

Was the Arsenal of Democracy an Engine of Mobility? Public Investment and the Roots of Mid-century Manufacturing Opportunity *

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Abstract

This paper examines the long-run effects of siting a large government-financed manufacturing facility in a region during World War II (WWII) on local development and on individual-level earnings mobility during the Postwar period. We test for market-level effects by comparing counties that received plants for idiosyncratic war-related reasons to counties that were observably similar in 1930. In counties where plants were sited manufacturing employment rose by 30 percent and average production wages rose by 10 percent after the war, with both remaining elevated through 2000. We find that sitings of both ordnance plants—which could not be converted to civilian use—and general-purpose plants had similar effects on Postwar manufacturing, but that only regions with ordnance plants remained oriented towards defense production during the Cold War. In addition, plant sitings led to permanent increases in regional population and employment and had long-lived effects on local incomes. We test for individual-level effects by studying the long-term earnings effects on workers based on where they resided before the war as children. While we find evidence of selective migration of better-educated individuals into regions with plants, we also find that men born in a locale where a large plant was constructed had 2–4% higher wage and salary earnings as adults in the 1970s and 1980s, though we find no such effects for women’s earnings. Moreover, plant sitings increased intergenerational household income mobility for pre-war resident children. For children with parents whose 1940 earnings were below the median, we find an effect of a plant siting on adult earnings ranks equivalent to their having parents 4 percentiles higher in the 1940 earnings distribution.

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1 Introduction

The past half-century in the United States has been marked by a dramatic decline of high-paying manufacturing jobs in once-booming industrial cities and towns (Krueger and Summers, 1988; Moretti, 2013). This decline has coincided with a secular increase in inequality and declines in the rate of wage growth and upward earnings mobility—children are far less likely to grow up to earn as much as their parents now than in 1950, particularly in the post-industrial heartland (Chetty et al., 2017).¹ However, taking a longer view, manufacturing work opportunities had been scarce in many regions prior to the Second World War (WWII). Between 1940 and 1950 the premium for skill fell dramatically, the pre-tax wage distribution underwent a “Great Compression”, and the employment level in middle-class production occupations reached unprecedented heights (Goldin and Margo, 1992). Over this same period, the U.S. government-financed a wartime industrial expansion that resulted in a threefold increase in manufacturing output over four years. While time-series evidence suggests a link between the War effort and post-war prosperity, it has been difficult to establish a direct causal link between wartime investment and the emergence of high-wage manufacturing regions.

This paper examines how siting a large, government-financed plant in a region during WWII impacted its labor market and the earnings of its pre-War residents in the long run. These plants were built as part of an industrial facilities expansion program that increased the value of the national manufacturing capital stock by approximately 50% between 1939 and 1944. While the majority of wartime production occurred at pre-existing plants that were retooled or and expanded, entirely new plants had to be constructed to meet government orders of airplanes, ships, ordnance, explosives, and related war goods. For security purposes, the military insisted that new plants should be sited outside of established manufacturing hubs in less-congested inland locales with sufficient access to water, power, labor, and housing. However, firms were hesitant to stake funds on new large war plants that were far from their existing facilities and had uncertain postwar value—thus, the government was left to fund the construction of these large new facilities. Public spending on the largest 301 plants comprised *half* of all wartime capital expenditure (United States Civilian Production Administration, 1945).

We test whether places that received more wartime investment supported better manufacturing jobs in the long run and whether pre-War residents benefited from their construction. The underlying policy question—can public efforts to increase local investment improve earning opportunities for local residents?—remains relevant today. The answer to this question is central to understanding whether place-based labor policies can help the economically disadvantaged reach the middle class (Austin et al., 2018; Bartik, 2019; Slattery and Zidar, 2020), whether industrial policy is justified in developing regions (Murphy et al., 1989; Rodrik, 2004; Lane, 2020), and the extent to which infrastructure shapes how urban clusters form

¹It is plausible that the decline of manufacturing hubs, which had once provided mid-century economic opportunity to local residents, directly contributed to the rise of local poverty traps that have hindered upward mobility (Wilson, 1990, 1997). Chetty et al. (2014) find that local manufacturing prevalence in 2000 is *negatively* correlated with upward mobility in recent years, during which time manufacturing jobs were in decline. Recent evidence suggests the manufacturing opportunity was an important driver of upward mobility in earlier periods (Tan, 2019).

and persist (Redding et al., 2011; Bleakley and Lin, 2012; Kline and Moretti, 2014; Severnini, 2014). Moreover, as evidence mounts that the places where children grow up have causal effects on their adult earnings (Chetty et al., 2016; Chetty and Hendren, 2018a,b), it is important to understand how policy interventions that affect local economic conditions might contribute to children’s long-run outcomes. Yet, opportunities to learn about potential interventions are extremely rare—“big pushes” are few and far between, and when they do occur in the form of plant openings or major infrastructure works, they are typically systematically targeted at places that are expected to grow or to stagnate.

To estimate the effects of siting a large plant in a specific locale, we compare counties that received a large, government-funded plant to counties that had similar access to basic resources and labor in 1940. Our conjecture, motivated by historical evidence, is that *neither* the places where such plants were sited nor observably-similar places would have been chosen as a plant site if not for the exigencies of the war emergency. Rather, any plant construction that would have occurred in the counterfactual would have taken place in the large industrial hubs where private firms actually made investments during the War. Government documents further suggest that conditional on access to basic resources, siting decisions for public plants were largely driven by a combination of strategic considerations and idiosyncratic factors pertaining to the ready availability of suitable parcels—neither of which would have had any bearing on economic outcomes if not for the war. This approach is supported by the data: all outcomes we observe for “treated” counties and similar comparison counties evolved in parallel between 1920 and 1940. By contrast, plants that received substantial *private* funding seem to be systematically located in regions experiencing higher pre-war employment growth, even conditioning on a wide array of observable characteristics.²

We first examine the impact on local economies in county-level panel data using a difference-in-differences design. The construction of a plant during the War had dramatic and persistent effects on the locales in which they were sited. Local manufacturing output increased by approximately 25% percent in the aftermath of war and only began to wane in the last decades of the century. Average labor productivity and average compensation of production workers in manufacturing both immediately rose by 10%—and, strikingly, the shifts in both were *permanent*. Yet we find little effect on the number of manufacturing establishments or on manufacturing activity in surrounding counties; this suggests that the prior results were driven primarily by the initial plant itself and not by broader agglomeration forces. However, plant sitings had permanent effects on regional development. Population, total employment, and economic activity in the service and retail sectors increased by 10% in the decades following the War and remained elevated even as manufacturing employment receded. Neighboring counties expanded permanently as well. Plant sitings increased median family income and median male wages by 3-5% through the late 1980s.

The persistent effects we find on places chosen as plant sites is not simply a result of ongoing military spending during the Cold War. While plants that made specialized war goods like ordnance and

²Both contemporaneous reports (United States War Production Board, 1945a) and recent work by Rhode et al. (2018) show that the geographic distribution of WWII procurement spending was largely driven by pre-war economic differences across regions. Our conjecture is not that all wartime spending and investment was randomly assigned, but rather that the subset of plants with no private investment were sited for idiosyncratic reasons peculiar to WWII.

explosives continued to supply the military after WWII, most other general-purpose plants that produced products related to civilian goods—largely metals and transportation vehicles—typically re-converted to civilian production after the War. We examine Vietnam War-era procurement data and find that the counties with ordnance plants continued to produce intensively for the military during the Cold War but the general-purpose plants did not.³ Nonetheless, the effects of plant sitings on long-run manufacturing employment were similar for both types of firms. Moreover, *only* the general-purpose plants that returned to civilian industry caused long-run growth in overall population, employment, and the number of additional manufacturing establishments. This latter set of findings suggests that one-time public investments in plants had long-lasting effects due to path-dependence in regional development processes.

A key question for policy-makers is whether economic development in a region actually benefits the people who live and work there. In spatial equilibrium, new high-wage jobs might be filled by highly-skilled migrants who would have found a high-wage job elsewhere had the war plants not been built. We study this question using two longitudinally-linked datasets that enable us to measure postwar earnings outcomes of individuals who resided in treatment and comparison counties *prior* to the war. We focus on individuals who were born before the war but were too young to work or serve during the War to isolate the impacts of the plant apart from any wartime work experience. The first data source tabulates adult household income ranks based on IRS tax returns from 1974 and 1979 by county of residence and parent earnings rank in the 1940 Census, drawn from Massey and Rothbaum (2020).⁴ To study individual-level mechanisms in greater detail, we then examine micro-data from the Current Population Survey Annual Social and Economic Supplements (CPS ASEC) linked to Social Security earnings histories in the Detailed Earnings Record (DER) and place of birth Numerical Identification System file (Numident) using Protected Identification Keys (PIKs) assigned by the Census Bureau.⁵ Though these post-war outcome data do not allow for a difference-in-differences design based on a parallel-trends assumption, we argue that a stronger conditional independence assumption is plausible in this setting.

We find that plant sitings directly raised the adult earnings of prewar residents. Men born before the War in counties where plants were built had 3-4% higher annual wage and salary earnings when we first observe them in 1978. These effects persist through the late 1980s. We find no effects on individual earnings of women—though we do find that plant sitings increased *household* adjusted gross income similarly for men and women. Although these earnings increases could in part reflect movement towards locations with higher costs of living, we find that affected children did not tend to move to counties with higher housing values as adults. We also find little evidence that the earnings effects were driven by increased educational attainment among affected individuals. While white men experienced the largest adult income gains, we find no effect on high school or college completion rates among this group. We do, however, find

³By contrast, we find clear evidence that counties with ordnance or explosive plants were far more dependent on military spending than comparison counties.

⁴These data are constructed to be analogous to the regional mobility measures in Chetty et al. (2014). “Household income” in this analysis is defined as the Adjusted Gross Income of the tax filing unit.

⁵Social security earnings are at the individual and include all earnings subject to payroll or self-employment taxes.

large effects on educational attainment for black men, though our estimates are less precise.

Moreover, plant sitings increased rates of upward intergenerational mobility. For children born into families that were below the median income in 1939, we find that plants raised their adult household income rank in the 1970s by a full percentile, but find little effect on those born to parents at the high end of the earnings distribution. The magnitude of the former effect is equivalent to being born to parents with a 1939 earnings rank that was four percentiles higher than ones' parents. We find similar effects on adult household earnings ranks for both men and women. We also find similar effects for children born in counties adjacent to those where plants were sited; although the effects on manufacturing jobs was concentrated close to the plant site, the exposure effects on the workforce propagated throughout a much broader region.

These individual-level earnings effects cannot fully account for the broader increase in prosperity in affected regions. Using the CPS-DER data, we test how plant sitings during WW2 changed the composition of workers within the same birth cohorts examined before that resided in those affected counties later in life (in their CPS year).⁶ We find that places where plants were sited had higher-paid and better educated workers in the late 20th Century, and that the size of these effects exceeds the within-individual effects on those born in the county pre-War. This is particularly true in regions where general-purpose plants were sited, which experienced dramatic population growth due to migration after the War. By contrast, counties where ordnance plants were sited, for which we observe no population growth effect after the War—saw little increase in the average wages and educational attainment of late-20th-Century residents. Yet, if anything, the causal impacts of the ordnance plants on pre-war residents' long run outcomes were larger than those of the general purpose plants. These findings imply that selective migration flows induced by the different kinds of plants were a major driver of the regional prosperity measured in county-level tabulations of contemporaneous residents.

This work contributes to a broader literature on the effects of place-based policies on regional development (Kline and Moretti, 2014; Neumark and Simpson, 2015; Austin et al., 2018; Bartik, 2019; Slattery and Zidar, 2020), and industrial policies that promote regional development through support for manufacturing firms (Rodrik, 2004; Lane, 2020). The potential for "big push" investments to have spillover effects throughout regions has been highlighted in influential work by Murphy et al. (1989).⁷ This work adds to earlier studies of plant sitings, most notably Greenstone et al. (2010) who found that new plant sitings during the 1980s and 1990s increased regional productivity, although subsequent work by Patrick (2016) found limited effects of these openings on local wage levels. It also complements work by Kline and Moretti (2014) who evaluated the long-run effects of a major regional development push in the 1930s, the Tennessee Valley Authority, and found it had long-run impacts on regional development.⁸ In addition, several re-

⁶In the CPS-DER sample, we only observe place of birth and adult place of residence reported in the post-1990 CPS; we observe earnings but not place of residence in the pre-1990 DER.

⁷This, in turn, is related to work on agglomeration, path dependence, and related mechanisms by which transient shocks to regions can have persistent shocks to development (Davis and Weinstein, 2002; Redding et al., 2011; Bleakley and Lin, 2012; Nunn, 2014; Hanlon, 2017).

⁸This work is also closely related to studies of industrial policies on regional development outside the United States.(Fan and Zou, 2018; Criscuolo et al., 2019; Lu et al., 2019; Lane, 2019)

cent paper have examined other industrial policies during World War II, including the Marshall Plan in Europe (Bianchi and Giorcelli, 2019) and efforts to promote scientific research by the U.S. Office of Scientific Research and Development (Gross and Sampat, 2020). Our work is most directly related to a contemporaneous study by Jaworski (2017), which examined variation in World War II capital expenditures among counties within the U.S. South and found no longer-term effects on long-run manufacturing expansion.⁹

Our paper joins a growing literature that explores the long-run economic effects of military spending and, in particular, the effects of World War II on various aspects of the post-war economy. Goldin and Margo (1992) were among the first to clearly document that a distinct “great compression” in the wage distribution occurred during the 1940s. Fishback and Cullen (2013) examine the relationship between aggregate local spending and post-war per-capita retail sales and population growth, and appear to find relatively small effects. Higgs (1992; 2004) and Mulligan (1998) argue that the effects of the war on the labor force and postwar growth were minimal. Recently, Brunet (2018) has studied the short-run macroeconomic stimulus effects of war spending using detailed data on military expenditures. Studies by Barro (1981), Ramey (2011), and Nakamura and Steinsson (2014) similarly examine the macroeconomic effects of military buildups later in the 20th century. Angrist and Krueger (1994) studied the effects of the postwar GI bill on education and earnings, finding that most of the effects appeared to be driven by selection.

The results also highlight how access to good-paying manufacturing jobs improved upward mobility in the postwar era. In doing so, our findings add to a growing literature on the causal drivers of intergenerational mobility (Black and Devereux, 2011)—and, in particular, the effects of place on long-run opportunity (Chetty et al., 2016; Chetty and Hendren, 2018a,b). While exposure to local manufacturing jobs is not associated with increased upwards mobility today (Chetty et al., 2014), our results suggest high-wage manufacturing opportunity helped create a ladder out of poverty in the postwar period. Yet, our results also indicate that “big-push” policies that successfully cause regions to become larger and higher-income in the long term do not necessarily foster upward mobility for incumbent residents. To the contrary, we find that the plants that did the most to create opportunity for locals did little to promote regional development and relied heavily on military spending during the Cold War.

The remainder of the paper proceeds as follows. Section 2 provides historical and institutional background on the economic mobilization for WWII places decisions to build plants with public funds in that context. Section 3 describes the data we study. In Section 4, we develop a research design that exploits the institutional context to obtain credibly causal estimates of the impact of wartime plant openings on post-war local economies. Section 5 presents the baseline results concerning regional development. Section 6 discusses the mechanisms of persistence. Section 7 presents and discusses results concerning the post-war earnings and upward mobility of pre-war residents. Section 8 discusses mechanisms by which plant openings impacted individual incomes and the relationship between these effects and regional income growth. Section 9 concludes.

⁹The difference in results appears to stem from the focus by Jaworski (2017) on the U.S. south, that paper’s definition of the dependent variable as $\log(1+\text{investment})$ where rather than investment per capita or a binary treatment measure (investment is 0 in most counties), and differences in the method of selecting comparison counties.

2 Historical Background

2.1 The World War II Industrial Expansion

The industrial mobilization during WWII was one of the most dramatic industrial expansions in United States history. To meet the needs of the war, US manufacturing output tripled between 1939 and 1942 and Gross National Product increased by 50 percent—both increases were driven entirely by government purchases. From 1940 to 1944, annual output of planes rose from approximately 6,000 military planes per year (out of 13,000 total civilian and military aircraft produced) to over 96,000 military planes per year—a sixteen-fold increase in output (Craven and Cate, eds, 1955). Employment in the chemical- and metal-working sectors had nearly tripled from about three million to nearly eight million, while the other industrial sectors expanded only slightly from the 1939 base employment of five million. The once-tiny aircraft manufacturers increased their employment fourteen-fold. Annual government purchases of these goods amounted to nearly half of the size of the entire US economy in 1939.

The vast majority of the production for the War was done by private firms under contract. Due to the urgency of the crisis, contracts were not allocated competitively; instead, “cost-plus-a-fixed-fee” contracts were directly negotiated by a wide array of government military agencies with manufacturing firms. The War Production Board (along with its predecessors, the National Defense Advisory Committee and the Office of Production Management) was established to help these myriad agencies connect with firms that had the capabilities to take on major projects without creating bottlenecks or misallocations of resources (White, 1980).

Coming off the heels of the Great Depression, private industry lacked the capacity to meet the dramatic surge in government demand.¹⁰ Moreover, the plant stock in 1939 was oriented primarily to activities such as agricultural processing, textiles, apparel manufacturing, and wood/paper processing that were difficult to convert to production of wartime goods like airplanes, ordnance, and explosives. Thus, the expansion required a massive investment in new industrial capacity. Most production contracts required some expansion of firms’ productive capacities. In many cases, firms simply enhanced existing plants to increase the output of pre-war goods (like canned food, uniforms, or iron) or converted and retooled factories to make different products. However, when a contract called for the production of a new type of product on an unprecedented scale, a completely new plant was often required.

This investment push resulted in a dramatic increase in the aggregate stock of industrial capital. By the end of the War, \$20.3 billion had been spent on new plant capital—of that, \$13.9 billion was spent on new plants, while the remainder was spent converting and expanding existing plants (U.S. Civilian Production Administration 1945). To put that amount in perspective, the *total* book value of manufacturing capital reported to the IRS in 1939 was \$39.5 billion (U.S. Department of Treasury 1942). That amount significantly exceeds New Deal (PWA and WPA) spending on public works, which totaled \$15 billion (1940 dollars)

¹⁰No full-scale industrial mobilization had occurred during the First World War.

from 1933-1939.¹¹ The vast majority of this investment—\$15.9 billion of the total \$20.3 billion spent in total, and \$12.3 billion of the \$13.9 billion spent on new plants—was financed by the U.S. government.¹² Notably, \$10 billion of government investment—roughly half of all industrial investment during the war expansion—was spent constructing 301 very large plants (U.S. Civilian Production Administration 1945).

2.2 WWII's Big New Plants

When it came to siting these new plants, military and private firms were often at odds. Strategic considerations led military planners to determine that new plants should be sited outside of established manufacturing hubs. The chief imperative was to reach maximum capacity as quickly as possible while maintaining continuity of output. It was therefore advantageous to site plants outside of labor markets where a large share of the workforce was already occupied with manufacturing work. Moreover, concentrating industry in large hubs posed major risks, both because of the vulnerability of a single city (e.g. bombings or power outages) and because of service interruptions due to urban congestion. Location of plants along coasts and borders raised the specter of bombing raids; hence, although many key industries were concentrated in coastal cities (aircraft, in particular), the military urged that all new expansions take place two hundred miles or more inland, if possible Craven and Cate, eds (1955). Further, assembly of airplanes typically required that plants be built alongside large airfields, which required rapid assembly of the sorts of large parcels that were difficult to find in major cities.

These factors that increased the strategic value of a plant tended to run against their financial value. Although private firms that made investments in service of war contracts were offered generous tax subsidies, firms and their financiers were much less eager to risk capital on very large industrial plants with highly uncertain post-war value like ordnance factories or bomber assemblies—especially if those facilities were located far from firms' existing operations. An unexpectedly quick end to the war could radically reduce such a plant's profitability since repurposing for civilian production would be costly. After the war, Air Force historians noted that "The industrialists' reluctance to invest in dispersed plant facilities was at odds with the government's hope that private capital could finance new inland construction; Hence, the War Department could carry out its policy only to the extent that the government was willing to put up the money" (Craven and Cate, eds, 1955).

In many cases, the military decided that such plants were sufficiently necessary to justify full government financing, even at extraordinary cost. Large bomber assemblies, ordnance works, aluminum and steel plants, chemical processing facilities, and other large plants were built in small cities that had little history of large scale chemical processing or metalworking. In these cases, the plants were Government-Owned and Contractor Operated (GOCO)—the contracted firm would typically construct and operate the facil-

¹¹In 2010 Dollars, the wartime capital expansion was approximately \$300 billion. In comparison, the 2009 stimulus measure (the American Recovery and Reinvestment Act) authorized a total of \$111 billion for infrastructure construction.

¹²During the wartime expansion, private investment expanded as well. While private capital expenditures on production facilities totaled \$1.2 billion in 1939, annual private investment in war manufacturing facilities was about \$1.8 billion per year on average throughout the war. Publicly-financed investment—approximately three times the amount of private investment—was supplemental.

ity, while the facility itself would be fully financed and owned by the US government, usually under the auspices of the Defense Plant Corporation (DPC). At the end of the war, the plants were to be auctioned off to private firms, with the right of first refusal offered to the operator. Such arrangements allowed the government to assume the full financial risk of these crucial plants (White, 1980).

Siting decisions for GOCO plants were largely driven by a combination of strategic considerations and idiosyncratic factors pertaining to the ready availability of suitable parcels. While siting efforts were coordinated by centralized boards, the actual process was fairly ad hoc; Appendix 9 provides a detailed examination of this process and the bodies involved. The chief concerns were: 1) location outside of congested industrial centers, 2) availability of parcel and community support to begin construction immediately, and 3) sufficient access to labor, housing, transportation, water, and power.¹³ In practice, the foremost challenge was to find a parcel spanning hundreds of acres that could be purchased or seized with minimal difficulty. In many cases, local officials or large landholders in small cities (that typically could not attract private investment outside of wartime) would offer military officials full disposal of local public services or free land if they located war plants in their jurisdictions. Appendix 9 presents several examples. Much to their chagrin, elected officials had limited ability to influence the siting process beyond recommending suitable parcels in their jurisdictions (Wilson, 2016). Rhode et al. (2018) find that political influence had little impact on the placement of WWII defense contracts more broadly.

After the war, most plants built during the war were either converted to civilian production or put to continued use for defense production as the Cold War began. Business interests were concerned about government ownership of productive facilities that might potentially compete with private interests in the post-war. Therefore, the authorizing legislation required that plants must be operated by private firms after the war. In practice, plants specializing in the production of ammunition and ordnance were often kept under government ownership, but operated by private firms under contract. Most other plants owned by the DPC and other government agencies were sold to private sector firms, generally at a small fraction of their construction price (United States War Assets Administration, 1947). While some wartime contractors purchased the plants they had operated, others declined to do so or were prohibited from doing so due to antitrust concerns.¹⁴ For example, Ford declined to purchase the massive Willow Run plant it built outside Ypsilanti, Michigan (on farmland personally owned by the Ford family) and it was instead sold to Kaiser-Frazer to produce the original consumer Jeep (which in turn sold the plant to General Motors several years later). Like many other plants, Willow Run remained in operation for over fifty years.

¹³According to a report by the successor agency to the WBP: "Such factors as availability of labor, transportation facilities, housing, water power, community services and attitude, sources of raw materials and destination of the finished products, and the general relation of the new plants to the over-all distribution of manufacturing facilities in the country were carefully examined. The board was anxious to avoid, if possible, the building of plants in already highly industrialized and congested areas. (McGrane, 1946)" See 9 for additional references. Recent work by Severnini (2014) highlights that proximity to power sources was an important consideration for plant siting prior to the development of high-tension transmission lines after 1950.

¹⁴In particular, Alcoa was deemed to have excessive market power over aluminum, and many plants it operated during the war were sold to competitors.(White 1980)

3 Data Sources

3.1 County-Level Panel Data

We build our county-level panel using Economic Census and the Decennial Population Census data compiled in the County Data books and in work by Haines (2005), which covering the years 1920 through 2000. We supplement these data with additional county-level variables and data from additional years obtained from the National Historical Geographic System (Minnesota Population Center, 2011). We also include data from Fishback et al. (2005) that account for geographical features, the severity of the Great Depression, and local exposure to various New Deal interventions. In addition, we collect postwar defense contracts reported on DD350 forms from the National Archives and compute county-level sums based on the locations of the contracting firms. The county median family income concept available from 1950 onwards is not available for the 1940 Census; we proxy for 1939 median family income by calculating tabulating median combined household 1939 earnings in each county using the full-count 1940 Census microdata from IPUMS.

We make several restrictions on the baseline analysis sample. We also exclude counties in Alaska, Hawaii, and Virginia due to significant changes in county definitions during the observation period. In addition, we exclude highly rural counties that were unsuitable to manufacturing—in particular, we drop counties that have no records of wages paid to manufacturing production workers in an Economic Census year between 1920 and 1955 or with fewer than 10 such workers in 1939. After applying these restrictions, there are 1,981 counties in our baseline sample.

3.2 World War II Investment Data

To examine the effects of large public plant sitings, we collected and digitized WPB data books that report all capital expenditures that were authorized by the government to support a war supply contract. In particular, we draw from a 1945 WPB data book, *War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator* (United States War Production Board, 1945b).¹⁵ The data book has plant-level detail on each plant’s operator, the 1939 industry of the operating firm, the city in which the plant is located, the plant’s war products and output volumes as specified in the operator’s contract, the date of completion, and the cost of facilities expansion. Importantly, all capital expenditures are broken out into privately financed and publicly financed amounts, and then are further subdivided into expenditures on structures and equipment.

We use the information contained in the data book to identify instances where new, large plants were constructed with public financing. First, we identify an investment as a “new plant” if over 40% of the investment was in structures rather than equipment, which is a ratio that is typical among the plants that we

¹⁵Very few new projects were authorized in the final year of the war—authorizations through October 1945 account for 90% of all wartime capital authorizations and virtually all public plant construction.

were able to identify as newly-built from archival sources. In some cases, plants were explicitly identified in the data book as “new plants,” though some newly-built plants were not labelled as such. If plants are marked as “new plants” we classify them as new regardless of their investment composition.¹⁶ Next, we identify plants as “publicly-financed” investments if 99 percent or more of capital expenditures on structures came were from public agencies; we classify remaining new plants (partially) “privately-financed”.¹⁷ Finally, to identify the most significant siting events, we define plants as “large” if the total investment was at least \$1 million in \$1940. This is meant to pick up on plants in the long tail of the expenditure distribution in Figure 1, which plots the distribution of expenditure amounts across plants on a log scale, illustrates how thick the upper tail is. We use these data to identify large, publicly-financed, new plants. We define these as plants costing at least \$1 million (\$1940) built at new sites, for which one-hundred percent of investment in durable, immobile structures was publicly financed. There are 582 plants fitting this definition in the data, concentrated in only 202 counties in our analysis sample. However, even within this group the bulk of expenditure occurred in a small subset of very large plants.

3.3 Individual-Level Earnings Records

We study long-run effects on individuals using two distinct data sets. The first data set is constructed from the CPS ASEC in 1991, 1994, 1996-2017. A subset of CPS ASEC respondents were linked to their SSNs and assigned a PIK. Using the PIK, we can link these individuals to two datasets provided by the Social Security Administration, the Numerical Identification System (or NUMIDENT) and Detailed Earnings Record (DER). The NUMIDENT contains information on the date and place of birth provided by each individual when they file for an SSN. We used the crosswalk from Black et al. (2015) that matches the strings in the NUMIDENT place and state fields to counties to identify the county of birth of each linked individual. The DER is a panel of job-level earnings histories from Form W-2 returns for each linked CPS ASEC respondent. For the linked individuals, the DER includes the W-2s for all jobs from 1978 to 2016. Although we do not observe individuals surveyed by the CPS prior to 1991, we do observe the full earnings history from 1978

¹⁶A 1945 WPB book of county-level tabulations subdivided plant investment into three categories: “expansions” of existing facilities producing similar goods, “conversions” of existing facilities producing substantially different goods, and “new plants” that were substantively new establishments (WPB 1945b). Those tabulations found that slightly over \$12 billion of the plant expenditures nationwide fell into the “new plant” category. Our “new plant” designation is meant to replicate that categorization as best as possible in the plant level data. Many plants are directly labelled as “new plants” in the plant level data; these account for \$8 billion of the total expenditure. There are numerous large plants in the data without this label that have been confirmed to be new plants based on external sources; a common and intuitive feature of such plants is that the expenditure on structures is very large relative to the expenditure on equipment (since the sites are being constructed from scratch). Using this threshold yields a total expenditure on plants classified as “new” that approximately matches the \$12 billion target. In comparison to the published county level tabulations, the county-level totals based on our micro-data classification has a .85 correlation (as opposed to .73 when only using plants listed as “new”).

¹⁷We only use expenditure on structures for this determination since they are the fundamentally immobile part of the investment; it is easier for private firms to recoup costs on equipment that can be transferred or sold to plants in other locations. Many plants had some, but not all, construction of new structures financed by private capital. Most large plants have multiple buildings or wings, and in some cases a firm was willing to finance buildings with high expected post-war value but not supplemental buildings or wings that were dedicated to very specialized parts of the production of war material. We distinguish between these “partially privately financed” plants from plants that received absolutely no private investment in any structure at the facility.

onwards for everyone who eventually appeared in the CPS sample in the available years.

We use these records to construct a sample with county of birth information for individuals who appeared in one of the available CPS files and who were born before the start of the War at the end of 1941, but were under 18 at the end of the War (born 1928 or later) and were therefore not directly involved in wartime activity. The result is a sample of approximately 100,000 individuals with earnings from the DER and demographic and education information collected in the CPS.

The second dataset links information on children in the 1940 Census of Population to information about their adult earnings reported on IRS 1040 individual income tax returns from 1974 and 1979, for individuals who could be assigned PIKs in both sources. Unlike the CPS-DER file, which builds off a representative sample of the population, the 1940 Census is a comprehensive population-level dataset. Massey and Rothbaum (2020) use these data to document county-level differences in intergenerational rank-rank mobility following Chetty et al. (2014). Adult income ranks in the 1970s are measured using tax-unit level Adjusted Gross Income (AGI) reported on the 1040 forms in the two available years. We use public-use data produced by Massey and Rothbaum on expected child income rank by parent earnings decile by county and for various subgroups, including child race, gender, and foreign-born status of parents.¹⁸ These data enable us to measure the 1970s household income (AGI) ranks of individuals based on their 1940 county of residence reported in the Census and their parents' 1939 earnings decile.¹⁹

The construction of this database is documented in detail in Massey and Rothbaum (2020) but we will provide a more concise summary here. In the 1940 census, all individuals 14 and over were asked their wages and salary earnings as well as whether they had \$50 or more of "sources other than money wages and salary." Furthermore, wage and salary earnings were top coded at \$5,000 and enumerators were instructed not to "include the earning of businessmen, farmers, or professional persons derived from business profits, sale of corps, or fees" Ruggles et al. (2020). In the full population data, 65 percent of the 41 million children have at least one parent who reported wage and salary earnings. Because over 30 percent of children have parents without reported wage and salary earnings, Massey and Rothbaum (2020) cannot easily include non-earners in the estimates. They attempt to control for selection into wage and salary earnings and child PIK assignment (needed for observing child adult income) by using inverse probability weights (IPW) conditioning on parent, child, and location characteristics. In practice, this results in additional weight given to children of parents with similar characteristics to parents without reported wage and salary earnings. This increases the weight given to children of Black parents, less educated parents, and parents with lower reported wage and salary earnings.²⁰

¹⁸This dataset also includes the addition of $(\epsilon-\delta)$ -differentially private Gaussian noise for disclosure protection.

¹⁹Unfortunately, the CPS-DER and the publicly released estimates from the linked 1940 Census—IRS datasets cannot be merged at the individual level.

²⁰Alternative strategies such as imputing the earnings from other data sets, such as the 1950 census are possible, and have been tried, for example by Feigenbaum (2018). However, any such strategy still requires modeling the relationship between parent self-employment earnings and child adult income based in part on the the relationships observed between parent wage and salary earnings and child adult income. In practice, we do not believe this would differ substantially from the IPW assumption, but do plan on implementing this imputation to test for the robustness of the Massey and Rothbaum mobility results.

4 Research Design

4.1 Empirical Strategy

Our empirical approach is to compare counties that received a large, government-funded plant to other counties that were observably similar in 1940 in terms of the plant siting criteria described in archival documents. In particular, officials sought secure locations outside of congested industrial hubs in smaller cities, preferably inland, with access to key resources: sufficient labor, housing, transportation, water, and power. Our conjecture, motivated by historical evidence, is that *neither* the places where such plants were sited nor similarly-suitable locales would have been chosen as a plant site if not for the exigencies of the war emergency.²¹ That is, conditional on the size of the workforce, the availability of water and power, and the state of local industrial infrastructure, siting was as good as random among similar counties. In that case, selected sites and comparable counties would have likely evolved similarly over time if not for the war emergency.

Our approach requires a region-level “treatment” or exposure measure. To do so, we study cross-county variation in exposure to plan investments using two complementary approaches. First, to study the effect of getting a large plant, we construct a binary treatment variable that indicates whether a county received a new large, public plant. Only 202 counties in our sample have any such plants, and since some plants were fairly small facilities there is a thick tail in the per-capita spending distribution visible in Appendix Figure A.1. We designated counties as “treated” if their public plant spending per 1940 resident was in this long tail—specifically we designate the half of counties with new plants with the most intensive investment levels (101 counties in total) and omit remaining 101 counties with plants that were not in the long tail from the analysis sample.²² We use this binary treatment definition to isolate the impacts of the plants that were large relative to their local economies, while ensuring results are not driven by a small handful of outliers. Figure 2 maps the location and type of each of the largest new public plants in each of the 101 treatment counties—most of the plants are sited in mid-sized, inland cities scattered throughout the the Midwest, the Plains States, and the Mountain West of the United States. Nonetheless, the variation in investment amounts along the intensive margin may also impact outcomes. Accordingly, we use the total spending on new government plants per 1940 resident as a regressor as a second approach as well.

To quantify the suitability of a potential site location, we estimate a propensity score using observable characteristics that were relevant for plant siting.²³ First, we include variables measuring the size of the city and available workforce in 1940: logged total population, employment, black population and their squares; logged urban population and immigrant population, the share of residents residing on farms, and the share

²¹In the no-war counterfactual, we argue that plant construction that would have occurred would have taken place in the large industrial hubs where private firms actually made investments during the War.

²²While large cities like Detroit and Chicago did receive large new public plants, this restriction excludes such locations from the treatment definition since these investments were small relative to the size of the existing manufacturing base..

²³We estimate propensity scores using predicted values from a probit regression of the binary treatment indicator on the specified observable characteristics of counties.

of adults with a high school degree. We also include the logged number of unemployed men and women in the 1937 Unemployment Census. To capture suitability of the geographic location we include indicators for access to major rivers, lakes, waterways, external coasts, and Great Lakes coasts. We infer infrastructure suitability using logged per-capita measures of New Deal public works spending and AAA road grants from Fishback et al. (2005), as well as the 1940 share of households with electricity, and the shares of rural households with electricity and with plumbing. We also include logged 1939 value added, the number of manufacturing establishments, and the size of the production workforce to capture the baseline level of manufacturing development. The key identification assumption is that conditional on this core set of pre-war observable characteristics, the remaining sources of variation in plant assignment are statistically independent of other latent determinants of post-war economic development.

We use these propensity score to re-weight comparison counties according to their suitability for receiving a plant. We define the propensity score weight W_i as equal to one for all treatment observations, and equal to $W_i = \frac{\hat{p}_i}{1-\hat{p}_i}$ for the remaining controls. Under the assumption that plant assignment is independent of all unobserved drivers of outcomes conditional on the propensity score, the re-weighted difference in outcomes for treatment and comparison counties with propensity scores in the overlapping region

$$AT\hat{E}T_t = \frac{1}{N_{Treat}} \sum_{i:Treat_i=1} Y_{it} - \frac{\sum_{i:Treat_i=0} Y_{it} \frac{\hat{p}_i}{1-\hat{p}_i}}{\sum_{i:Treat_i=0} \frac{\hat{p}_i}{1-\hat{p}_i}} \quad (1)$$

is a consistent estimator of the average treatment effect on treated counties (Hirano and Imbens, 2005). In our analysis, we examine how estimates differ under different specifications of the propensity score and alternative estimators including nearest-neighbor matching on the propensity score and OLS estimation conditional on control variables.

Under the conditional independence assumption and correct specification of the propensity score, the treatment group and re-weighted comparison group should be observably similar and should evolve similarly prior to the War. Table 2 examines covariate balance in the unweighted and re-weighted overlap subsamples. Columns 1-4 test for balance on the siting-relevant characteristics included in the propensity score. In unweighted comparisons, the 94 treatment counties in the overlap sample were larger and higher income than the average comparison county—this is because the large majority of counties in the comparison group were highly rural and unlikely to be suitable for a plant under any circumstances. Once we re-weight comparison counties using the propensity score, the treatment group and comparison groups are well-balanced—this is largely by construction. As a stronger test of our identifying assumption, we also test for balance on a wide range of county characteristics that were *not* included in the propensity score, including income and wage metrics, occupational composition, industry composition, and house prices. Consistent with idiosyncratic assignment conditional on the propensity score, re-weighted comparison counties are statistically indistinguishable from the treated counties. Figure 3 maps variation in the propensity score weights across counties. Similar to the treatment counties, the highest-weighted control

counties tend to be in small or middle sized heartland cities with a similar geographic distribution. We additionally test whether outcomes evolved in parallel for the treatment counties and re-weighted comparison counties prior to the War in our analysis below.

We supplement this baseline analysis by examining the effects of more intensive investment using OLS regressions where the covariates used in the propensity score are included as linear controls. We measure spending intensity as total expenditure on new government-funded plants per 1940 resident—this is positive for the 202 counties with new plants and 0 for the rest of the sample. We estimate equations of the form

$$\ln Y_{it} = \alpha_t + \beta_t \frac{\text{Expend}}{\text{Capita}_i} + \gamma_t X_i^{1940} + \epsilon_{it} \quad (2)$$

t is an outcome year, X_i^{1940} is the 1940 vector of observable covariates, and β_t is the year specific treatment effect. We winsorize the exposure measure $\frac{\text{Expend}}{\text{Capita}_i}$ at the 95th percentile among nonzero observations. We estimate these regressions on the full sample, including all 202 counties with treatment spending.

Since we have panel data for many of our county-level outcomes, we can relax the identification assumption by using a difference-in-differences design. We implement this design by replacing county level outcomes in year t , Y_{it} , with differences in outcomes relative to 1940 levels, $\Delta Y_{it} = Y_{it} - Y_{i1940}$. This facilitates causal inference under a weaker assumption—rather than assuming that treatment and control outcomes would have been identical conditional on observed covariates, it only needs to be the case that outcomes would have changed the same between 1940 and the outcome year. However, we cannot apply the same differences-and-differences approach in our individual-level analysis, because we do not observe pre-War outcomes for individuals in our data sources. Causal inference in this latter setting requires that the conditional independence holds.

5 Long Run Impacts on Regional Development

We first examine how plant sitings during WWII impacted local manufacturing output, employment, and wages in treatment regions in the long run. Any plausible effect on the broader labor market should likely stem from the direct impacts on the manufacturing sector. One should note that effects on county-level manufacturing outcomes may reflect the influence of the treatment plant itself, in addition to any spillover effects on neighboring firms. Without comprehensive longitudinal data on plants and their ownership that goes back to the 1930s, it is difficult to distinguish between these internal and external effects. Thus, in this section, we study how plant sitings impacted local manufacturing work on the whole.

Figure 4 plots the reweighting estimates of plants on county-level manufacturing activity for each year available from from 1900 to 2000.²⁴ Several findings stand out. The effects of a plant siting on aggregate manufacturing output is large—value added increases in treatment counties by approximately 30–40 per-

²⁴These outcomes are derived from establishment level surveys; county aggregates are therefore tabulations across establishments located each county.

cent in the years following the reconversion period during the late 1940s. These effects are highly persistent. As late as 1997, manufacturing value added remains about 25 percent higher in treatment counties than in the re-weighted comparison group.²⁵ The effects on production employment follow a similar time pattern, rising to be 20–30 percent in the 1950s compared to the control group, and remaining roughly 15 percent higher at the end of the century. However, we find that plant sitings had little to no effect on the number of manufacturing *establishments* in first decades following the war. The simplest explanation for this latter finding is that the massive new plants built during the war by themselves accounted for most of the increase in manufacturing activity after the war. Later in the century, however, the effect on establishments increases and converges to the effect on employment, suggesting that the long-run effects are less likely to be driven by a single plant.

The second panel of Figure 4 examines the effects of plant sitings on the average production wage. Plant sitings had an effect on average manufacturing wages that was large, immediate, and persistent. After the postwar reconversion, production wages increased by 8-10 percent in treatment counties relative to comparison counties, after evolving in parallel from 1900 to 1940. Notably, these pay differentials persistent through the *entire* remainder of the century—even as the effect on the number of production jobs fades somewhat over time, the remaining jobs continue to pay the same premium relative to wages in comparison counties. This increase in pay closely tracks the changes in county-level manufacturing labor productivity (value added per production employee) resulting from the siting of a war plant.

The results from the continuous exposure variable are qualitatively similar and display comparable time patterns; given the mean treatment levels documented in Table 1, the magnitudes are roughly consistent with those found in the analysis of the binary treatment. These estimates indicate that the size of the plant relative to the local labor market is indeed a key determinant of the effect size. The effects on productivity and wages are more persistent than the effects on output and employment—even as manufacturing jobs became more scarce later in the century, the remaining manufacturing jobs in these locations continued to be more productive and pay better throughout.

Importantly, the treatment and re-weighted control counties evolve in parallel prior to the outbreak of the war. This finding, which holds for all four outcomes examined, is consistent with the identification assumption that public plants were sited for idiosyncratic reasons, and not based on differences in latent trends in regional performance or manufacturing productivity. We further examine the robustness of our identifying assumptions in Figure 5, which shows how the employment and wage results change under alternative specifications. Our re-weighting estimates are very similar to OLS estimates or propensity-score nearest-neighbor matching estimates of the same treatment-control comparison. Results are stable across both more parsimonious and more extended specifications of the propensity score.

We examine these baseline results in detail in 3. The left-hand panel reports ATET effects on changes in the outcome variables from 1939 to either 1963 or 1982. The right hand panels show OLS estimates of the

²⁵To interpret the magnitude of these effects, the median treatment county had 5566 production workers employees in 1954, which accounts for roughly one-fifth of employment in the typical treatment county. 20% fewer production employees in the typical treatment county would amount to over one thousand fewer jobs.

effects of an addition \$1000 (2014\$) per capita in public plant spending for the same years and outcomes. For each specifications, we also present estimates under an alternative specification that adds additional income and sectoral variables to the baseline set of covariates.²⁶ Appendix Table A.1 presents further specifications using alternative estimators. We find effects that are consistent across all specifications—the treatment effect of a new plant is roughly equivalent to the effect of spending \$10,000 (2014 Dollars) per capita. Notably, total wage bill grows in close proportion to total value added and therefore capital intensity remains fairly constant in postwar period—suggesting factor payment shares are similar in counties with and without new war plants.

As a comparison, we identify large new plants in our data that drew at least some private financing and define a “private plant” treatment constructed identically to the main treatment variable. Appendix Figure A.2 replicates the main findings for value added and production wages using this alternative treatment and a corresponding re-weighting control. While these “private plant” produce more after the war, they are on a clear upward trend from 1930 to 1940, with no clear break in that trend during WWII. Thus, for these private plants, it is difficult to interpret the full effects on postwar outcomes as the causal effect of the war. The idiosyncratic siting of the public plants, which could not attract private investment, is crucial to the causal interpretation of our results.

We examine the effects on broader regional development in Figure 6 and Table 4.²⁷ Government-funded construction of war plants had permanent effects on the size of the surrounding economies. Total population and employment grew in proportion with one another over the first two decades following the war relative to the comparison counties, which never caught up afterward. In 1960, the populations of counties where plants were sited were nearly 10% larger than in control counties. While the share of local residents who were employed in the manufacturing rose by several percentage points, employment growth spilled over to other sectors such as retail, which grew in close proportion to employment.

While the employment effects spread far beyond the manufacturing sector, increases in manufacturing wages were not shared by all other sectors. The second panel of Figure 4 also reports effects on average wages in the retail and wholesale sectors—in almost all years these effects are close to zero with tight confidence intervals. These findings imply that plant sitings caused the *premium* paid by manufacturing jobs relative to jobs in other sectors to increase. Why did manufacturing wages track the productivity of local firms, rather than wages in other sectors (and manufacturing wages in places that did not get plants)? One possibility is that plant sitings led to a rapid “upskilling” of manufacturing jobs and that higher wages reflect the market value of enhanced skill. Another possibility is that labor market institutions enabled workers to bargain for wages tied to their output. Wartime labor policies—particularly those of the National War Labor Board—facilitated rapid union expansion in war-related industries (Goldin and Margo, 1992;

²⁶In particular, we additionally include 1940 median household income and housing values as well as employment and average wages in the retail, service, and wholesale sectors. In OLS specifications the covariates used in our propensity score estimation are included as controls.

²⁷These outcomes are derived from population surveys, which are aggregated to the county level by individuals’ places of residents

Wilson, 2016; Farber et al., 2018). As a result, by 1946, over 80% of production workers in the industries that participated in war production—including automobile, aircraft, shipbuilding, steel, and aluminum manufacturing—were covered by union contracts (United States Bureau of Labor Statistics, 1947). This latter explanation is consistent with a long literature interpreting cross-industry wage differentials—and, in particular, a sizable manufacturing wage premium in the 1970s and 1980s—as arising due to imperfect competition in segmented or unionized labor markets (Krueger and Summers, 1988).

Though we do not find effects on wages outside of the manufacturing sector, the treatment nonetheless has a significant effect on median family incomes. The effects on median earnings largely reflect the increases in manufacturing wages: the magnitude of this effect is about one-fourth of the effect on manufacturing wages, which constituted about one-fourth of employment in treatment counties in 1950. It is possible that the rise in household incomes might reflect a rise in the local cost of living. We find that the median housing value in treated counties did in fact rise proportionally to the median incomes, though the former estimates are more imprecise and less robust to specification than the latter. While higher values might reflect a higher cost of living, this need not be the case, as higher housing values could simply reflect increased consumption of higher-quality housing due to increases in income.

Our focus on counties is somewhat arbitrary—it is possible that plants had impacts beyond county borders. It is also possible that the growth that occurred in counties with war plants merely displaced activity from other counties in the same region. To test whether this is the case, we directly examine regional spillovers in Table 5 and further study how our results change when we omit counties bordering treated counties from our comparison group in Appendix Tables A.1 and A.2. We test for regional spillovers by creating a binary indicator that is one if a county is adjacent to a treatment county (and had no public plant investment) and zero for other counties in our sample with no public plant investment. We then construct a propensity score using the same baseline covariates and use this to obtain re-weighting estimates analogously to our baseline analysis. Consistent with a story in which manufacturing-sector effects are driven by the plant built during the war itself—which would not have been built in the region if not for the war—we find no evidence of any effect on manufacturing activity at establishments in surrounding counties. However, we do find evidence that surrounding counties shared in the broader regional development experienced by the counties where plants were sited, particularly in the longer run. Notably, we find effects on manufacturing employment in neighboring counties when examining metrics tabulated by place of worker *residence* rather than by establishment locations; this suggests residents of surrounding counties commuted to expanded manufacturing employment opportunities in treatment counties.

In the Appendix, we examine how the effects of government-financed plant construction compared to other forms of war spending. In particular, we identify large new plants in our data that drew at least some private financing and define a “private plant” treatment constructed identically to the main treatment variable. Appendix Figure A.2 replicates the main findings for value added and production wages using this alternative treatment and corresponding re-weighted control group. While counties that received privately-financed plants experienced employment growth after the war, employment was on a clear upward trend

from 1930 to 1940, with no clear break in that trend during WWII. Thus, for these private plants, it is more difficult to interpret the full effects on postwar outcomes as the causal effect of the war.

6 Mechanisms of Local Persistence

The findings above show that wartime plants construction had large effects on the size of local labor markets and on the availability of particularly “good-paying” manufacturing jobs, and that these effects were extremely persistent. However, the implications of these findings hinge on the reasons the effects persisted. Were these war plants really one-off investments, or were these plants supported by ongoing US spending throughout the Cold War? Did effects persist simply because the initial investments were durable and depreciated slowly, or did treatment counties also attract ongoing private investment at higher rates? Were these effects internal to a single plant, or were there external economies of scale that generated path dependence in regional evolutions? This section addresses each of these questions in turn.

To test whether treated counties continued to benefit from ongoing military spending, we draw on the Military Prime Contract File database of DD350 forms, which documents all Military contracts between 1966 and 1975. This database lists the name and county of the prime contractor on each defense contract, as well as the value of each contract. Using these records, we measure the total defense contracts received by prime contractors in each county, broken out for 1966–1970 and 1971–1975. An important limitation of these data is that they only report prime contractors and not subcontractors that worked on military projects. Nonetheless, these data allow for a simple test of whether treatment plants were likely to be prime contractors during the Cold War.

Propensity score re-weighting estimates are presented in Panel A of Table 6. These estimates are simple treatment-control comparisons, not difference-in-differences estimates. We find that treatment counties get more defense contracts than the counterfactual control group. However, such an effect might occur mechanically due to the increased manufacturing presence in those counties—this would occur, for example, if contracts were distributed randomly across regions in proportion to manufacturing employment. Thus, we test whether the treatment counties had higher ratios of defense contracts to overall manufacturing value-added. We find that the effect is much smaller and not statistically significant from zero, suggesting the rise in value added was not driven primarily by military spending.

However, as mentioned above, it is well documented that several plants in the treatment group continued to supply the military as Government-Owned Contractor-Operated (GOCO) plants throughout the Cold War. This was particularly common among the ordinance and ammunition plants built during the war. This was in large part because, unlike aircraft assemblies and steel plants, ammunitions plants and ordnance works bear little resemblance to facilities that produce consumer goods and could not be converted to civilian use. As the primary consumer of ammunition and ordnance, the military continued to generate most of the demand for these facilities after 1945. Thus, one should expect to see differences between such plants and other general manufacturing plants built during the war. We use descriptions in

the WPB data books to classify the largest new public plant in each treatment county as either an ordnance works or a general manufacturing plant, and implement the propensity score reweighting estimator separately for each treatment type.²⁸ Panel A of Table 6 further presents the effects of each type of plant on Cold War defense spending. We find that whereas the 44 ordnance works in our sample draw substantially more military contracts than the counterfactual control group, the other manufacturing plants are no more likely to cause counties to attract defense contracts—both in absolute terms and relative to total manufacturing output—than other comparable counties.

Although ordnance plants get substantially more military spending after 1945, these plants alone do not drive the main results of the paper. Panel B of Table 6 and Figure 7 separately estimate the effects of each kind of plant on local development. Both types of plants have similar effects on postwar manufacturing activity. The time path of the effects of ordnance plant sitings largely reflects the history of the Cold War—the largest spikes are during the wars in Korea and Vietnam, and the effects dissipate afterwards. Yet the effects of the plants that were largely converted to civilian production were similar; if anything, the effects on wages were larger. Moreover, this latter group of plants has significantly larger effects on broader regional development. Population and employment outside of manufacturing grew dramatically in places where plants were converted to private industry. These findings indicate that one-time government investments in such manufacturing plants did in fact have transformative effects on local labor markets, which were not driven by continued military spending. By contrast, there was no effect on broader population or employment near ordnance plants. Rather, total employment in these areas skewed significantly more towards manufacturing.

Next, we test whether the initial investments resulted in higher rates of private industrial investment after the war. While county-level data on manufacturing capital expenditures is available after the War, there are no comprehensive data from before the war—as a result, it is not possible to implement the baseline difference-in-differences design. Instead, we implement a simple comparison of the treatment counties and the propensity score re-weighted control in the available postwar years. These differences will yield biased estimates of the effects of plant sitings to the extent that there were any pre-existing differences between treatment and control counties. For comparison, we plot these estimates alongside simple-difference estimates of the effects on manufacturing value added in Figure A.3. We find that the effects on log capital expenditures were slightly larger than the effects on value added and follow a similar time pattern. If the (true, unobserved) initial effects on capital expenditures are assumed to be proportional to the estimated effects on value added, then the implied effects of wartime investment on postwar investment are large. Figure A.3 shows that the investment effects were particularly large for general manufacturing plants. These findings indicate an important role for path-dependence—the initial investment by the government increased the returns to future investment by the private sector.

These dynamic complementarities may be purely internal to the original plant—once the fixed cost of assembling a site and establishing a supply chain is sunk, it might be most profitable for the firm that

²⁸Treatment counties of either type are never included in any control group.

owns the plant to continue to reinvest in it. However, path-dependence might operate through external channels as well if agglomeration externalities are at play. The standard examples are those highlighted in Alfred Marshall's 1890 textbook: productivity enhancements via local know-how and social interactions, easier searches for specialized labor due to the initial attraction of a base pool of skilled laborers, access to dense markets at low cost, or construction of high-fixed cost core infrastructure that benefits other business. If there are agglomeration effects, then the plants were an unambiguous win for local counties—even beyond the profits they provided to their owners. On the other hand, if there were no externalities or local market-level effects, and all wage increases simply reflect workers with specific skills or tolerance for industrial work moving into the county to take jobs at the plant, then there are no direct gains to local residents—beyond perhaps the basic advantages of having a larger population and tax base. Since we only observe aggregate earnings and employment in our data, we cannot determine whether the employment and wages effects we find are evidence of true non-pecuniary agglomeration spillovers, market-level impacts, or simply a reflection of activity at one large plant.

Another potential driver of persistence is investment in infrastructure constructed to support the war plants, which could in turn increase productivity at other plants. One particularly salient example is the construction of the Interstate Highway System (IHS). As noted in Baum-Snow (2014), the early plans for the IHS were strongly influenced by the experience of the war. A key goal of the IHS was to facilitate continuity of supply to the armed forces in case of war—which meant creating good connections to war production facilities such as the treatment plants. In simple re-weighted treatment-control comparisons of counties in 2000, we find that treatment counties are 8 percent more likely to have an IHS connection ($p = 0.05$), and that this effect rises to 12 percent ($p = 0.01$) when focusing on non-ordnance plants. Treatment counties also have 9 additional IHS miles ($p = 0.05$) relative to the counterfactual comparison group. These connections may certainly have contributed to the persistence of the effects identified above.

7 Effects on Individual Earnings

7.1 Effects on People Versus Places

We find that plant sitings had large and persistent effects on local manufacturing development and regional growth. But do such changes benefit the people who live and work in those regions? In spatial equilibrium, utilities are invariant to the geographic distribution of production so long as agents are sufficiently mobile. In this section, we examine whether plant sitings raised typical incomes in regions—and, more specifically, whether plant sitings directly increased the incomes of the *individuals* who lived nearby before the war began. We test for individual-level effects using two data sources that link information on individuals' pre-war place of residences to administrative earnings records from the 1970s onwards. This enables us to compare adult outcomes for individuals who had a public war plant built in their pre-war hometown—for idiosyncratic reasons—to others in the same cohort who did not.

However, we cannot apply the same difference-and-differences approach used in the county-level analysis for this individual-level analysis, as we do not observe pre-War outcomes for individuals in our data sources.²⁹ Our approach, therefore, is to measure post-period differences between individuals who resided as children in counties that received plants compared to those who resided in counties that did not. These analyses require the conditional independence assumption to hold—specifically, it must be the case that, conditional on the propensity score or the corresponding covariate set, plant assignment is as-good-random. If this assumption holds, difference-in-differences (DD) and post-period differences (PPD) estimates should be identical in expectation—up to noise in pre-period outcome levels. This is partially testable through comparison of PPD and DD effects for outcome variables for which we have full panel data.

We examine the plausibility of the post-period difference estimates in Table 7. First, we compare DD and PPD estimates of our main county-level wage effects under various sets of covariates, focusing on the period around 1980 for which we observe individual earnings outcomes. Starting with our baseline covariate set (which excludes pre-war income variables) we find that PPD and DD propensity score estimates of effects on manufacturing and retail wages and median family incomes are practically identical. Including pre-war earnings and industry covariates has essentially no impact on the PPD estimates.

We next explore PPD estimates on other county-level earnings outcomes available in 1970 and 1980—variables for which we do not have pre-war measurements at the county level. Consistent with breadwinners in most households during this period being male, we find the magnitude of the effect on median male wages is close to that of the effect on median family incomes (about 4 log points), but find no effect on women’s wages. We also find that plant sitings are associated with nearly a full percentage point reduction in the poverty rate. These effects are highly robust as we vary the covariate set. The similarity between DD and PPD results and the robustness to controls for lagged dependent variables support the use of PPD estimators (using the full control set) to identify the causal effects of plant assignment.

From these county-level results, it is not possible to tell whether plant sitings increased the earnings of any particular resident. On one hand, the increase in local average wages may reflect an increase in earnings opportunities for individuals who would have resided near the plant in any case and therefore gained access to better jobs and work experience with the plant siting. On the other hand, the rise in local wages might merely reflect an inflow of skilled workers drawn to the new plant that would have earned the same higher wage in any case—but perhaps in another location. In this latter scenario, the investment might only change where people work without impacting any single individual’s earning opportunities; it might affect the places but not individual people who live there. In order to distinguish between these two stories, it is necessary to study individuals longitudinally.

²⁹If the impacts grew linearly with time of exposure, one could use variation in exposure length across cohorts to identify the causal exposure effect conditioning on a place effect, as in Chetty and Hendren (2016). In this setting, there is no reason to think effects should grow monotonically with exposure; by contrast, it is plausible that the cohorts entering the workforce right after the War—who had the least time exposure to the plants—may have been best positioned to get started on a successful career as these plants were starting to expanding civilian production.

7.2 The Effects of Plant Sitings on Individuals' Long-Run Outcomes

We test for individual-level effects using longitudinally linked data that enables us to study post-war outcomes of individuals based on where they lived prior to the war. We focus on individuals who were born before the war but were too young to work or serve during the War (1928-1941) in order to isolate the impacts of the plant apart from any wartime work experience. We then observe their adult earnings outcomes beginning in the late 1970s, when members of the relevant cohorts were between 35 and 50 years old. We observe earnings regardless of where these individuals locate as adults, which enables us to identify the causal effect of exposure to a plant siting apart from selection biases arising from adult location decisions. The effects we are able to recover are therefore the total effects of exposure beginning in World War II—beyond the direct effects of gaining increased access to local high-paying manufacturing jobs. Our estimates will thus also reflect indirect effects from human capital accumulation, learning, or increased family resources that may also benefit workers who choose to work.

Our baseline analysis uses the CPS-DER database, which contains individual level administrative earnings records from 1978 to 2016 for the sample of respondents from the CPS-ASEC from 1991, 1994, or 1996-2016 that could be linked to a PIK. Individuals with a PIK were linked to the NUMIDENT to identify their places of birth, which we in turn assign to counties. The ability to follow individual wage and salary earnings over time is a unique advantage of this data source. However, there are several limitations. First, even pooling the CPS ASEC samples across years, the number of individuals in the relevant cohorts is nonetheless relatively small—our analysis sample contains fewer than 100,000 individuals born between 1928 and 1941. Among this sample, 52,000 are male, and only about 10% were born in a county that received a large public plant. Second, we only observe outcomes for individuals who survive until their CPS year in the 1990s or later. However, we find no evidence of selective attrition that may bias our estimates—there is no relationship between county-level public investment and the share of county-level birth cohorts that eventually appear in the CPS-DER file. Third, the DER file contains total wage and salary earnings at an annual level, so we cannot differentiate between part-time work and low-wage work. We therefore classify individuals as full-time wage workers in a year if their earnings were at least the level one would earn working 2,000 hours at the Federal minimum wage in that year; we then test separately for earnings affects among full-time workers and for effects on the probability of participating in the full-time workforce on the extensive margin.³⁰ Finally, though we observe wage and salary earnings levels in each year 1978–2016, we only observe information on occupation, industry, and place of residence in the year when individuals appear in the CPS-ASEC, which is after 1990 and generally very late in workers' careers.

In our baseline analysis, we assign treatment status based on individuals' county of birth. We estimate regressions of the form:

$$\ln Y_{ict} = \alpha_t + \beta_t \text{Treat}_c + \gamma_t X_i^{1940} + \sum_{\tau=1928}^{1941} \delta^\tau \mathbf{1}[YOB_i = \tau] + \epsilon_{it} \quad (3)$$

³⁰We winsorize earnings at the 99th percentile.

where i denotes an individual and c denotes county of birth. We include controls for year of birth indicators to account for age differences in the outcome year. We estimate this individual-level specification using OLS, clustering standard errors at the county level.³¹

Effects on earnings in 1978 (the earliest year for which we observe earnings) are reported in Table 8. Plant sitings had significant effects on the earnings of male prewar residents, even more than three decades after the plants were constructed. Conditional on working full-time, the 1978 wage earnings of men born in treatment counties were approximately 3-4% higher than those born in comparable control counties. In Appendix Figure A.4, we find no effect on the extensive margin in almost every year. Figure 8 plots how the earnings effect evolves over time. We find that the 1978 effect—equivalent to roughly \$3,000 per year in current dollars—largely persists throughout the mid-1980s for men who continue to work.

Table 8 displays how these earnings effects differ across demographic subgroups. As in the county-level analysis, we find no individual-level earnings effects for women—if anything, we find that women born in places chosen as plant sites were less likely to work. In addition, we find that the effects for black men are small and statistically insignificant.³² However, we do find evidence that plant sitings during WWII increased educational attainment among black men—those born in treatment counties were 5 percentage points more likely to report having completed high school as well as 5 percentage points more likely to report having attended college in the CPS-ASEC. The effects on educational attainment are smaller for white men and zero for women of any race.

7.3 Manufacturing Opportunity and Upward Mobility

Our findings indicate that access to the high-paying manufacturing jobs stemming from wartime public investments causally increased workers' earnings in the decades following World War II. Does this imply that the availability of such jobs was an important driver of upward mobility in the midcentury period? If manufacturing jobs offered high-wage jobs to individuals regardless of their educational attainment or family background, it is possible that workers from lower-income families benefits relatively more from the construction of a large manufacturing plant. Accordingly, we use parental earnings information on 1940 Census forms linked to child earnings information from administrative household tax returns from the 1970s to test whether public plant construction caused an increase in upwards intergenerational earnings mobility. We draw on data from Massey and Rothbaum (2020) who, similar to Chetty et al. (2014), calculate the expected 1970s income rank (based on AGI reported on IRS tax returns) of individuals by their parents' 1939 wage and salary earnings decile for all individuals born from 1921-1940 with PIKs in the 1940 census.

Using these data, we test whether the construction of war plants had differential effects on the adult income rankings of individuals depending on the pre-war earnings rank of their parents. For each county in our sample, we calculate the average adult earnings rank for children who resided in that county during

³¹We find that our estimates are robust to alternative weighting schemes and control sets. Results are displayed in Appendix Table A.3

³²The sample of black men is substantially smaller than the sample of white men, leading to more imprecise results

the 1940 Census among subgroups split by parent earnings rank and other child demographics. As before, individuals are assigned to counties based on where they resided as children prior to the War (in 1940), not based on where they resided as adults. We estimate the effects of plant sitings using the baseline propensity score reweighting estimator in Equation 2. Our primary estimates are presented in Table 10 and alternative specifications are displayed in Appendix Table A.5.

For men born into families that were below the median income in 1939, we find that plants raised their adult earnings rank in the 1970s by nearly a full percentile. This effect is substantial, and equivalent to raising the earnings rank of ones parents by four percentiles. We also find positive, but smaller, effects on men born to parents with above-median incomes in 1939. The magnitude of these effects are consistent with our results from the CPS-DER file—below the 95th percentile, an increase in rank by one percentile corresponds to a \$1,700 increase (2017 dollars) in AGI on average.

Figure 9 plots effects further split out by parental earnings decile. While the treatment effect is roughly constant across deciles in the lower half of the parental earnings distribution, we find these effects drop off substantially at the high end of the parental earnings distribution. We show in Appendix Figure A.5 that individuals who lived adjacent to treatment counties as children in 1940 experienced comparable effects, consistent with our findings above that manufacturing employment increased among post-war residents of neighboring counties. These estimates indicate that the postwar manufacturing opportunities created as a result of wartime plant construction during World War II did in fact foster upward earnings mobility for children born in the surrounding region, increasing adult earning for individuals with parents at the lower end of the earnings distribution relative to those with parents at the top of the earnings distribution.

8 Mechanisms

The effects on individual earnings could reflect several different mechanisms. The simplest mechanism is that workers born in treatment counties had better access to the increased number of higher-paying manufacturing jobs created in their treatment counties.³³ Results in Table 8 show treatment had significant effects on the probability that individuals remained in their birth county through in year they appeared in the CPS, over fifty years after the start of the War. When we stratify the sample of men on the endogenous decision to remain in one’s county of birth in Table 8, we find the effects are concentrated entirely among the “stayers”, with little to no effect on individuals who move from their birth county before their CPS year. Although consistent with a story in which only individuals remaining to work locally benefit from high-wage work opportunity, this finding might simply reflect positive selection into staying. This explanation is also consistent with recent work by Tan (2019), who documents economic mobility in the early 20th century was less related to childhood environments and more reflective of adult work opportunity.

³³We do not observe any information about the industry or firms in which individuals worked except in the post-1990 CPS-ASEC itself. We find no effect of the treatment on the probability individuals worked in the manufacturing sector in or after the 1990s, though most workers were at retirement age at this point.

An alternative mechanism is that growing up near a big good-paying plant increased resources individuals had access to during childhood. These additional resources—both household income and improved local services—may have facilitated additional investment in human capital, which could have increased workers’ earnings opportunities both in and beyond the local manufacturing sector. Yet our findings in Table 8 indicate the plant sitings had no effect effects both on high school completion and college enrollment for white men, the group that experienced the largest earnings gains. We do, however, find large effects on educational attainment among black men despite finding smaller and statistically insignificant impacts on their earnings, though our sample of black men is small and the estimates are imprecise. .

Do these effects on the individuals’ earnings explain the rise in median county incomes reported above? A direct comparison of the two results is not possible, since the individual-level results reported in this section are for selected cohorts, while the county-level median income tabulations are based on the full workforce. Nonetheless, the CPS-DER allow us to estimate the effect of plant sitings on average earnings of post-war residents *among the same subset of cohorts* used in the place-of-birth analysis. While we do not observe place of residence in each of the years for which we observe earnings information in the DER, we can use individuals’ place of residence in the year they appear in the CPS-ASEC after 1990 as a proxy. Doing so allows us to examine how compositional changes contribute to observed postwar earnings patterns in the places where plants were sited during WWII.

Accordingly, we estimate a version of the regression equation (3) where the treatment status is assigned based on where individuals in our sample lived during the year they responded to the CPS-ASEC, instead of their place of birth. Although adult place of residence is only measured once in the CPS-ASEC year, earnings for these individuals are measured in each year after 1978. We plot effects for each year, along with the baseline effects with treatment assigned by place of birth, in Panel A of Figure 10. The effects on average earnings of post-war residents are significantly larger than the earnings effects on pre-war residents—roughly twice the magnitude—in most years we observe. One explanation for this finding is that workers who moved into treatment counties after the war were positively selected, increasing average local earnings beyond the amount due to a causal impact on individuals. However, an alternative explanation is that the place-of-birth effects are attenuated due to “imperfect compliance” with the natural experiment—if individuals born in treated counties moved away for idiosyncratic reasons, then these estimates may be better interpreted as “intent to treat” effects than a proper average treatment effect.

To better understand the relationship between regional development, increased earnings for incumbent residents, and selective migration, we return to the distinction between ordnance plants and general manufacturing plants, where only the latter could be converted to civilian production. In Section 6, we found that although only regions with ordnance plants directed production towards military contracts during the Cold War, only places with general manufacturing plants that could be converted to civilian production experienced population and employment growth. Are these latter regions, which expanded significantly, the ones where incumbent residents fared the best?

The results, presented in Table 9 and Panels B and C of Figure 10, indicate that the investments that had

the largest impact on regional development were distinct from those that provided the greatest benefit to local residents. Specifically, we find that ordnance plant sitings had the largest effects on the adult earnings of pre-war residents, increasing wage and salary incomes by about 4 percent in every year 1978–1990. By contrast, the effect of general manufacturing plant sitings is only half the magnitude and the point estimates are typically not statistically different than zero. Likewise, we only find evidence of increased educational attainment for men born in the counties where ordnance plants were sited. Moreover, these individuals fared better regardless of where they lived later in life. While we find that both kinds of plants increased the adult earnings of individuals who remained in their birth counties throughout adulthood in Appendix Table A.4, only the ordnance plants increased earnings for those who migrated elsewhere as well.

Strikingly, however, the general manufacturing plants had a dramatic effect on the composition of post-war residents. These plant sitings led to an influx of higher-earning workers born in other counties, suggesting that the large long-run effects of general manufacturing plants on local median family incomes in Table 6 in no small part reflect an inflow of positively-selected workers as those regions grew. Consistent with this explanation, we find that these plants significantly increased the educational attainment of the post-war population, despite not increasing attainment among pre-war residents. In sharp contrast, ordnance plant sitings had *no* effect on the earnings levels among those residing nearby in the CPS. This finding—that ordnance plants increase the long-run earnings of pre-war residents but not post-war residents—suggests that the positive effect on the earnings of long-term stayers is primarily an artifact of positive selection into staying rather than a causal effect of the plant siting. Taken together, our results imply that mechanisms by which incumbent residents benefited from plant sitings were largely disjoint from the mechanisms that drove the post-war increases in regional growth and incomes of contemporaneous residents observed in our county-level analysis.

9 Conclusion

This paper has examined the extent to which durable investments in productive capacity made as part of the World War II mobilization effort had local labor market effects that long outlived the war itself. We have argued that the highly idiosyncratic location and investment decisions concerning a specific subset of plants built during the war—large plants that were constructed in new locations with absolutely no private capital that were necessitated by short-run strategic concerns for the war—are the closest to a random “helicopter drop” of major industrial infrastructure improvements that has ever occurred in the Western world. This claim is backed up both by the qualitative history of the war and, more importantly, by robust absence of pre-trends across treatment and non-treatment counties. Using this “natural-experimental” setting, we test for the causal, long-run impacts of receiving a large industrial plant for idiosyncratic reasons.

We find that post-war manufacturing output, employment, and payroll in the recipient county are markedly higher in recipient counties than in similar, untreated counties. While a short-run increment to manufacturing activity is not surprising, we also find that these effects are remarkably persistent over

the course of five decades. These effects in the manufacturing sector carry over to aggregate labor-market outcomes, such as total employment and median wages. Yet, wages in other sectors in the recipient counties remain unaffected. We then test whether regional improvements in wages results in better earnings outcomes for specific individuals by studying the long-term earnings effects on workers based on where they resided before the war as children. We find that growing up in a locale where a large plant was constructed had an economically significant impact on men's adult wage incomes. These plants also increased upwards intergenerational household income mobility for children born to parents with below-median family incomes in 1939.

These findings highlight the large role that government-supported manufacturing expansions played in the spread of "good-paying" midcentury manufacturing jobs. Moreover, these findings highlight the role that access to high-paying manufacturing jobs played in fostering upward mobility during the middle of the 20th Century. However, whether these results imply that investments in World War II affected middle-class wage growth and upward income mobility in the *aggregate* depends on the channels by which county-level effects impacted other regions in general equilibrium. Even if externalities and market-mechanism effects were present that rationalized policy intervention from the perspective of local authorities, these plant openings may not have been net gains from a national perspective if they simply shifted activity away from another locality. From the perspective of the federal government, this would appear as near-perfect crowd-out. Any productivity spillovers and wage effects gained in the winning county are spillovers and wage increases lost in the county that was "crowded-out." This point about the general equilibrium consequences of place-based policies was examined in depth in (Kline and Moretti, 2014) who note that the gains in the "winning" county have to be disproportionately larger than the losses in the county that was "crowded-out" to justify public influence in where manufacturing investment takes place (for economic, not military-strategic reasons). Thus, beyond determining the form of local spillovers, an important goal for future work is to infer the extent to which investments would have been made elsewhere in the counterfactual scenario, in order to determine the aggregate economic benefit of local investments.

This work provides clear evidence that a one-time surge in public investment motivated by a crisis can have transformative effects on local economies and support "good"-paying jobs for decades—and, moreover, that military spending promoted upward mobility during the midcentury period. Nonetheless, it is important to note that these plants may have been successful in boosting regional labor markets not despite, but *because* they were built to overcome a very short-term crisis. The big plants of WWII were not built with an eye to maximize long-run efficiency and profitability, but rather to put every potential worker or resource at hand in the moment to work as effectively as possible. This imperative resulted in plants designed to put people in the community to work. Moreover, in order to ensure production would continue without interruption by strikes despite wartime wage freezes, wartime labor policies (particularly those of the National War Labor Board) facilitated rapid union expansion in war-related industries (Goldin and Margo, 1992; Wilson, 2016; Farber et al., 2018). By 1946 over 80% of production workers in the industries that participated in war production—including automobile, aircraft, shipbuilding, steel, and aluminum

manufacturing—were covered by union contracts (United States Bureau of Labor Statistics, 1947). Modern-day sitings of highly-automated plants in labor markets with little union presence may have different effects on work opportunity and earnings outcomes.³⁴ These considerations should be kept in mind when relating the findings of this research to evaluations of contemporary placed-based interventions.

³⁴For example, Patrick (2016) finds that modern “million-dollar plant” sitings increased productivity among firms in a region but *not* average wages.

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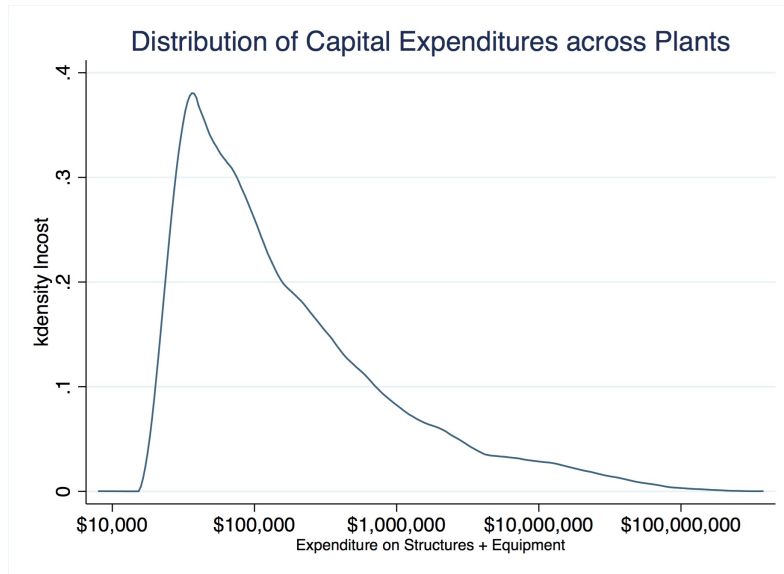
Wilson, Mark R., *Destructive Creation: American Business and the Winning of World War II*, Philadelphia: University of Pennsylvania Press, August 2016.

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—, *When Work Disappears: The World of the New Urban Poor*, 1st edition ed., New York, NY: Vintage, July 1997.

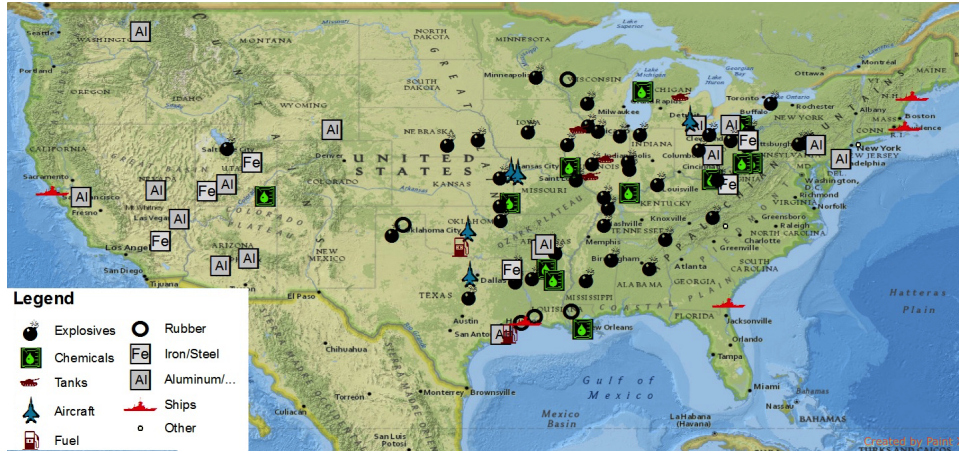
Figures and Tables

Figure 1: Distribution of War-Necessitated Capital Expenditures across Plants



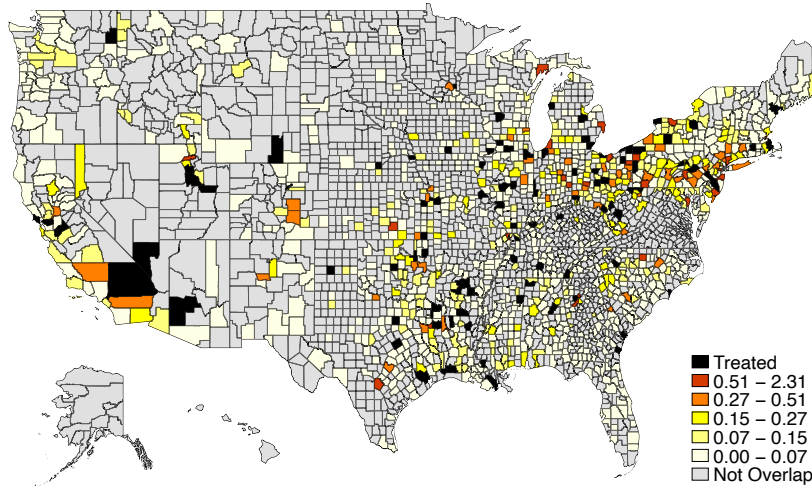
Notes: Figure is kernel density plot of the distribution of wartime all capital expenditures (structures and equipment, in logged 2014 dollars) across all establishments listed in WPB records.

Figure 2: Location of Large Publicly-Financed Plants



Notes: Map displays the largest new publicly-financed war plants, coded by primary product produced during the War.

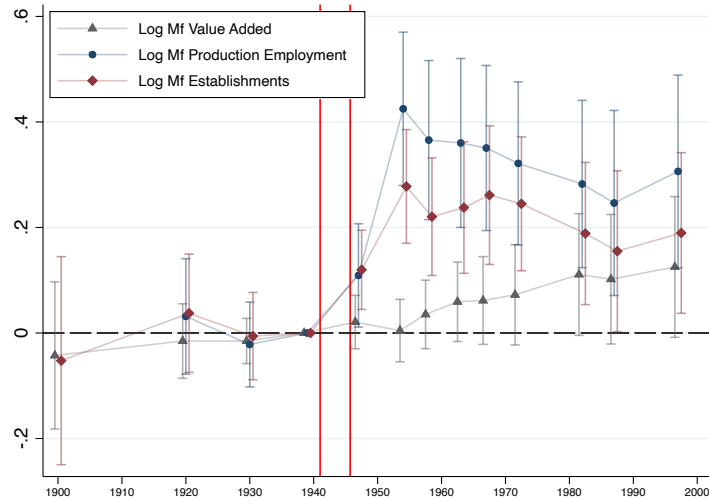
Figure 3: Propensity Score Weights of Control Counties



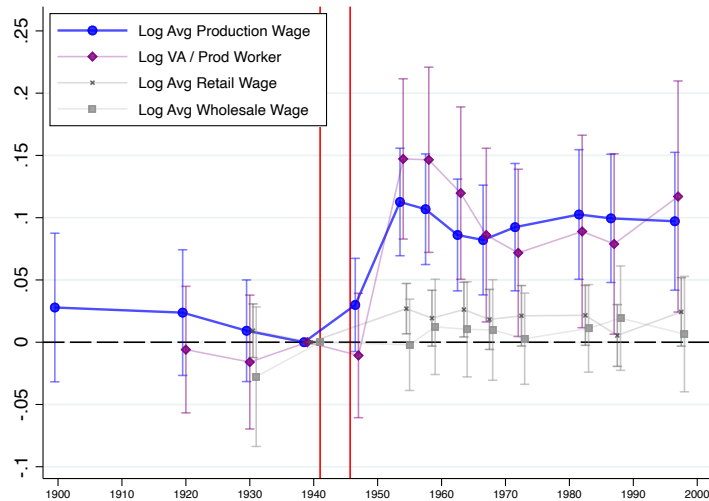
Notes: Map displays propensity score weights from baseline specification. All 101 treatment counties are presented in black. The 1,117 counties in the overlap sample are color-coded by quintile of the propensity score. Counties outside the overlap region or otherwise omitted from analysis sample are colored gray. Baseline set of conditioning variables is select logged 1930 variables—population, employment, manufacturing value added and establishments, manufacturing production worker employment and payroll, retail employment, agricultural land value, and the black, immigrant, and urban populations—as well as measures of New Deal and related spending from Fishback et al. (2005).

Figure 4: Effects on Local Manufacturing

(a) Establishments, Output, and Employment



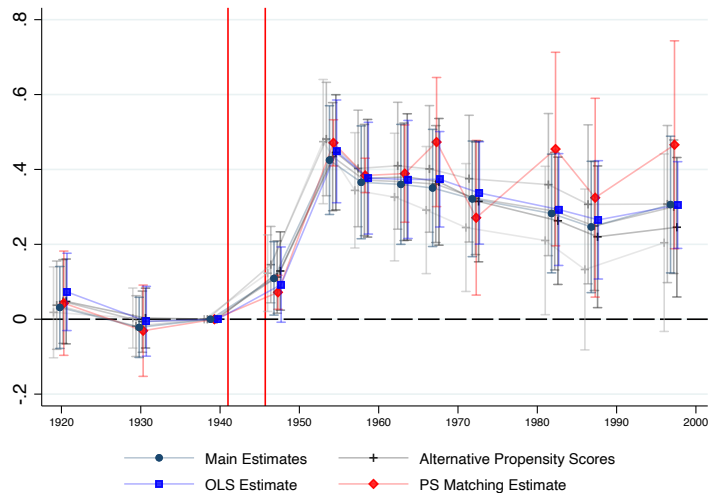
(b) Manufacturing Wages



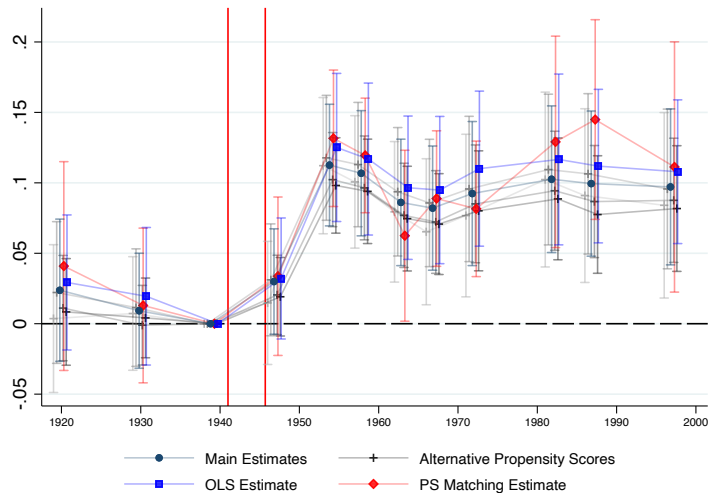
Notes: Figure displays propensity score re-weighted ATET estimates of plant sitings on outcomes differenced over 1939 levels. Outcomes are tabulations from establishment surveys. Propensity score is estimated using baseline covariates in Table 2. Sample is $N = 1,179$ counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. Each effect is estimated separately.

Figure 5: Robustness of Results to Specification

(a) Effect on Log Production Employment

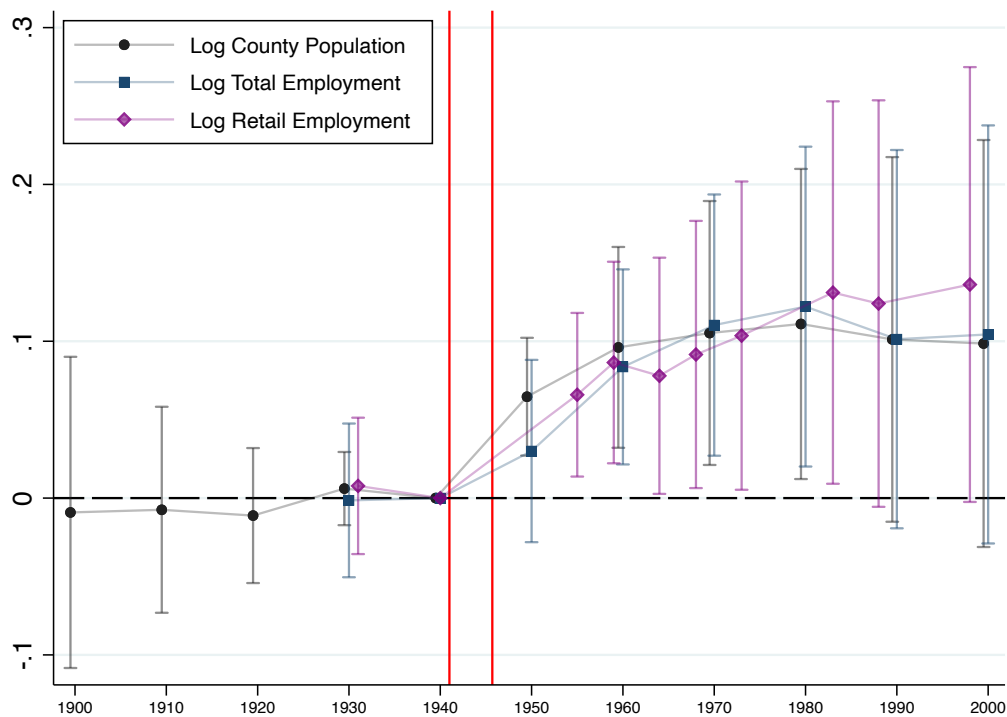


(b) Effect on Log Production Wage Bill Per Worker



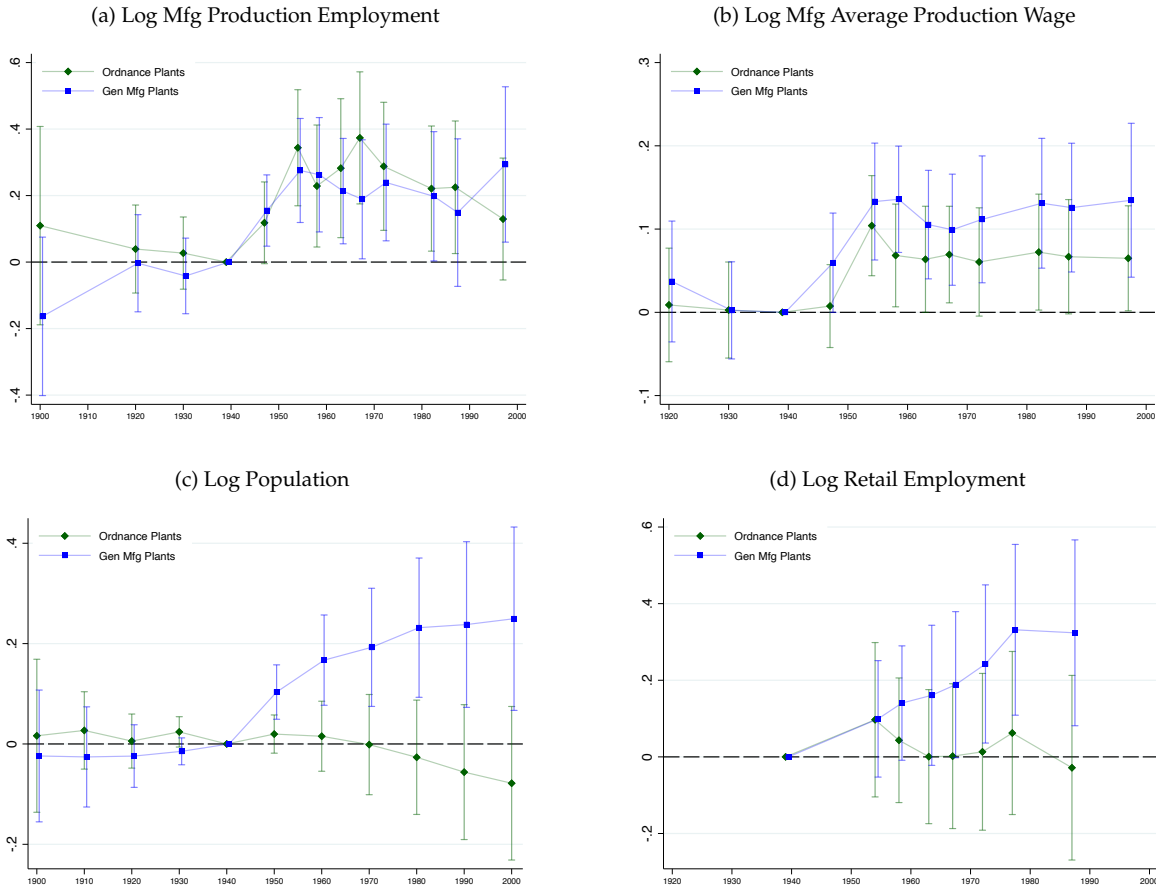
Notes: Figure displays estimates of ATET effects of the binary treatment on changes in county total log manufacturing value added and log average production wage from 1939 to each outcome year with different sets of conditioning variables and/or different estimators. The navy circles are the baseline re-weighting estimates. The blue squares and red circles display OLS and propensity-score nearest-neighbor matching estimates of the same treatment indicator on the same overlap sample. The gray plus (+) symbols are re-weighting estimates under four alternative specifications of the propensity score; darker shades of gray indicate a larger number of included covariates. These specifications include: 1) log population only, 2) population, employment, geographic features, rural/urban population, and access to power and water, 3) baseline specification plus 1940 median household income and housing value, and 4) the prior specification plus retail, service, and wholesale employment and pay rates. Sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. Each effect is estimated separately.

Figure 6: Broader Employment Outcomes: Reweighting Estimates



Notes: Figure displays propensity score re-weighted ATET estimates of plant sitings on outcomes differenced over 1940 levels. Propensity score is estimated using baseline covariates in Table 2. Sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. Outcomes are tabulations from population censuses. Each effect is estimated separately.

Figure 7: Heterogeneity by Plant Type



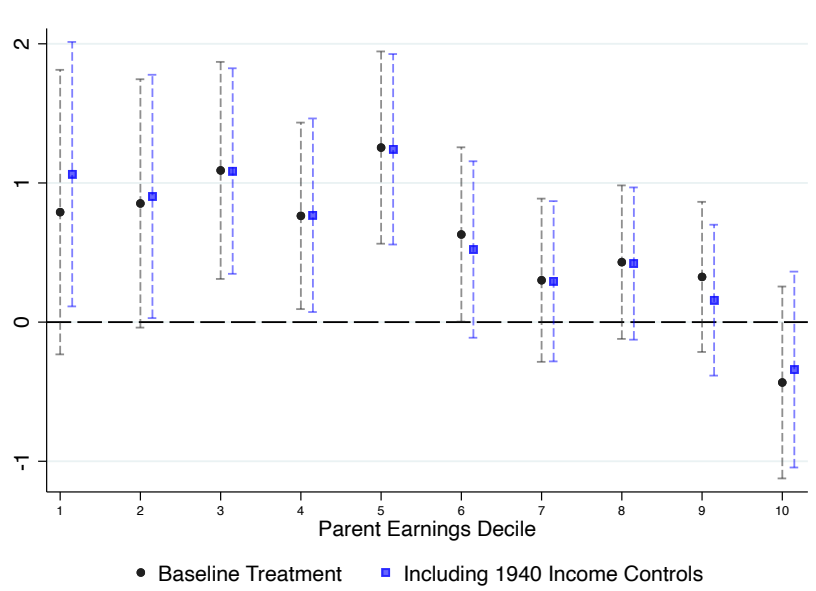
Notes: Panels plot propensity-score reweighting estimates of ATET effects of one of two binary treatment variables: 1) treated and largest plant is ordnance/ammunition facility (“Ordnance Plants”), and 2) treated and largest plant is other general-purpose manufacturing facility (“Other Plants”). For each of these treatments, separate propensity scores are estimated and reweighting estimates are calculated on corresponding overlap samples. There are 45 counties with the “Ordnance Plant” treatment, in the overlap sample there are N=1,108 counties, 40 of which are treated. There are 45 counties with the “Ordnance Plant” treatment, in the corresponding overlap sample there are N=1,108 total, 40 of which are treated. There are 55 counties with the “Other Plant” treatment, in the corresponding overlap sample there are N=1,168 counties, 52 of which are treated. All other details are as in Figure 4.

Figure 8: Effects on Adult Earnings of Men Born in County



Notes: Figure plots OLS estimates of specification in Equation 3 by outcome year. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT born in a county in the main analysis sample (N=37,000). Wages are defined for individuals with annual wage/salary earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year. Annual earnings are winsorized at the 99th percentile. Each estimate is from a separate regression.

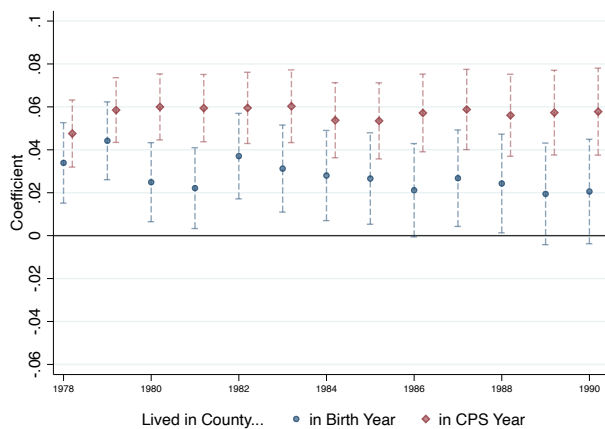
Figure 9: Effects on Mens' Adult Earnings Rank by Parent Earnings Decile



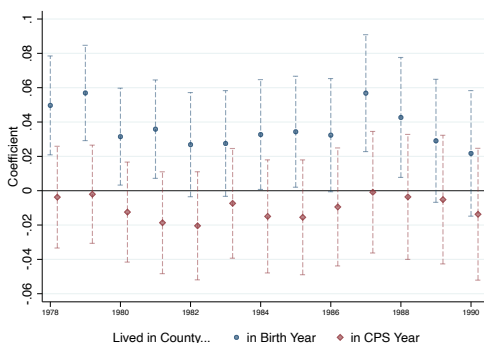
Notes: Figure reports propensity-score reweighting estimates of ATET effects on levels (not differenced) of the outcome variables, using the same county-level propensity score specifications in Table 3. Outcomes are county-level averages (weighted by match rates) of adult income ranks based on 1975 and 1979 AGI as calculated by Massey and Rothbaum (2020) among the specified parental-earnings decile. Each point estimate is calculated separately. Sample is 101 treatment counties and 1117 control counties in overlap region. Each estimate is from a separate regression.

Figure 10: Effects on Pre-War Residents and Post-War Composition

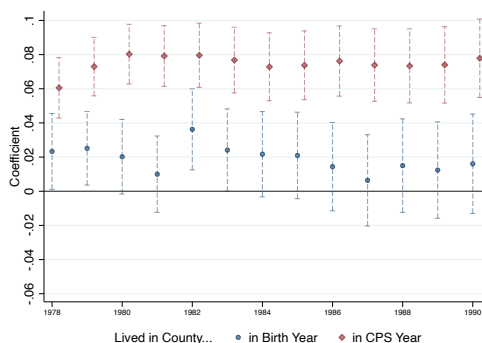
(a) Baseline



(b) Ordnance Plants



(c) General-Purpose Plants



Notes: Figure displays the effects of plant sitings on the average earnings in the specified year of individuals, with treatment assigned either based on their place of birth or their place of residence in their CPS-ASEC year. Each estimate is from a separate OLS regression of individual earnings on the specified treatment variable, year-of-birth indicators, and the baseline 1940 covariate set. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT; N = 37,000 such individuals have birth counties in the analysis sample and N = 34,500 have CPS-year residences in counties contained in the analysis sample. Wages are defined for individuals with annual wage/salary earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year. Annual earnings are winsorized at the 99th percentile.

Table 1: Summary of Wartime Industrial Capital Spending

(a) Summary of Plants

Category	Cost (Million \$1940)	Cost (Million \$2014)	# Establishments
All Cap Expenditures	\$20,597	\$299,274	12,906
Matched to County	\$19,916	\$289,379	11,738
New Plants	\$10,904	\$158,435	5,508
No Private Capital	\$7,002	\$101,739	535
Cost > \$1 Million	\$6,945	\$100,910	341
In Treatment County	\$4,735	\$68,774	159

(b) Big New Plant Spending in Treatment Counties

	Mean		Median	
	(Million \$1940)	(Million \$2014)	(Million \$1940)	(Million \$2014)
Total Spending	\$46,884,000	\$680,936,000	\$33,790,000	\$490,755,200
Per Capita Spending	\$823	\$11,959	\$423	\$6,141

Table 2: Covariate Balance

1940 Covariates in Propensity Score					Covariates Excluded from Propensity Score				
Variable	Unadj Diff	t-stat	RW Diff	t-stat	Variable	Unadj Diff	t-stat	RW Diff	t-stat
Log Population	0.686	8.734**	0.021	0.705	1940 Log Median Household Incomr	0.284	7.715**	0.002	0.130
Log Employment	0.704	7.948**	0.036	0.860	1940 Log Median Housing Value	0.252	5.911**	-0.035	-1.077
Share of Housing Units w/ Electricity	0.149	6.981**	0.001	0.125	1940 Births Per Capita	-0.001	-2.094*	-0.000	-0.469
Share of Rural H Units w/ Electricity	0.137	4.770**	0.000	0.022	1939 Retail Sales Per Capita	123.098	4.416**	-16.808	-1.147
Share of Rural H Units w/ Water	0.078	3.281**	0.005	0.573	1939 Log Retail Establishments	0.718	8.996**	0.007	0.196
Log Mfg Establishments	0.815	7.938**	0.025	0.680	1939 Log Retail Employment	0.946	8.433**	-0.004	-0.074
Log 1939 Mfg Production Emp	1.137	8.269**	0.010	0.207	1939 Log Retail Average Wage	0.090	5.016**	-0.020	-1.879*
Log 1939 Mfg Value Added	1.352	9.969**	0.041	0.767	1939 Log Service Establishments	0.759	8.533**	0.012	0.274
Log Black Pop	0.964	3.376**	0.070	0.795	1939 Log Service Employment	0.922	6.784**	-0.020	-0.285
Log Urban Pop	1.393	11.262*	0.029	0.657	1939 Log Service Average Wage	0.151	6.119**	-0.019	-1.061
Log Foreign Born Pop	1.377	7.185**	0.001	0.021	Log County Land Area	-0.064	-0.875	0.019	0.273
Log PC 1930's AAA Grants	-0.861	-5.770*	0.030	0.507	1940 Log # Male Craftspersons	1.002	9.982**	0.037	0.930
Log 1930s WPC/WPA Pub Wks \$	0.223	4.000**	-0.005	-0.245	1940 Log # Female Craftspersons	1.065	7.728**	-0.007	-0.120
Log 1937 Unemployed Men	0.800	9.807**	0.028	0.826	1940 Log # Male Operatives	0.968	9.762**	0.030	0.653
Log 1937 Unemployed Women	0.806	8.777**	0.035	0.903	1940 Log # Female Operatives	1.010	7.075**	-0.021	-0.311
Share of Pop 25+ w/ HS Degree	0.032	4.160**	0.005	1.459					
Share of Pop on Farms	-0.146	-8.285*	-0.003	-0.545					
<i>N treated in overlap sample</i>	94								
<i>N control in overlap sample</i>	1085								
<i>Sum of control weight</i>	95.6								

Notes: Sample is subset of 101 treatment counties and comparison counties in main sample with propensity scores contained in the overlapping interval. The propensity score is estimated using the variables in the left-hand column, plus the indicators for presence of rivers, swamps, lakes, bays, beaches, external coasts, and great lakes coasts. ** indicates $p < .05$, * indicates $p < .10$.

Table 3: Difference in Differences Estimates of Effects on Manufacturing Outcomes

	<i>PSWT Binary Treatment Effect</i>				<i>OLS Effect of \$1000 (2014\$) Per Capita</i>			
	1963 Effect (1)	1982 Effect (2)	1963 Effect (3)	1982 Effect (4)	1963 Effect (5)	1982 Effect (6)	1963 Effect (7)	1982 Effect (8)
Outcome:								
Log Mfg Value Added	0.360** (0.082)	0.282** (0.081)	0.380** (0.086)	0.263** (0.087)	0.031** (0.008)	0.027** (0.007)	0.031** (0.008)	0.027** (0.007)
Log Mfg Establishments	0.059 (0.038)	0.111* (0.059)	0.058 (0.043)	0.096 (0.064)	0.005* (0.003)	0.011** (0.004)	0.005 (0.003)	0.011** (0.005)
Log Mfg Production Payroll	0.327** (0.072)	0.296** (0.076)	0.328** (0.074)	0.255** (0.078)	0.024** (0.008)	0.025** (0.007)	0.025** (0.007)	0.026** (0.007)
Log Mfg Production Employment	0.238** (0.064)	0.189** (0.069)	0.250** (0.066)	0.159** (0.072)	0.018** (0.005)	0.019** (0.006)	0.018** (0.005)	0.018** (0.006)
Log Mfg Production Avg Wage	0.086** (0.023)	0.103** (0.027)	0.075** (0.019)	0.089** (0.022)	0.005* (0.003)	0.006** (0.003)	0.006** (0.002)	0.007** (0.002)
Log Mfg VA Per Prod. Employee	0.120** (0.035)	0.089** (0.039)	0.126** (0.038)	0.096** (0.040)	0.012** (0.003)	0.008* (0.004)	0.012** (0.003)	0.008** (0.004)
Log Mfg Prod Labor Share of VA	-0.034 (0.031)	0.014 (0.037)	-0.051* (0.027)	-0.008 (0.036)	-0.007* (0.002)	-0.002 (0.003)	-0.006* (0.001)	-0.001 (0.004)
Baseline Covariates	x	x	x	x	x	x	x	x
1940 Industry / Income Covariates			x	x			x	x

Notes: Table displays propensity score re-weighted ATET estimates of plant sitings and OLS estimates of effects of per capita investment in new public plants on outcomes differenced over 1939 levels. Outcomes are tabulations from establishment surveys. Baseline propensity score is estimated using baseline covariates in Table 2. Baseline sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. "1940 Industry / Income Covariates" adds the following variables to the propensity score: logged 1940 median household income and housing value, and logged 1939 average wages and employment in each of the retail, service, and wholesale sectors. In these specifications, propensity scores are re-estimated on the full analysis sample with additional covariates and the overlap region is constructed accordingly. OLS estimates use the same covariates as linear controls, and are run on the full analysis sample including all 202 counties with any public plant spending. Each effect is estimated separately. ** indicates p < .05, * indicates p < .10 .

Table 4: Difference in Differences Estimates of Effects on Broader Employment

	<i>PSWT Binary Treatment Effect</i>				<i>OLS Effect of \$1000 (2014\$) Per Capita</i>			
	1960 Effect (1)	1980 Effect (2)	1960 Effect (3)	1980 Effect (4)	1960 Effect (5)	1980 Effect (6)	1960 Effect (7)	1980 Effect (8)
<i>Outcome:</i>								
Log Population	0.096** (0.033)	0.111** (0.050)	0.107** (0.035)	0.122** (0.055)	0.008** (0.003)	0.011** (0.003)	0.009** (0.003)	0.011** (0.003)
Log Total Employment	0.084** (0.032)	0.122** (0.052)	0.105** (0.035)	0.140** (0.057)	0.009** (0.003)	0.013** (0.004)	0.009** (0.003)	0.013** (0.004)
Mfg Share of Employment	0.030** (0.010)	0.030** (0.011)	0.023** (0.008)	0.022** (0.009)	0.002** (0.000)	0.001** (0.001)	0.002** (0.000)	0.001** (0.001)
Log Retail Employment	0.078** (0.038)	0.131** (0.062)	0.073* (0.040)	0.110* (0.063)	0.009** (0.003)	0.013** (0.004)	0.009** (0.003)	0.013** (0.004)
Log Avg Retail Wage	0.026** (0.011)	0.022* (0.012)	0.008 (0.008)	-0.002 (0.010)	0.001* (0.001)	0.001 (0.001)	0.000 (0.001)	-0.000 (0.001)
Log Median Family Income	0.033** (0.013)	0.040** (0.014)	0.024** (0.011)	0.022* (0.013)	0.004** (0.002)	0.003** (0.001)	0.003** (0.001)	0.003** (0.001)
Log Median Housing Value	0.042 (0.027)	0.056* (0.034)	0.016 (0.022)	0.011 (0.030)	0.003 (0.002)	0.003 (0.002)	0.003** (0.001)	0.003** (0.001)
Baseline Covariates	x	x	x	x	x	x	x	x
1940 Industry / Income Covariates			x	x			x	x

Notes: Table displays propensity score re-weighted ATET estimates of plant sitings and OLS estimates of effects of per capita investment in new public plants on outcomes differenced over 1940 levels. Outcomes are tabulations from establishment surveys. Baseline propensity score is estimated using baseline covariates in Table 2. Baseline sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. "1940 Industry / Income Covariates" adds the following variables to the propensity score: logged 1940 median household income and housing value, and logged 1939 average wages and employment in each of the retail, service, and wholesale sectors. In these specifications, propensity scores are re-estimated on the full analysis sample with additional covariates and the overlap region is constructed accordingly. OLS estimates use the same covariates as linear controls, and are run on the full analysis sample including all 202 counties with any public plant spending. Log median family income for each postwar year is differenced over author-tabulated median 1939 household income in the 1940 census public-use full-count microdata as a proxy. Retail wages are from 1939, 1963, and 1982. Each effect is estimated separately. ** indicates p < .05, * indicates p < .10.

Table 5: Spillovers to Adjacent Counties

	<i>Treated Counties</i>		<i>Adjacent Counties</i>	
	Med Run Effect (1)	Long Run Effect (2)	Med Run Effect (3)	Long Run Effect (4)
Log Mfg Esablishments	0.059 (0.038)	0.111* (0.059)	0.010 (0.026)	0.066* (0.036)
Log Mfg Production Employment	0.238** (0.064)	0.189** (0.069)	-0.014 (0.043)	0.033 (0.049)
Log Mfg Production Avg Wage	0.086** (0.023)	0.103** (0.027)	-0.019 (0.015)	-0.017 (0.019)
Log Population	0.096** (0.033)	0.111** (0.050)	0.024 (0.017)	0.082** (0.028)
Log Total Employment	0.084** (0.032)	0.122** (0.052)	0.024 (0.017)	0.087** (0.030)
Mfg Share of Employment	0.030** (0.010)	0.030** (0.011)	0.017** (0.004)	0.016** (0.005)
Log Median Family Income	0.033** (0.013)	0.040** (0.014)	0.025** (0.011)	0.038** (0.012)
Log Median Housing Value	0.042 (0.027)	0.056* (0.034)	0.040** (0.018)	0.075** (0.023)

Notes: Table displays propensity score re-weighted ATET estimates of plant sitings as well as ATET effects of being adjacent to a county where a plant was sited. In adjacency specifications, we use the covariates in the baseline propensity score model to estimate a new propensity score on an adjacency indicator on the main sample, where the 202 counties with public plant spending are omitted from the comparison group. There are 352 counties in the main sample that are adjacent to treatment counties; in the propensity score overlap sample there are N = 1,653 counties total, 336 of which are adjacent to treatment counties. Log median family income for each postwar year is differenced over author-tabulated median 1939 household income in the 1940 census public-use full-count microdata as a proxy. ** indicates $p < .05$, * indicates $p < .10$.

Table 6: Effects on Postwar Defense Contracts

Panel A: Vietnam War Spending Effects

	<i>Ordnance Plant</i>	<i>General Manufacturing Plant</i>
	(1)	(2)
Log Defense Contracts 1966-1970	1.318** (0.375)	0.142 (0.260)
Log Ratio to 1967 Mfg VA	0.871** (0.358)	-0.156 (0.215)
Log Defense Contracts 1971-1975	1.034** (0.357)	0.170 (0.246)
Log Ratio to 1972 Mfg VA	0.699* (0.363)	-0.013 (0.217)

Panel B: Manufacturing and Total Employment Effects

	<i>Ordnance Plant</i>		<i>General Manufacturing Plant</i>	
	Med Run Effect	Long Run Effect	Med Run Effect	Long Run Effect
	(1)	(2)	(3)	(4)
Log Mfg Esablistments	0.054 (0.047)	0.016 (0.069)	0.056 (0.060)	0.189** (0.087)
Log Mfg Production Employment	0.282** (0.107)	0.221** (0.096)	0.213** (0.081)	0.198** (0.099)
Log Mfg Production Avg Wage	0.064* (0.033)	0.072** (0.036)	0.105** (0.033)	0.131** (0.040)
Log Population	0.015 (0.036)	-0.027 (0.058)	0.167** (0.046)	0.232** (0.071)
Log Total Employment	0.024 (0.034)	0.002 (0.062)	0.151** (0.045)	0.249** (0.075)
Mfg Share of Employment	0.038** (0.011)	0.046** (0.012)	0.021** (0.009)	0.013 (0.012)
Log Median Family Income	0.036* (0.019)	0.041** (0.019)	0.053** (0.024)	0.060** (0.022)
Log Median Housing Value	0.018 (0.041)	0.016 (0.048)	0.083** (0.034)	0.113** (0.046)

Notes: Panels plot propensity-score reweighting estimates of ATET effects of one of two binary treatment variables: 1) treated and largest plant is ordnance/ammunition facility (“Ordnance Plants”), and 2) treated and largest plant is other general-purpose manufacturing facility (“Other Plants”). For each of these treatments, separate propensity scores are estimated and reweighting estimates are calculated on corresponding overlap samples. There are 45 counties with the “Ordnance Plant” treatment, in the overlap sample there are N=1,108 counties, 40 of which are treated. There are 45 counties with the “Ordnance Plant” treatment, in the corresponding overlap sample there are N=1,108 total, 40 of which are treated. There are 55 counties with the “Other Plant” treatment, in the corresponding overlap sample there are N=1,168 counties, 52 of which are treated. Panel A displays effects on aggregates of Defense Department prime contracts awarded to firms based in each county; outcomes are in levels. Panel B displays effects on outcomes from Tables 3 and 4 differenced over 1939 or 1940 levels as specified in those tables; see notes to those tables for additional details. “M. Run” denotes postwar year is 1960 or 1963 as specified in prior tables, “L. Run” denotes postwar year is 1980 or 1982 as specified in prior tables. ** indicates p < .05, * indicates p < .10 .

Table 7: Comparison of Difference-in-Differences and Post-Period-Difference Estimates of Earnings Effects

<i>Outcome:</i>	<i>Estimator</i>			
	PPD (1)	DD (2)	PPD (3)	DD (4)
1982 Production Wage	0.086** (0.021)	0.090** (0.025)	0.102** (0.021)	0.079** (0.021)
1982 Retail Wage	-0.002 (0.010)	0.018 (0.013)	0.007 (0.010)	-0.002 (0.011)
1980 Median Family Income	0.042** (0.011)	0.040** (0.014)	0.040** (0.011)	0.022* (0.013)
1980 Median Male FT Earnings	0.046** (0.010)		0.046** (0.010)	
1980 Median Female FT Earnings	0.010 (0.008)		0.015* (0.008)	
1980 Shr Fam Below Pov Level	-0.008* (0.003)		-0.007* (0.003)	
1970 Avg Income, Male Operators	0.041** (0.010)		0.051** (0.010)	
1970 Avg Income, Male Craftspers.	0.046** (0.010)		0.054** (0.010)	
Baseline Covariates	x	x	x	x
1940 Industy / Income Covariates			x	x

Notes: Table reports propensity-score reweighting estimates of ATET effects using the specifications described in 3. “PPD” post-period difference specifications present effects on the outcome variables in levels, “DD” difference-in-differences specification examine effects on outcomes differenced over 1939 levels when available. In DD specifications, log median family income for is differenced over author-tabulated median 1939 household income in the 1940 census public-use full-count microdata as a proxy. ** indicates $p < .05$, * indicates $p < .10$.

Table 8: Effects on Adult Earnings by Demographic Group

	<i>Men</i>					<i>Women</i>		
	<i>All</i>	<i>Black</i>	<i>White</i>	<i>Stayers</i>	<i>Movers</i>	<i>All</i>	<i>Black</i>	<i>White</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log 1978 Wage	0.034** (0.010)	0.015 (0.033)	0.032** (0.010)	0.080** (0.016)	0.017 (0.012)	-0.002 (0.012)	-0.001 (0.034)	0.002 (0.013)
<i>N</i>	30,000	2,900	27,000	8,800	21,500	15,500	2,600	13,000
HS Degree in CPS	0.012 (0.007)	0.056* (0.030)	0.002 (0.007)	0.019 (0.014)	0.010 (0.008)	0.003 (0.007)	0.005 (0.025)	-0.004 (0.007)
<i>N</i>	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Any Clg in CPS	0.011 (0.009)	0.052* (0.027)	0.002 (0.009)	0.046** (0.014)	0.006 (0.011)	0.004 (0.008)	0.021 (0.023)	-0.002 (0.008)
<i>N</i>	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Stays in Birth Cty	0.037** (0.008)	0.052** (0.026)	0.036** (0.009)	-	-	0.027** (0.007)	0.007 (0.023)	0.029** (0.008)
<i>N</i>	37,000	4,000	32,500			43,500	5,900	37,000

Notes: Table reports OLS estimates of specification in Equation 3. Each estimate is from a separate regression of individual earnings on the specified treatment variable, year-of-birth indicators, and the baseline 1940 covariate set. Sample is all individuals born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT. Wages are wage and salary earnings reported on forms W-2 and are defined for individuals with annual earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year; as a result N is smaller for this outcome. Wage in dollar levels is in CPI-adjusted 2017 dollars. Annual earnings are winsorized at the 99th percentile. Each estimate is from a separate regression.

Table 9: Effects on Pre-War Residents and Post-War Composition

	1978 Wage/Salary Earnings			CPS Year Characteristics			1980 Med Housing
	Logs	2016 \$	Earnings > MW	HS grad	Any College	In Birth County	Value in CPS County
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Treatment Plant in...							
Birth County	0.033** (0.010)	2,067** (735.1)	0.009 (0.007)	0.012 (0.007)	0.011 (0.009)	0.037** (0.008)	-0.008 (0.006)
<i>N individuals</i>	30,000	30,000	37,000	37,000	37,000	37,000	33,000
Adult CPS County	0.048** (0.008)	2,736** (621.3)	0.008 (0.006)	0.020** (0.006)	0.036** (0.007)	-0.019** (0.007)	0.104** (0.005)
<i>N individuals</i>	28,000	28,000	34,500	34,500	34,500	34,500	34,500
B. Ordnance Plant in...							
Birth County	0.050** (0.015)	3,635** (1,129)	-0.012 (0.011)	0.026** (0.011)	0.026** (0.013)	0.026** (0.013)	0.015* (0.009)
<i>N individuals</i>	30,000	30,000	37,000	37,000	37,000	37,000	33,000
Adult CPS County	-0.004 (0.015)	-936.4 (1,178)	0.005 (0.011)	0.012 (0.011)	0.004 (0.014)	0.064** (0.013)	-0.007 (0.009)
<i>N individuals</i>	28,000	28,000	34,500	34,500	34,500	34,500	34,500
B. Other Plant in...							
Birth County	0.023** (0.011)	1,194 (871.5)	0.020** (0.008)	-0.0001 (0.009)	0.002 (0.010)	0.026** (0.010)	-0.018** (0.007)
<i>N individuals</i>	30,000	30,000	37,000	37,000	37,000	37,000	33,000
Adult CPS County	0.061** (0.009)	3,776** (705.5)	0.007 (0.007)	0.021** (0.007)	0.043** (0.008)	-0.056** (0.008)	0.142** (0.005)
<i>N individuals</i>	28,000	28,000	34,500	34,500	34,500	34,500	34,500

Notes: Table displays the effects of plant sitings on individuals in the CPS-DER sample, with treatment assigned either based on their place of birth or their place of residence in their CPS-ASEC year. Each estimate is from a separate OLS regression of individual earnings on the specified treatment variable, year-of-birth indicators, and the baseline 1940 covariate set, see Equation (3) in the paper. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT; N = 30,000 such individuals have birth counties in the analysis sample and N = 28,000 have CPS-year residences in counties contained in the analysis sample. Wages are defined for individuals with annual wage/salary earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year. Annual earnings are winsorized at the 99th percentile.

Table 10: Effects on Upward Mobility

	<i>Benchmark</i>		<i>Ordnance Plant</i>		<i>General Manufacturing</i>	
	<i>OLS</i>	<i>PSWT</i>	<i>OLS</i>	<i>PSWT</i>	<i>OLS</i>	<i>PSWT</i>
	(1)	(2)	(3)	(4)	(5)	(6)
<i>1940 Parent Income Rank</i>			A. Men			
Below Median	1.046** (0.331)	0.900** (0.338)	1.004** (0.397)	1.224** (0.457)	1.113** (0.466)	0.354 (0.526)
Above Median	0.496* (0.254)	0.458** (0.231)	0.548* (0.299)	0.438 (0.371)	0.600* (0.343)	0.224 (0.362)
<i>1940 Parent Income Rank</i>			B. Women			
Below Median	0.919** (0.313)	0.745** (0.354)	0.941** (0.420)	1.162** (0.480)	0.852* (0.437)	0.123 (0.518)
Above Median	0.667** (0.224)	0.626** (0.210)	0.886** (0.286)	0.844** (0.319)	0.603** (0.285)	0.260 (0.318)
N	1172	1172	715	715	1161	1161

Notes: Table reports propensity-score reweighting estimates of OLS and propensity score reweighted ATET estimates of effects on levels (not differenced) of the outcome variables. Outcomes are county-level averages (weighted by match rates) of adult income ranks based on 1975 and 1979 AGI as calculated by Massey and Rothbaum (2020) among the specified demographic group. N indicates number of counties in analysis sample with disclosable outcomes and available covariates.

Appendix A: Supplemental Tales and Figures

Figure A.1: County-Level Distribution of Spending on Large Publicly-Financed Plants

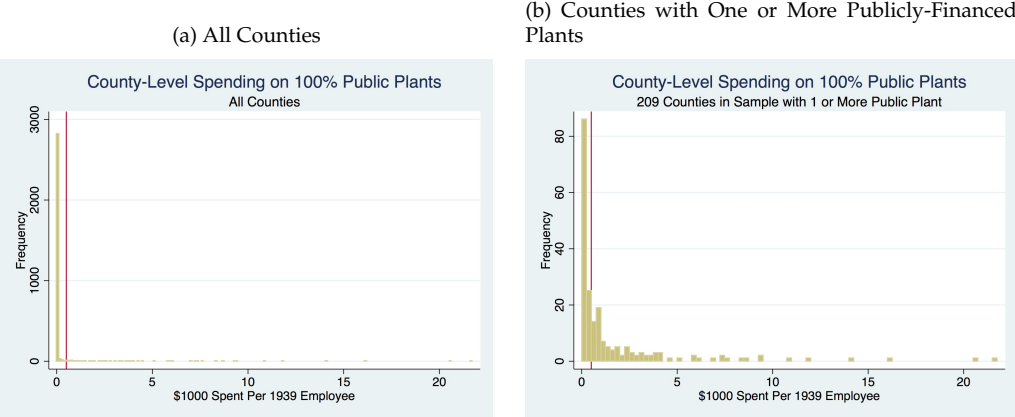
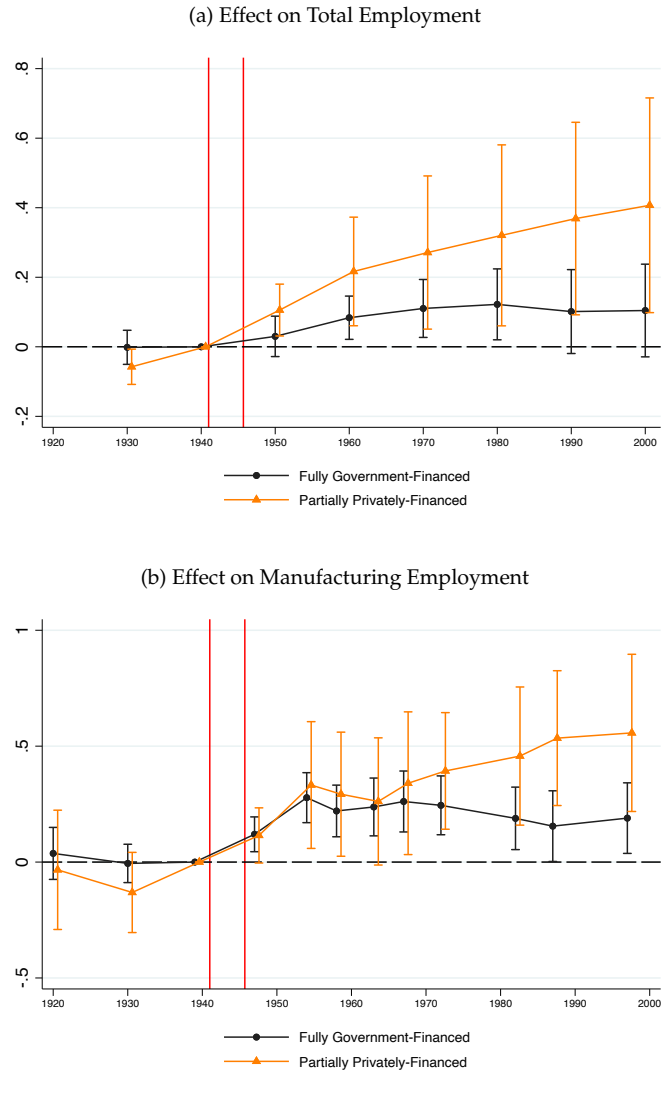


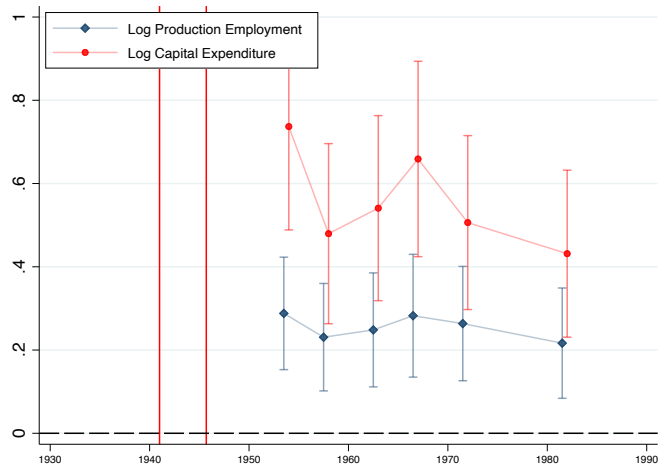
Figure A.2: Comparison: Effect of Big New Plants with Private Financing



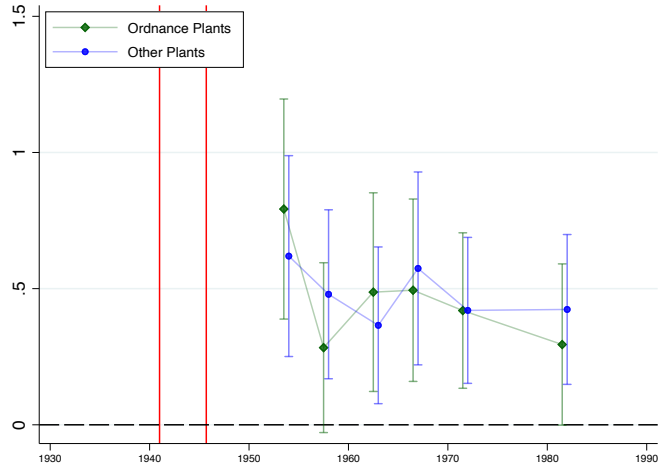
Notes: Figure presents estimation of specification identical to that in Panel A of 4, but binary treatment defined by presence of large, new plant with at least some private financing, using fully analogous definition, and propensity score is re-estimated accordingly. Treatment counties from baseline analysis are excluded from control set. Blue line reproduces select estimates from Panel A of Figure 4, see notes for details.

Figure A.3: Effects on Postwar Capital Expenditures

(a) PSWT Treat-Control Differences in Post-Period



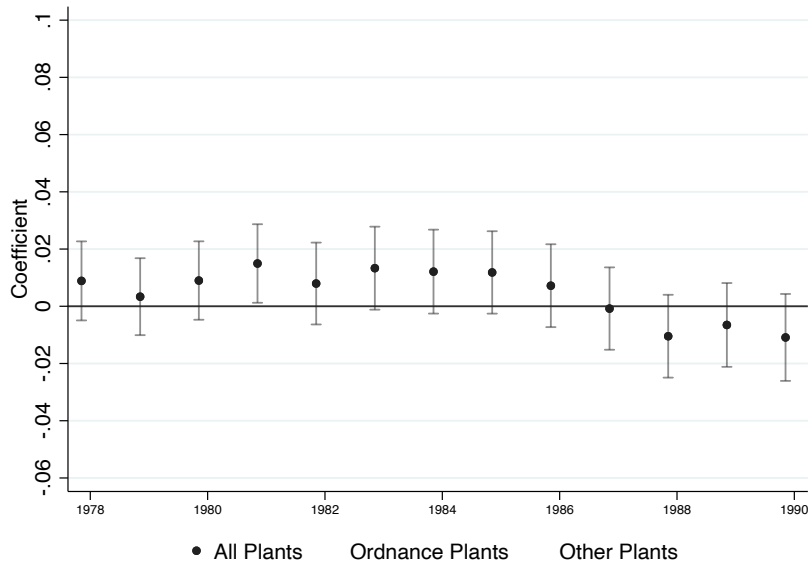
(b) By Plant Type



Notes: Figure displays estimates of ATET effects of the binary treatment on levels (not changes) of outcome variables. All other details are as in Figure 7.

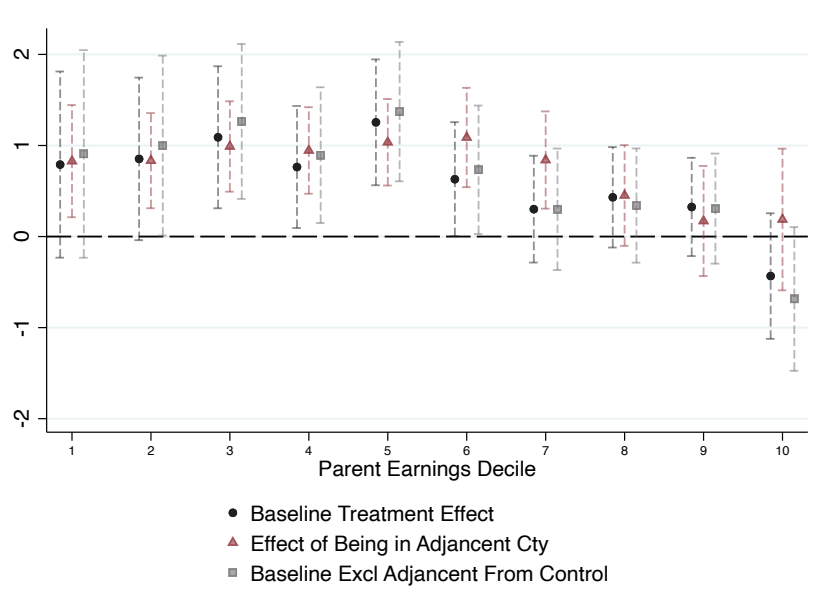
Figure A.4: Extensive Margin Effect, Men Born in County

Outcome: 1 (Has Earnings > Full-time Minimum Wage)



Notes: Figure plots OLS estimates of specification in Equation 3 by outcome year. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT born in a county in the main analysis sample (N=30,000). Outcome is an indicator for whether individuals' annual wage/salary earnings exceeded the equivalent of 2,000 hours work at federal minimum wage in the corresponding year. Each estimate is from a separate regression.

Figure A.5: Effects on Mens' Adult Earnings Rank by Parent Earnings Decile



Notes: Table reports estimates from Figure 9 as well as estimates of spillovers to adjacent counties using the estimator described in Table 5 and the outcomes from Figure 9.

Table A.1: Effects on Manufacturing Outcomes: Alternative Estimates

	PS Re-Weighting				OLS		PS Match	
	<i>Binary Treatment Effect</i>		<i>Excluding Adjacent Counties</i>		<i>Binary Treatment Effect</i>		<i>Binary Treatment Effect</i>	
	1963	1982	1963	1982	1963	1982	1963	1982
	Effect	Effect	Effect	Effect	Effect	Effect	Effect	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:								
Log Mfg Value Added	0.360** (0.082)	0.282** (0.081)	0.369** (0.093)	0.286** (0.092)	0.373** (0.080)	0.293** (0.076)	0.389** (0.066)	0.454** (0.132)
Log Mfg Esablishments	0.059 (0.038)	0.111* (0.059)	0.068 (0.044)	0.124* (0.067)	0.051 (0.035)	0.086* (0.052)	0.081** (0.040)	0.103 (0.071)
Log Mfg Production Payroll	0.327** (0.072)	0.296** (0.076)	0.334** (0.081)	0.291** (0.085)	0.332** (0.072)	0.290** (0.075)	0.288** (0.076)	0.397** (0.118)
Log Mfg Production Employment	0.238** (0.064)	0.189** (0.069)	0.247** (0.072)	0.180** (0.077)	0.232** (0.058)	0.164** (0.063)	0.284** (0.093)	0.176 (0.134)
Log Mfg Production Avg Wage	0.086** (0.023)	0.103** (0.027)	0.083** (0.025)	0.103** (0.029)	0.097** (0.026)	0.117** (0.031)	0.062** (0.031)	0.129** (0.038)
Log Mfg VA Per Prod. Employee	0.120** (0.035)	0.089** (0.039)	0.118** (0.039)	0.098** (0.044)	0.138** (0.038)	0.120** (0.042)	0.164** (0.045)	0.185** (0.062)
Log Mfg Prod Labor Share of VA	-0.034 (0.031)	0.014 (0.037)	-0.034 (0.034)	0.005 (0.042)	-0.041 (0.034)	-0.003 (0.037)	-0.101* (0.043)	-0.058 (0.046)

Notes: Table displays alternative estimates of effects of per capita investment in new public plants on outcomes differenced over 1939 levels. Outcomes are tabulations from establishment surveys. Baseline propensity score is estimated using baseline covariates in Table 2. Baseline sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. "1940 Industry / Income Covariates" adds the following variables to the propensity score: logged 1940 median household income and housing value, and logged 1939 average wages and employment in each of the retail, service, and wholesale sectors. In these specifications, propensity scores are re-estimated on the full analysis sample with additional covariates and the overlap region is constructed accordingly. ** indicates $p < .05$, * indicates $p < .10$.

Table A.2: Effects on Broader Employment: Alternative Estimates

	PS Re-Weighting				OLS		PS Match	
	Binary Treatment		Excluding Adjacent		Binary Treatment		Binary Treatment	
	Effect		Counties		Effect		Effect	
	1963	1982	1963	1982	1963	1982	1963	1982
	Effect	Effect	Effect	Effect	Effect	Effect	Effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:								
Log Population	0.096** (0.033)	0.111** (0.050)	0.099** (0.038)	0.121** (0.057)	0.079** (0.027)	0.078** (0.038)	0.105** (0.031)	0.127** (0.048)
Log Total Employment	0.084** (0.032)	0.122** (0.052)	0.089** (0.037)	0.133** (0.059)	0.082** (0.026)	0.102** (0.040)	0.104** (0.033)	0.170** (0.053)
Mfg Share of Employment	0.030** (0.010)	0.030** (0.011)	0.037** (0.012)	0.039** (0.013)	0.025** (0.005)	0.026** (0.008)	0.038** (0.003)	0.044** (0.013)
Log Retail Employment	0.078** (0.038)	0.131** (0.062)	0.076* (0.044)	0.134* (0.070)	0.066* (0.034)	0.094** (0.047)	0.052 (0.041)	0.115* (0.061)
Log Avg Retail Wage	0.026** (0.011)	0.022* (0.012)	0.027** (0.013)	0.021 (0.014)	0.028** (0.009)	0.019** (0.009)	0.023** (0.006)	0.016 (0.021)
Log Median Family Income	0.033** (0.013)	0.040** (0.014)	0.030** (0.015)	0.035** (0.016)	0.033** (0.012)	0.043** (0.014)	0.041 (0.026)	0.063** (0.031)
Log Median Housing Value	0.042 (0.027)	0.056* (0.034)	0.044 (0.029)	0.069* (0.035)	0.031 (0.027)	0.036 (0.032)	-0.007 (0.035)	0.014 (0.046)

Notes: Table displays alternative estimates of effects of per capita investment in new public plants on outcomes differenced over 1940 levels. Outcomes are tabulations from establishment surveys. Baseline propensity score is estimated using baseline covariates in Table 2. Baseline sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. "1940 Industry / Income Covariates" adds the following variables to the propensity score: logged 1940 median household income and housing value, and logged 1939 average wages and employment in each of the retail, service, and wholesale sectors. In these specifications, propensity scores are re-estimated on the full analysis sample with additional covariates and the overlap region is constructed accordingly. Log median family income for each postwar year is differenced over author-tabulated median 1939 household income in the 1940 census public-use full-count microdata as a proxy. Retail wages are from 1939, 1963, and 1982. Each effect is estimated separately. ** indicates p < .05, * indicates p < .10.

Table A.3: Robustness of Estimates of Effects on Men's Earnings

	(1)	(2)	(3)	(4)	(5)
<i>Outcome</i>					
Log Wage/Salary Earnings	0.034** (0.010)	0.038** (0.010)	0.040** (0.010)	0.036** (0.012)	0.034** (0.006)
Wage/Salary Earnings	2,067** (735.1)	2,348** (736.2)	2,475** (744.5)	2,557** (927.2)	2,749** (510.2)
Wage/Salary Earnings >= Annual Minimum	0.009 (0.007)	0.009 (0.007)	0.007 (0.007)	0.005 (0.009)	0.005 (0.005)
Wage/Salary Earnings > 0	0.003 (0.006)	0.003 (0.006)	0.002 (0.006)	0.004 (0.007)	0.004 (0.004)
Any College	0.011 (0.009)	0.012 (0.009)	0.012 (0.009)	0.015 (0.011)	0.009 (0.006)
HS Graduate	0.012 (0.007)	0.013* (0.007)	0.012* (0.007)	0.020** (0.010)	0.020** (0.005)
Baseline Controls	x	x	x	x	x
+ 1940 Income Controls		x	x		
+ 1940 Industry Controls			x		
<i>Sample Weights</i>					
Baseline	x	x	x		
CPS Survey Weights				x	
PIK Propensity Weights					x

Notes: Table reports OLS estimates of specification in Equation 3. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT. Wages are wage and salary earnings reported on forms W-2 and are defined for individuals with annual earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year; as a result N is smaller for this outcome. Wage in dollar levels is in CPI-adjusted 2017 dollars. Annual earnings are winsorized at the 99th percentile. Each estimate is from a separate regression.

Table A.4: Effects on Adult Earnings by Demographic Group and Plant Type

	<i>Men</i>					<i>Women</i>		
	All	Black	White	Stayers	Movers	All	Black	White
	(1)	(4)	(5)	(4)	(5)	(6)	(6)	(7)
Panel A. Ordnance Plants								
Log 1978 Wage	0.050**	0.064	0.046**	0.069**	0.043**	-0.017	-0.000	-0.017
	(0.015)	(0.047)	(0.015)	(0.024)	(0.018)	(0.018)	(0.048)	(0.020)
N	30,000	2,900	27,000	8,800	21,500	15,500	2,600	13,000
HS Degree in CPS	0.026**	0.058	0.023**	0.024	0.027**	0.017*	0.018	0.014
	(0.011)	(0.042)	(0.011)	(0.022)	(0.013)	(0.010)	(0.036)	(0.010)
N	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Any Clg in CPS	0.026**	0.044	0.021	0.023	0.035**	-0.006	0.027	-0.015
	(0.013)	(0.037)	(0.014)	(0.022)	(0.016)	(0.012)	(0.033)	(0.013)
N	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Stays in Birth Cty	0.02635**	-0.01601	0.03689***			0.01852	-0.01519	0.02606**
	(0.013)	(0.037)	(0.013)			(0.011)	(0.032)	(0.012)
N	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Panel B. Other Plants								
Log 1978 Wage	0.023**	-0.003	0.021*	0.070**	0.005	0.013	-0.004	0.019
	(0.011)	(0.044)	(0.012)	(0.019)	(0.014)	(0.014)	(0.045)	(0.015)
N	30,000	2,900	27,000	8,800	21,500	15,500	2,600	13,000
HS Degree in CPS	-0.000	0.046	-0.011	0.009	-0.003	-0.002	-0.003	-0.010
	(0.009)	(0.041)	(0.009)	(0.017)	(0.010)	(0.008)	(0.033)	(0.008)
N	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Any Clg in CPS	0.002	0.036	-0.006	0.048**	-0.011	0.011	0.028	0.008
	(0.010)	(0.037)	(0.011)	(0.017)	(0.013)	(0.009)	(0.031)	(0.010)
N	37,000	4,000	32,500	11,000	26,000	43,500	5,900	37,000
Stays in Birth Cty	0.026**	0.084**	0.020**			0.018**	0.000	0.020**
	(0.010)	(0.036)	(0.010)			(0.009)	(0.030)	(0.009)
N	37,000	4,000	32,500			43,500	5,900	37,000

Notes: Table reports OLS estimates of specification in Equation 3. Sample is all individuals born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT. Wages are wage and salary earnings reported on forms W-2 and are defined for individuals with annual earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year; as a result N is smaller for this outcome. Wage in dollar levels is in CPI-adjusted 2017 dollars. Annual earnings are winsorized at the 99th percentile. Each estimate is from a separate regression.

Table A.5: Effects on Upward Mobility: Alternative Estimators

	<i>Benchmark</i>		<i>Ordnance Plant</i>		<i>General Manufacturing</i>	
	<i>OLS</i>	<i>PSWT</i>	<i>OLS</i>	<i>PSWT</i>	<i>OLS</i>	<i>PSWT</i>
	(1)	(2)	(3)	(4)	(5)	(6)
<i>1940 Parent Income Rank</i>			A. Men			
Below Median	1.046** (0.331)	0.900** (0.338)	1.004** (0.397)	1.224** (0.457)	1.113** (0.466)	0.354 (0.526)
Above Median	0.496* (0.254)	0.458** (0.231)	0.548* (0.299)	0.438 (0.371)	0.600* (0.343)	0.224 (0.362)
<i>1940 Parent Income Rank</i>			B. Women			
Below Median	0.919** (0.313)	0.745** (0.354)	0.941** (0.420)	1.162** (0.480)	0.852* (0.437)	0.123 (0.518)
Above Median	0.667** (0.224)	0.626** (0.210)	0.886** (0.286)	0.844** (0.319)	0.603** (0.285)	0.260 (0.318)
N	1172	1172	715	715	1161	1161

Notes: Table reports propensity-score reweighting estimates of ATET effects on levels (not differenced) of the outcome variables. Outcomes are county-level averages (weighted by match rates) of adult income ranks based on 1975 and 1979 AGI as calculated by Massey and Rothbaum (2020) among the specified demographic group. Baseline sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded plant and comparison counties have none. N indicates number of counties in sample with disclosable outcomes and available covariates.

Appendix B: Historical Appendix

This appendix provides additional historical evidence about the planning, siting, and construction of new government-financed industrial plants during WWII.

B.1 Oversight of Plant Construction During WWII

The industrial mobilization for war can be roughly divided into four periods corresponding to progression of the administrative structure overseeing the production effort:

- **The National Defense Advisory Council (NDAC) era, 1940:** Focus on initial preparedness for war and production for allies.
- **The Office of Production Management (OPM) era, 1941:** Industrial expansion in expectation of joining the War.
- **The War Production Board (WPB) era, 1942-1945:** Full-scale production for war.
- **The Civilian Production Board (CPB) era, 1945-1946:** Reconversion of industrial capacity towards civilian production.

Each of these four organizations were outgrowths of their predecessors adapted to changing circumstances.(Fesler, 1947) They were responsible for coordinating the allocation of war supply contracts to private industry as well as the siting and construction of new industrial plants. New government-funded plants were approved and sited under the supervision of each of the NDAC, OPM, and WPB; however, the peak of centralized planning occurred during the OPM era when a Plant Site Board within OPM was formed to scout and approve new plant sites. The history of these organizations and their activities were extensively documented by the CPB in a series of studies published shortly after the war.

While these organizations played an important coordination role and exercised veto power over plant siting decisions, none had direct control over decisions of when and where to build new plants. According to one CPB history (*“Industrial Mobilization for War”*):

The Plant Site Board cooperated with similar boards set up by the War and Navy Departments in the review of locations for defense plants, and did not hesitate to withhold its approval where sites were deemed unsatisfactory. The Plant Site Board had its own research staff, which analyzed all proposals in the light of availability of labor, transportation, housing, power, raw materials, supply and destination of product, and other relevant factors. So far as possible, an attempt was made to locate plants away from highly industrialized areas. Other agencies of the Government, such as the Federal Power Commission, the Coordinator of Defense Housing, the Bureau of Labor Statistics, and the National Resources Planning Board were consulted for factual information, as were the various divisions and branches of OPM. ...

The Plant Site Board actually exercised a species of over-all planning function, although it was done in negative terms. The Board could not initiate anything, but by rejecting proposals offered, asking reexamination, and recommending specific changes, it did exercise a guiding influence in plant location, which prevented many bottlenecks and much undue concentration of industry. (Fesler, 1947)

Another CBP document, *The Facilities and Construction Program of the War Production Board and Predecessor Agencies*, provides additional detail:

In evaluating the work of the Plant Site Board it is well to remember certain things. In the first place the Plant Site Board was a negative planning unit. The initiation of proposals for the type of war plants needed and the selection of their locations were in the hands of the technical agencies, such as the War and Navy Departments, and the Maritime Commission. The Plant Site Board occupied more or less of a "veto" position. In view of the urgency for speeding up production, however, the Plant Site Board naturally was reluctant to exercise this power for fear of impeding the defense effort. Nevertheless, the establishment of the Plant Site Board was a recognition of the fact that a central planning unit was needed for the industrial expansion program. (McGrane, 1946)

Accordingly, there was no centralized procedure or systematic rule for plant siting decisions. In practice, vetos were typically exercised in cases where proposed plants were to be located in large industrial hubs deemed too congested for additional construction.

B.2 Plant Siting Considerations of Coordinating Bodies

The central concerns of the OPM Plant Site Board and its predecessors/successor bodies were to avoid redundancy and spread out new plants geographically:

Insofar as it was consistent with the primary objective of expediting the national defense program and with due regard to appropriate military factors, the Committee was to be guided "in approving plant site locations by a policy of wide geographic decentralization of defense industries and full employment of all available labor." In other words, the Plant Site Committee was to review all facilities projects financed by the Government with two objectives in mind: (1) No new facilities should be created as long as alternate capable facilities were available; (2) no facilities should be located in inappropriate spots relative to the supply of labor, power, utilities or housing. (Fesler, 1947)

The push for dispersion arose from concerns about supply chain security. If production facilities were excessively concentrated, localized attacks or blackouts could severely disrupt the war effort. A December 1941 letter by Major T.A. Sims, Assistant Technical Executive and later Deputy Chief of Staff in the Army

Air Force Material Command (which oversaw aircraft procurement), suggested that aircraft producers that had factories along the coasts should construct new modification centers in the interior to ensure continuous operations:

It is obvious that our aircraft factories located along the coast lines are going to be working under unfavorable conditions, such as blackouts and wide dispersion of their products just as soon as it becomes flyable. ... It is therefore proposed that we face this situation on a semipermanent basis, and require that each airframe manufacturer within 200 miles of our oceanic coastline establish an inland modification and dispersal base to which flyable airplanes awaiting the completion of certain installations to make them completely acceptable articles can be flown and completed at the inland modification base. (Fesler, 1947)

The same principle guided the recommendations of the OPM Plant Site Board.

What factors guided the selection of sites outside of congested industrial hubs? The primary considerations were easy access to key resources, including water, housing, labor, and transportation. The process was described by the CBP (emphasis added):

It was the function of the board to work with the site boards of the War and Navy Departments in the review and approval or disapproval of proposed locations for additional plants or facilities required for the national defense program. The board met with representatives of the Ordnance Department, the Army Air Corps, and the Navy Department and surveyed their overall general plans for additional war industrial plants. Upon receipt of these plans, E. M. Martin, who was both assistant to the chairman of the Plant Site Board and the board's research director, carefully analyzed the proposals with a view to locating the new plants most advantageously for the defense program. *Such factors as availability of labor, transportation facilities, housing, water power, community services and attitude, sources of raw materials and destination of the finished products, and the general relation of the new plants to the over-all distribution of manufacturing facilities in the country were carefully examined. The board was anxious to avoid, if possible, the building of plants in already highly industrialized and congested areas.* (McGrane, 1946)

The Plant Site Board, in parallel with the War and Navy departments, worked to identify parcels available for speedy acquisition in regions that met these criteria.

Congressional pressure had minimal influence on siting decisions. Although powerful legislators did try to influence siting decisions, there was little they could do besides make a strong case for locations in their home States. According to the CPB,

The OPM was deluged with requests from Congressmen and Senators from various parts of the country suggesting the location of defense plants in their respective Districts and States. Such requests were received from members of Congress from Wisconsin, Arkansas, Louisiana, Montana, Kansas, Indiana and Connecticut. Senator Arthur Capper of Kansas stressed the importance of locating plants in the Middle West. He asserted the Middle West possessed the

following advantages for national defense: (1) The greatest safety from foreign invasion and sabotage; (2) a large number of vacant housing facilities; (3) many idle schools, churches, stores, public utilities; (h) excellent transportation facilities; (5) abundant fuel; (6) low living costs; (7) good native American labor; and (8) a great supply of easily accessible raw materials. Residents of Kansas and Nebraska complained that their region did not receive its share of defense plants; yet, as a matter of fact, the Government spent large sums in the expansion of aircraft assembly plants at Wichita, Kansas City, and Omaha. Likewise, representatives from the South protested that the OPM had established a policy that no defense industries should be located within a 200 mile zone of the coast line of the Gulf of Mexico. There was no such fixed policy, for the Government financed the expansion of shipbuilding, ship repair, and magnesium facilities in Louisiana and Texas along the coasts. (McGrane, 1946)

As noted by Mark Wilson in *Creative Destruction*, this lack of influence was much to the chagrin of legislators:

[Senators] Stefan, Truman, and many of their peers remained dissatisfied and critical of the distribution of war work because their own influence was limited. The location of new plants was influenced less by the pull of congressmen and governors than by the calculations of military and civilian officials in the executive branch. Those officials often did favor the South and West because they endorsed a policy of decentralization, for strategic as well as political reasons. However, even this spreading of the work failed to placate many congressmen because, in most cases, it was the military and its contractors who selected sites using calculations of available transport, power, water, and local labor supply. Internal Navy correspondence from early 1941 shows that the Navy believed that it, and not Congress or even civilian mobilization officials, controlled the choice of plant sites. Under these conditions, even the most powerful congressmen might be stymied. (Wilson, 2016)

Thus, strategic considerations largely trumped political and economic considerations in the siting of publicly-funded plants.

B.3 Plant Siting in Practice

The push to site new plant construction in dispersed locations outside of established manufacturing hubs was met with strong resistance by private industry. Firms expected new facilities to be most valuable in the long run if sited in productive hubs where they already had major operations underway. This led firms to generally refuse to finance new construction in dispersed locations:

The War Department had decided that new defense plants should be built in the interior of the country at least 200 miles from the borders, and the Air Corps selected Omaha and Tulsa as

the sites for the two new plants. But the hard facts of the nation's economic structure made the policy difficult to follow. The greater part of American industry was concentrated along the Atlantic and Pacific coasts or in the Great Lakes region. Manufacturers in general resisted proposals for a transfer of their operations to areas remotely situated from established centers of labor and technical skills, and not without reason. As Knudsen once explained to General Marshall: "We can't move Detroit." *The industrialists' reluctance to invest in dispersed plant facilities was at odds with the government's hope that private capital could finance new inland construction; hence, the War Department could carry out its policy only to the extent that the government was willing to put up the money.* (Craven and Cate, eds, 1955; emphasis added)

Thus, private investment by firms in service of war contracts, even when generously subsidized, tended to be located in hubs that were expected to experience productivity growth in the long run, while investment in large new facilities in dispersed areas had to be fully financed by the government.

As a result, although many of the new, large, government-financed plants were constructed in dispersed locations, the majority of private investment in both the conversion of existing plants and the construction of new facilities occurred in well-established industrial hubs like Detroit and Chicago. Hence, the CPB noted

[S]upply contracts followed the location of industry and the workers; but new facilities were planned to follow at least partial decentralization. (McGrane, 1946)

During the war 1944 study by the War Production Board observed that wartime production had largely reinforced prewar patterns of industrial concentration, and that the government-funded construction of new plants was largely an exception to that rule:

Military and economic consideration resulted in a heavy concentration of these war expansions in the same states and areas where specific industries had chiefly operated before the war. These conclusions runs counter to impressions that a widespread relocation of industrial plant has occurred. Actually, effective dispersion has been the exception rather than the rule. Certain exceptions are important, however; new facilities for various industries now exist in areas previously not devoted to such industry. May such new (or greatly expanded) industrial areas are almost certain to continue in importance after the war. (United States War Production Board, 1945a)

Thus, although the geographic distribution of *production* and *private investment* during World War II largely reflected the prewar distribution of industrial activity, public spending on new plant construction tended to occur in regions that likely would not have been major industrial sites if not for wartime exigencies.

Outside of major manufacturing hubs, siting decisions were driven by fairly idiosyncratic factors so long as locations were deemed to have sufficient access to labor, housing, transportation, and power. As an example, consider the Geneva steel mill built in Utah (near Provo), which opened in 1942 and was the largest steel plant ever built west of the Rockies. Whetten (2011). notes that while private financiers had

seen little prospect in such a large steel plant in Utah, the federal government stepped in for reasons of short-run necessity:

The officials at the OPM did not aim to foster regional industry or to bring the American West out of the third world and into the first; they simply wanted to address national defense contingencies and the supply and demand issues that loomed ahead of the attack on Pearl Harbor.

With the Panama Canal closed due its vulnerabilities, moving steel from existing hubs in Ohio and Pennsylvania to Pacific shipyards in California, Oregon, and Washington States was impractical, necessitating new steel production sites in the West. These priorities created a unique opportunity for political entrepreneurs to attract investment, even when efforts to attract private capital had come up empty handed. Whetten notes that:

Local powers in Utah County attempted to both facilitate and benefit from federal use of power. They were not a colony that accepted federal choice and watched powerlessly, and they were not capitalists who spent their own capital to build the plant. ... Local businessmen and politicians tried to both support and steer federal decisions by suggesting locations, adapting local infrastructure, and attempting to sway public opinion." (Whetten, 2011).

Had the War not occurred, such a plant would likely not have been sited in the outskirts of Provo. The Geneva plant remained in operation until 2001.

Similarly, the siting of Ford's massive Willow Run plant in Ypsilanti was the result of idiosyncratic factors. Prior to the War, Willow Run was the site of an agricultural camp for boys established by Henry Ford towards the end of his life:

Using the same principles and methods as Camp Legion, this second camp taught farm training, self-reliance, management, and salesmanship. Like the first camp, the boys governed themselves, appointing a foreman and field foreman from their own ranks. They lived in tents, with a mess hall and a chapel on-site, and sold their produce from a roadside stand built by Ford. Boys in both camps had time for recreation as well as work, each camp had a baseball diamond and the boys participated in a softball league, there was also volleyball and handball, movies were shown, and each camp also hosted harvest dances, inviting nearby high school students to join. (The Henry Ford Archives)

At the outbreak of the war, the Ford family pledged this land to the war effort as a show of good faith:

Beset by Henry Morgenthau's treasury department sleuths investigating fmc ties to Ford of France and Germany- was fmc cooperating willfully with the Nazis?- and by the Truman committee, and the FBI (what were Henry Ford's and test pilot- consultant Charles Lindbergh's loyalties?), Edsel persevered to turn Henry's farm camp for disadvantaged boys into the largest

aircraft factory in the world— Willow Run. To please father, the plant was configured to stay within Washtenaw county which had voted Republican in 1940. ...

Like other war factories built in rural areas, Willow Run had no housing and workers could not commute to work from Detroit. Perhaps because of the importance of the b-24, the government agreed to release materials to build housing—“Bomber City.” Needing ten thousand workers, fmc turned to recruiting and training southern whites and blacks—a hypergolic racial mix. They hired a very large number of women, again against social norms. (Fitzharris, 2017)

Ford constructed and operated the Willow Run plant during World War II, but declined to purchase the facility at the end of the War. The plant was initially purchased by Kaiser-Frazer who in turn sold it to Ford’s rival General Motors, where the plant remained in operation until 2010.