Occupational Exp.	Gifts	Food	Autos	Clothing	Travel	Personal Care	Equipment
Shipyard	12.53	22.30*	363.94***	−4.24	75.60	76.32***	21.30***	4.04
	(30.72)	(13.57)	(64.85)	(66.03)	(49.83)	(15.87)	(7.85)	(6.30)
Overlap	0.03	0.06***	0.26***	0.32***	0.27***	0.00	0.04***	−0.00
	(0.03)	(0.01)	(0.06)	(0.06)	(0.05)	(0.02)	(0.01)	(0.01)
Shipyard*Overlap	−0.02	0.02	0.66***	−0.09	0.04	0.01	0.05**	−0.01
	(0.08)	(0.04)	(0.17)	(0.17)	(0.13)	(0.04)	(0.02)	(0.02)

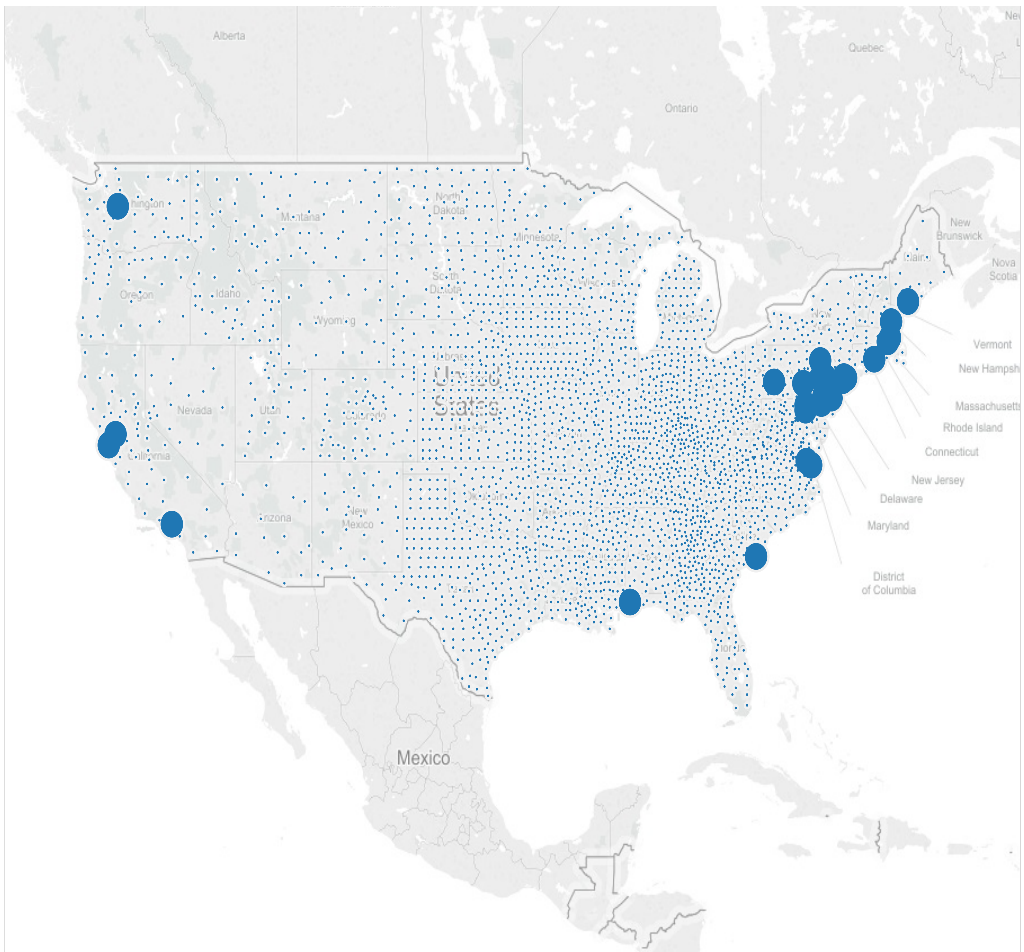
The table gives coefficient estimates from regressions of Equation 3. Ordinary least squares regressions include controls for the age and age squared of the husband and wife of the household and household income as well as a dummy for whether the household is not white and state dummies. *Overlap* and its interaction terms are described in the text. Standard errors are in parentheses. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Table VIII: Effect of "Per-County" Shipyard Spending on Growth in Manufacturing Output

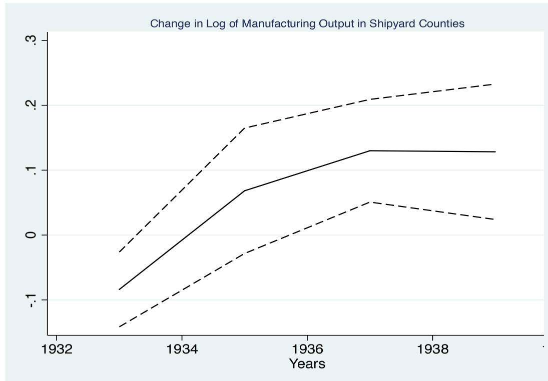
$Shipyard_{1934}$	-0.035^{**}
	(0.014)
$I(Year = 1935) *$ "Per-County" Spending	0.293^{**}
	(0.116)
$I(Year = 1937) *$ "Per-County" Spending	0.844^{***}
	(0.100)
$I(Year = 1939) *$ "Per-County" Spending	1.051^{***}
	(0.097)

This table gives the coefficient estimates from a regression of the two year change in manufacturing output on dummy variables for *Shipyard* and *BordersShipyard* and interaction of "per-county" spending associated with the Vinson-Trammell Act (defined in the text) interacted with year fixed effects. Standard errors clustered at state level are in parentheses. ***, **, and * indicate significance at the 1,5, and 10 percent levels, respectively.

Figure I: Locations of Shipyards Active in 1934

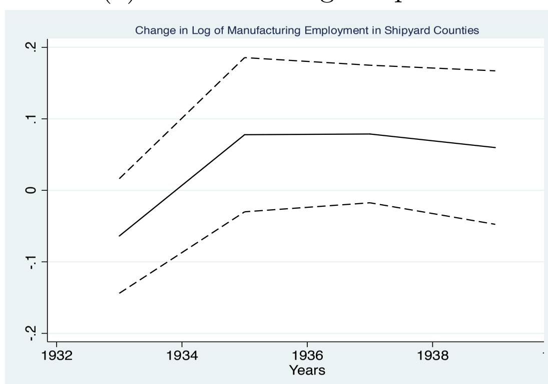


Large dots indicate the locations of shipyards and major shipyard suppliers, as indicated in Lane (1951) and contemporary news sources.

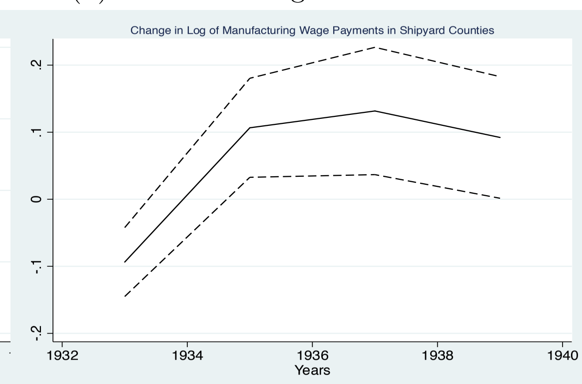


(a) Manufacturing Output

(b) Manufacturing Value Added



(c) Manufacturing Employees

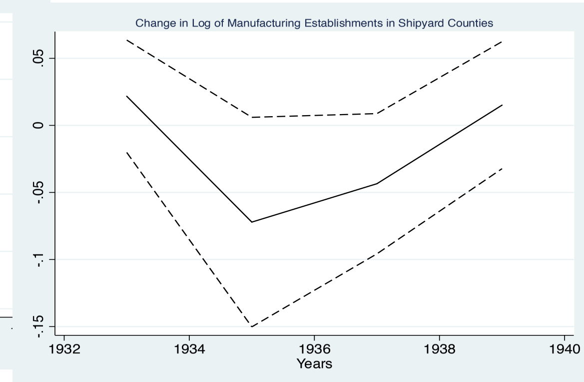


(d) Manufacturing Wage Payments

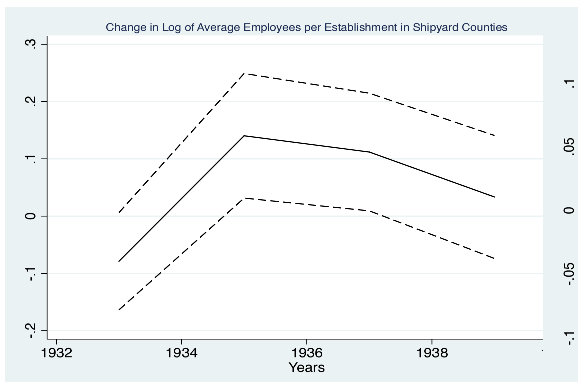
Figure II: Predicted Additional Growth in Manufacturing Outcomes Associated with Shipyard Counties

Figure plots coefficients on $Shipyard_{1934}$ dummy variable (corresponding to the year 1933) and coefficients on interaction terms between $Shipyard_{1934}$ dummy and dummy variables for 1935, 1937, and 1939. Figure contains 95% confidence bands estimated with standard errors clustered at the state level. Regression includes controls for state fixed effects, whether county is "industrialized," on a coast, and its urban percentage in 1930.

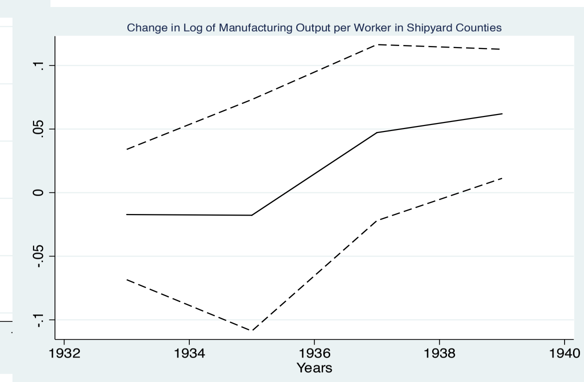
(a) Average Earnings per Manufacturing Employee



(b) Manufacturing Establishments



(c) Average Employees per Manufacturing Establishment



(d) Manufacturing Output per Employee

Figure III: Predicted Additional Growth in Manufacturing Outcomes Associated with Shipyard Counties

Figure plots coefficients on $Shipyard_{1934}$ dummy variable (corresponding to the year 1933) and coefficients on interaction terms between $Shipyard_{1934}$ dummy and dummy variables for 1935, 1937, and 1939. Figure contains 95% confidence bands estimated with standard errors clustered at the state level. Regression includes controls for state fixed effects, whether county is "industrialized," on a coast, and its urban percentage in 1930.