2024

Stimulus on the Home Front: the State-Level Effects of WWII Spending

Gillian Brunet
Smith College, gbrunet@smith.edu

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Stimulus on the Home Front: the State-Level Effects of WWII Spending

Gillian Brunet†

January 19, 2024

Abstract

I use newly-digitized contract data on U.S. war production spending over 1940-1945 to analyze the macroeconomic effects of U.S. military spending in World War II. I find personal income multipliers of 0.34 over two years and 0.49 over three years. Personal income multipliers may substantially understate GDP multipliers, perhaps by as much as 50%. Employment estimates imply costs per job-year over the same time horizons of $405,013 and $232,268 in 2015 dollars, suggesting job creation was limited. I also find evidence of negative scale effects: larger positive spending shocks are associated with systematically smaller multiplier estimates.

JEL E62, E65, H56, N12, N42

†Assistant Professor of Economics, Smith College. Wright Hall # 226, Northampton, MA 01063. Email: gbrunet@smith.edu. This research was supported by dissertation fellowships from the Federal Reserve Board of Governors and the Economic History Association, as well as an NBER Postdoc funded by the Peter G. Peterson foundation. I am grateful to Christina Romer for outstanding guidance and support, and to Olivier Coibion, J. Bradford DeLong, Barry Eichengreen, Yuriy Gorodnichenko, Martha Olney, Valerie Ramey, David Romer, and Noam Yuchtman for detailed comments and advice. I would also like to thank Sandile Hlatshwayo, Joshua Hausman, Erik Johnson, Dmitri Koustas, Elisabeth Perlman, Sarah Quincy, Nicholas Sander, Joachim Voth, and Colin Weiss for their comments, as well as several anonymous referees and numerous seminar participants. Joseph West provided excellent research assistance. All errors are my own.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

1 Introduction

World War II was the single largest fiscal shock in the twentieth century. The U.S. spent $305 billion on national defense over 1940–1945, or $4.5 trillion in 2015 dollars. Many estimates of the fiscal multiplier rely on military spending because it is more plausibly exogenous than domestic spending. The scale of WWII spending was so large that it dominates all other shocks when included in military spending data. The variance of yearly changes in U.S. defense spending over 1938–2013 is 21 times higher than the variance over 1947–2013.

This paper uses panel data to study the fiscal multiplier in the United States during WWII. I assemble panel data from an ex-post summary tabulation of war supply contracts published by the Civilian Production Administration—the successor agency to the War Production Board (WPB)—in 1946. My data include all war production contracts for $50,000 or more awarded between June 1940 and September 1945, excluding only food products and electricity generation. Contracts run the gamut from blankets, screws, and sheet iron to B-17 flying fortresses and aircraft carriers. The total value of contracts in the sample is $183 billion, or almost 60 percent of all U.S. military spending over 1940–1946. I use these data to construct a state-level panel of U.S. war production spending. Previous work on this topic has always relied on either aggregate or cross-sectional data; this paper is the first to use panel data on U.S. military spending in WWII.

Panel data are particularly helpful for disentangling the many unusual features of the wartime economy. With panel data one can estimate the effects of government spending using within-panel variation, controlling for time fixed effects (time trend effects, with differencing) and state fixed effects (state-specific underlying trends, with differencing), making the argument for identification much stronger. Many WWII policies had coincident timing because they were all driven by the realities of the war (e.g. many policy changes were enacted after Pearl Harbor). Using variation across locations as well as across time helps isolate the effects of fiscal shocks from the effects of other policies.

Because state-level GDP is not available for WWII, I estimate a personal income multiplier in lieu of a GDP multiplier. I find personal income multipliers of 0.34 over a two-year time horizon

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

and 0.49 over a three-year time horizon. It is somewhat difficult to compare personal income multipliers to GDP multipliers because GDP includes consumption of fixed capital (highly relevant to the production of military goods) while personal income does not. An empirical exercise suggests that using personal income in lieu of GDP may reduce estimates by up to half. Doubling my estimates suggests a GDP multiplier consistent with many standard estimates in the literature. My findings thus suggest that the GDP multiplier on military purchases was either the same or somewhat smaller during WWII than during other periods. Despite this ambiguity, it seems implausible that the WWII multiplier was substantially larger than the multiplier during other periods.

I also estimate employment multipliers, which are directly comparable to estimates for other periods. I find employment multipliers of 0.0359 and 0.0626 over time horizons of two and three years, corresponding to costs per job-year of $405,013 and $232,268 in 2015 dollars. Compared to employment multipliers estimated for more recent fiscal shocks, these estimates are low (and costs per job-year are high). The three-year employment multiplier is at the low end of the range found in the recent literature, while the employment multipliers for shorter time horizons are significantly smaller than any estimated for modern time periods. The employment multipliers are substantially smaller than the likely GDP multipliers. This may be because productivity increased more sharply in locations with capacity constraints, as shown for the WWII aircraft industry by Ilzetzki (2023).

The paper finishes with an exploration of factors which may have influenced the WWII fiscal multiplier. Because the scale of WWII spending was so large compared to other fiscal shocks, I examine whether scale effects may have been present, i.e. whether larger increases in WWII production are associated with systematically smaller multiplier estimates. I find strong evidence for negative scale effects on personal income at all time horizons, and on employment at short time horizons. I also discuss several features of the WWII economy: the conversion of industrial capacity from civilian manufacturing to war production, the unusually high household savings rate, and the extremely tight labor market over 1942–1945. While I cannot directly estimate the influence of these factors on the multiplier, there are good reasons to believe that these features of the economy may have influenced the multiplier process.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

While recent research in applied microeconomics has examined aspects of WWII (including Goldin and Olivetti, 2013; Acemoglu, Autor, and Lyle, 2004; Goldin, 1991; Collins, 2001; and Fetter, 2016), most economists have avoided working on the U.S. macroeconomy during WWII for the last half century, largely because the complexities of the wartime economy could not be disentangled using aggregate data. There are, however, a few notable exceptions. Higgs (1992) argues broadly that wartime living standards and economic conditions were considerably less rosy in fact than in popular perception, but focuses more on consumption, rationing, and household purchasing power than on the macroeconomic effects of war spending. He challenges the notion of wartime prosperity based on how much of the wartime growth in both output and employment was directly attributable to war production and consequently did not contribute to civilian economic wellbeing. Jaworski (2017) examines the role of WWII facilities spending on capital deepening and economic development in the American South, and finds no systematic long-term effects. Rhode et al. (2017) examine the political economy of war production and facilities spending, but do not address the macroeconomic effects of the spending.

Earlier papers on the WWII multiplier use either cross-sectional or aggregate time series data. Gordon and Krenn (2010) address only the defense period, before the U.S. formally entered the war. They find that the aggregate economy's response to fiscal policy was already constrained by raw materials shortages in 1941. Fishback and Cullen (2013) examine the economic effects of war production spending on retail sales using a county-level cross-sectional comparison of economic outcomes in 1939 and 1948. They find no effect of war spending on the change in county retail sales. Given differences in methodology and time horizons, their findings are broadly consistent with my panel estimates of the fiscal multiplier for the WWII period.

This paper is structured as follows. Section 2 provides a brief overview of U.S. war production and describes the data. Section 3 discusses identification, estimation, and results. Section 4 examines factors that may have influenced the WWII multiplier, including the scale of spending and features of the wartime economy. Section 5 concludes.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

2 Historical Context and Data

2.1 Historical Context

Until Pearl Harbor, American entrance into WWII was not a foregone conclusion. Throughout 1940 and most of 1941 it was far from clear—even to many of the planners in Washington—that the U.S. would engage in "a shooting war" abroad rather than just supplying its future allies with arms and mounting a preemptive defense at home. It was nearly impossible for planners to (1) obtain accurate estimates of production requirements from a military leadership with no concrete sense of its goals or strategy, or (2) convince Congress and the successive agencies responsible for war planning to identify and stockpile the raw materials that would be needed for an all-out war.

When war came after Pearl Harbor, war needs could not be met solely through expansions of production capacity. Building new facilities requires time and strategic materials, both of which were scarce in 1942. While war production began during 1940 and 1941, the scale was very small relative to the eventual scale of production needed at the height of the war. Conversion of existing manufacturing facilities was necessary for dramatically accelerating production in early 1942. This history is explained in greater depth in online appendix B.

In aggregate terms, the U.S. spent $305 billion on national defense over 1940–1945, or $4.5 trillion in 2015 dollars[1]—a massive increase over the $8 billion total U.S. defense spending over the entirety of the 1930s.[2] Nominal GDP grew from $103 billion in 1940 to $228 billion in 1945. Real GDP grew by more than 75% over the same period, at an annual average rate of 11.8%. Total non-farm civilian employment grew by 8 million over the same period, despite the large number of Americans serving in the Armed Forces.

[1] OMB Historical Table 3.1, adjusted using BLS CPI calculator.

[2] *Budget of the U.S. Government for the Fiscal Year Ending June 30, 1940*, table 2, p. VII.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

2.2 Data

My panel of defense spending is built from individual military contracts with private firms. The data come from an ex-post summary tabulation of war supply contracts published by the Civilian Production Administration (which succeeded the WPB). Contracts are listed by the establishment of main production. See Figure 1 for an example of contract listings from the data. The dataset includes all contracts for $50,000 or more awarded between June 1940 and September 1945, excluding only food products. Contracts include airplanes and victory ships, but also intermediate goods (e.g. propellers and gun fittings), raw materials (e.g. aluminum and leather), and smaller items (e.g. mattresses, gloves, insect repellent, and toilet paper). In total, the data include more than 190,000 contracts, worth $183 billion—a huge sum given that U.S. GDP was only $103 billion in 1940. Most contracts were relatively small, though numerous contracts are recorded for many firm/location pairs. More than 90% of contracts were for less than $1 million; fewer than 200 contracts were for more than $100 million. The largest contract was for Boeing to build B-17 bomber planes in Seattle for $669 million.

While in many ways very similar to the modern data on military contracts used by Nakamura and Steinsson (2014),[3] WWII contract data have one distinct advantage: precise timing. Instead of an approximate date associated with a paperwork filing, each contract listing in the WWII data includes the month and year of both contract award and contract completion. The median contract length is 7 months, with a range of 1 to 83 months. 95% of contracts lasted 18 or fewer months. Larger contracts lasted longer on average: when weighted by dollar value, the median contract was 16 months. This is consistent with the narrative record: WWII contracts were intentionally of short duration to avoid renegotiations due to price changes (specifically wartime inflation).

Figure 2 shows the distribution of war supply contract spending by state. War production contracts were concentrated in the industrial northeast, in the midwest, and along the Pacific coast. Online appendix A provides an in-depth discussion of data cleaning and preparation.

To construct a panel from the contract data, I first take the total value of each contract and

[3]Nakamura & Steinsson's contract data begins in 1966.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Figure 1: Example of Civilian Production Administration Contract Data

NAME OF MANUFACTURER / LOCATION OF MANUFACTURE / PRODUCT AND CONTRACT NUMBER		AGENCY	FOOTNOTE	VALUE THOUSANDS DOLLARS	AWARD DATE		COMPLETION DATE	
					MO.	YR.	MO.	YR.
INSECT POWDER	TPS77605L	T		130	1	45	3	45
DDT INSECTICIDE	28021 QM29451	A		471	1	45	4	45
INSECTICIDE	28021 QM26736	A		170	1	45	1	45
DDT INSECT POWDER	28021 QM30839	A		93	2	45	9	45
INSECTICIDE POWDER	28021 QM33715	A		282	3	45	9	45
INSECTICIDE	28021 QM33751	A		314	3	45	7	45
LIQUID INSECTICIDE	28021 QM41648	A		135	6	45	12	45
INSECTICIDE POWDER	28021 QM40418	A		90	6	45	12	45
DDT INSECT POWDER	28021 QM38508	A		72	7	45	12	45
				4852				
MCCORMICK B B SONS								
JACKSONVILLE FLA								
PRECUT BARRACKS	8123ENG 45	A		244	6	44	10	44
				244				
MCCORMICK BAXTER CREBOTING CO								
PORTLAND OREG								
CREOSOTING WOOD PILES	406XSY12045	N		1367	6	44	4	45
LUMBER	406XSX13307	N		163	6	44	9	44
LUMBER	406XSX13489	N		55	8	44	10	44
LUMBER	406XSX13727	N		141	8	44	10	44
LUMBER	406XSX13719	N		141	8	44	11	44
WOOD PILING	130XSX21070	N		150	7	45	12	45
				2017				
MCCORMICK BAXTER CREOSOTING CO								
STOCKTON CAL								
RAILROAD CROSS TIES	35052ENG 477	A		71	8	44	11	44
RAILROAD CROSSTIES	35052ENG 526	A		73	9	44	12	44
				144				
MCCORMICK BROS CO								
ALBANY IND								
METAL SLEEVES	ORD 7592	N	3	1950	11	44	7	45
METAL SLEEVES	ORD 9378	N	3	1322	7	45	1	46
				3272				
MCCOY COUCH FURNITURE MFG CO								
BENTON ARK								
LOCKERS	586 QM 272	A		302	2	43	8	43
LOCKERS	586 QM 387	A		116	4	43	8	43
				418				

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

distribute it uniformly over all of the months in which the contract was open (inclusive of the award and completion months). I then sum spending by state and quarter and/or year.

I focus on two outcome measures: employment and personal income. Employment data was collected and published monthly at the state level in the Current Employment Statistics (CES), administered by the Bureau of Labor Statistics (BLS). Because employment data are noisy and because using monthly data may exacerbate timing problems in spending data, I will generally use quarterly employment data (employment totals averaged across each quarter).

My other outcome variable is personal income, which was estimated annually at the state level by the Bureau of Economic Analysis (BEA).[4] This is the measure closest to GDP available for the 1940s. At the national level, changes in personal income track changes in output very closely over the relevant period. However, personal income differs from GDP in several ways, most crucially for this paper in that personal income does not include either the consumption (depreciation) of physical capital through the production process or profits retained and reinvested by firms. It is also noteworthy that personal income is measured before taxes are paid.

2.3 Subcontracting

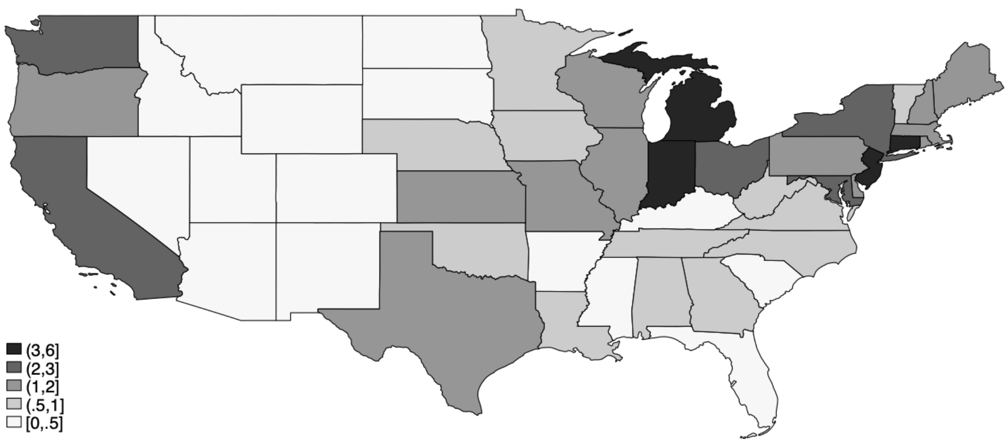
One concern with using contract data to construct geographically disaggregated data on war production is that there might be systematic measurement error caused by subcontracting. Several types of subcontracting could theoretically cause these measurement errors.

First, a primary contractor might subcontract part or all of their production to another firm, so that the subcontractor produces (some portion of) the product. The WWII contract data account

[4]BLS payroll data were used in conjunction with other sources used to construct personal income. These other sources included data from income taxes, the Bureau of Agricultural Economics, and surveys conducted directly by the BEA. Thus there may be some cointegration between the two outcome measures. The sources used to construct personal income data for this period are discussed in the April 1940 *Survey of Current Business*, pp. 10–11.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Figure 2: Total Per Capita War Supply Spending (1940s dollars in thousands)



Source: Author's calculations based on 1946 contract listings from the Civilian Production Administration. While war production was concentrated in the industrial northeast and the Pacific coast, there was significant variation across both states and time.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

for this kind of subcontracting: contracts placed indirectly through other firms (rather than directly by military procurement agencies) are included with a footnote indicating that they were placed in this way. Of the more than 191,000 contracts in the data, roughly 12,000 fall into this category, with a total value of just over $6 billion.

Second, when producing more complex goods such as tanks, guns, and ships, firms often purchase intermediate inputs from other firms and outsource the manufacture of simpler components. While this could potentially cause systematic measurement errors in contract data, in practice these supply chains are unlikely to cause systematic errors in state-level data for two reasons.

Subcontracting was channeled directly through primary contractors for ease of administration. It seems that many firms relied on preexisting supply chains, which tended to be geographically proximate to the industries with which they were connected. Where new supply chain relationships were established, policies of the WPB strongly incentivized prime contractors to subcontract with local firms. As WPB Chairman Donald Nelson explained in his 1946 memoir:

> We were able also to induce large manufacturers to spread the work to smaller firms... letting [large firms] overload somewhat on orders and then refusing permission to expand their facilities when the facilities of small plants were available in the vicinity of the larger plants. (Nelson, 1946, p. 275)

While fortuitous for my purposes, the logic behind this policy was sound given the goals of war production planners. Given the large number of small firms with preexisting industrial capacity, utilizing those small firms was essential to American efforts. This was especially true given the complexity of many essential war products: smaller firms usually produced the simpler components, freeing the large firms to specialize in the more complex components (and assembly of final goods) that required more extensive equipment and knowledge. The Herculean administrative task of directly managing so many small contracts (and the related materials allocations) would have quickly overwhelmed the already strained administrative capacities of the WPB and military procurement agencies. Subcontracting was the clear solution to this problem.

Why did the WPB choose to incentivize *local* subcontracting? Planners were concerned with

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

minimizing the strain on shipping capacity, even within the U.S. While shortages of freight capacity were not as acute as shortages of critical raw materials, they were linked in the sense that expansions of shipping capacity required the same raw materials that were so critical for war production. Thus expansions of domestic shipping capacity were carefully calibrated. Local subcontracting meant less shipping within the production process, freeing up raw materials.

The biggest concern about locations in the data is flows of raw materials. There is good reason to believe that the geographic distribution of raw materials production looked very different from the geographic distribution of war production. This suggests that contract data may systematically misrepresent economic activity associated with war production because the production of raw materials is not accounted for.

The procurement agencies did buy some raw materials directly, and these contracts are reflected in the data. However, contracts for materials account for only a small fraction of the materials used for the war effort. Data on manufacturers' shipments show $50 billion in "iron and steel and their products" and $9 billion in textile-mill products for war use (Industrial College of the Armed Forces, 1947 p. 289). The contract data contain $3 billion for iron and steel products[5] and nearly $6 billion for textiles.[6] Thus the contract data account for roughly 6% of war-related manufacturers' shipments for iron and steel and their products, and roughly 63% of war-related textile shipments. This suggests that economic activity relating to war production is systematically undercounted in places where metals were produced and over-counted in the places where final goods were produced.

[5]This includes all products with descriptions including the terms "iron" or "steel," plus selected products that appear in listings for either Bethlehem Steel or Carnegie Steel, such as forgings, armor, shapes, axles, and rails. This is very likely an undercount, but the vast majority of iron and steel and their products used for war production were purchased directly by firms and are not accounted for in the contract data.

[6]Textiles are defined as products whose descriptions include any of the terms "cloth," "wool," "cotton," and "duck" (a type of cotton fabric frequently appearing in the data).

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

One way to crudely account for these missing flows of raw materials is to interact the state's 1939 mining employment rate (share of population employed in mining) with the time fixed effects, essentially allowing a separate underlying trend for the mining sector. This is similar to the controls for the farm sector discussed below in Section 3.2. Including mining fixed effects modestly increases estimates of the income and employment multipliers, as shown in online appendix C.3. The point estimate almost always increases by less than one standard error.[7] This suggests that failing to account for flows of raw materials introduces a small downward bias to my estimates.

Note that because military spending data almost universally record government payments rather than value-added by firms, the measurement problems posed by subcontracting—particularly for raw materials—seem likely to apply to *all* geographically disaggregated datasets constructed from military contracts. Like other measurement issues, these problems seem likely to grow as the geographic unit of measurement shrinks, suggesting benefits to focusing on state-level outcomes.

2.4 Other Potential Data Concerns

This section briefly addresses other potential data concerns, particularly those about the timing, location, and completeness of the data.

One possible concern might be whether the locations listed in the data accurately reflect where goods were produced. In this respect, the WWII production data are far better than most other firm-level data sets: contracts were recorded by establishment of main production, *not* by the location of the firm's headquarters. So there is an unusually high degree of certainty that production took place in the state to which each contract is assigned.

Another possible concern is how the contract data capture the timing of the spending. The data record both the award month/year and completion month/year for each contract. As explained in Section 2.2, I distribute the value of each contract uniformly over its length (inclusive of both starting and ending months—necessary as there are a non-trivial number of contracts with a length of 1

[7]The exception is employment multipliers estimated at horizons above 2 years. At a time horizon of 12 quarters, the employment multiplier decreases slightly, from 0.0626 to 0.0618.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

or 2 months). This is not the only reasonable approach. Online appendix A.3 presents alternative timing assumptions and shows how they affect the baseline results.

Finally, one might be concerned about the completeness of the war production data, and especially about the omission of other government purchases not captured in the data. The war production included in my contract data accounts for more than 59% of total federal (US) military spending over 1940 through 1946. Facilities spending (building both military installations and industrial facilities within the United States) accounts for another 8.7%. The remaining 32% of U.S. military spending over 1940–46 is likely dominated by personnel costs: the size of the military peaked at 12.1 million personnel in 1945, not counting local workers on military payrolls in U.S.-occupied areas Asia, Africa, and Europe. Unfortunately it is difficult to find data on total personnel costs; if all military personnel were paid $50/month (starting pay for enlisted soldiers), military payrolls would have accounted for more than $21 billion over 1940–44 (another 7% of total defense spending over 1940–46), but this is surely a lower bound (and also excludes the substantial cost of feeding the armed forces). One can safely conclude that the contract data used in this paper must account for a large majority of U.S. WWII production.

As documented by historian Mark Wilson (2016), the "government-owned, company-operated" model, abbreviated GOCO, dominated WWII production. This explains why the contract data are so complete: both production by private firms and GOCO production is included in the war production data. While the data exclude war production at sites both owned *and* operated by the government, such arrangements were relatively rare. Apart from war production at facilities both owned and operated by the government, the major omissions from the contract data are food production and the Manhattan Project.

By far the largest category of war production omitted from the data is food production, which is the only category of production entirely excluded from the WPB contract listings. With over 12 million Americans serving in the Armed Forces during the war—most of them overseas in Europe, north Africa, and the Pacific—U.S. food production for the war effort was significant. As the war progressed and the U.S. took on responsibility for military governance of occupied territories—

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

including many areas where local food production had been disrupted by the war—the military's demand for food only grew. Unsurprisingly, U.S. agriculture boomed during the war years. This confounding factor and its role in my identification strategy are discussed in detail in Section 3.2.

The WPB contract listings ostensibly include all other final goods (and some services) purchased by the U.S. government (and foreign governments via Lend-Lease) for the war effort, as well as raw materials and intermediate goods purchased directly by the government rather than via contractors. However, some top secret projects were excluded from the WPB contract listings.

The most obvious omission from the contract data is the Manhattan Project, as it later came to be called.[8] As large as the Manhattan Project looms in historical memory, its fiscal impact was comparatively modest: it is equivalent to just over 1% of the war production listed in the data.

The Manhattan Project appears to be the exception rather than the rule: the WPB contract listings include 271 contracts with product descriptions given as "Classified," totaling $449 million (about $6.5 billion in 2015 dollars). The companies supplying these classified contracts include aircraft manufacturers—Bendix, Douglas, Hughes, Lockheed, and Northrup—telephone and radio companies—Belmont Radio, Bell Telephone Labs, Hazeltine Electronics—electric companies—General Electric, Western Electric—manufacturing companies—Diebold, Raytheon—and a handful of universities—Harvard, Duke, and the California Institute of Technology. The breadth of contractors for these confidential contracts suggests that outside of top secret projects, classified projects were generally included in the WPB contract listings.

[8]The contract listings do not include any war production contracts in Los Alamos, NM or Oak Ridge, TN, the main locations of the Manhattan project. Spending on the Manhattan Project was small relative to the spending represented in the data: total spending on the Manhattan Project was slightly under $2 billion in 1940s dollars, compared to a total of almost $181 billion in war contracts listed in the data for the continental United States. The government spent more on apparel and other textile products (SIC 23, which accounts for over $3.1 billion in contracts) than it did on the Manhattan Project.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

3 Identification & Results

3.1 Identifying Assumption

Because my identification strategy includes both time and state (location) fixed effects, the identifying assumption is that differential growth of military procurement across states over time is independent of other determinants of economic conditions. Both the historical narrative and empirical testing support this identifying assumption.

The geography of war production does not appear to have been substantially influenced by politics. War production contracts were made directly by military procurement agencies, which reduced the influence of Congress and elected officials on contract placements. Rhode et al. (2017) study the political economy of WWII spending and find that political factors do not predict the distribution of war production. Specifically, they "find robust evidence that war contract spending [was] not allocated to enhance the president's electoral chances" (Rhode et al., 2017, p. 41). As they note, this finding stands in contrast to studies on the geographic distribution of federal spending during some other periods (such as the New Deal), though it is consistent with the (relatively limited) previous literature on the political economy of spending in WWII. Other evidence also suggests that the geographic distribution of war spending was not politically motivated. Panel II of Table 2 (below) shows that state-level war production spending did *not* predict increases in government employment, as one would expect if war production spending were strongly correlated with other shocks to government spending. War production spending is associated with tiny reductions in government employment ($\beta < 0.01$) at time horizons of 2 to 4 quarters; the effect is not statistically significant over other time horizons, and is never economically significant.

While civilian planners at both the WPB and War Manpower Commission did urge military procurement agencies to place contracts in locations with more available labor, in practice the effects of these efforts were negligible. Civilian production planners expressed frustration over military attitudes that civilians should tighten their belts and be prepared to make any sacrifices the

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

military deemed necessary (with a very low valuation on civilian needs).[9] A retrospective on the war effort published in 1946 by the Bureau of the Budget describes efforts to distribute contracts to areas with available labor supply in 1942 and 1943:

> Labor scarcity was greatest in a few areas ... into which facilities and contracts had been crowded and which had become congested with war workers and military personnel until no more could be absorbed...
>
> Previous efforts to reduce the demand for labor in areas of shortage had met with only limited success. In October 1942 the WPB, by its Directive No. 2, instructed the procurement services to consider the adequacy of labor supply in particular areas as a factor in contract placement... The impact of this directive on actual procurement practice, however, was not large. Among the reasons were the traditional independence of the procurement services, preoccupation with price and delivery considerations, reluctance to give up customary sources of supply, reluctance to give up a facility which may then be taken over by a competing procurement branch, and the absence of any continuous policing by Army Service Forces headquarters of compliance by contracting officers with Directive 2. (Bureau of the Budget, 1946, pp. 432–433)

Other attempts to place contracts according to local economic conditions were similarly ineffectual, due to a combination of inter-agency power struggles, competing policy objectives, and opposition from both industry and organized labor. Even after war production had reached its peak and civilian planners had begun serious planning for reconversion, the Army remained unwilling to consider local labor market conditions when placing contracts. As WPB Chairman Donald Nelson recounted:

> ... the Army's one answer to excessive turnover and localized labor shortages was to

[9] Nelson's characterization of the dynamic between the military and civilian planners (see pp. 109–112) is corroborated by the Bureau of the Budget (chapter 5) and consistent with the Army's own history (Smith, chapter 7).

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

> create unemployment and let its pressure force people into jobs. A more wasteful or dangerous solution of the matter could hardly be imagined. It was a method which involved the use of maximum pressure to produce minimum results... [Y]ou couldn't employ idle die-setters in Cleveland if a factory in Phoenix needed welders—and this illustration is by no manner of means farfetched. (Nelson, 1946, p. 405)

I test the identifying assumption by examining whether spending changes are forecastable using the state's employment rate from period $t-1$. Specifically, I put the cumulative change in war production spending—as defined below in Section 3.3—on the LHS and then include the state's employment rate (lagged by one period) and all control variables (again as defined below in Section 3.3) on the RHS. I use the state employment rate because it was the highest quality and highest frequency data available at the state level during WWII. The narrative record indicates that employment statistics were particularly salient to policymakers. I lag employment data by one period to mirror the delay in data available to policymakers. At every time horizon (from 0 periods up to 3 years), the absolute value of the estimated coefficient on the lagged state employment rate is smaller than 0.00001. This is unsurprising given the lagged values included in the controls, but it is nonetheless reassuring for the validity of the identifying assumption.

Another concern is that even if contract placement was not driven by economic conditions, Americans systematically moved to locations where employment in war production was available. Online appendix C.2 presents robustness checks relating to this concern. First, I find that war production does not predict changes in reported population. Second, fixing each state's population at 1939 levels (when computing real per capita spending in each period) does not significantly alter my results. Both of these findings suggest that within the identification framework of this paper, migration does not significantly alter the results. However, population data are smoothed between benchmark measurements, so it is impossible to accurately measure the differential effects of wartime relocations. In the absence of better population data, one can only speculate that migration to locations with higher levels of war production could be a source of bias.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

3.2 Controlling for the Farm Sector

Exposure to agriculture presents a significant confounding factor for panel identification. First, underlying trends in agricultural income were influenced by the war through channels separate from production of war goods and not accounted for in my data. Second, because agriculture was an essential economic activity for the war effort, wartime constraints on the economy were much looser in the agricultural sector than in other sectors of the economy, suggesting that both underlying trends and the effects of war production spending may have been quite different in areas with significant agricultural activity.

Agriculture presents an omitted variable bias problem because agricultural income grew very quickly at the same time war production was ramping up, making it important to allow for different underlying trends in agricultural states. Several factors drove the expansion of the agricultural sector during WWII: natural rebuilding of the farm sector after the Dust Bowl, the need to provision the armed forces, food aid to European allies (where war significantly interrupted agriculture in many areas), and the need to provide food for civilians in areas under Allied control, starting with North Africa in 1942. Real net farm income more than doubled over 1940–43. Net farm income over 1940–45 totalled $57 billion, or roughly one third the value of war supply contracts. This is particularly important because the passthrough from commodity prices to farm incomes was much larger for the farm sector than for other types of employment, as argued by Hausman et al. (2021).

The war production contracts recorded by the WPB (and used in this paper) do not include food or food processing (often undertaken near agricultural areas), so the increases in farm income are not reflected in my measure of war production. To the extent that increased spending on food was a result of wartime stimulus, food production was still concentrated in certain states and regions (often very different from the states and regions where the production of war materials was concentrated) and systematically shipped across state borders, so it is still a confounding factor.

In addition, wartime rationing affected the farm sector much less intensely than it affected most other sectors of the civilian economy. Food production was deemed essential for the smooth functioning of the civilian economy, which meant strategic materials such as metals were more

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

readily available for producing agricultural equipment than for other civilian manufacturing. In fact, farm implements were one of the few categories of new durable goods for which metals were allocated (along with repairs and replacement parts).

To allow for the differences between the agricultural and non-agricultural sectors, I interact the time fixed effects with the share of the state's population employed exclusively in agriculture in 1939.[10] The variation in 1939 farm employment is shown in Figure 3. This allows for a different underlying time trend in the agricultural sector of the economy. I use agricultural employment in 1939 to avoid endogeneity problems.

3.3 Estimation

I start from an estimation approach detailed in Ramey (2016): specifically, I estimate cumulative multipliers using local projections with the variable transformations originated by Hall (2009) and Barro and Redlick (2011). The resulting specifications are similar to the panel data specifications popularized by Nakamura and Steinsson (2014), but include controls for lag-lead exogeneity and directly estimate the cumulative multiplier at time horizon h.

Cumulative changes to government spending and personal income are defined as

$$\tilde{y}_{it}^h = \sum_{j=0}^{h} \frac{y_{i,t+j} - y_{i,t-1}}{y_{i,t-1}} \text{ and } \tilde{g}_{it}^h = \sum_{j=0}^{h} \frac{g_{i,t+j} - g_{i,t-1}}{y_{i,t-1}}, \tag{1}$$

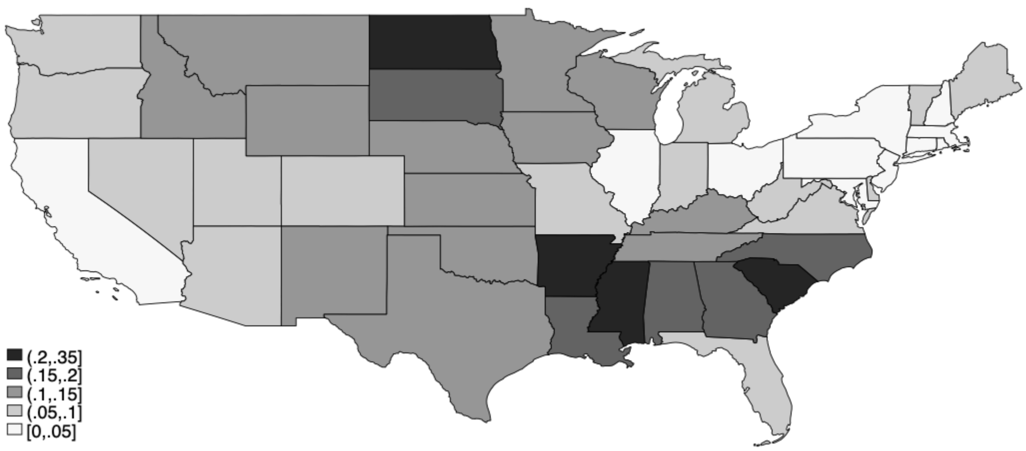
where i is the location (state) and t is the time period. Variables y and g are per capita personal income and war production measured in 1942 dollars. To control for lag-lead exogeneity, I include lagged values of the variables of interest using the transformations

$$\breve{y}_{itk} = \frac{y_{i,t-k} - y_{i,t-k-1}}{y_{i,t-k-1}} \text{ and } \breve{g}_{itk} = \frac{g_{i,t-k} - g_{i,t-k-1}}{y_{i,t-k-1}}, \tag{2}$$

[10]These figures are constructed using estimates from the 1940 *Census of Agriculture*, which measured agricultural employment by state in 1939. I adjust totals for agricultural employment downwards to remove workers who also worked off-farm.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Figure 3: Share of State Population Employed in Agriculture in 1939



Source: Author's calculations based on the *1940 Census of Agriculture*, as reported by the *Statistical Abstract of the U.S.*. Estimates have been adjusted to remove workers who also held non-agricultural employment.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

where $k = 1, 2, ...$ up to the total number of lags (generally two years). This is similar to Ramey and Zubairy (2018), who use a simple log transformation for lagged values of g and y. I use a slightly different transformation for the lagged values to avoid distortions from observations with state-level war production of $0.[11] While it is necessary to include these lags due the presence of serially correlated shocks, the combination of fixed effects and lagged values of (a close transformation of) the dependent variable is known to bias coefficient estimates towards zero (see Alvarez and Arellano, 2003). For this reason, the estimates that follow should be interpreted as lower bounds. This is discussed further in Section 3.4.

3.3.1 Personal income multipliers estimated with annual data

When using personal income as the outcome variable, the analysis uses annual (calendar year) data, which is available by state from 1929 onwards. I estimate

$$\tilde{y}^h_{it} = \beta^h_y \, \tilde{g}^h_{it} + \eta_{k,y} \sum_{k=1}^{2} \breve{y}_{itk} + \eta_{k,g} \sum_{k=1}^{2} \breve{g}_{itk} + \alpha_i + \gamma_t + F_i * \gamma_t + \epsilon_{it}. \tag{3}$$

F_i is the share of state i's population employed in agriculture in 1939; including $F_i * \gamma_t$ essentially allows separate time fixed effects for the agricultural and non-agricultural sectors, which is necessary for the reasons given in Section 3.2.[12]

[11]Military contract data begin in 1940, so I assume $0 war production in 1938 and 1939. Total authorizations for U.S. military spending—including both contracts and direct government spending—averaged $1.1 billion per year in 1938 and 1939, compared to $20.5 billion in 1941 and $110.8 billion in 1942—so the bias from assuming $0 contracts prior to 1940 should be relatively small. Given these $0 values, a log transformation to lagged values would make it necessary to choose between possible distortions or dropping all observations from the earliest years of war production. Using one-period differences scaled by personal income avoids this problem.

[12]Because a state's pre-war rate of agricultural employment is time-invariant, F_i is collinear with state fixed effects. Thus it would be redundant to include F_i separately in the estimating equation.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Table 1: Personal Income Multipliers (Annual)

time horizon h (years)	0	1	2	3
β_y^h	0.0316	0.124*	0.336***	0.485***
	(0.0544)	(0.0615)	(0.0813)	(0.135)
Personal income lag 1	-0.203**	-0.793***	-2.112***	-3.647***
	(0.0955)	(0.166)	(0.216)	(0.322)
Personal income lag 2	-0.220***	-0.809***	-2.197***	-3.097***
	(0.0411)	(0.115)	(0.307)	(0.428)
Spending lag 1	0.186***	0.471***	1.092***	2.032**
	(0.0490)	(0.121)	(0.314)	(0.774)
Spending lag 2	0.0478	0.00673	0.784*	1.327
	(0.0565)	(0.123)	(0.452)	(1.181)
Observations	343	294	245	196
Within R-squared	0.764	0.842	0.907	0.934

Standard errors clustered by state. Personal income and war production are measured in real 1942 dollars. Time fixed effects and interaction between time fixed effects and pre-war agricultural employment shares estimated but not shown. *** p<0.01, ** p<0.05, * p<0.1

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

The resulting cumulative multiplier estimates over time horizons of 0, 1, 2, and 3 years are shown in Table 1. As one might expect, the cumulative fiscal multiplier grows over time. The contemporaneous personal income multiplier is near zero. At a one-year time horizon, the personal income multiplier is 0.12. The cumulative multiplier then jumps to 0.34 over two years and 0.49 over a three-year time horizon.

Personal income is a narrower measure of output than GDP, so estimating the multiplier using personal income rather than GDP may bias estimates. While I cannot use state GDP data for the 1940s U.S. because they do not exist, I can learn about the likely direction and scale of this bias by reestimating multipliers using personal income instead of GDP for a period when state GDP data are available. Replicating Nakamura and Steinsson (2014) using their public replication files but replacing state GDP with state personal income reduces Nakamura and Steinsson's estimates of the output multiplier by about 50%. The relative magnitude of this effect is consistent across specifications. The true effect of measuring output using personal income rather than GDP may differ due to changes in the U.S. economy between the 1940s and 1969–2006 (the period of Nakamura and Steinsson's data), specifically the rate of consumption of physical capital, changes in taxation, the scope of transfer payments, and retained corporate profits. However, this exercise does suggest that using personal income in lieu of GDP may introduce a substantial downward bias.

The definitional differences between GDP and personal income help explain why the GDP multiplier may be substantially larger than the personal income multiplier. At first glance it is not obvious why the personal income multiplier might be smaller: typically GDP and personal income change roughly 1-1. However, GDP includes consumption of fixed capital (allowance for depreciation), while personal income does not. Since production of military goods is substantially more capital-intensive than the overall economy, it follows that the response of GDP to military contracts should be larger than the response of personal income, since the former includes the consumption of fixed capital while the latter does not. Personal income also excludes corporate profits, which could similarly reduce the personal income multiplier relative to the GDP multiplier when government purchases are military contracts.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Given these conceptual differences and the lack of state GDP data for the 1940s, it is reasonable to assume that the personal income multiplier is smaller than the GDP multiplier. If one believes that the personal income multiplier is half the GDP multiplier, the cumulative GDP multiplier would be negligible at the time of the shock, about 0.25 after 1 year, about 0.67 after 2 years, and about 0.95 after 3 years.

3.3.2 Employment multipliers

I define cumulative and one-period changes in employment by:

$$\tilde{e}_{it}^{h} = \sum_{j=0}^{h} \frac{e_{i,t+j} - e_{i,t-1}}{e_{i,t-1}} \text{ and } \breve{e}_{itk} = \frac{e_{i,t-k} - e_{i,t-k-1}}{e_{i,t-k-1}}, \tag{4}$$

where e is the total civilian employment rate in period i and period t. The cumulative employment multiplier at time horizon h can then be defined

$$\tilde{e}_{it}^{h} = \beta_e^h \, \tilde{g}_{it}^{h} \times \frac{y_{i,t-1}}{e_{i,t-1}} + \eta_{k,g} \sum_{k=1}^{8} \breve{g}_{itk} + \eta_{k,e} \sum_{k=1}^{8} \breve{e}_{itk} + \alpha_i + \gamma_t + F_i * \gamma_t + \epsilon_{it}. \tag{5}$$

Because $\tilde{g}$ is constructed as war production relative to personal income, it is necessary to scale $\tilde{g}$ by y/e so that units are comparable. I measure employment on a quarterly basis, so I include 8 lagged values of both war production spending and employment to control for two years of lag-lead exogeneity. For ease of interpretation, both personal income y and war production g are reported in annualized amounts. In all other respects Equation 5 is directly analogous to Equation 3.

Employment multipliers for various time horizons and the corresponding cost per job-year are reported in Panel I of Table 2. The cumulative effect of war production on employment is statistically significant after two quarters and grows monotonically over time horizons up to 12 quarters. These employment multipliers can be translated into a "cost per job year" by taking $\$1,000/\beta_e^h$ and then adjusting for inflation since 1942. The implied cost per job year falls as the time horizon lengthens. Even at fairly long time horizons, the cost per job year is quite high: $405,013 over two years and $232,268 over three years. These estimates use CPI inflation to adjust from 1942 to 2015

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Table 2: Multipliers Estimated Using Employment Data (Quarterly)

time horizon h (quarters)	0	4	8	12
Panel I. Employment multiplier				
β_e^h	0.00755	0.0237**	0.0359***	0.0626***
	(0.00771)	(0.00981)	(0.0123)	(0.0103)
Cost per job-year (2015$)	$1,925,828	$613,502	$405,013	$232,268
Observations	1,127	931	735	539
Within R-squared	0.684	0.731	0.860	0.953
Panel II. Employment effects by sector				
Manufacturing (SIC 3)	0.0160***	0.0419***	0.0596***	0.0627***
	(0.00594)	(0.00599)	(0.00443)	(0.00851)
Ratio $\beta_{e\,mfg}^h/\beta_{e\,total}^h$	2.1	1.8	1.7	1.0
Observations	1,097	901	705	509
Within R-squared	0.674	0.817	0.903	0.961
Private Non-Manufacturing	-0.0104**	-0.0150	-0.0233*	-0.00555
(SIC 1-2, 4-6)	(0.00505)	(0.00929)	(0.0124)	(0.0111)
Observations	1,097	901	705	509
Within R-squared	0.600	0.575	0.737	0.910
Government (SIC 7)	-0.00307	-0.00738*	-0.00757	-0.000900
	(0.00190)	(0.00389)	(0.00556)	(0.00577)
Observations	1,058	874	690	506
Within R-squared	0.601	0.609	0.645	0.843

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Panel III. Personal income multiplier estimated using employment data

$\beta_y^h/((1-\xi)(1+\chi))$	0.0177	0.0750**	0.121***	0.237***
	(0.0238)	(0.0333)	(0.0432)	(0.0353)
Observations	1,127	931	735	539
Within R-squared	0.684	0.730	0.859	0.954

Standard errors clustered by state. Personal income and war production are measured in real 1942 dollars. Time fixed effects and interaction between time fixed effects and pre-war agricultural employment shares estimated but not shown, as are lagged one-period changes of both war production spending and the outcome variable.

*** p<0.01, ** p<0.05, * p<0.1

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

dollars. If I used the GDP Deflator or PCE index instead, the cost per job-year would be roughly $325,000 over two years and $186,500 over three years in 2015 dollars. On the other hand, on average real wages were lower in the early 1940s than they are today: hourly manufacturing wages rose by a factor of 27 between 1942 and 2015, nearly double the increase of the CPI over the same period. This implies that the indirect costs of job creation were larger than a naïve interpretation of inflation-adjusted figures might suggest. On balance, then, the CPI-adjusted figures seem to be a reasonable estimate for comparison, though the uncertainties introduced by comparing figures from such disparate eras erode precision.

Panel II of Table 2 shows the effect of war production on different sectors of employment (1-digit SIC codes). Specifically, employment in each sector or set of sectors is always calculated as the cumulative change in that sector relative to total civilian employment, i.e. the denominators are always total employment so that estimates are comparable across sectors. Lagged one-period changes (as shares of total employment) of both total employment and employment in the relevant sector are included as controls.

The effect of war production on manufacturing employment is consistently larger than the effect of war production on total employment. This suggests that war production displaced non-manufacturing jobs, which is consistent with the historical narrative. Interestingly, the ratio of the multiplier on manufacturing employment and the multiplier on total civilian employment shrinks over time, from just over 2 at the shortest time horizons down to 1.0 after 12 quarters. This helps explain why the total cost per job-year shrinks over time: initially, a state loses one non-manufacturing job for every manufacturing job created by war production. Over time, non-manufacturing jobs recover, until finally after 3 years war production has no significant effect on non-manufacturing jobs but is associated with a sustained increase in manufacturing employment.

3.3.3 Estimating the output multiplier using employment data

Chodorow-Reich (2019) describes the relationship between the output and employment multipli-

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

ers:

$$\beta_y^h \approx (1-\xi)(1+\chi)\frac{y_{t-1}}{e_{t-1}}\beta_e^h \quad (6)$$

where ξ is capital's share of income in the aggregate production function and χ is the elasticity of hours per worker to total employment. Close examination of Equation 5 shows that $\tilde{g}_{it}^h$ is already scaled by y_{t-1}/e_{t-1}; thus it is straightforward to estimate the output multiplier by substituting $\beta_y^h/((1-\xi)(1+\chi))$ for $y_{t-1}/e_{t-1}\ \beta_e^h$. These results are shown in Panel III of Table 2.

In the post-WWII U.S., $\xi \approx 1/3$ and $\chi \approx 1/2$, so that $(1-\xi)(1+\chi) \approx 1$. Under the assumption that these parameters are correct for WWII, the cumulative output multiplier at horizon becomes statistically significant after 4 quarters and grows to 0.24 after 12 quarters. χ can be directly estimated for WWII using monthly data on employment and hours of "production and nonsupervisory employees" in manufacturing. The estimates are highly sensitive to the period chosen: over January 1939 to December 1945, $\chi = 0.38$, somewhat below the post-WWII average of 0.5. But restricting the sample to the period in which the war economy was in full swing, February 1942 (when most wartime restrictions were introduced) to August 1945 (the official end of the war) produces an estimate of $\chi = 0.56$, slightly above the post-WWII average. Either way, the WWII parameter is not dramatically different from the post-WWII parameter, so Chodorow-Reich's formula should be reasonably accurate for WWII.

Robustness checks are presented in online appendix C. Appendix C.1 further discusses the effects of war production spending on government employment and the concern that the geographic placement of war production could be politically motivated. Appendix C.2 explores the role of wartime population flows, while C.3 attempts to account for the production of raw materials by adding an additional set of interactions, between time fixed effects and 1939 state mining employment rates. Appendix C.4 adds controls for tax changes, and C.5 adds controls for military service by state. None of these robustness checks substantially alter the main results presented above.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

3.4 Reconciling Panel vs. Time Series Multiplier Estimates

As mentioned in Section 3.3, the combination of fixed effects and lagged values of (a close transformation of) the dependent variable is known to bias coefficient estimates towards zero (Alvarez and Arellano, 2003). The within-panel multiplier estimates presented above should be understood as lower bounds for this reason. Personal income multipliers may also be smaller than GDP multipliers, as discussed in Section 3.3.1.

To understand the extent of the downward bias in these panel estimates, it is helpful to compare them to time series estimates. Table 3 presents the results using time series identification, calculated using the same underlying data, with war production, personal income, and employment summed across all states. Time series estimation directly follows the the Hall-Barro-Redlick approach detailed in Ramey (2016). With time series data it is also possible to directly estimate the GDP multiplier for WWII. Over one year, the aggregate GDP multiplier is considerably larger than the aggregate personal income multiplier, though they appear to converge at longer time horizons. Even given imprecise time series estimates, this comparison of time series multipliers does suggest that while the GDP multiplier may be larger than the personal income multiplier, it may be less than twice as large.

Aggregate time series estimates—the traditional approach to measuring the fiscal multiplier—identify effects based on variation in timing, implicitly assuming that the dependent variable (the change in government spending) is one of the main influences on the outcome variable (whether GDP, employment, or personal income), attributing all coincidental changes to the dependent variable. Thus aggregate multiplier estimates may misattribute growth to war production when even if it is driven by other factors with coincident timing. This misattribution is less likely with panel estimates, which identify effects based on within-panel variation.

In this sense, the aggregate time series estimates of the personal income and employment multipliers reported in Table 3 may be viewed as an upper bound for the panel estimates. While far less precisely estimated than the panel multipliers, the time series estimates are larger—often substantially larger—at every time horizon. This is unusual, as local multiplier estimates tend to be larger

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Table 3: Bounding the Effects of War Production Spending on Total Non-Farm Employment and Personal Income: Time Series Estimates

time horizon h (years)	0	1	2	3
Panel I. Personal income multiplier				
β_y^h, time series (using personal income)	0.350	0.613	0.821**	
	(0.264)	(0.256)	(0.0425)	
Observations	9	8	7	
Within R-squared	0.6564	0.8097	0.9768	
β_y^h, time series (using gdp)	0.548**	0.573	0.863	
	(0.155)	(0.200)	(0.141)	
Observations	9	8	7	
Within R-squared	0.6461	0.8682	0.9607	
Panel II. Employment multiplier				
β_e^h, time series	0.0565	0.215*	0.171*	0.192**
	(0.0610)	(0.0998)	(0.0720)	(0.00962)
Observations	31	27	23	19
Within R-squared	0.0529	0.5088	0.8731	0.9966

Standard errors clustered by state in panel regressions. Personal income and war production are measured in real 1942 dollars. Baseline regressions include time fixed effects and interaction between time fixed effects and pre-war agricultural employment shares. All regressions include two years of lagged one-period changes for both war production spending and the outcome variable. Time series estimates using annual data are not reported for $h = 3$ because the number of observations is too small given the lagged controls. *** p<0.01, ** p<0.05, * p<0.1

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

than time series estimates as a general rule (Ramey, 2019). Part of this discrepancy is explained by a methodological difference: local multiplier estimates often neglect to control for lag-lead exogeneity. Omitting appropriate lagged values typically seems to increase local multiplier estimates by a factor of two (Ramey, 2020).

What accounts for the gap between the time series and panel multiplier estimates? First, the panel estimates may be downward biased by the (necessary) confluence of lagged values and fixed effects. Second, the effects other factors contributing to economic growth and coincident with war production—but not correlated with the within-panel variation in war production—would be incorporated into the time series estimates but not the panel estimates. In order to bias the time series estimates upwards, the timing of these omitted variables would need to be highly correlated with war production—and thus with American involvement in WWII—and they would have had to have had substantial positive macroeconomic effects. Two factors plausibly meet these criteria.

One likely factor is the wartime farm boom described in section 3.2. While net farm income over 1940–45 was $57 billion, about one-third the total value of war supply contracts, the passthrough from commodity prices to farm incomes was much larger for the farm sector than for other types of employment, as argued by Hausman et al. (2021), so increased income in the farm sector could easily have had an outsized effect on consumption, indirectly boosting non-farm incomes and employment in agricultural areas, just as in 1933 (Hausman et al., 2019). For this reason, the multiplier on government purchases of agricultural products (excluded from the contract data) may be substantially higher than the multiplier on war production. This seems particularly likely given that rationing was less binding in agricultural areas than elsewhere in the country, both because farm equipment was exempt from rationing and because food rationing was much harder to enforce in areas where people produced food and could trade it informally.

Another likely factor is the growth in the size of the military. Military employment grew from roughly 500,000 in mid-1940 to just over 12.2 million in 1945[13], an increase of 11.7 million service members. While military pay was relatively low and some of it was spent overseas, this was

[13] I am indebted to Valerie Ramey for sharing data on military employment during WWII.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

a huge increase in the number of wage earners. Like war production, military employment grew substantially in the early 1940s and then accelerated dramatically after Pearl Harbor. Military pay is not included in my contract data, nor is military employment included in my state-level employment data (which follows the usual conventions of non-farm *civilian* employment), but military pay does factor into personal income (or GDP).

There are also conceptual differences between the aggregate multiplier and the time series multiplier, as discussed by Nakamura and Steinsson (2014) and Chodorow-Reich (2019). In the particular context of WWII, another difference may also matter. In theory, the fiscal multiplier is a marginal effect, but not in practice: it's simply not possible to measure the effect of a marginal dollar of government spending in aggregate data. By construction, aggregate time series estimates of the fiscal multiplier measure the average treatment effect of changes in spending. But with panel data, local multiplier estimates are identified based on relative spending changes across locations. In the case of WWII, every state in the data received war contracts by 1942 at latest.[14] So while still far from a true marginal effect, the panel-data estimates of the multiplier are be closer to measuring a marginal effect than the time series estimates. If there are non-linearities in the fiscal multiplier—as shown later in Section 4.2—this econometric difference may also account for some of the discrepancy between the time series and panel estimates.

4 Discussion

How do the local multiplier estimates reported in Tables 1 and 2 compare to multiplier estimates for other periods? Because the literature typically reports GDP multipliers rather than personal income multipliers, it is difficult to draw meaningful conclusions about the relative size of the personal income multipliers reported in Table 1.

The total employment multipliers presented in Section 3.3.2 are particularly low—and costs per job-year correspondingly high. Even at the longest time horizon considered (12 quarters), the

[14]Only two states report zero contract dollars through 1941, New Mexico and South Dakota.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

estimated cost per job-year is $232,268 in 2015 dollars; it is even higher at every shorter time horizon.[15] These results imply quite small output multipliers, as shown in Panel III of Table 2 (discussed in Section 3.3.3). Indeed, they suggest significantly smaller output multipliers than those found in Section 3.3.1.

Given the higher frequency and likely higher quality of the employment data relative to the personal income data, these small employment multipliers warrant further consideration. Small local multiplier estimates can be interpreted in several ways. One possibility is that they reflect a low aggregate multiplier during WWII—at least for government spending on war manufacturing. Another possibility is that the aggregate multiplier was larger to begin with and quickly pushed the economy onto a very steep section of the aggregate supply curve. This scenario would also produce small local multipliers. Another way to reconcile small employment multipliers with larger personal income multipliers (and potentially much larger GDP multipliers) would be if productivity increased more in locations where capacity constraints were tighter. Ilzetzki (2023) finds exactly these patterns of productivity increases in the WWII aircraft industry.

The remainder of this section explores factors which may have influenced the fiscal multiplier in WWII. Section 4.1 takes a closer look at manufacturing employment and its implications. Section 4.2 discusses the scale of the WWII fiscal shock. Finally, Section 4.3 discusses features of the WWII economy: conversion (4.3.1), the extremely high household saving rate (4.3.2), and the extremely tight labor market during 1942–1945 (4.3.3).

[15]These costs per job-year are high relatvie to recent estimates of job creation from the 2009 American Recovery and Reinvestment Act (ARRA). At the high end, Feyrer and Sacerdote (2011) find an overall cost of $170,000 per job-year and Wilson (2012) finds an initial cost of $125,000 per job-year, both using fairly general measures of ARRA spending distributed through states. At the low end, Chodorow-Reich et al. (2012) find a $26,000 cost per job-year for the Medicaid expansion in the ARRA, or 3.8 job-years per $100,000 of spending, and Shoag (2013) finds a cost of $22,000 per job-year using shocks to state pension fund returns during the Great Recession.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

4.1 Displacement of Non-Manufacturing Jobs

Examining the effect of war production spending on manufacturing employment specifically—as opposed to total non-farm civilian employment—helps explain why estimates of the (total) employment multiplier are so small. Panel II of Table 2 shows that at every time horizon, the response of manufacturing employment is (weakly) larger than the response of total (non-farm civilian) employment (shown in Panel I).[16] In other words, the increase in manufacturing jobs in response to war production spending is larger than the increase in total (non-farm civilian) jobs. At short time horizons, the response of manufacturing employment to war production spending is about twice the response of total employment. The ratio $\beta^h_{e\,mfg}/\beta^h_{e\,total}$ falls as the time horizon grows, reaching 1.0 at 12 quarters. Because the direct effect on manufacturing employment is consistently larger than the total effect, the indirect employment effect of WWII production must have been negative—especially at shorter time horizons. These indirect effects are distinct from conversion, discussed below in Section 4.3.1: conversion specifically involved the displacement of civilian manufacturing by war production, so the employment effects of conversion occur entirely within manufacturing.

4.2 The Scale of the Fiscal Shock

A distinguishing feature of U.S. fiscal policy in WWII is the sheer scale of the expansionary shock. Even if the U.S. was substantially below potential output when the WWII expansion began, it is possible—even likely—that total economic activity was constrained by the production possibilities frontier. In 1942—the year in which the U.S. transitioned to a war economy—$59.5 billion in war

[16]This is not a mechanical effect from a smaller denominator for manufacturing employment. The denominators for cumulative changes in total employment and manufacturing employment (and every other employment subgroup, for that matter) are always total non-farm civilian employment in year $t = 0$, i.e. all employment variables are constructed with the same denominator variable for purposes of comparison.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

contracts were awarded and war production (measured as the uniform distribution over contracts) totaled \$37.1 billion. Contract awards in 1942 totaled a whopping 46% of 1941 GDP.

There is almost certainly a hard limit to how much output can expand in a short time horizon—especially without prior efforts to expand capacity. If the aggregate economy hit this constraint in WWII—and there reason to think it may have given the sheer size of the fiscal shock—we might see lower multiplier estimates due to the influence of this constraint.

Estimating all scale effects on the multiplier would be quite difficult because spending occurs over multiple time periods, the change in spending over each time horizon can be either positive or negative, and theory suggests that effects should be asymmetric. Ceteris paribus, larger spending increases are more likely to hit binding constraints on aggregate output, but there is no theoretical reason to expect scale effects for spending cuts. Rather than attempting to estimate scale effects at all time horizons, I introduce a quadratic term for the initial one-period change in spending from period $t-1$ to t, *only* when that initial change is positive.

Specifically, I add the following quadratic term:

$$(\breve{g}_{it0})^2 \times \mathbb{1}\{\breve{g}_{it0} > 0\} = \left(\frac{g_{i,t} - g_{i,t-1}}{y_{i,t-1}}\right)^2 \times \mathbb{1}\left\{\frac{g_{i,t} - g_{i,t-1}}{y_{i,t-1}} > 0\right\} \quad (7)$$

This approach has several advantages. First, it is relatively simple and can be used at all time horizons, for both the annual and quarterly versions of the data. Second, if the scale of the shock does indeed affect the multiplier, the recent history of shocks should matter, making the initial shock relevant even at longer time horizons. For example, consider two regions that start with identical economic conditions and experience a series of positive fiscal shocks (as occurs in a major war such as WWII). Suppose region A experiences a much larger initial shock than region B, and then later on both regions experience identical shocks. All else equal, the larger initial spending increase in region A should make it more likely that region A will hit the binding constraint on aggregate activity—whether in response to the initial shock or a later shock—because the cumulative shock to region A is larger. Thus, the initial shock should remain relevant even at longer time horizons.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Estimates including the quadratic term given in equation 7 are shown in Table 4.[17] The results suggest that the massive scale of war production did influence its aggregate effects. For personal income, the coefficient on the quadratic term is always negative and significant at the 95% level and the point estimate of β_y is larger at all time horizons. For employment, the coefficient on the quadratic term is strongly negative at shorter time horizons, reaching a minimum of -1.851 at a time horizon of two quarters. At these short time horizons estimates of β_e are also larger, consistent with the personal income results. At longer time horizons, however, the coefficient on the quadratic term loses significance and becomes substantially less negative, becoming positive (though never statistically significant) at time horizons of 8 quarters and longer.

Larger positive spending shocks clearly reduce the personal income multiplier at all time horizons up to three years and reduce the employment multiplier at time horizons under one year. The quadratic coefficients for longer-term employment effects are not precisely identified, so it is not possible to draw definite conclusions. One interpretation is that extensive-margin employment adjusted to large aggregate shocks within two years, but that expansion of output (as measured by personal income) remained limited by the scale of the shock even after three years. This interpretation is plausible if the workers drawn into the workforce after these large shocks were significantly less productive than the workers who were already employed (given the differences in work experience between the two groups), or if larger expansions of war production displaced more productive industries (e.g. the U.S. auto industry's productivity at war production was significantly lower than its productivity at auto production, as suggested by the narrative record).

[17]Separately including a quadratic term for spending decreases does not substantially alter the coefficient estimates reported in Table 4. An even simpler approach—including the square of total war production spending (for location i and time t) produces qualitatively similar results—with the difference that the imprecisely estimated point estimates of the quadratic coefficients for employment at longer time horizons remain negative.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Table 4: Personal Income and Employment Multipliers Allowing for Scale Effects

Panel I. Personal income multiplier

time horizon h (years)	0	1	2	3
β_y^h with quadratic term included	0.120**	0.169**	0.421***	0.592***
	(0.0575)	(0.0678)	(0.0994)	(0.145)
Quadratic (+ shock only)	-0.386**	-0.539**	-0.905**	-0.928**
	(0.152)	(0.259)	(0.351)	(0.376)
Observations	343	294	245	196
Within R-squared	0.767	0.843	0.909	0.935

Panel II. Employment multiplier

time horizon h (quarters)	0	4	8	12
β_e^h with quadratic term included	0.0254***	0.0264**	0.0358***	0.0617***
	(0.00895)	(0.0105)	(0.0130)	(0.0107)
Quadratic (+ shock only)	-0.651***	-1.592	0.0364	1.269
	(0.152)	(0.964)	(1.349)	(1.594)
Observations	1,127	931	735	539
Within R-squared	0.688	0.731	0.860	0.953

Standard errors clustered by state. Personal income and war production are measured in real 1942 dollars. Time fixed effects and interaction between time fixed effects and pre-war agricultural employment shares estimated but not shown, as are lagged one-period changes of both war production spending and the outcome variable. *** p<0.01, ** p<0.05, * p<0.1

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

4.3 Features of the WWII Economy

In addition to the sheer scale of fiscal expansion in WWII, specific features of the WWII economy may also influence estimates of the multiplier. I address the three features most relevant to the fiscal multiplier: conversion (4.3.1), the extremely high household saving rate (4.3.2), and the extremely tight labor market during 1942–1945 (4.3.3).

4.3.1 Conversion

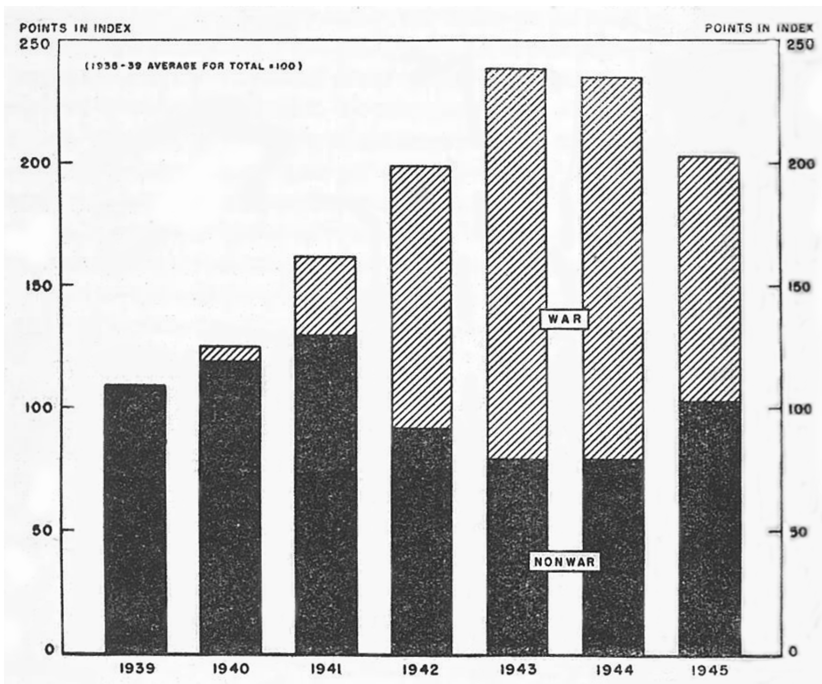
Before U.S. entry into the war, and especially before the defense buildup which began early in the summer of 1940, the American defense industry was tiny. Many defense production facilities from WWI had been abandoned, or in some cases intentionally demolished (to avoid capital taxes) during the interwar years (Nelson, 1946, p. 32). While the defense industry dramatically expanded its production capabilities, these additions were far from adequate for supplying military needs in the necessary timeframe.

Thus *conversion* of existing manufacturing facilities from civilian to military production was necessary. Conversion is defined as the retooling of facilities previously in use for civilian production to produce goods for the war effort. Figure 4 shows a significant drop in non-war industrial production after 1941. This reduction in non-war industrial production reflects conversion of non-war industrial capacity for war production. Conversion came about both through direct government intervention and through the indirect pressures imposed by shortages of strategic materials.

Production of certain civilian goods was expressly prohibited for the duration of the war. The most famous example of this is the automotive industry. Consumer demand for vehicles was quite high: auto sales broke records in 1941, exceeding 1929 sales in nominal terms for the first time (prices were still below 1929 levels). Automakers would have liked to keep producing and selling cars and consumers would have kept buying them. But planners quickly recognized that there was no way to acquire sufficient rubber and steel for war production without completely diverting raw materials from the auto industry, so civilian automobile production was prohibited in early February 1942 and did not resume until the summer of 1945. American auto producers retooled

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Figure 4: Industrial Production 1939–1945 (United States)



Source: Bureau of the Budget, based on data from the Federal Reserve Board of Governors

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

their assembly lines and switched to producing planes, tanks, etc. In short, automakers maximized their profits subject to the constraints imposed by wartime rationing, though not producing new civilian vehicles imposed a significant loss in welfare.

In practice, firms could not obtain raw materials with strategic value for any purpose other than war production or essential civilian manufacturing: these materials were not available at any price unless their use had been certified as necessary to the war effort. In many cases (especially metals), there were also shortages of substitutes. Many firms faced a choice between sitting idle for lack of inputs or retooling to take on defense production. Shortages of strategic materials also made it difficult to expand total industrial capacity; thus materials shortages and the conversion of industrial capacity were tightly intertwined. The speed of conversion forced suboptimal allocations and distorted markets by making it impossible to fully increase productive capacity. The explicit policy goal of conversion was to maximize war production, not to maximize economic activity.

One can understand conversion as an application of Pareto's theory of second best: given the economic distortions introduced by war production, conversion (and associated control of strategic materials) was an attempt to minimize the economic costs of war production, specifically preventing production bottlenecks. With extreme shortages of strategic materials—particularly most types of metal (iron, steel, aluminum, copper, etc.) and rubber—conversion reduced competition for these materials (at the cost of eliminating production of civilian goods). In this sense, conversion either reduced the scale of the aggregate demand shock due to war production or made a larger aggregate demand shock possible in the short run. If the aggregate supply curve was locally very steep (as suggested by the extreme shortages of materials), conversion likely had much larger effects on prices—specifically, minimizing inflation—than on quantities.

Conversion may affect local multiplier estimates because conversion was far from evenly distributed across states. Larger spending shocks were possible in locations with more conversion, with effects as described in Section 4.2.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

4.3.2 Household Saving

Household saving increased dramatically during WWII. The personal savings rate jumped from 6.8% of after-tax income in 1940 to 26.2% in 1942, peaking at 27.9% in 1944. The pattern is very similar for net private saving, which jumped from 6% of GDP in 1939 to more than 20% of GDP in 1942, 1943, and 1944. WWII is a clear aberration, with substantially higher savings rates than any other period in modern U.S. experience (including 2020).

Why did saving behavior alter so dramatically during the war? Wartime restrictions on purchases of durable goods provide an obvious explanation: savings rates increased dramatically when rationing was imposed. Because the manufacture of durable goods was so restricted due to materials shortages during the war, households could not respond to rising incomes by increasing their purchases of durables. Consumption of nondurable goods was less affected by rationing, though few sectors were completely untouched by the war effort. But nondurables are not close substitutes for durables. It appears that many consumers saved instead—and likely used those savings to purchase durable goods after the war.

To affect local multiplier estimates, saving responses to war production must have been differentially larger in times and locations where per-capita war production was higher. Panel analysis of bond purchase data (substituting bond purchases for the outcome variable) does show a positive correlation between cumulative war production spending and cumulative bond purchases over most time horizons (Brunet and Hlatshwayo, 2023).

4.3.3 Labor Market Conditions

While unemployment was still quite high at the start of 1940, it diminished rapidly during the "defense period" before Pearl Harbor. By November 1941—the month before the U.S. formally entered the war and defense spending took off—unemployment had fallen to 3.8%, already very low by historical standards. By September 1942 unemployment had fallen below 2%, and it stayed below 2% until January 1946. While many women did enter the workforce, during the early years of the war women entered approximately 1:1 with men exiting the labor force as they entered the

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Armed Forces. By 1945 the U.S. Armed Forces had grown to over 12 million—removing almost 12 million people (mostly prime age men) from the civilian labor force.

While the empirical evidence on this question is mixed (see Ramey and Zubairy, 2018; Auerbach and Gorodnichenko, 2012, 2013; Auerbach et al., 2020), there are theoretical arguments that the fiscal multiplier should be smaller when the unemployment rate is low due to lower excess capacity in the economy. Given the extraordinarily low unemployment rate prevailing throughout much of WWII, this argument should be relevant to WWII if it is applicable in any setting.

5 Conclusion

When assessing the macroeconomic effects of WWII, it is essential to remember that the policy goal was winning the war. Economic stimulus was incidental to the war effort, though appreciated by policymakers at the time. Yet WWII did stimulate the American economy, at least to some extent, even with rationing, price controls, and other economic interventions that constrained normal market mechanisms.

I find cumulative local personal income multipliers of 0.34 over two years and 0.49 over three years. If the GDP multiplier is twice the personal income multiplier, as suggested by the empirical exercise in Section 2.2, this implies GDP multipliers of 0.68 over two years and 0.98 over three years. However, I find much smaller employment multipliers than these GDP multipliers might suggest. I estimate a cumulative local multiplier of 0.036 after 8 quarters and 0.063 after 12 quarters, corresponding to costs per job year of $405,013 and $232,268 in 2015 dollars, respectively. Quarterly employment data can also be used to indirectly estimate the output multiplier. These results are smaller than the direct results for personal income: 0.12 after 8 quarters and 0.24 after 12 quarters. If both sets of estimates are correct, they may imply differential productivity growth in locations with more war production.

WWII was the largest fiscal shock in modern American history. My findings suggest that while WWII military spending did expand both unemployment and personal income, the aggregate

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

effects—particularly on employment—may have been muted by the sheer scale of the fiscal shock, with war manufacturing displacing non-manufacturing jobs. I find evidence for asymmetric scale effects: larger positive spending shocks are associated with smaller increases in personal income and employment (while I find no evidence of non-linearities in negative spending shocks). These findings suggest that it becomes increasingly difficult to expand economic activity beyond a certain point in the short run, even with extraordinarily large spending increases.

References

Acemoglu, Daron, David Autor, and David Lyle (2004) "Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury," *Journal of Political Economy*, 112 (3), 497–551.

Alvarez, Javier and Manuel Arellano (2003) "The Time Series and Cross-Section Asymptotics of Dynamic Panel Data Estimators," *Econometrica*, 71 (4), 1121–1159.

Auerbach, Alan and Yuriy Gorodnichenko (2012) "Measuring the Output Responses to Fiscal Policy," *American Economic Journal: Economic Policy*, 4 (2), 1–27.

——— (2013) "Output Spillovers from Fiscal Policy," *American Economic Review*, 103 (3), 141–146.

Auerbach, Alan, Yuriy Gorodnichenko, and Daniel Murphy (2020) "Local Fiscal Multipliers and Fiscal Spillovers in the United States," *IMF Economic Review*, 68 (1), 195–229.

Barro, Robert J. and Charles J. Redlick (2011) "Macroeconomic Effects from Government Purchases and Taxes," *Quarterly Journal of Economics*, 126 (1), 51–102.

Brunet, Gillian and Sandile Hlatshwayo (2023) "War, Pandemic, and Household Saving: the COVID-19 Pandemic through the Lens of WWII," unpublished working paper.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Bureau of the Budget (1946) *The United States at War: Development and Administration of the War Program by the Federal Government*.

Bureau of Labor Statistics, United States Department of Labor (1947a) *Activities of the Bureau of Labor Statistics in World War II: Development and Administration of the War Program by the Federal Government*.

Chodorow-Reich, Gabriel (2019) "Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?," *American Economic Journal: Economic Policy*, 11 (2), 1–34.

Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Gui Woolston (2012) "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act," *American Economic Journal: Economic Policy*, 4 (3), 118–145.

Civilian Production Administration (1946) *Alphabetic listing of major war supply contracts: cumulative June 1940 through September 1945*.

Collins, William J. (2001) "Race, Roosevelt, and Wartime Production: Fair Employment in World War II Labor Markets," *American Economic Review*, 91 (1), 272–286.

Fetter, Daniel K. (2016) "The Home Front: Rent Control and the Rapid Wartime Increase in Home Ownership," *Journal of Economic History*, 76 (4), 1001–1043.

Feyrer, James and Bruce Sacerdote (2011) "Did the Stimulus Stimulate? Real Time Estimates of the Effects of the American Recovery and Reinvestment Act," *NBER Working Paper 16759*.

Fishback, Price and Joseph A. Cullen (2013) "Second World War spending and local economic activity in US counties, 1939–58," *Economic History Review*, 66 (4), 975–992.

Goldin, Claudia (1991) "The Role of World War II in the Rise of Women's Employment," *American Economic Review*, 81 (4), 741–756.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Goldin, Claudia and Claudia Olivetti (2013) "Shocking Labor Supply: A Reassessment of the Role of World War II on Women's Labor Supply," *American Economic Review*, 103 (3), 257–262.

Gordon, Robert J. and Robert Krenn (2010) "The End of the Great Depression 1939–41: Policy Contributions and Fiscal Multipliers," *NBER Working Paper 16380*.

Hall, Robert E. (2009) "By How Much Does GDP Rise If the Government Buys More Output?," *Brookings Papers on Economic Activity*, fall, 183–231.

Hausman, Joshua K., Paul W. Rhode, and Johannes F. Wieland (2019) "Recovery from the Great Depression: The Farm Channel in Spring 1933," *American Economic Review*, 109 (2), 427–472.

——— (2021) "Farm Product Prices, Redistribution, and the Early U.S. Great Depression," *Journal of Economic History*, 81 (3), 649–687.

Higgs, Robert (1992) "Wartime Prosperity? A Reassessment of the U.S. Economy in the 1940s," *Journal of Economic History*, 52 (1), 41–60.

Ilzetzki, Ethan (2023) "Learning by Necessity: government demand, capacity constraints, and productivity growth," unpublished working paper.

Industrial College of the Armed Forces (1947b) *Applied Military Economics: A Case Study of the American War Economy (Preliminary Draft)*.

Internal Revenue Service (1936–1946) *Statistics of Income Report, Part 1*.

Jaworski, Taylor (2017) "World War II and the Industrialization of the American South," *Journal of Economic History*, 77 (4), 1048–1082.

Jordà, Òscar (2005) "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95 (1), 161–182.

Nakamura, Emi and Jón Steinsson (2014) "Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions," *American Economic Review*, 104 (3), 753–792.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01432

Nelson, Donald M. (1946) *Arsenal of Democracy: the Story of American War Production*: Harcourt, Brace and Co.

Ramey, Valerie A. (2016) "Macroeconomic Shocks and Their Propogation," in Taylor, John B. and Harald Uhlig eds. *Handbook of Macroeconomics, Vol. 2*, 71–162, https://doi.org/10.1016/bs.hesmac.2016.03.003.

——— (2019) "Ten Years After the Financial Crisis: What Have We Learned from the Renaissance in Fiscal Research?," *Journal of Economic Perspectives*, 33 (2), 89–114.

——— (2020) "Comment on "What Do We Learn from Cross-Sectional Empirical Estimates in Macroeconomics?"," in *NBER Macroeconomics Annual, Vol. 35*, 232–241.

Ramey, Valerie A. and Sarah Zubairy (2018) "Government Spending Multipliers in Good Times and in Bad: Evidence from U.S. Historical Data," *Journal of Political Economy*, 126 (2), 850–901.

Rhode, Paul W., James M. Snyder Jr., and Koleman Strumpf (2017) "The Arsenal of Democracy: Production and Politics During WWII," *NBER Working Paper 24158*.

Shoag, Daniel (2013) "Using State Pension Shocks to Estimate Fiscal Multipliers since the Great Recession," *American Economic Review*, 103 (3), 121–124.

Smith, R. Elberton (1959) *United States Army in World War II: the Army and Economic Mobilization*: Center of Military History, United States Army.

Wilson, Daniel J. (2012) "Fiscal Spending Jobs Multipliers: Evidence from the 2009 American Recovery and Reinvestment Act?," *American Economic Journal: Economic Policy*, 4 (3), 251–282.

Wilson, Mark R. (2016) *Destructive Creation: American Business and the Winning of World War II*: University of Pennsylvania Press.