

WWII Contract Spending and Labor and Capital Income.

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Abstract: In this paper I show a counter-intuitive, negative, state level income multiplier for WWII contract spending. This negative multiplier shows that the war had a strong, permanent convergence effect on states that received less war spending, a difference in growth rates driven by both the direct political economy effect of the war and across-state migration. As a shock to manufacturing, the war had a strong temporary positive effect on the growth of manufacturing income that evaporated by 1945. State level per capita nonmanufacturing income, meanwhile, experiences permanently slower growth in response to war spending. I show this slower growth in nonmanufacturing income is explained 50/50 between migration and the direct fiscal policy effect. Lastly, personal capital income (interest, dividends and rent) grows more slowly in response to WWII spending. This effect is explained mostly by migration before 1947. After 1947 the direct fiscal effects of the war explain slower capital income growth.

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1 Introduction

In this paper I show that wartime contract spending results in long term slower growth of personal income relative to 1940, an effect that is “permanent” in the sense that by 1957—the end of the available panel—personal income growth is slower in states with more war spending. For every dollar of wartime spending, total personal income grows 7.3 cents slower by 1945, and grows 13.4 cents slower by 1957. While the per dollar multiplier is small, the sheer size of the wartime shock means a large negative effect on the growth of nominal income in states that received war spending. A hypothetical state with average per capita personal income that received an average amount of war spending per capita personal income would be \$90 or 13% lower by 1945. By 1957 personal income growth would be lower by \$165.93 or 12% of personal income. This slower growth in personal income from wartime contract spending is primarily driven by two components of income: slower growth in nonfarm ownership income and slower growth in income from private nonfarm, nonmanufacturing industries¹.

The negative state level multiplier driven by migration helps resolve an outstanding puzzle among economic historians. Much current research reinforces a long standing disconnect between the popular assumption that “WWII ended the Great Depression” and the fact that no clear mechanism explains how that happened. Fishback and Cullen (2013) and Fishback and Jaworski (2016) find no postwar intensive county level growth in retail sales, income and home prices in response to the war. Jaworski (2017) finds no effect of wartime investment spending on long term economic development in the south. As well, Paul Rhode (1994, 2003) finds that west coast states, particularly California, experienced extensive economic growth due to the war but no long term change in their industrial composition. Further, the dynamics of war spending itself makes it difficult to identify a clear mechanism for how the war undid the secular stagnation of the Depression. The war had a large, but ultimately temporary effect on manufacturing, a fact evident in the response of manufacturing income to wartime contract spending discussed below. Despite the fact that military spending remained

¹Referred to as simply “nonmanufacturing income” in the rest of the paper.

elevated after the war, the industries that saw the largest wartime boom in employment—aircraft manufacturing and shipbuilding—saw a collapse of demand after the war. It was just not possible for domestic civilian or foreign demand to make up for wartime government demand of planes and ships. Employment in these industries increased from 118 thousand in 1939 to a peak of 2.25 million in 1943, but the end of 1947 employment in these industries had contracted to 285 thousand (US Dept of Commerce, 1949). The temporary nature of the wartime shock is likely why Rhode finds no long run changes to the industrial structure of the west coast and Jaworski finds no long run change in southern industrial development. As well, as Alexander Field (2019) points out taht, if anything, war spending had a negative effect on productivity. The industries that saw the largest productivity gains during the war shrank back into relative irrelevancy in the aggregate after the war.

Figure 1 makes the basic point of this paper. Figure 1 demonstrates the negative relationship between war spending and nominal income growth from 1940 to 1947. States are grouped by whether they experienced net in-migration (black) or out-migration (red). The shock of the war is very large with states receiving average contract spending from 1940-1945 of 211% of 1940s per capita income. Average per capita income growth in the US between 1940 and 1947 was 123%. The 28 states with net out-migration had average per capita income growth of 166% while the 20 states with net in-migration had an average per capita income growth of 115% from 1940-1947. All states during this period experienced rapid nominal income growth, but as Figure 1 shows, states with net out-migration are largely clustered in the bottom right of the graph, having received lower levels of per capita war spending but also having experienced faster income growth.

The negative nominal income multiplier in this paper, on the other hand, elucidates the mechanisms of how the war alleviated the the economic pathology of the Depression. It also provides insights into the mechanisms for the post-war regional convergence and the post-war across state dynamics of inequality (Bossie and Kuehn, 2021; Farber et al, 2021). As Robert Higgs (1999) argues, WWII did not permanently increase the level of potential output

relative to the pre-war trend. To put this differently, as Field (2008) does, war spending did not provide a meaningful permanent boost to production on the supply side. Rather, WWII was a shock that lifted the economy out of the chronic, below potential, hysteresis of the Depression (Mathy, 2018). The wartime shuffling of labor and concomitant more productive use of the labor force, and the corresponding increases in per capita income set the economy back onto its pre-Depression trajectory. The relative multipliers in this paper show that wartime migration played as large role as the direct fiscal shock to manufacturing in the reshuffling of labor that broke the secular stagnation of the Depression.

To estimate the effect of war spending on state level growth I use a simple fixed effects specification, with state and year dummies to establish the basic dynamics of the war's effect on personal income. The central identifying assumption is the sheer size of the shock. On average total contract spending is 211% of of average state personal income in 1940, which accounts for almost all of the variation in the economy at least through 1947. It is well known that consumption hovered around its 1939/1940 level throughout the war (Higgs, 1999; Edelstein, 2000) and that private investment, both business and residential, was almost totally crowded out. After the war ends, the reconversion period 1945-1949 is characterized by two mild reconversion recessions driven entirely by the contraction in military spending. Under the assumption that the war is a large exogenous shock, there are two main potential sources of bias for the effect of contract spending. Rhode et al (2017) find that political distribution of contracts is not a source of bias, but that war shocks are correlated with state industrial structure. State fixed effects control for aspects of industrial structure that do not change over time. Since industrial structure across states is highly correlated within census divisions I control for the evolution of industrial structure over time by including an interaction of a time dummy with census division dummy (Allegreto et al 2011). Tests for “historical endogeneity” find that the shock of the war, controlling for industrial structure, is not correlated with income variables during the late 1930s. Finally, since my panel extends to 1957, I control for the shock of the Korean War—which is highly correlated with the

WWII shock—after 1950.

Using this FE specification I establish the following pattern of income response: total income grows 11.5% slower by 1957 in a hypothetical average state that receives an average amount of war spending. Such an average state would see manufacturing income 34.2% higher in 1943, but this temporary growth effect completely dissipates by 1946. In contrast, nonmanufacturing income growth in an average state would be 10.8% lower in 1944 and 15.3% lower in 1957.

To decompose the underlying mechanisms driving the effect of wartime contract spending on income I employ mediation analysis using a three equation structural equation model (SEM). This SEM model is identified under the same exogeneity assumptions as the FE model which is incorporated into the SEM model. This SEM technique can be thought of as analogous to VAR variance decomposition with a Cholesky ordering of war spending \rightarrow migration \rightarrow income. The migration variable used here is “excess migration” from 1940-1947 above the across state migration experienced during the depression migration trend of 1935-1939.

Slower growth in nonmanufacturing income is explained in equal parts by the direct fiscal and migration effects during the reconversion period. However, only migration has any statistical explanatory power after 1950. The effects across nonmanufacturing industries is heterogeneous and is concentrated in transportation and public utilities payrolls as well as wholesale and retail trade payrolls. The concentration of this effect is consistent with wartime excess demand for labor in these industries as well as the secular post-war national shifts in industrial structure towards those industries. Additionally, I find a statistically weak effect of migration on mining payrolls. In contrast, during the reconversion process the direct fiscal effect is the sole driver of the slower growth of service sector payrolls in response to the wartime shock.

I also find a substantial, negative response of ownership income to the shock of the war which explains about 30% of the effect of the war on total per capita personal income. By

1957 an average state receiving an average amount of war spending would see capital income grow (interest, dividend and rental income) 31% slower by 1957. Nonfarm proprietor income would be roughly 11% lower, though this result is less statistically robust than the response of capital income. Evidence from state level corporate income reinforces the evidence from personal income. Post-tax net corporate profits in our hypothetical average state grew 53% more slowly by 1951. This slower growth in profits is reflected in retained earnings, which grow 7% slower in an average state. Dividends also grow more slowly in response to the war shock, though this effect is statistically weak. To the extent that state level dividends paid out by corporations is comparable to state level personal capital income, the behavior of dividends explains roughly one-third of the total capital income response to war spending. Bossie (2020) shows that interest payment growth, at least from national banks, is not different across states in response to war spending, but most of the difference in commercial bank balance sheet growth comes from demand deposits. Give the Regulation Q prohibition on interest payments for demand deposits it makes sense that local interest income was close to zero. This implies two-thirds of the capital income response is from rental income. This is consistent with the effect of rent controls during the war found in Fetter (2016). He finds that rental controls both lowered relative rent payments and also induced landlords to sell. This would permanently reduce per capita rental income.

Finally, I show a surprising response of farm income to war spending. Farm wages are not, in a statistically meaningful way, affected by either migration or direct spending. Farm proprietor income, on the other hand, shows a relatively strong statistically significant response to the direct fiscal effect of the war. The lack of an across state migration effect on farm incomes is striking given the historical narrative that lack of postwar farm labor lead to structural changes in farming techniques, due to migration, towards greater capital intensity (Grove, 2000 for instance).

2 Historical Background

This historical background does three things. First, it provide a justification for using nominal and not real figures, which is that there is no credible or agreed upon national wartime deflator, let alone a useful state level deflator. A rough calculation suggest that the nominal effect discussed below can be decomposed into a two-third real and one-third inflation effect. Second, it fleshes out the labor demand forces likely driving the migration effects on nonmanufacturing income which is that sectors with a strong migration effect are also those nonmanufacturing sectors that show both strong wartime and postwar labor demand. Further, we can identify two likely channels through which migration would affect per capita income in an industry, the first is an effect on the composition of employment and the second is a shift of the labor demand curve to the left. There is more clear historical evidence for the compositional effect. Finally, I discuss the two possible mechanism that explain the direct fiscal effect of the war on state level capital income. Here there seems to be a strong “political economy” effect of the war from the heavy hand of the federal government in war industries. However, much of the direct effect on ownership income is driven by the postwar business cycle. The collapse in employment in major war industries after the war suggest capital income also fell in those industries.

2.1 Understanding Nominal Results

as discussed above, this paper deals in nominal income because there is no credible state level deflator available². It is important to bear in mind that the empirical results in this paper show “relative open economy multipliers” and measure differences in relative growth rates of state level income in response to war spending. To gauge the relative inflationary and real output growth of the results below it makes the most sense to look at the behavior of nominal income and prices from 1940 to 1947 because it is not until 1947 that the full

²See Bossie (2020a) Appendix A for a discussion of the various attempts to establish a national level deflator that takes into account he various wartime distortions of the price level

inflation of the war manifested itself during the decontrol process. The CPI increased from Jan 1940 to Jan 1948 by 43% suggesting real per capita income in the US grew by 80% between 1940 and 1947. Another way to think about this is that for the nominal results below one can make a rough assumption that 2/3rds of the relative multipliers represent disparate growth in real variables.

To the extent that the wartime inflation was local, this average national inflation rate probably overstates inflation in states losing migrants. While income increased on average for those who remained it is unlikely their increased demand for food—a major source of national WWII inflation from 1940-1947—was enough to offset the decline in food demand as migrants left. Second, the inflationary effects on housing of migration into war spending states is well known. This effect is ambiguous, though. There likely would have been a relative disinflationary effect on housing in states with net out migration and sometimes an absolute decline in population during the war. However, rent control in war centers would have slowed down the inflationary impulse of housing shortages in war production centers (Fetter, 2016) Bossie (2020) offers some hints on the possible inflationary effects of the monetary shock of the war at the state level. Using a similar empirical strategy as this paper, shows slower growth in deposits in local banks in response to war spending that is roughly the same magnitude as the slower growth of income presented here. This suggests that the excess growth in income in states not receiving war spending was primarily saved. This did not result in increased local lending—deposit increases were held as paper assets, namely Treasury bond and reserves. This short circuit between the increase in the local money supply without an increase in local lending would have mitigated the potential inflationary impact of the excess growth in income in non-warspending states.

2.2 National context for migration's effect on non-manufacturing income

The effect of war spending on nonmanufacturing income is heterogeneous across various nonmanufacturing industries. The strong effect of war spending through migration in transportation and utilities and in wholesale and retail trade is consistent with the fact that, nationally, those industries saw the strongest excess demand during the war, excluding manufacturing. Unlike manufacturing, however, demand for labor in these industries remained consistent after the war was over. An increase in per capita income from specific industries in response to out-migration can come from two possible mechanisms. Out-migration of a worker in a given industry can increase per capita income mechanically if workers move into a industry from another industry. It is also possible for per capita income in an industry to increase if out-migration causes a shift of the supply curve of labor for that industry. This also holds more broadly for the behavior of capital income, which does show a clear wartime response to contract spending. Here, the compositional effect is easiest to if relatively low wage workers who are not wealthy enough to own assets migrate this would mechanically increase the share of per capita capital income. The empirical evidence below also points to a nonuniform effect on wages and salaries across non-manufacturing industries. The historical evidence from US Employment Services on placements and other labor market statistics show that employment growth is more consistent with a compositional explanation for faster income growth in specific sectors.

The second way in which migration can affect relative incomes is through shifting the labor supply curve in out-migration states to the left. Here, the historical evidence for what the effects were on labor supply are unclear. Historical evidence on the skills mix of job demand and wage increases offer indirect evidence that migration during the war was being driven by strong job demand across all skill levels. However, across sector wage data suggests that wages for unskilled labor grew the fastest through the war and reconversion period.

Table 1 shows placement data from the U.S. Employment Service (USES). This placement

data can be used as a measure for labor demand pressure from various industries. Monthly placements peak in August of 1944 so placement from the last two years of the war offer us a useful picture of marginal job demand during the period of the war where the economy was running above potential. For our purposes this table offers two different insights. The first is that—excluding manufacturing—trade and transportation and utilities placements account for the largest shares of placements during 1944 and 1945. This offers a limited explanation for why migration affects per capita income in those industries so strongly. These sectoral effects are also consistent with evidence from Kuznets et al, (1960) that shows shifting nonmanufacturing industrial structures from 1940-1950 in out-migration regions, specifically the South, brought them more in line with the rest of the country.

The second insight is that the skills mix of labor demand is broadly based. Half of placements were unskilled. It is common among economic historians of this period to point out that skilled and/or more educated workers were more likely to migrate long distances (Boustan, 2017). Further evidence from “The Labor Market”, a monthly publication from the U.S. Department of Labor and the U.S. Employment Service, helps flesh out the summary in Table 1. First, from monthly data on placements we can disaggregate the trade and services categories. Placements for trade make up about two thirds of that category. The skills mix from placements in September of 1944 (US Department of Labor, January 1945) offers some additional anecdotal evidence for skills mixes across industries. The proportion of skills mix in trade and transportation and utilities are almost reciprocal. Transportation and utilities placements are over three quarters unskilled labor while trade placements are two thirds of skilled placements. Further, Table 2 shows the evolution of employment from 1944 to 1948. In contrast to the steep decline in manufacturing employment during the reconversion process, Table 2 shows that strong demand for labor was maintained for nonmanufacturing sectors through the postwar recession, though the increase in employment in trade is almost 10 times that of transportation and utilities.

The ambiguity of the relationship of the mix of skills demanded in industries that are

significantly affected by migration is compounded by the pattern of wage growth during the war. Figure 2 shows the changes in hourly earnings from 1940-1980 for a selection of industries. It should be acknowledged that there is some arbitrariness to the industries listed. What emerges most clearly is that in nonmanufacturing industries wages grew fastest in low skilled occupations. Farm wages grew fastest of all. This effect of the shifting supply of agricultural labor is ubiquitous in the historical literature but I find no evidence for there to be an across state migratory effect on farm incomes below. Figure 2 shows that in addition to agricultural workers hotel workers, road builders, common construction laborers and coal and quarry miners all show hourly earnings growth much faster than the average of the industries presented in Figure 2. However, it is not possible to clearly tie this to the skills makeup of industries showing the strongest response to out-migration.

The key takeaway for the reader is that national level evidence strongly supports the compositional explanation for the effect of migration on per capita income in nonmanufacturing industries. However, a clear leftward shift of the labor supply curve for trade and transportation and utilities is not as obvious. While the skills mix from USES data suggests broad based demand for labor at all skill levels, the strongest hourly earnings growth during the war are for unskilled occupations. I leave speculation about how this paper suggest mechanisms that lead to excess pressure on unskilled wages to the discussion section.

2.3 Context for direct fiscal effects on capital income

It is useful to discuss the various factors underlying the “direct” effects of the fiscal policy shock. Here we are most interested in explaining the behavior of the personal capital and corporate income responses to the shock. There are a number of overlapping factors that can generally be labeled the “political economy” factors. One possible limiting factor is that the impact of the war shock is limited by supply constraints. Brunet (2022) argues the small and or negative relative multipliers could be driven by the fact that the huge shock of the war simply pushed the economy up along the aggregate supply curve. This would

mitigate the per dollar effect of war spending as it pushed against hard local supply constraints. Outside of supply constraints, robust labor markets in war centers was a significant channel of redistribution from capital to labor (Bossie and Mason, 2020). Wartime labor markets also provided a strong tailwind to the post-Wagner-Act increase in union density. The strong hand of the Federal Government also encouraged the increase in union density to keep industrial peace. The Federal government also intervened directly in wartime labor markets in other ways. Much of this was aimed directly at companies receiving war contracts through such devices as the National War Labor Board (Farber et al, 2020). As well, the Fair Employment Practices Committee discouraged defense contractors from racial wage discriminating, limiting the ability of defense contractors to increase profits through discriminatory wage policy (Collins, 2001). While this paper does not show a difference in corporate taxes, across state corporate profits were controlled by other means. Wilson (2010) argues that the military was quite conscious of war profiteering and often clawed back profits from firms they believed received profits that were too high. Rent controls similarly limited profits for landlords in boom cities (Fetter, 2016).

However, these factors only explain the temporary wartime effect on capital. Much of the postwar differential in growth rates can be traced to the way in which the temporary shock of the war dissipated during the postwar business cycle adjustment back to civilian production. Table 2 makes the basic point that during the postwar reconversion, employment in both aircraft manufacturing and shipbuilding collapsed, with employment in both industries shrinking by over 80% by the end of 1948. This decline in employment was permanent in those industries, since there was simply no way civilian markets could make up for military demand. This collapse in employment can also be thought of as a proxy for what happened to ownership income in those industries.

3 Baseline Empirical Results

3.1 Empirical methodology

The basic empirical model and military contract data used to study the effect of WWII military contract spending on the income (and the components of income below) is the same as that used to study the response the effect of WWII on county level southern industrial development in Jaworski (2017) and the effect of military contract spending on state level bank balance sheets in Bossie (2020).

The basic empirical specification is as such:

$$INCOME_{it} = \beta_{1t}(y_t * WWII_i)s_i + y_t + \delta_{1t}(y_t * CENSUS_j) + \delta_{2t}(y_t * KOREA_i) + \epsilon_{it} \quad (1)$$

All variables are population adjusted and in logs³. $INCOME_{it}$ is a personal income variable: total income or its sub-components. This specification includes a dummy variable each for state (s_i) and year (y_t). These control for time invariant individual state characteristics as well as policy changes across years. This paper, as with the other two papers mentioned, can be seen as employing a variation of the methodology used in Nakamura and Steinsson (2012) to calculate state level “open economy” or “relative” fiscal policy multipliers. The effect of interest is β_{1t} which captures the effect of $WWII_i$ contract spending interacted with a time dummy. The contract spending variable, $WWII_i$, is a single observation of total contract spending per state. $WWII_i$ includes military contracts of \$50,000 or more for the period June 1940 - September 1945 (Haines, 2010). There is also data on military contract spending for some subcategories of contract spending. Total war contract spending can be broken down into four subcategories and so it makes sense to explore the differences in the effect of plant and equipment spending, heavy equipment and non-heavy equipment supply

³It should be noted that it is occasionally necessary to add one to a variable to make log transformations tractable. When this is necessary it will be noted in the text.

contracts, and military facilities spending. In this section the focus is on total war spending. These subcategories should be thought of as contracts for investment spending, production of durable and nondurable goods, and government military infrastructure respectively. One challenge of the data is that the wartime contract spending variable is time invariant.

Normally, when estimating an interaction effect it is common practice to also include the “main effect” as well as the interaction effect in the estimate of a total effect. However, the time invariance of $WWII_i$ makes it pointless to include it individually as time invariant state level variation is absorbed by s_i . This does not pose a problem for our estimate because the effect of the contract spending variable is captured adequately by the interaction with the time dummy. The concern of this paper is the change in the components of income for 1941 to 1957 relative to 1940 attributable to war spending and the interaction effects captures this when we drop the 1940 year dummy. As well, since the state fixed effects already includes the variance of time invariant war spending variable it does not provide any new information to the regression. Given that the individual effect of $WWII_i$ is neither material to the question of interest nor useful in controlling for variation in our regression, not including the individual effect is costless. Additionally, $KOREA_i$, a single observation of total war spending per state is included to control for the effect of the secondary shock of the Korean War (Secretary of Defense, 1962). This variable is zero before 1951 and positive through 1957.

There are two main sources of potential endogeneity that have been well discussed in the literature (Jaworski, 2017; FC, (2013); Bossie, (2019); Rhode Snyder and Strumpf (RSS, 2017)). The first is potential endogeneity from the political process of spending allotments. As with all types of fiscal spending, political considerations potentially play a role in the distribution of military contract spending. The second source of endogeneity comes from the fact that the contract spending shock was largely a shock to manufacturing in urban economies and thus war spending is endogenous to the existing industrial structure of a given state. Borrowing from the minimum wage literature (Allegretto et al, 2011), where the

confounding effects of industrial structure are a similar concern, I have included a dummy variable for Census districts $CENSUS_j$ interacted with a time trend to capture changes in industrial structure over the time. The census and state level dummies provide adequate control for geographical difference in industrial structure.

Political considerations, on the other hand, did not seem to play a strong role in the allocation of military contracts. Koisenstien (2004) argues that military control of production removed, to a significant extent, the role of politics in deciding where military counteracts would be sourced. Rhode et al (2017) confirm this empirically. However, both authors find that industrial structure was a major determinant of contract spending. This is perhaps obvious. Speed of production was the overarching concern of military planners and so production was centered largely where production capacity already existed. The main role of the state fixed effects is in controlling for the existing industrial structure of states in 1939. This is, of course, likely to be a imperfect device. Appendix B is dedicated to establishing that, given the fixed effects specification used here, the shock of the war can be reasonably assumed to be an exogenous shock to total personal income. This claim is based on a test for "historical endogeneity". Simply, the regression in Equation 1 is run backwards on the period 1929 to 1940, with the 1940 year dummy excluded (and without the Korean War variable). This allows us to measure the correlation of income variables from before the war with the WWII contract spending shock. In general, as Appendix B establishes, correlation between the 1930s and WWII contract spending is not of much of a concern. However a potential issue that state level fixed effects does not address stems from the fact that states with different industrial compositions are also likely to experience different post-1940 trends.

There are two other possible issues when thinking about the effect of the war on the two very different (war and postwar) subperiods. First, the operating assumption is that the single observation per state is a reasonable proxy for annual wartime spending. If that assumption does not hold then estimates are likely to be biased. However, comparing the single observation of total contract spending to data from preliminary estimates of annual

contract spending as well as annual “war industry” payrolls suggest that the single observation of total spending is a reasonable proxy for annual spending during the war. Appendix A of Bossie (2020) shows comparisons between the single observation per state and annual contemporary wartime spending for 1940-1945 and shows that the single observation per state produces comparable estimates to annual series. This is particularly true for the more complete war industry payrolls series. As well, Appendix A of this paper establishes that estimates for the full 1939-1957 period also hold when the full period is divided up into a war (1939-1945) and a postwar (1946-1957) periods.

It should be pointed out that while there is readily available data for all variables for the District of Columbia, it has been dropped from the analysis. The effects of war spending on income and the components of income are systematically different when DC is included. The fact that DC can obviously be regarded as a special case given that it is both not a state and the seat of the Federal Government means it is reasonable to drop DC as an outlier.

3.2 The Response of total income and private income

The negative effect of wartime contract spending on total income is economically and statistically robust. Of more interest here is the effect of contract spending on the nonfarm private economy. Wartime contract spending generates a temporary positive growth effect on manufacturing during the war. Nonmanufacturing income, meanwhile, grows more slowly in states that receive war spending. This negative growth effect of the war on nonmanufacturing income carries through until 1957.

Figure 3 shows the response of total personal income to war contract spending as well as the response of private income divided up into its major components: manufacturing and nonmanufacturing income. Following Higgs (1999) the focus of this paper is on the effect of wartime contract spending on private income. However, it is of interest to look at the response of total income to total war spending. Results from total war spending are presented in two basic ways. First is the per dollar multiplier. As you can see from the first

panel of 3 nominal per dollar multipliers are negative and small. The war contract effect on total income peaks in 1956 at -15.7 cents per dollar. However, the shock of the war was so large that this small per capita dollar multiple translates into a large total effect. To capture the total size of the effect of the war on income it is useful to show the growth differential of an average state receiving and average amount of war spending. For instance, total per capita average state income in 1956 was \$1991.93. Average total wartime contract spending was \$1233.67 in 1940 per capita terms. The implied total effect of the war shock on an average state receiving an average amount of war spending was to slow total income growth by 14.1%. This measure is somewhat crude and contains some double counting, since the effect of the war is already included the average state level income. However, such a measure is necessary for clearly communicating what I shall refer to as the “average total relative growth effect” of a large shock that has a small per dollar effect. From the first panel of 3 it is clear that the effect of war spending is immediate and negative. The total relative growth effect peaks in 1947 with an implied slower growth of total per capita income of 20.5% (12 cents per dollar). The peak multiplier effect of 1956 is somewhat misleading as to the long run effect of the war on total income. On average from 1949-1957 the multiplier is -11.5 cents and the total relative growth effect on total income is -12.3%.

The second panel of Figure 3 shows the effect of total war spending on private income. It makes sense to focus on private income because there were also large expansion of the federal government, both in military terms and in terms of nominal transfers. These non-private sources of income as well as the dynamics of farm income (discussed below) add noise to estimates of the “development effect” that wartime spending had on the private economy. The effect of total wartime contract spending on private income is significantly different during the war than the response of total income. First, private nonfarm earnings shows a small but positive wartime multiplier in 1942 and 1943 of -2.1 and -2.4 cents per dollar respectively. After the war the relative growth effect is negative. Given the multiplier estimates for total income above, the average 1949-1957 multiplier of -8.9 cents per dollar

of war spending for private income suggests a large portion of the total income effect is accounted for by the effect on private income.

The bottom two panels of 3 further break down private income between manufacturing and nonmanufacturing nonfarm earnings. The response of manufacturing income is drives the positive wartime private income response. The multiplier effect on manufacturing peaks in 1943 at 4.5 cents per dollar of war spending. An average state receiving an average total contract spending shock would see manufacturing income 34.2% higher in 1943. After 1943, however, the effect of war spending dissipates and the long term effect of the war on manufacturing income is statically zero through 1957. In contrast, by 1957 an average state would see its nonmanufacturing income grow 15.2% slower. While the response of total income contradicts the positive wartime relative multiplier in Brunet (2022), who shows a two year multiplier of 34 cents per dollar of supply contracts, the temporary positive response of manufacturing earnings here is smaller but in the same direction. The smaller effect makes sense, given the reliance in this paper of a single shock per state compared with the more detailed monthly data in her paper.

3.3 The response of personal and corporate ownership income

Both state level personal capital income and state level corporate income suggest a strong negative effects on the returns to ownership. The slower growth in nonfarm ownership income explains about 40% of the total post-1949 slower growth in total income. The response of corporate net income and profits is in line with state level capital income. State level dividend payments by corporations are negatively related, but much smaller than personal capital income. Personal capital income consists of three components: dividends, interest and rent. Under the weak assumption that dividend payments are paid within states we can estimate that dividend payments are driving about 40% lower growth of capital income both during and after the war. Bossie (2020) shows that, at least for national banks, the growth of interest rate payments by banks on deposits is not different across states. The

dividend and interest rate effect of state level war spending thus suggests that roughly two thirds of the dynamics of personal capital income are being driven by rent payments. This estimate is consistent with Fetter (2016) who argues that rent controls, more likely to be implemented in wartime boom cities, lowered the return to renting housing and to higher levels of home ownership. Both the lower direct return to renting and the mechanical effect of per capita landlord holdings drove down per capita rental income. Again, the conclusion that capital income is driven by rental income should be considered very tentative and there is some contradictory evidence presented below.

The first panel of Figure 4 shows the response of total nonfarm ownership income to total war contract spending. The response pattern is similar to that of nonmanufacturing income in that per dollar nominal growth of nonfarm ownership income gets slower over time. In 1944 nonfarm ownership income is -1.6 cents slower per dollar of war spending and by 1957 nonfarm ownership income growth is -6.3 cents slower. The average total relative growth effect varies less with ownership income is 21.5% lower in 1944 and 23.2% lower in 1957. The trends for capital income and nonfarm proprietor income follow the same trend as total nonfarm ownership income with the response of capital income to total war spending is stronger, both economically and statistically, than the response of nonfarm proprietor income.

The second panel of Figure 4 shows the response of capital income to total wartime contract spending. In 1944 capital income is 0.8 cents lower per dollar of war spending. By 1957 the nominal per dollar effect had increased to -4.6 cents per dollar. However, the average total relative growth effect in 1944 reduced capital income by 46% in an average state. This total effect is reduced to 31.4% by 1957. The effect of war spending on nonfarm proprietor income is much smaller. The multiplier effect of contract spending is not statistically significant and above 1 cent per dollar until 1953. This small effect translates to a fairly stable average relative growth effect of war spending on nonfarm proprietor income of -10% between 1944 and 1957.

Figure 5 shows state level corporate income, based on data collected from the IRS Statistics of Income II for 1940-1951. The effect of war spending on corporate income follows Equation 2. The results for corporate income should be approached with caution. First, the state in which a corporation registers need not be the state where most of its income, or dividend payments manifest. To this end, Delaware, along with DC, has been dropped from the analysis of corporate income. Second, the panel for corporate income is limited to 1951 and thus only captures the medium term response of corporate income to the wartime shock.

Net income, post-tax net income, and retained earnings all display a similar response to total wartime spending. A slower growth of pre-tax and post-tax earnings is only evident after the war, as is a differential in the growth of tax payments (not shown). The average total relative growth effect shows a average slower growth of post-tax corporate earnings of 51% between 1946 and 1951. Retained earnings are statically below zero even during the war: lower by 1.5 cents per dollar of war spending in 1943 and 3.2 cents lower in 1951. Of interest, the total average growth effect suggest that corporations in an average state would have retained earnings that were 45% lower in 1943 and 69.1% lower in 1951. While the multiplier effect on per capita corporate dividends is relatively small, only larger than -1 cent after 1948 the total average growth rate of dividends is very large. Dividends are 92% lower in an average state receive average war spending. However, total per capita dividends fluctuate significantly and so this estimate should be approached with caution.

One further point should be noted regarding the dynamics of the corporate and personal capital income. It is clear from Figures 4 and 5 that there is wartime crowding out of corporate and personal profits. In addition to this wartime crowding out retained earnings and capital income also experience a significantly slower growth during the postwar business cycle. From 1944 to 1947 the per dollar effect of war spending increases from -1.4 cents to 4.3 cents. The total average growth effect. The change in the total average growth effect is less dramatic, from -47.6% in 1946 to -55.1% in 1948. The behavior of capital income and

profits suggest that the direct of war spending on output is a complex phenomenon driven by both wartime capital crowding out as well as the business cycle effects of the postwar structural adjustment away from wartime industries.

4 The direct and migration effects of war spending on income

4.1 Empirical methodology

In this section I turn to the underlying mechanisms that is driving the response of income to war spending. The war had an obvious direct effect on output, but as discussed above migration plays a clear role in the across state response of income to war spending. To disentangle the effects of migration from the direct fiscal effects the following system of equations is estimated:

$$INCOME_{it} = \beta_{1t}(y_t * WWII_i) \tag{2}$$

$$+ s_i + y_t + \delta_{1t}(y_t * CENSUS_j) + \delta_{2t}(y_t * KOREA_i) + \epsilon_{it}$$

$$MIGRATION_i = \alpha_1(WWII_i) + \delta_{3t}CENSUS_j + \mu_i \tag{3}$$

$$INCOME_{it} = \beta_{2t}(y_t * WWII_i) + \alpha_{2t}(y_t * MIGRATION) \tag{4}$$

$$+ s_i + y_t + \delta_{4t}(y_t * CENSUS_j) + \delta_{5t}(y_t * KOREA_i) + \sigma_{it}$$

Equations 2 to 4 are estimated as a simultaneous equation model (SEM) to evaluate the relative explanatory power of both the direct and migration factors. Other disciplines refer to this technique as mediation analysis (Barron and Kenny, 1986). For economists it likely makes more sense to think of it as a variance decomposition with a Cholesy order: war spending \rightarrow migration \rightarrow income. Standard errors for this system of equations are clustered

at the state level and calculated by the delta method. This produces our three estimates of interest: A total effect (β_{1t}) that has already been discussed at length above. This total effect is decomposed into the direct effect of war spending on income (β_{2t}) and the effect of war spending on income that is “mediated” through migration ($\beta_{1t} - \beta_{2t} = \alpha_1 \alpha_{2t}$). The direct and migration effects are identified under two conditions. The same assumptions of exogeneity of the war shock from the total effect in Equation ???. The Cholesy ordering assumption which is that migration is the only “nonfiscal” mechanism through which war spending had an effect on income.

$MIGRATION_i$ is the “excess migration” from 1940-1947 (Census, 1948) in excess of migration from 1935-1939 (Census, 1946). This controls for migration from 1935-1939 as the “steady state” migration pattern of the Depression that is disrupted by the shock of the war. Both periods of migration are as the percentage of civilian migration relative to their base year populations (1940 and 1935 respectively). Data is available for migration from 1940-1945, which at face value is more comparable to the earlier migration panel. However, statistics for 1940-1947 are more reliable, as well, since the concern here is largely with the long term effect of of the war shock, net migration of 1940-1947 better captures the total shifting of migrants as it accounts for the post-war reconversion migration at the end of the temporary wartime employment shock.

In the original specification $WWII_i$ is total war spending. This is done to keep the results consistent with those in Bossie (2020). When decomposing fiscal and migration effects, however, $WWII_i$ excludes military facilities spending (about 8% of total spending). This does not effect the dynamics of the total effect significantly (presented below as elasticities and not multipliers) but it does have some implications for the direct and migration effect. Including military facilities spending tends to exaggerate the direct effect of war spending since facilities spending went to the building of government owned infrastructure. As with above, the focus here is on the effects of contract spending on long term private economic development and so it makes sense to exclude the economic effects of direct government

spending. To be clear, $WWII_i$ in the SEM specification is “private” contract spending that includes supply contracts, equipment supply contracts and plant and equipment contracts.

4.2 Total income, manufacturing, and nonmanufacturing income

Figure 6 shows the total, direct and migration effect of private war contracts on total income and manufacturing and nonmanufacturing income. Total income is included to establish a baseline pattern for the response of the economy to war spending shocks both directly and mediated through migration. When discussing the migration and direct effects it is helpful to divide our results into three broad periods. The first is the war period (1941-1945), the second reconversion period (1946-1950), and the third long run (1951-1957). The direct effect of war spending dominates during the war, explaining on average 93% of the total effect. During the reconversion period there is a shift in the explanatory power of the variables so that by 1951 both the migration and direct effects explain roughly 50% of the total effect each. The statistical significance of the effects after 1950 is statically marginal, but the consistency of the responses after 1950 suggest that both the direct effect and longer term effect have equal shares in the determination the evolution of the long term response of total income to private war contract spending.

The bottom two panels of Figure 6 show the response of manufacturing income and nonmanufacturing income respectively. The temporary shock to manufacturing is dominated by migration, which explains about 60% of the total effect. It is also somewhat difficult to make causal claims about the effect of migration on manufacturing income. This is due to a weakness in the SEM specification above. For the war period, it makes sense to include a lag of income. Much of migration would be driven directly by war spending, but migration would also be driven by a general increase in income. However, including a lag of income produces a well-known consistency problem. As well, the coefficient on lagged income absorbs most of the variation in current income. As such, the migration and direct effects on manufacturing income should be consider correlational and not causal. At any rate, the migration effect is

not statically significant. The direct effect, on the other hand, is statistically significant at the 5% level for the first two years under consideration (for 1943 the p-value is 0.0507) and thus we can more reliably make correlational claims about the wartime direct effect of war spending on manufacturing, at least during the 1940-1943 buildup of productive capacity.

The effect of private war spending on nonmanufacturing nonfarm income is fairly precisely estimated at zero during the war. Early during the transitional reconversion period, the direct effect is statistically significant; by 1948, the migration effect is also statically significant and stays significant through the end of our sample. As with the total income after 1948, the direct and migration effects explain equal parts of the effect of the war on nonmanufacturing, though from 1951-1953 migration explains around 70% of the effect on nonmanufacturing income before settling back into a more even split in the explanatory power of the migration and direct effects.

We have detail on the different sectors that comprise nonmanufacturing. Given the role of migration in slowing the growth of nonmanufacturing income, the focus here is on wages and salaries and not total income in these industries. Most striking from Figure 7 is the heterogeneity in the response of the various selected components of nonmanufacturing wages and salaries to the direct and migration effects. Wholesale trade and transportation and utilities show the strongest response to migration, though in both cases this is a post-1947 response. It is also notable is that during the reconversion period the direct effect, likely driven by business cycle dynamics, dominates the response from these two sectors. However, after 1950 there is a fairly precisely estimated zero direct effect of war spending on the two industries.

Mining is worth discussing because Brunet (2022) argues that mining is a potential source of bias in relative state level multipliers because contractors who needed raw materials would need to contract across state lines. First, the relatively precise zero estimate of the total effect of private war spending on mining⁴ suggests that this is unlikely a source of much

⁴A 1 has been added to mining wages and salaries to make the log transformation possible

bias. Second, this zero estimate is the product of complex underlying effects. The direct and indirect effects have opposite signs. The direct effect, however is not statically significant. The migration effect is only marginally significant but it does provide us with some suggestion that slower growth in mining wages and salaries are being driven by out-migration all other things equal.

Services payrolls are included because they behave very differently than the other three nonmanufacturing subindustries in Figure 7. The response of services wages and salaries is driven, during reconversion at least, by the direct effect. Under the assumption that whole-sale trade and transportation and utility workers were higher skilled than service workers, along with the lack of across state migration effect on farming wages (discussed below) this lends support to well know migration patterns in which relatively higher skill workers and/or educated workers (Boustan, 2017; Collins, 2007) were more likely to migrate, at least out of the south.

4.3 Personal and corporate capital income

The dynamics of the effect of war spending directly and through migration on ownership income are also complex. Figure 8 decomposes the total effect of private war spending on capital income and nonfarm proprietor income. The migration effect drives the response of capital income to war spending throughout the war and into the reconversion period. However, after a relatively small and immediate effect at the start of the war, the effect does not evolve very much through 1957. During reconversion the direct effect is more dynamic. This is consistent with the “business cycle” explanation for the behavior of capital income. This “business cycle” effect is permanent and carries through to 1957.

Nonfarm proprietor income is of interest because, like mining, it shows a complex interaction of direct and migration effects. The reader will notice that the total effect is only statistically significant, more or less, for the reconversion period. Around 1947 the direct and migration effects diverge, with the direct effect positive but not statistically significant.

The migration effect becomes consistently statistically significant after 1950 and produces a larger effect than the total effect once the direct effect is factored out.

The long run direct effect of the war on capital income is consistent with the the direct effect on retained earnings shown in Figure 9. The total effect of war spending on retained earnings is being driven by the direct effect after 1945. Somewhat surprisingly, per capita dividend payments are driven very strongly by migration effects. The reader will notice that the direct effects of dividend payments is positive (though not statistically significant). I take this relationship between dividends and migration to be strong evidence of a compositional effect of migration. Here, I make the assumption that the relative level of dividend payments did not change that much and/or corporate behavior is relatively exogenous to population changes. If that assumption holds, then Figure 9 shows that the per capita level of dividends would be diluted by increased migration into states. Equivalently, per capita dividends would become more concentrated on a per capita basis in states with net out-migration.

5 Empirical Effects of secondary interest: Farm and government income.

The total effect, direct effect, and migration effect on farm income⁵ is show in figure 10. The most striking takeaway from Figure 10 is that migration plays, for all intents and purposes, no role in determining the across state relative multiplier for farm income. In fact, Figure 10 shows a fairly precisely estimated zero effect of migration on relative farm proprietor income. Neither migration nor the direct effects of war spending show any significant relationship with farm wages and salaries. The direct effect of war spending is on the income of farm proprietors, with farm owners in states receiving war spending seeing lower income growth than farmers in nonwar states. The per dollar multiplier (not shown) is relatively small peaking at 1.4 cents per dollar during the war in 1944. An average state with

⁵All three farm income varibales have had a 1 added to them to make log transformation tractable.

average farm proprietor income would see income growth slower by 22%. The long term effect is even larger, in 1957 an average state receiving an average amount of war spending would see farm propriety income growth 45% lower. These estimates should be approached with some caution since farm proprietor income is one of the only categories to see an absolute decline in per capita income over the period under consideration here.

Nationally, farm income began to shrink in per capita terms after 1952. This effect may have been slightly faster in states that experience more war spending. However this effect is only statistically significant a decade after the war and so any causality of slower farm ownership income growth and the war is tentative at best. Generally speaking, the effect of war contract spending on farm earnings shown in Figure 10 reinforces the idea that farm income plays little role in explaining multiplier effects of the war. In fact a reasonable and likely interpretation for the direct effect of war spending on farm income is compositional.

Figure 11 shows the response of military wages and salaries along with wages and salaries for federal civilian and state and local governments. This has been included to make two basic points. First, military wages are not correlated geographically with private war spending contracts. Military wages could be a possible source of bias, but it is clear from the first panel of 11 they can be thought of as a separate economic phenomenon. The claim that federal civilian wages were not correlated with wartime private spending is somewhat weaker but it is similarly not much of a concern. The second point to be made is in the response of state and local government wages and salaries. As Figure 11 shows, the negative relative multiplier for state and local spending is being driven by migration. I believe this relationship is explained much in the same way as retained earnings. That is, the level of state and local spending is relatively population inelastic and so what Figure 11 is showing is a compositional effect from the dilution or concentration of state and local wages and salaries in in-migration and out-migration states respectively. This interpretation is perhaps somewhat complicated by the fact that during the reconversion period state and local spending show a clear business cycle response to direct war spending.

6 Discussion

In this paper I have demonstrated that the war is a temporary positive shock to state level manufacturing but a permanent, negative shock to state level nonmanufacturing income. The permanent shock of the war to nonmanufacturing income offers an explanation for post-war regional convergence as well as a mechanism for how “WWII ended the Great Depression”. Differential growth rates point to this permanent effect on nonmanufacturing income ended the hysteresis of the depression. While this is certainly a useful finding, it is also important to elucidate the underlying mechanisms that caused the changes in income. The slower growth in income from war spending is due in equal parts to the direct effects of the fiscal shock of the war and also indirectly through across state migration. The long run effect of the war on nonmanufacturing income is dominated by the indirect migration effect and is concentrated in transportation and utilities and wholesale and retail trade.

About a third of the total effect of the war on personal income can be attributed to slower growth in personal capital income. The short term effect of the war on capital income is driven by the direct fiscal shock, but migration explains the slower growth in capital income in the long term. The results for state level corporate income should be approached with some caution, but the negative across state response of corporate profits correlates with the effect of personal capital income and suggests generally fast growth of per capita ownership income in states that received lower amounts of war spending.

The analysis here can be considered “wide” but not “deep” in the sense that it establishes a broad set of empirical facts about the effect of WWII and manages a coherent narrative about the complex underlying mechanisms by which WWII had an economic impact on the sustained postwar golden age. As such, it offers a number of empirical facts that can serve as guideposts to future research. Most obviously, deeper analysis is called for regarding the effects of migration on income during the war. Now that the 1950 Census has been made publicly available that richly detailed cross section is likely provide much insight in how the shuffling of labor within and across regions during the war ended the Depression hysteresis.

To end, I would like to use it to offer some speculation on the mechanisms that this paper points to for the the dynamics of labor markets during and after the war. While it is not possible to make robust claims about how the the skills mix of migration led to changing incomes in out-migration states, a circumstantial picture emerges. First, the reader is reminded that 12.4 million people migrated across states during the war while 13.1 million migrated across counties within states (Census, 1948) between 1940 and 1947. The reader is also reminded that county level studies like those of Fishback and Cullen (2013), Jaworski (2017) and Fischback and Jaworski (2016) find no county level extensive growth. This should be contrasted with the much larger (though opposite signs) state level multipliers estimated in this paper and in Brunet (2022). It is useful to construct an artificial example to illustrate the dynamics between across state and across county, within state, migration. Suppose a train engineer in South Carolina in 1943 moves to fill the need for engineers in Oregon. This skilled worker migrates to take advantage of the wage differential, which is exaggerated during the war. After the war, his skills are still sought after to power the postwar full employment economy. The railroad in SC now needs an engineer, because the market for skilled workers is tight. The railroad in SC is more open to promoting and training unskilled labor, so they promote a train yard worker and train them as an engineer. They also stays in this job after the war. This leads to a job opening for a yard workers, and the wage differential is enough to draw a rural worker off the farm into the train yards. Assuming all three of these workers are paid the average for their job and skill level, it is the employer of the farm worker who must increase wages to replace the worker who moved off the farm and into the mill. Along this entire chain all workers are better off, earning higher wages (why else would they change jobs?) but wage growth at the industrial level is only experienced by agricultural workers. Here we have a generalized mechanism for why wage growth does not necessarily need to be above average in skilled professions. Workers are already getting a pay raise by moving into these occupations. However, low skilled labor does experience growth as a residual labor markets are forced to raise wages to attract additional workers.

Again, this is speculation. The role of skill mix in the migration of workers and changes in earnings during WWII is of obvious interest and is a fruitful area of future research.

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7 Figures and Tables

Table 1: Distribution of USES Placements by industry and skill level 1944 and 1945.

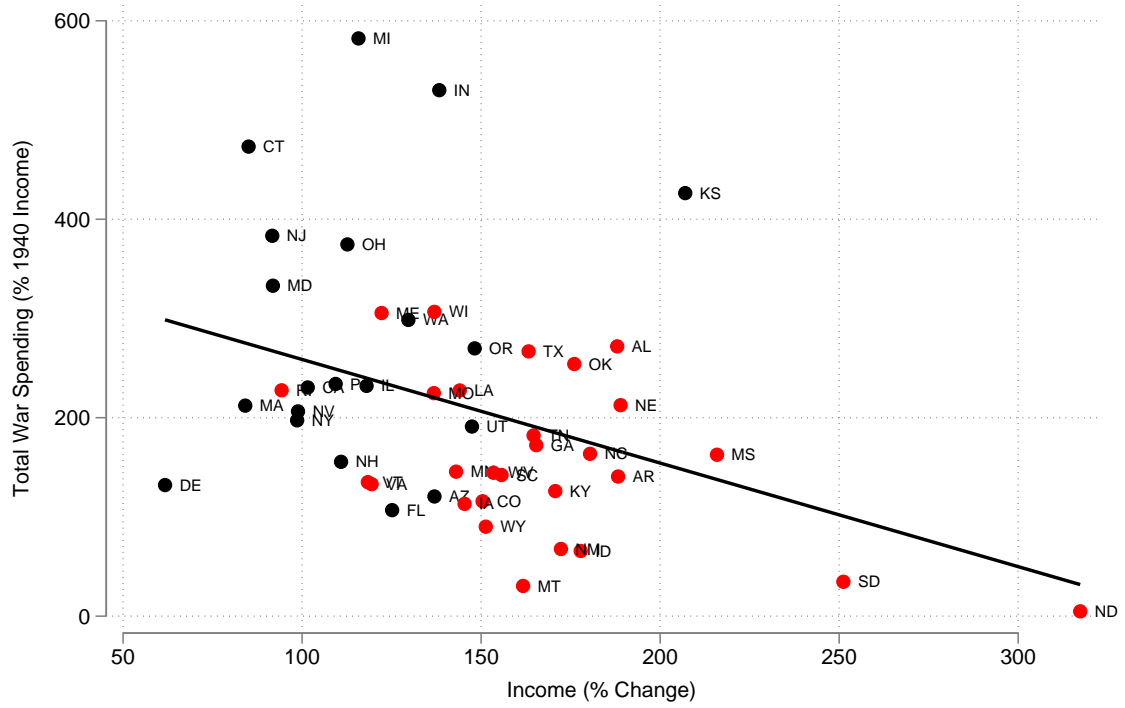
	Percent of total		Percent Women		Percent Nonwhite	
	1944	1945	1944	1945	1944	1945
Total (Number)	11,446,007	9,808,476	33.2%	30.2%	18.0%	19.1%
Manufacturing	59.7%	55.8%	33.8%	30.2%	13.8%	14.4%
Trade and service	18.7%	22.2%	50.2%	45.6%	33.5%	32.7%
Transportation, communications and other public utilities	7.8%	8.1%	12.0%	11.1%	14.9%	18.2%
Construction	6.3%	6.7%	3.2%	2.9%	20.7%	21.3%
Government	5.9%	5.4%	41.2%	38.0%	15.1%	15.3%
Mining	1.5%	1.5%	2.7%	2.5%	5.0%	5.0%
Other	0.1%	0.2%	26.1%	16.2%	14.8%	9.7%
Distribution by skill level						
Professional and managerial	1.2%	1.2%				
Clerical and sales	9.0%	9.1%				
Service	10.3%	11.3%				
Skilled	11.6%	10.7%				
Semi-skilled	14.7%	14.8%				
Unskilled and other	53.3%	52.8%				

Source: The Labor Market, April 1946, pages 44-46; US Department of Labor and US Employment Service.

Table 2: Change in Employment Relative to 1944

	October 1945		November 1948	
	Thousands	% of 1944	Thousands	% of 1944
Total Employment (1944 Total: 41.5mil)	-3,091	-7.5%	4,275	10.3%
Total War Manufacturing	-3270	-39.4%	-2423	-29.2%
Total Non-auto Transportation	-1704	-71.1%	-1965	-82.0%
Aircraft and Shipbuilding	-1571	-75.0%	-1835	-87.6%
Automobile Manufacturing	-255	-34.8%	55	7.5%
Iron and Steel	-455	-26.2%	-133	-7.7%
Electrical Machinery	-282	-36.9%	-230	-30.1%
Non-electrical Machinery	-306	-24.6%	-37	-3.0%
Total Nonwar Manufacturing	-218	-3.7%	1283	22.0%
Total Nonmanufacturing	-949	-5.2%	5243	28.6%
Mining	101	11.7%	75	8.7%
Construction	-158	-14.4%	1068	97.6%
Transport and Utilities	-63	-1.7%	268	7.1%
Trade	-541	-7.3%	2637	35.6%
Finance	-42	-3.1%	346	25.2%
Services	-246	-6.5%	849	22.4%
Civilian Government	236	3.9%	-312	-5.2%

Source: 1949 Statistical Supplement to Survey of Current Business



See text for sources. Red dots indicate states with net out-migration 1940-1947

Figure 1: Total Contract Spending and the Change in Income and Capital income 1940-1947

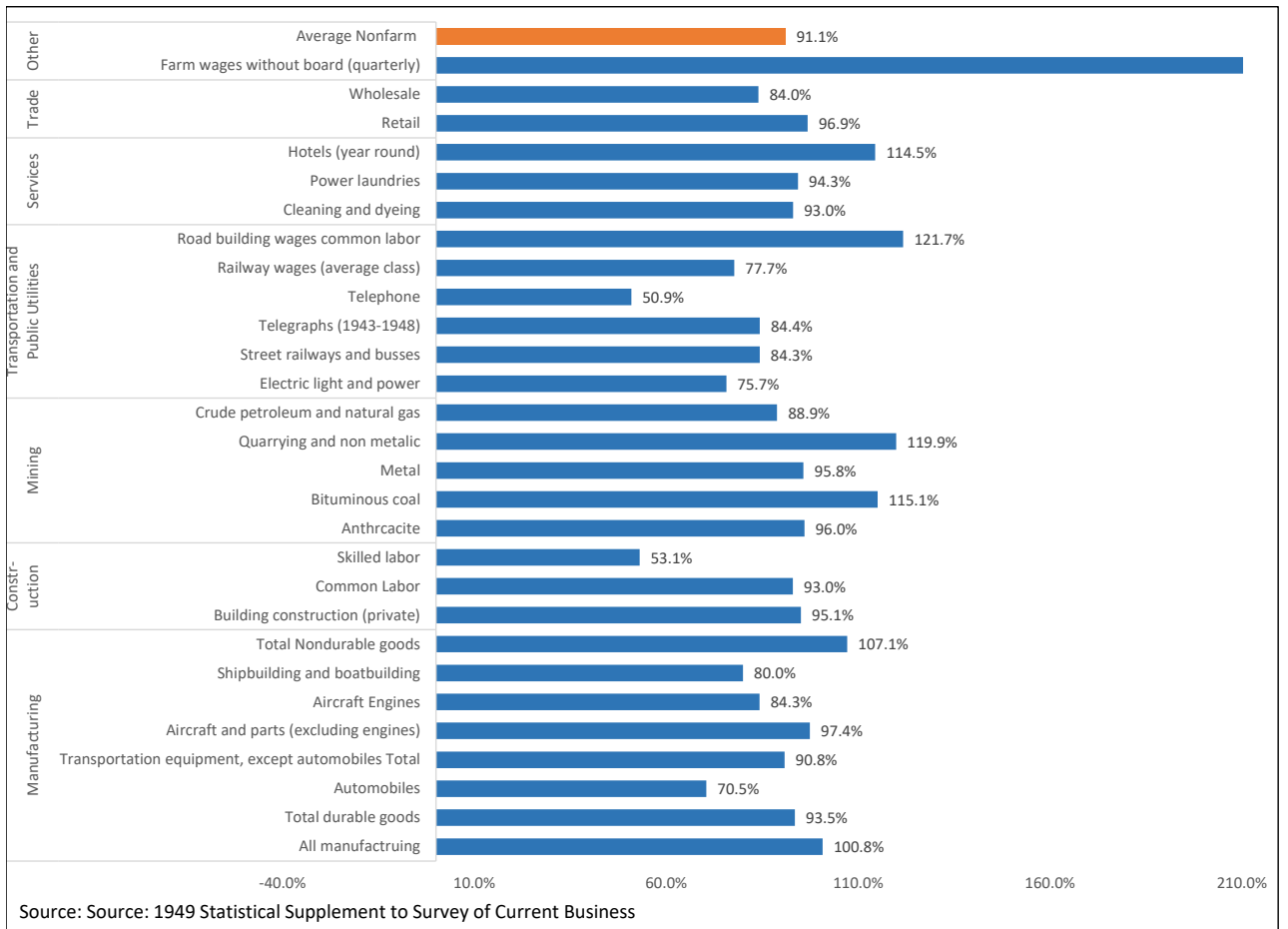
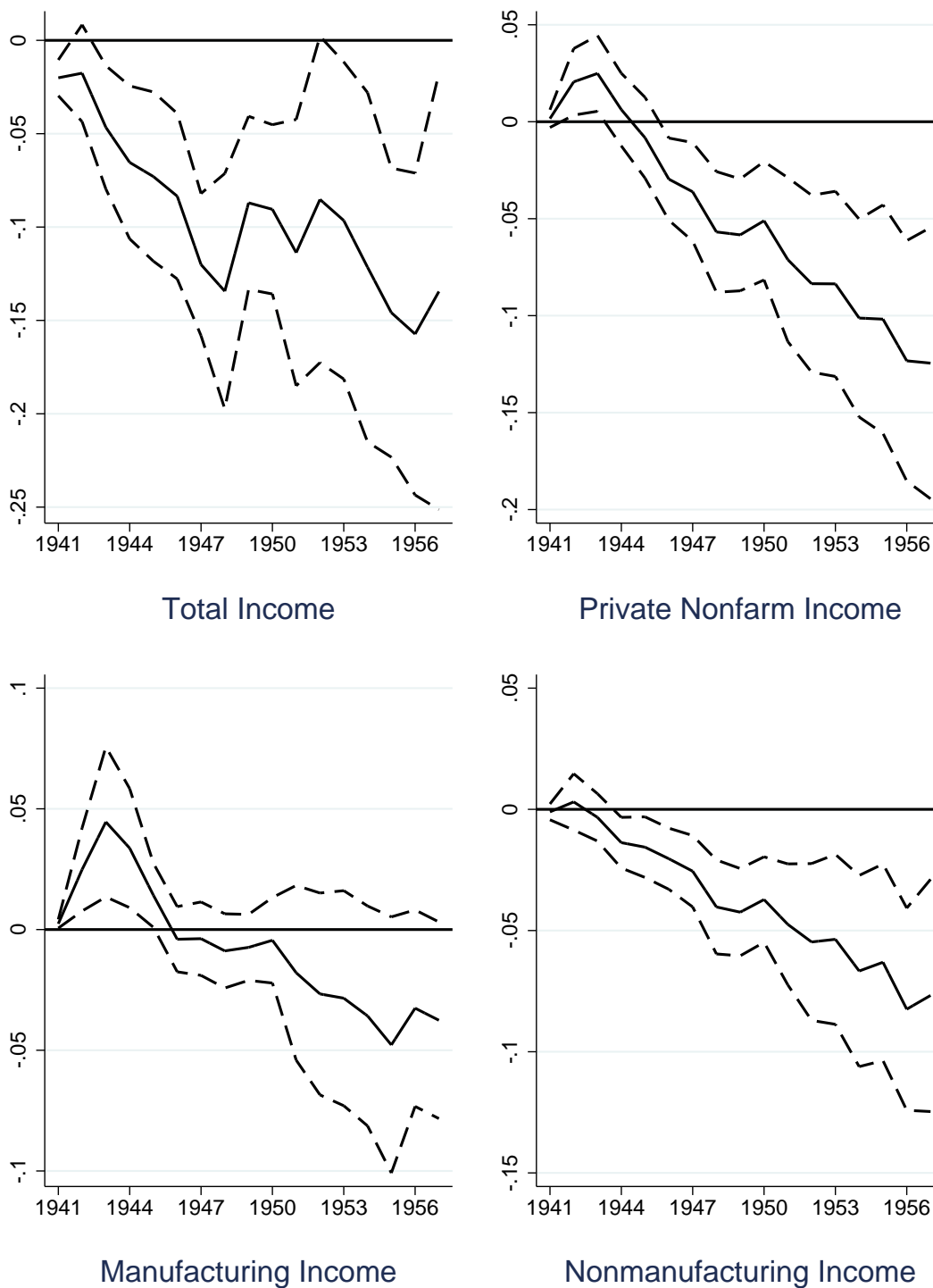
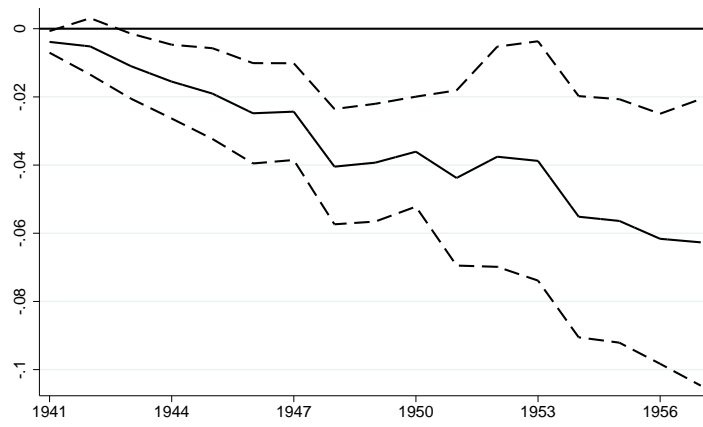


Figure 2: Hourly wage growth by industry 1940-1948

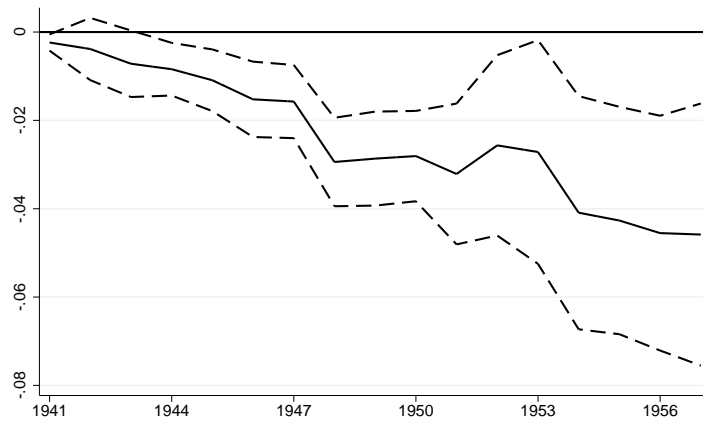


Graphs show the estimated dollar response to \$1 of war spending per capita.
Dashed lines are 95% confidence intervals.

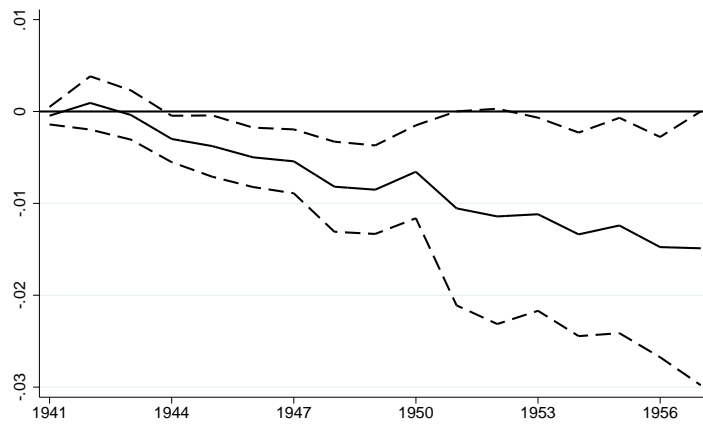
Figure 3: The Effect of WWII Contract Spending On Total Income and Private Income



Nonfarm Owner's Income



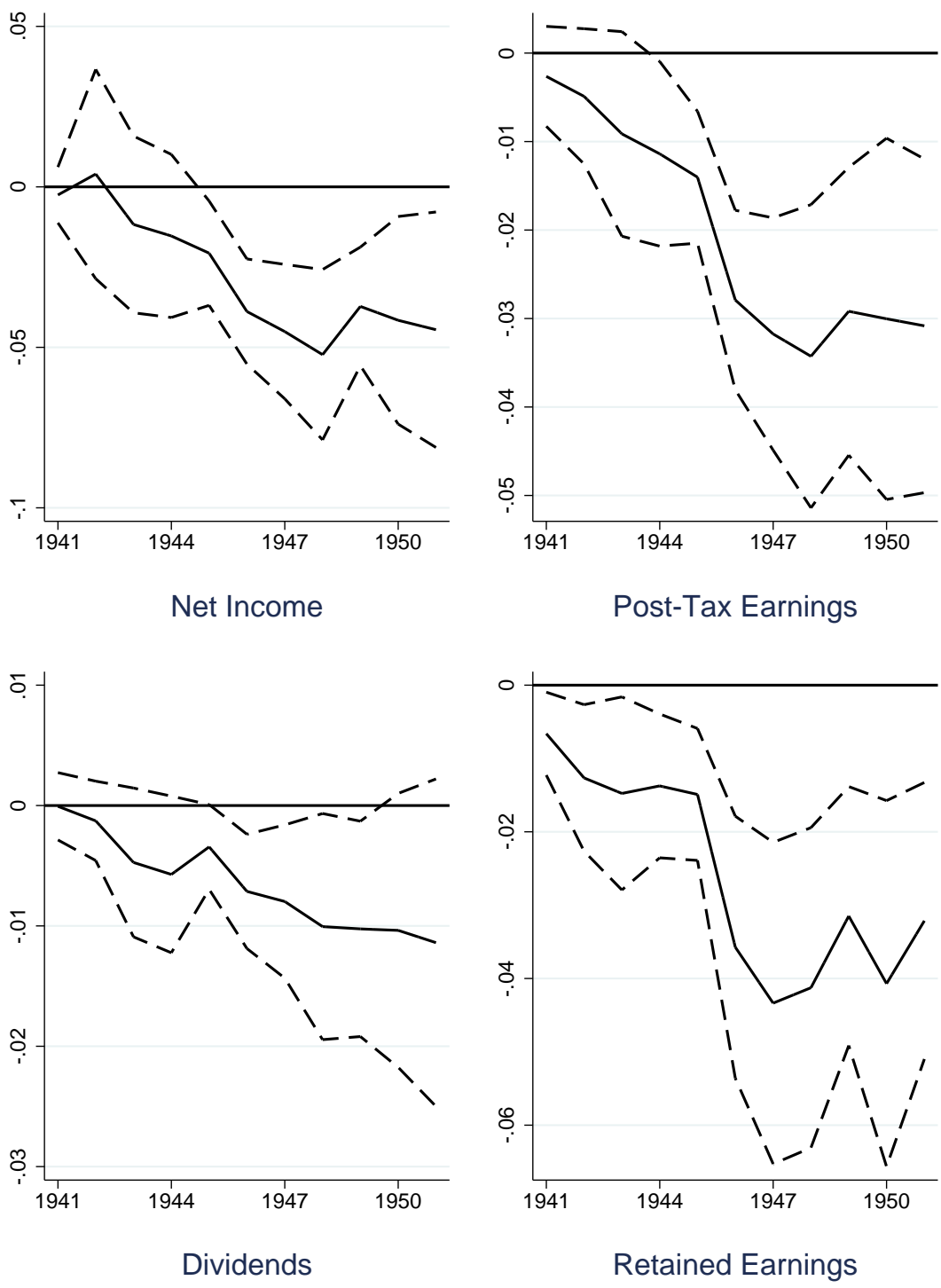
Capital Income



Nonfarm Proprietor Income

Graphs show the estimated dollar response to \$1 of war spending per capita. Dashed lines are 95% confidence intervals.

Figure 4: The Effect of WWII Contract Spending On Nonfarm Ownership Income



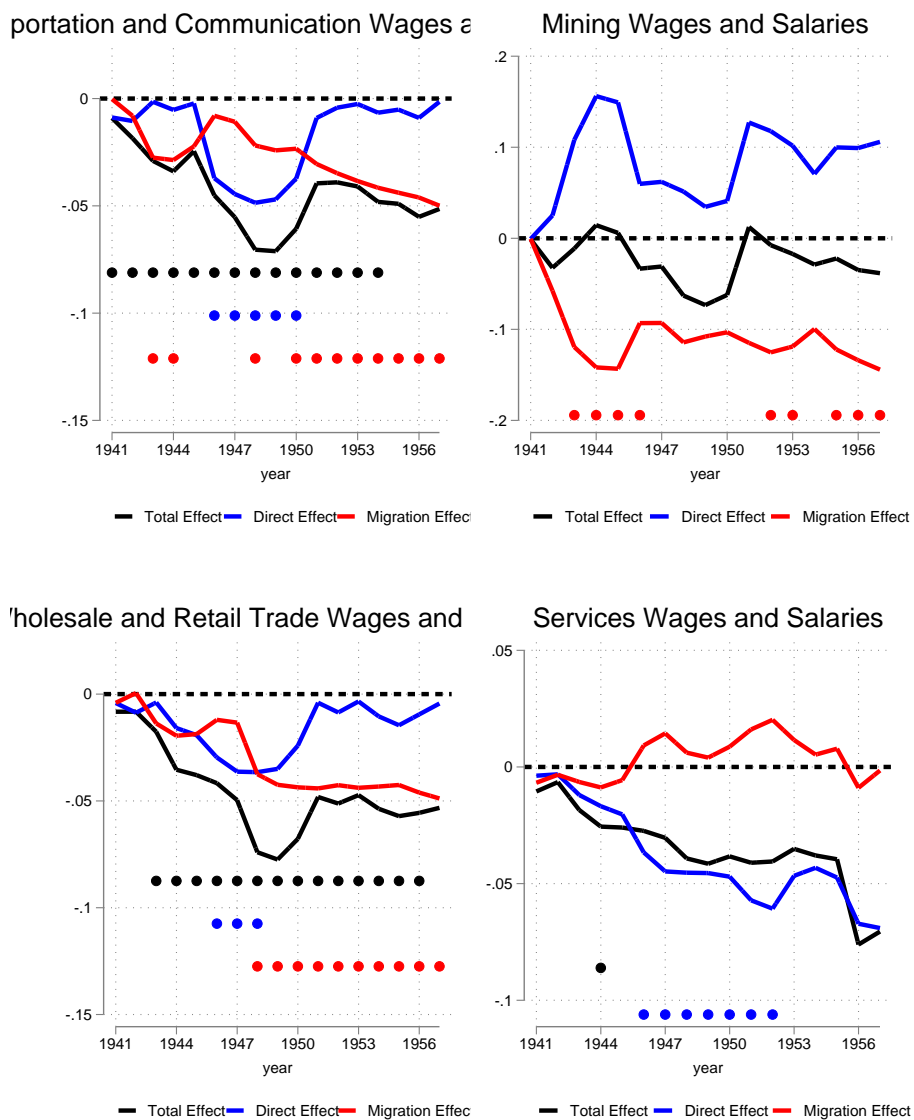
Graphs show the estimated dollar response to \$1 of war spending per capita.
Dashed lines are 95% confidence intervals.

Figure 5: The Effect of WWII Contract Spending on Corporate Income



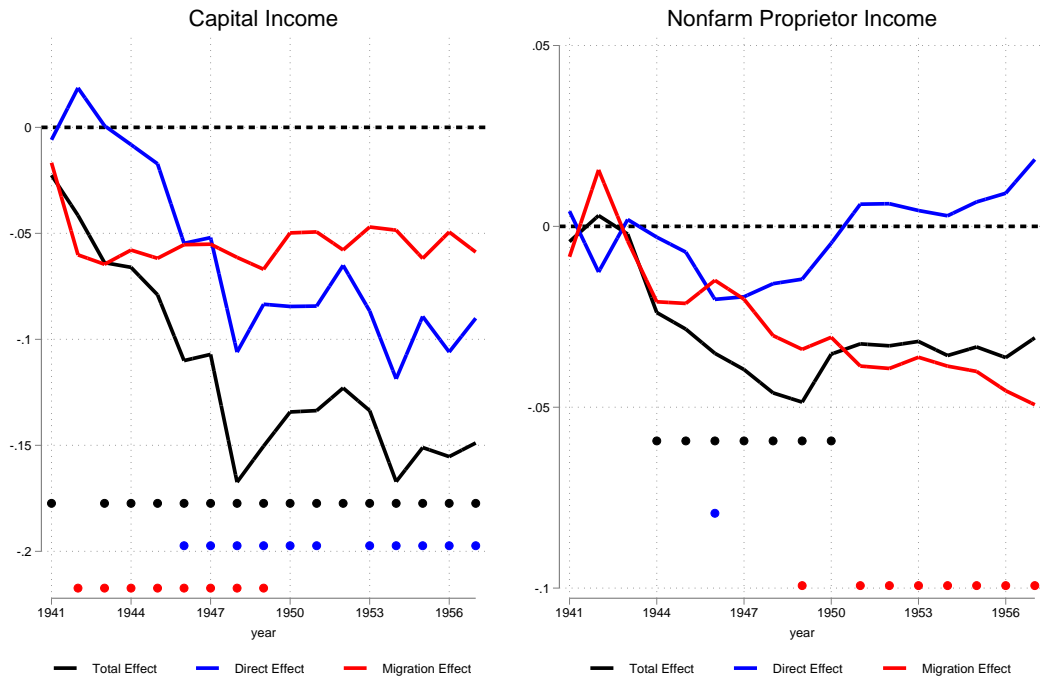
Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 6: Direct and Migration Effects On Total Income and Manufacturing and Nonmanufacturing Income



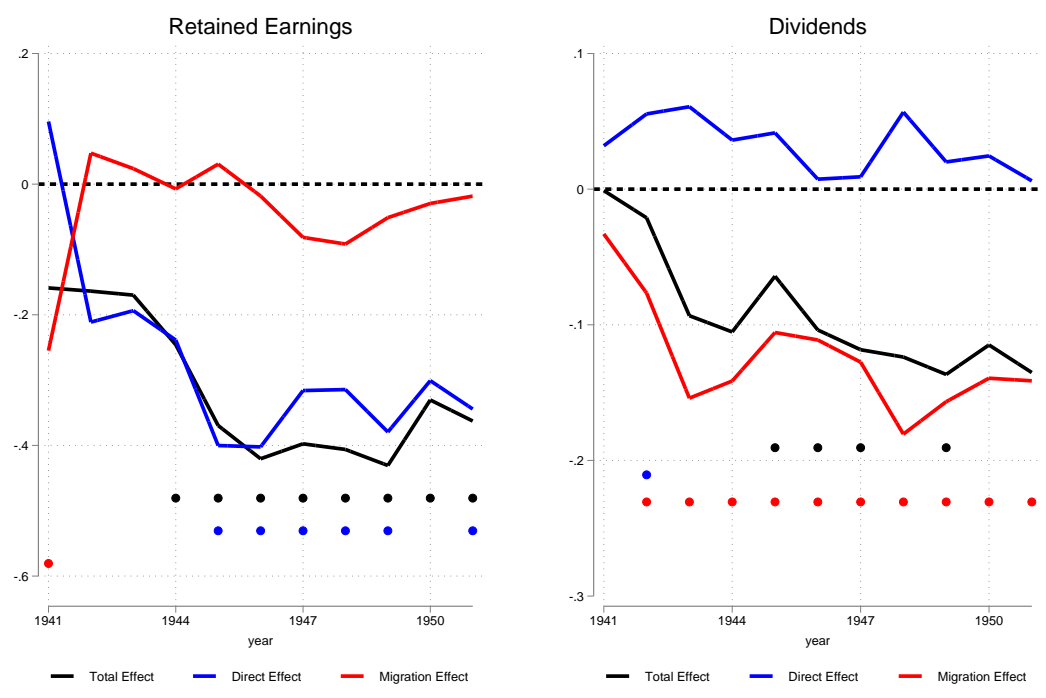
Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 7: Direct and Migration Effects On Nonmanufacturing Sector Wages and Salaries



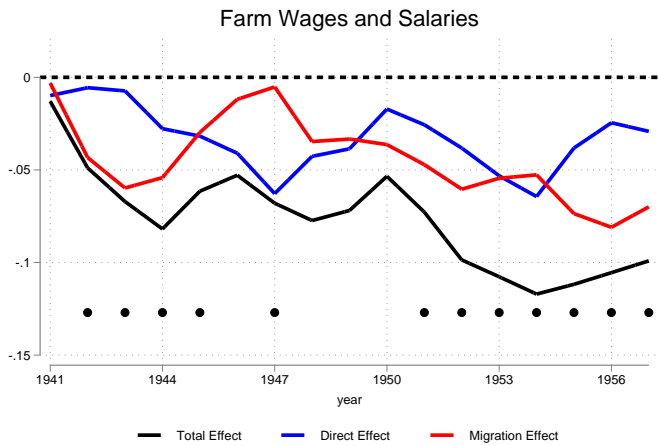
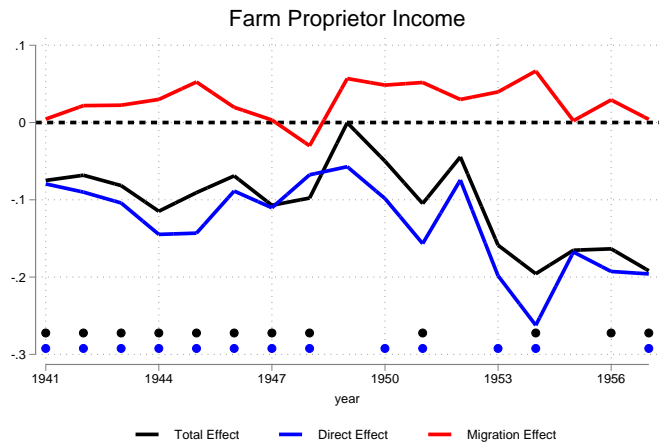
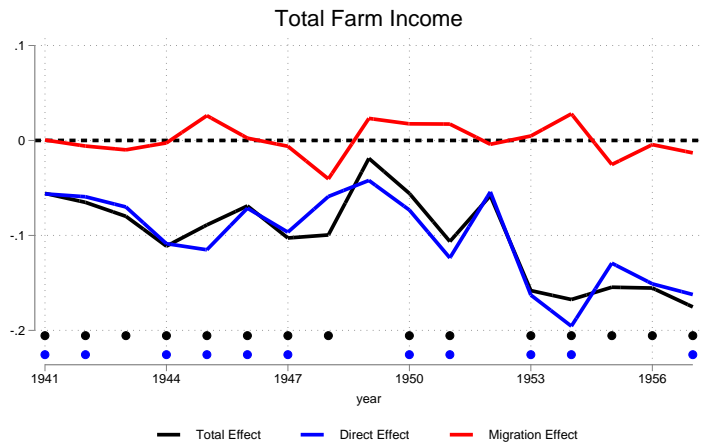
Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 8: Direct and Migration Effects On Personal Ownership Income



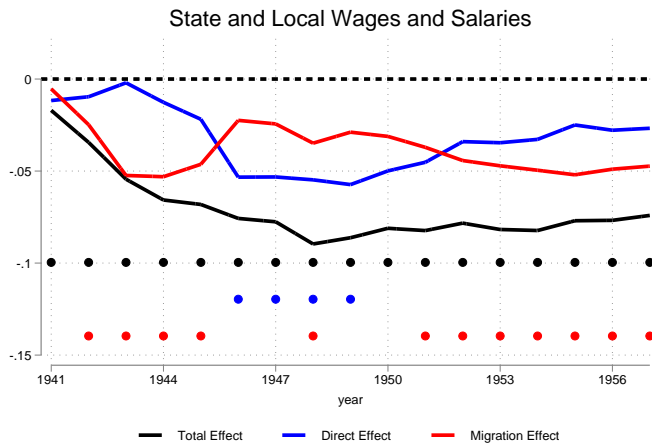
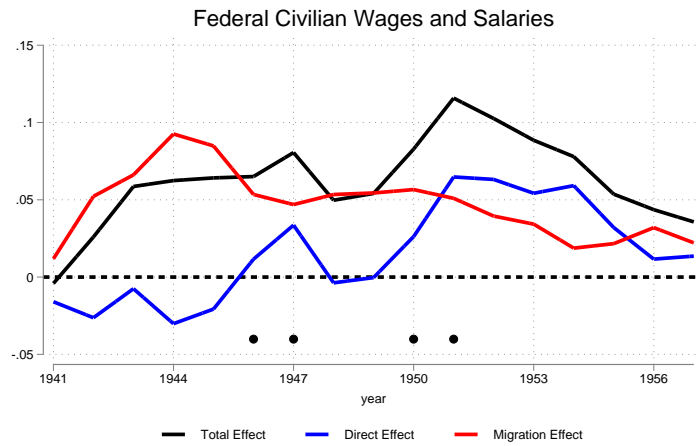
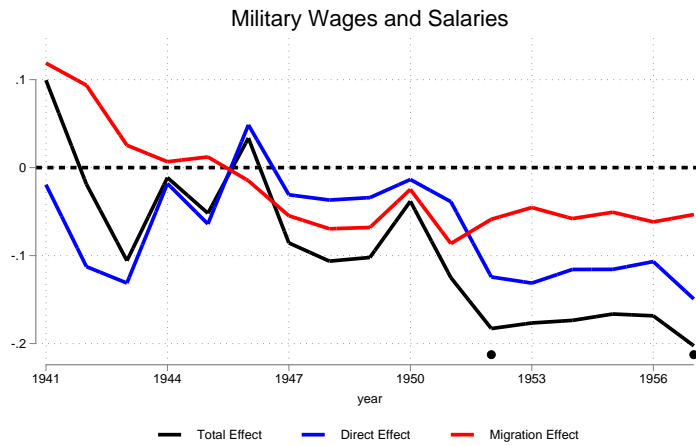
Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 9: Direct and Migration Effects On Corporate Profits



Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 10: Direct and Migration Effects on Farm Income



Graphs show elasticities and dots indicate where series are significant at the 5% level.

Figure 11: Direct and Migration Effects on Government Wages and Salaries