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 the publicly-financed construction of large manufacturing facilities during World War II (WWII) on regional development and on individual-level earnings mobility during the Postwar period. We study the wartime construction of large, new plants in that the United States military could not incentivize private firms to stake any capital on, and likely would not have been built in the selected locations if not for the war. 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, and find that plant sitings caused manufacturing employment to rise by 30 percent and average production earnings to rise by 10 percent after the war and to remain elevated through the year 2000. These manufacturing-sector effects are associated with a general increase in median family incomes and, to a lesser extent, with higher wages in other sectors. If individuals are spatially mobile in the long-run, these county-level earnings effects may be driven by selective migration rather than within-individual earnings growth. 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. Growing up in a locale where a large plant was constructed had an economically significant impact on mens' adult wages. These plants also increased upwards intergenerational household income mobility for children born to parents with below-median family incomes in 1940. Although general-purpose plants that generally reconverted to civilian industry drive the effects on regional development, only the ordnance plants that continued to produce for the military during the

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Cold War had economically or statistically significant effects on pre-war residents' earnings and upward mobility.

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 upwards 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, since firms saw little long-run value in large war plants far their pre-existing operations, 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.

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. Understanding the answer is vital 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 and persist (Davis and Weinstein, 2002; Bleakley and Lin, 2012). Moreover, as evidence mounts that the places

[1] It 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. This, however, does not rule out the possibility that manufacturing presence was an important driver of upward mobility in earlier periods.

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.

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 1980s; though this increase was also reflected in a similar increase in housing values over the same period.

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 explosives continued to supply the military after WWII, most other 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 where the latter group

of general-purpose plants were no more oriented towards military production than the comparison group was.[2] Yet, the effects of plant sitings on long-run manufacturing employment were similar for both types of firms. Moreover, *only* the 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 earnings ranks based 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).[3] To study individual-level mechanisms in greater detail, we then examine micro-data from the Current Population Surveys (CPS) 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. 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 individuals who lived in places where plants were built had 3-4% higher incomes as adults. Specifically, these plant openings had impacts on upward intergenerational mobility. For children born into families that were below the median income in 1939, we find that plants raised their adult earnings 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. Using the linked CPS data, we find that increases in tax-unit income were driven by increases in men's earnings; we only find significant effects for white men. We find similar results for those individuals residing in adjacent counties prior to the War.

However, we only find large effects on upward mobility in locations that became sites of ordnance plants that continued to produce for the military after the war—locations which did *not* experience significant population growth or increases in housing costs after the war. This suggests the plants that had the largest effects on local residents' earnings were distinct from those that had the largest impact on regional development.

These results 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 intergen-

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

[3]These data are constructed to be analogous to the regional mobility measures in Chetty et al. (2014).

erational 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 expansion helped created a ladder out of poverty in the postwar period. In contrast to contemporary work on the effects of place, we do not find larger earnings effects for children who were younger when plants were built. This suggests that plant construction may have benefited local residents through improved local access to good-paying jobs, rather than through development of human capital. Consistent with this explanation, we find the largest effects on individuals who remained in their country of birth through adulthood.[4]

Our work also contributes to a broader literature on the 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). 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. Several recent papers have found substantial effects of industrial policies on regional development outside the United States as well (Fan and Zou, 2018; Criscuolo et al., 2019; Lu et al., 2019; Lane, 2019) In addition, several recent 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 no longer-term effects on long-run manufacturing expansion.[5]

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

[4]Though this finding is consistent with improved access to high-paying jobs, we note that these results are based on stratification of the sample by an outcome (post-war location) and can only be interpreted as causal under very strong assumptions.

[5]The 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+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.

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 remained 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 of pre-war residents. Section 8 concludes.

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.[6] 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

[6]No full-scale industrial mobilization had occurred during the First World War.

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) from 1933-1939.[7] 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.[8] Notably, $10 billion of government of 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

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

[8] During 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.

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 facility, 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 8 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.[9] 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 8 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).

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

[9] According 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 8 for additional references.

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.[10] For example, Ford declined to purchased 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.

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 is 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*

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

1944 by General Type of Product Operator (United States War Production Board, 1945b).[11] 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 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.[12] 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".[13] 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.

[11] Very 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.

[12] A 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").

[13] We 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.

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 Current Population Survey Annual Social and Economic Supplement (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 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 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. Because individual demographics and parents' earnings are observed in 1940 Census, Massey and Rothbaum (2020) use these data to document county-level differences in intergenerational rank-rank mobility following Chetty, Hendren, Kline, and Saez (2014). For the children, adult income is measured using tax unit level Adjusted Gross Income (AGI) reported on the 1040 forms in the two available years. The construction of this database is documented in detail in Massey and Rothbaum (2020). We use this database 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. [14]

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

[14] Unfortunately, the CPS-DER and the 1940 Census—IRS linked datasets were provided to us under distinct authorizations and cannot be merged.

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.[15] 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.[16] 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.[17] 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 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

[15] In the no-war counterfactual, we argue that 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.

[16] While 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..

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

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

$$\hat{ATET}_t = \frac{1}{N_{Treat}} \sum_{i:Treat_i=1} Y_{it} - \frac{\sum_{i:Treat_i=0} Y_{it} \frac{\hat{p_i}}{1-\hat{p_i}}}{\sum_{i:Treat_i=0} \frac{\hat{p_i}}{1-\hat{p_i}}} \tag{1}$$

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 the propensity score weights vary 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 pos-

itive for the 202 counties with new plants and 0 for the rest of the sample. We estimate equations of the form

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

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 $\frac{Expend}{Capita}_i$ 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

5.1 Manufacturing Sector Outcomes

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.[18] 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 percent 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.[19] The effects on production employment follow a similar time pattern,

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

[19] To 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 treat-

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 examines the effects of plant sitings on aggregate labor productivity in manufacturing (value added per production employee) and average pay (total production wage bill per production employee). After war, treatment counties experienced a sharp and highly persistent increase in average pay. Wages rose by an additional 8–10 percent in treatment counties, and that same difference persists through 1997. This increase in pay closely tracks output per-worker over the same period.

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. Result 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 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.[20] Appendix Table 12 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,

ment county would amount to over one thousand fewer jobs.

[20] In particular, we additional 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.

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.[21] 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. Table 4 reports effects on retail-sector wages that are dramatically smaller than the effects on manufacturing wages and are not significantly different from zero. This result implies that the manufacturing labor market was sufficiently segmented from other sectoral labor markets that substantial wage differentials persisted. This finding is consistent with earlier studies of labor market segmentation and cross-industry wage differentials that find a significant manufacturing wage premium in the 1970s and 1980s (Krueger and Summers, 1988). The treatment does have a significant effect on median family incomes, the evolution of this effect appears to reflect the rise and fall of the manufacturing employment level over time, and perhaps a rise in the cost of living reflected in the cost of housing.[22] Since manufacturing jobs paid better in all postwar years, the effect on typical incomes was largely governed by the share of people who had access to such jobs.

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

[21] These outcomes are derived from population surveys, which are aggregated to the county level by individuals' places of residents .

[22] We find the median housing values rise by a similar around to median incomes, though the former estimates are more imprecise and less robust to specification than the latter. An increase in housing values may reflect a higher cost of living, but it may also reflect increased consumption of housing due to increases in income.

treated counties from our comparison group in Appendix Tables 12 and 13. 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.[23] 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

[23] Treatment counties of either type are never included in any control group.

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 owns the plant to continue to reinvest in it. However, path-dependence might operate though 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 data in the current outcome 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 Individual-Level Effects

7.1 Effects on Earnings of Postwar Residents

The evidence presented so far indicates that one-time public investments during World War II had dramatic and persistent effects on regional economies. While we find large, long-run increases in manufacturing wages in locales where plants were constructed, the welfare implications of these wage increases depend strongly on whose wages were rising. 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.

We therefore test for individual-level effects by studying the long-term outcomes of workers based on where they resided as children prior to the war. We focus on individuals who were too young to directly participate in the war and the associated mobilization effort, but would have been entering the workforce in the decades following the war. In two distinct data sources, we are able to match information on pre-war places of residence to administrative earnings records from the 1970s onwards, beginning when members of the relevant cohorts were between 35 and 50 years old. This enables us to compare adult outcomes for individuals who had a public war plant built in their childhood hometown—for idiosyncratic reasons—to other children in the same cohort who did not. Importantly, 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 outside the local manufacturing sector.

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.[24] Our approach, therefore, is to measure post-period differences between individuals who resided

[24] If the impacts grew linearly with time of exposure, one could use variation in exposure length across cohorts to identify the causal

as children in counties that received plants compared to those who resided in counties that did not. In these analyses the identifying assumption is that, conditional on a basic set of observable characteristics, plant sitings were fully as good as random. If this assumption holds, then, conditional on the sufficient set of covariates, 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.

Table 7 compares of 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. We first use our baseline covariate excluding pre-period income variables to ensure there is no mechanical equivalence between the DD and PPD estimates. We find that PPD and DD propensity score estimates of effects on manufacturing and retail wages are highly similar. Including pre-war earnings covariates has close to no impact on the PPD estimates. For comparison, we examine PPD effects on 1980 median male and female earnings, the 1980 poverty rate, and average 1970 male earnings in select production occupations—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 effects on median male wages are similar in magnitude to the effects on median family incomes 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.

7.2 Individual Outcomes in the CPS-DER [25]

We first examine individual level effects on individuals present in the CPS-DER database, which contains individual level administrative earnings records from 1978 onwards for all individuals surveyed in the CPS is 1991, 1994, or 1996-2017. We focus on the subset of observations that could be linked using a PIK to identify individuals' places of birth in the NUMIDENT files, 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 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% reside 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

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.

[25] Results from this section are from an earlier draft of the paper. Updated results, currently undergoing disclosure review, are qualitatively similar.

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. We winsorize earnings at the 99th percentile.

Using these data, we estimate the following individual-level post-period differences regression:

$$\ln Y_{ict} = \alpha_t + \beta_t Treat_c + \gamma_t X_i^{1940} + \sum_{\tau=1928}^{1941} \delta^\tau \mathbf{1}[YOB_i = \tau] + \epsilon_{it} \tag{3}$$

where i denotes an individual and c denotes a county. 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.

The baseline results presented in Table 8 indicate that the construction of large, high-paying manufacturing plants had significant effects on the earnings of 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 3.5% higher than those born in comparable control counties; we find no effect on employment. Figure 8 plots how these effects evolve over time. We find that the 1978 effects—equivalent to roughly $3,000 per year in current dollars—persist throughout the mid-1980s and dissipate around 1990. However, the results in Table 9 show the effects are concentrated among white men. We find no average effect for either black men or women of any race.[26] We find the effects are larger for the older cohorts who were entered the workforce shortly after the start of the war, though Appendix Figure A.4 shows that effects for older and younger cohorts converge in subsequent years.

These effects on earnings could reflect several different mechanisms. The simplest mechanism is that workers born in treatment counties had better access to the high-paying jobs created in their treatment counties. Results in Table 10 show treatment has only small effects on the probability that individuals remained in their birth county through the year they appeared in the CPS. We do not observe any information about the industry or firms in which individuals worked except in the CPS 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. However, when we stratify the effects on the endogenous decision to remain in one's county of birth, 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. This finding implies that at least one of the following two things holds: either staying in the birth county—and potentially working at one of the created manufacturing jobs—was necessary to benefit from

[26] In this dataset, tests of heterogeneous effects within genders are potentially underpowered due to small sample size.

the plant openings, or out-migrants were strongly negatively selected in a manner that offset any causal gains of childhood exposure to the plant.

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. Our findings in Table 10 indicate the plant sitings had marginally-significant effects both on high school completion and college enrollment. These heightened levels of educational attainment can explain some of the effect on earnings we find; however, at plausible levels of the returns to increased education, the small magnitude of the point estimates limit the extent to which this channel can account for the full earnings increase.

7.3 Implications for 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, following Chetty, Kline, Hendren, and Saez (2014), calculate the earnings ranks of children's parents ranks in the 1940 Census as well as children's own adult household income ranks for all individuals born 1921–1940 with PIKs in both the 1940 Census and the IRS tax return data. Massey and Rothbaum (2020) calculate children's household earning ranks among all individuals in the same birth cohort using adjusted gross income (AGI) reported on IRS 1040 forms in 1974 and 1979.

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 the 1940 Census among subgroups split by parent earnings rank and other 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 11 and alternative specifications are displayed in Appendix Table 14.

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 combined 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. Our estimates indicate that the manufacturing opportunities created by public investment during World War II increased upward earnings mobility more for those at the low end of the parental distribution relative to those at the top of the earnings distribution.

Figure 9 also displays separate estimates by plant type. The results are striking: only ordnance plants increase upward mobility or affect adult incomes significantly. By contrast, the general purpose plants—the subset of plants that spurred long-run regional development—did not impact the post-war earnings of pre-war residents. This suggests the plants that had the largest effects on incumbent residents' earnings were distinct from those that had the largest impact on regional development. Although manufacturing employment and wages grew similarly in both cases, local residents benefited the most in the regions with plants that continued to supply the military. We found above that those plants attracted few new migrants and had the effect of increasing the share of the local population employed in high-wage manufacturing. Access to these jobs through accident of birth, it seems, created a ladder of opportunity for residents born to parents at the lower end of the earnings distribution.[27]

8 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

[27] We find similar results in the CPS-DER data. Results are currently undergoing disclosure review.

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 mens' adult wage incomes, but not those of women. 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 turn around local economies and support "good"-paying jobs for decades, which in turn provide important work opportunities to local residents. 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. With less urgency and more time to plan, firms may have invested in more automated continuous processing technologies that created less demand for blue-collar workers (Goldin and Katz, 1998). Such considerations may be important in assessing the labor market effects of a given public investment

program.

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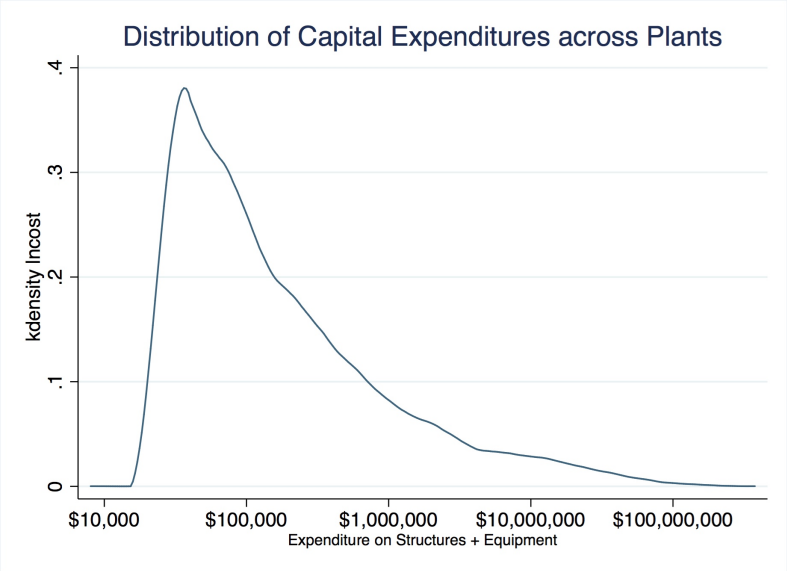
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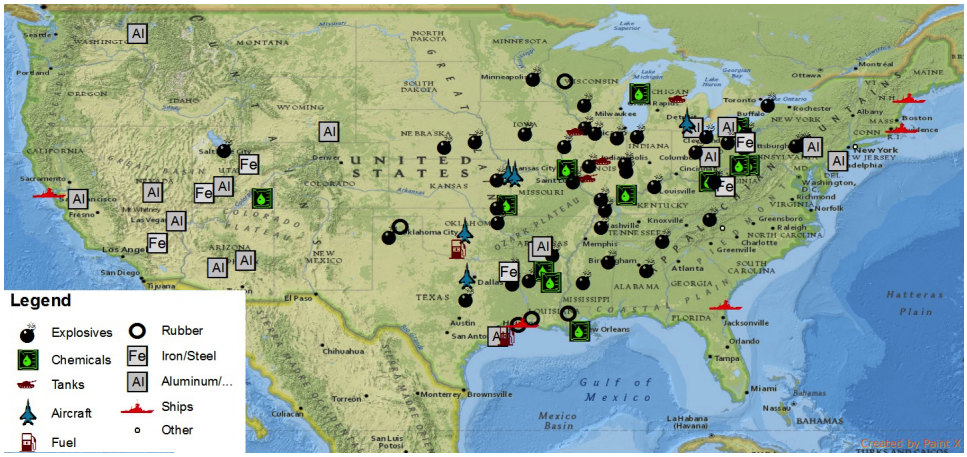
Figures and Tables

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



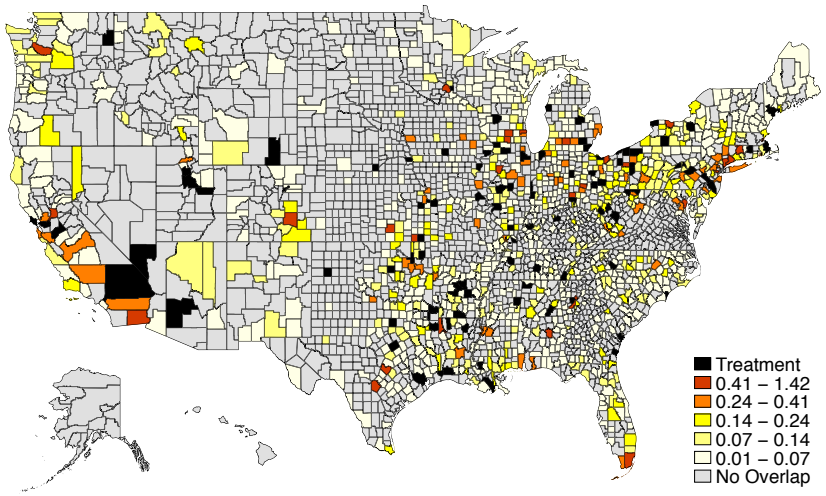
Notes: Figure is kernel density plot of the distribution of wartime all capital expenditures (structures and equipment, in logged 2014 dollars) across all establishments listed in WPB records.

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



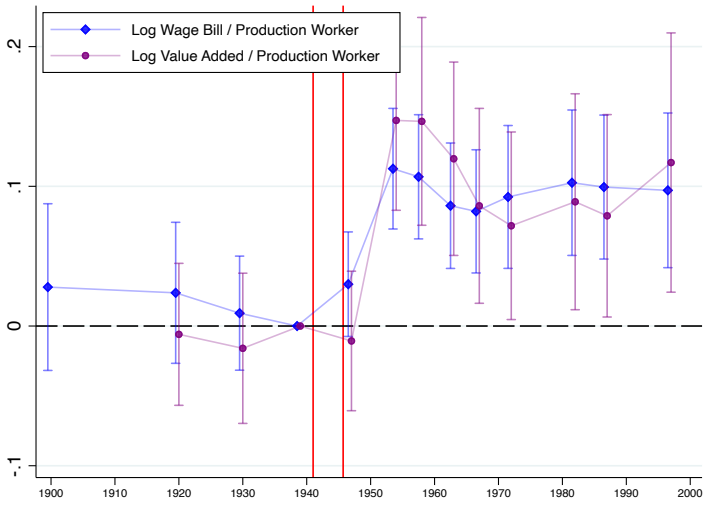
Notes: Map displays propensity score weights from baseline specification. All 101 treatment counties are presented in black. The 1,117 counties in the overlap sample are color-coded by quintile of the propensity score. Counties outside the overlap region or otherwise omitted from analysis sample are 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



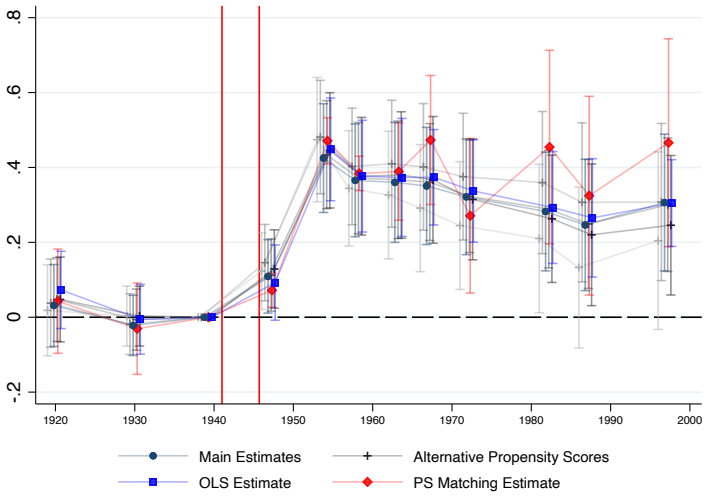
(b) Labor Productivity and Average Pay



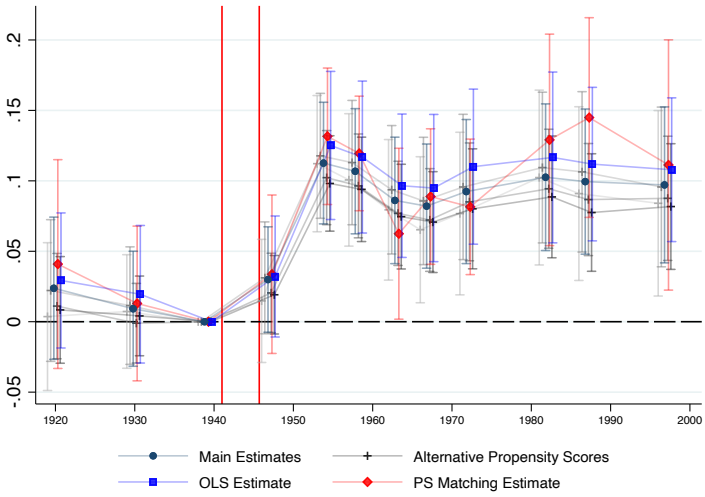
Notes: Figure displays propensity score re-weighted ATET estimates of plant sitings on outcomes differenced over 1939 levels. Outcomes are tabulations from establishment surveys. Propensity score is estimated using baseline covariates in Table 2. Sample is N = 1,179 counties in propensity score overlap 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 Value Added

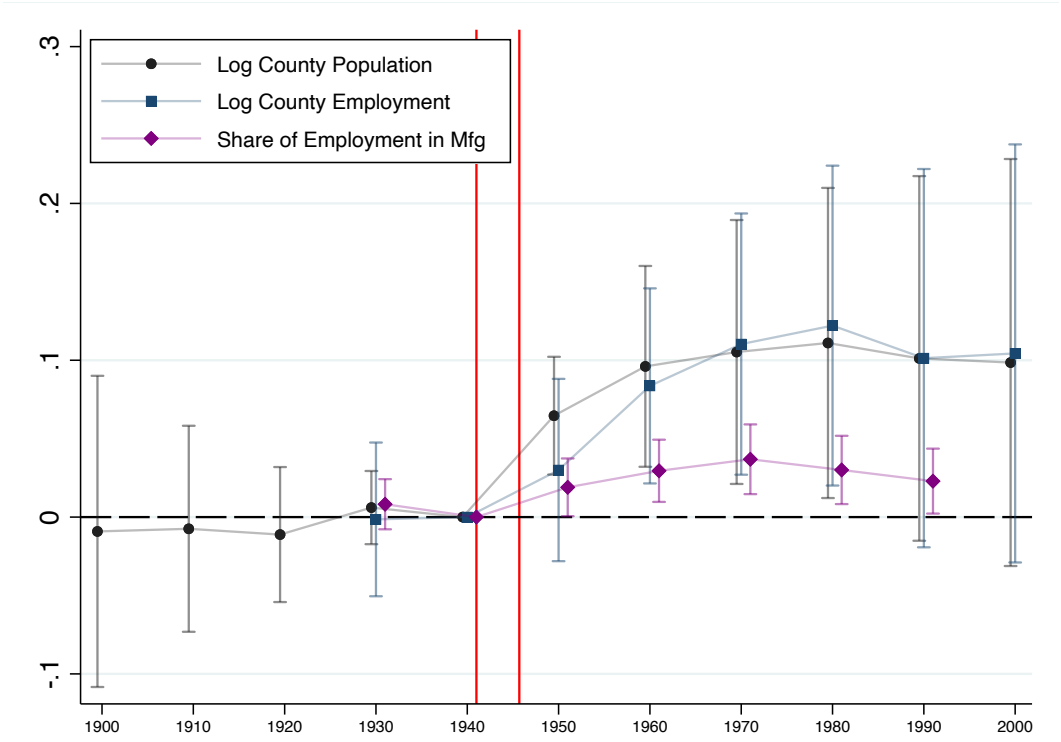


(b) Effect on Log Production Wage Bill Per Worker



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.

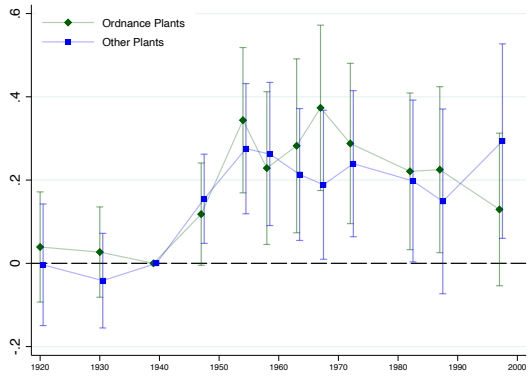
Figure 6: Broader Employment Outcomes: Reweighting Estimates



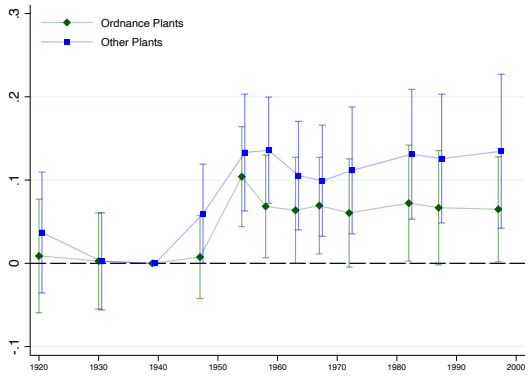
Notes: Figure displays propensity score re-weighted ATET estimates of plant sitings on outcomes differenced over 1940 levels. Propensity score is estimated using baseline covariates in Table 2. Sample is N = 1,179 counties in propensity score overlap sample, 94 are treated with a large government-funded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

Figure 7: Heterogeneity by Plant Type

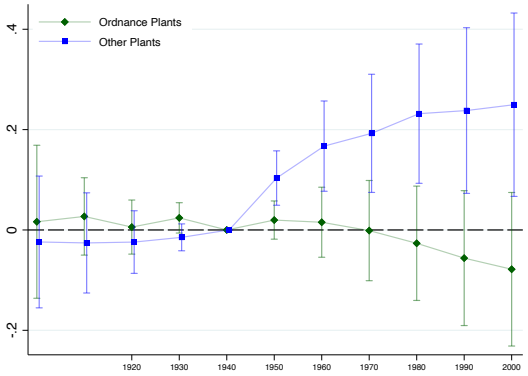
(a) Log Mfg Production Employment



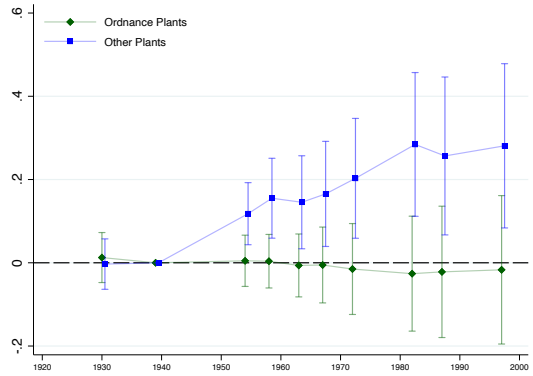
(b) Log Mfg Average Production Wage



(c) Log Population



(d) Log Retail Employment



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.

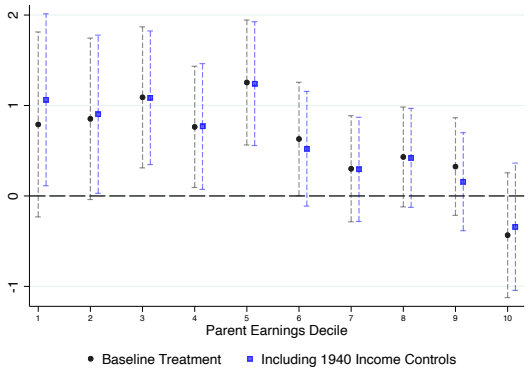
Figure 8: Effects on Adult Earnings of Individuals Born in County



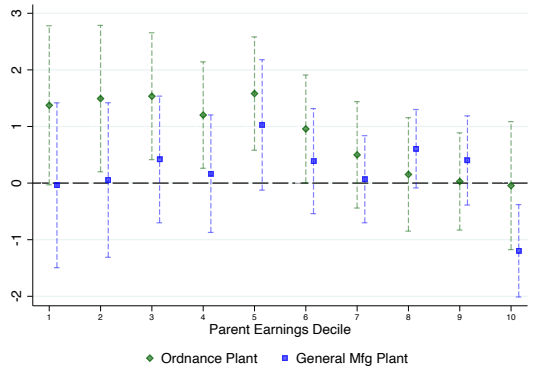
Notes: Figure plots OLS estimates of specification in Equation 3 by outcome year. Sample is all men born 1928-1941 in CPS-DER file with PIK links to the NUMIDENT. Wages are defined for individuals with annual wage/salary earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year. Average wages are taken over years in which wages exceed this threshold. 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

(a) Baseline Effect



(b) Effect by Plant Type, Baseline Specification



Notes: Table 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.

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 Incom	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					
N treated in overlap sample	94								
N control in overlap sample	1085								
Sum of control weight	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

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

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

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

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

Panel B: Manufacturing and Total Employment Effects

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

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

	1978 Wage		*Average Wage for Years > MW*		
	Logs	2016 $	1978-1981	1982-1985	1986-1989
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Treat	0.0340***	2,236***	4,383*	2,314**	-86.14
	(0.0104)	(808.6)	(2,661)	(1,172)	(1,677)
N	*42,500*	*42,500*	*48,000*	*46,500*	*45,500*

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. Dollar levels are in CPI-adjusted 2017 dollars. Average wages are taken over years in which wages exceed this threshold. Annual earnings are winsorized at the 99th percentile. Each estimate is from a separate regression.

Table 9: Effects on Adult Earnings: By Demographic Group

	Men					*Women*	
	All	0 to 6	7 to 12	Black	White	Black	White
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
1978 Outcome:							
Log Wage	0.0340**	0.0185	0.0556**	-0.0162	0.0338**	-0.0041	-0.0091
	(0.0104)	(0.0124)	(0.0153)	(0.0324)	(0.0114)	(0.0280)	(0.0132)
N	*42,500*	*24,500*	*18,000*	*3,800*	*38,000*	*3,400*	*18,500*
Wage	2,236**	749.2	4,319**	-1,366	2,227**	-990.5	-734.3
	(808.6)	(973.3)	(1,267)	(2,054)	(892.4)	(1,434)	(742.7)
N	*42,500*	*24,500*	*18,000*	*3,800*	*38,000*	*3,400*	*18,500*
Employed	-0.0045	-0.0048	-0.0039	-0.0407	-0.0058	-0.0506**	-0.0086
	(0.0087)	(0.0097)	(0.0106)	(0.0253)	(0.0090)	(0.0253)	(0.0084)
N	*52,000*	*30,000*	*22,000*	*5,200*	*46,000*	*7,600*	*52,000*

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. 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 10: Effects on Adult CPS Outcomes

	CPS Year Characteristics				*Log Wage, by CPS Residence*	
	HS grad	Any College	In Manuf Job	In Birth County	Not In Birth County	In Birth County
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Treat	0.0177*	0.0214*	0.0034	0.0204	0.0145	0.0793***
	(0.0092)	(0.0109)	(0.0059)	(0.0167)	(0.0121)	(0.0155)
N	*52,000*	*52,000*	*52,000*	*52,000*	*30,000*	*12,500*

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. Log 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. CPS year outcomes are variables reported in the post-1990 year in which the individuals appears in the CPS ASEC file. "In birth county" indicates whether the individual reported residing in their place of birth during the CPS year. Each estimate is from a separate regression.

Table 11: Effects on Upward Mobility

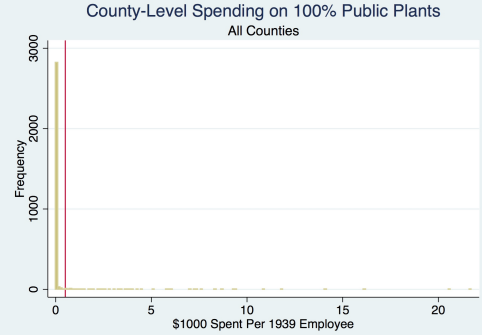
	Benchmark		*Ordnance Plant*		*General Manufacturing*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			A. Men			
1940 Parent Income Rank						
Below Median	0.900**	0.943**	1.224**	1.169**	0.354	0.294
	(0.338)	(0.330)	(0.457)	(0.500)	(0.526)	(0.503)
Above Median	0.458**	0.461*	0.438	0.465	0.224	0.192
	(0.231)	(0.236)	(0.371)	(0.377)	(0.362)	(0.332)
			B. Women			
1940 Parent Income Rank						
Below Median	0.745**	0.808**	1.162**	1.158**	0.123	0.073
	(0.354)	(0.342)	(0.480)	(0.526)	(0.518)	(0.491)
Above Median	0.626**	0.591**	0.844**	0.834**	0.260	0.305
	(0.210)	(0.210)	(0.319)	(0.338)	(0.318)	(0.297)
N	1172	1072	715	582	1161	814
Baseline Covariates	x	x	x	x	x	x
1940 Income Covariates		x		x		x

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 A: Supplemental Tales and Figures

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

(a) All Counties



(b) Counties with One or More Publicly-Financed Plants

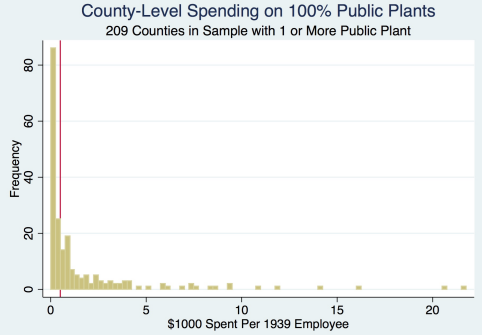
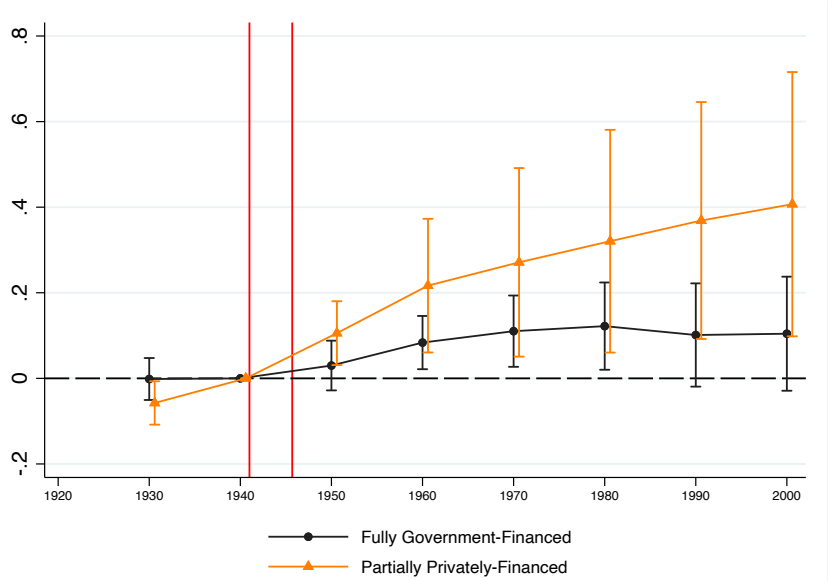
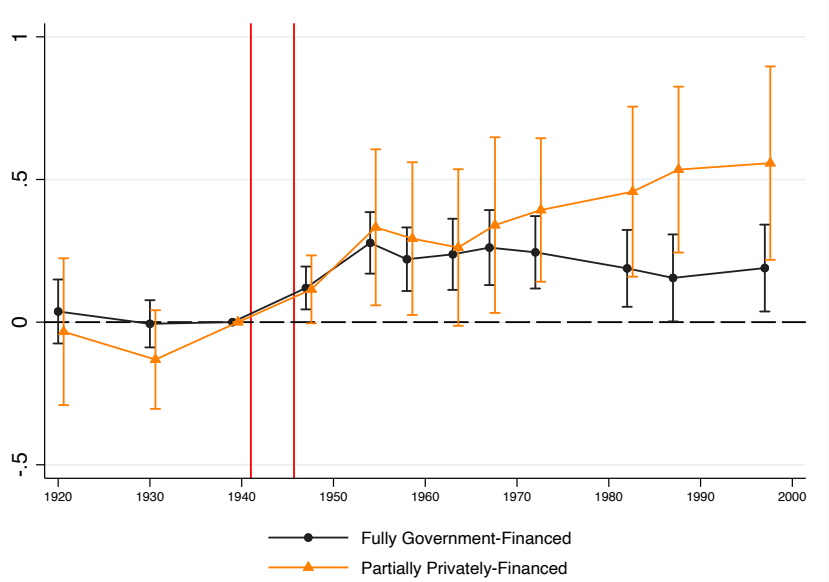


Figure A.2: Comparison: Effect of Big New Plants with Private Financing

(a) Effect on Total Employment



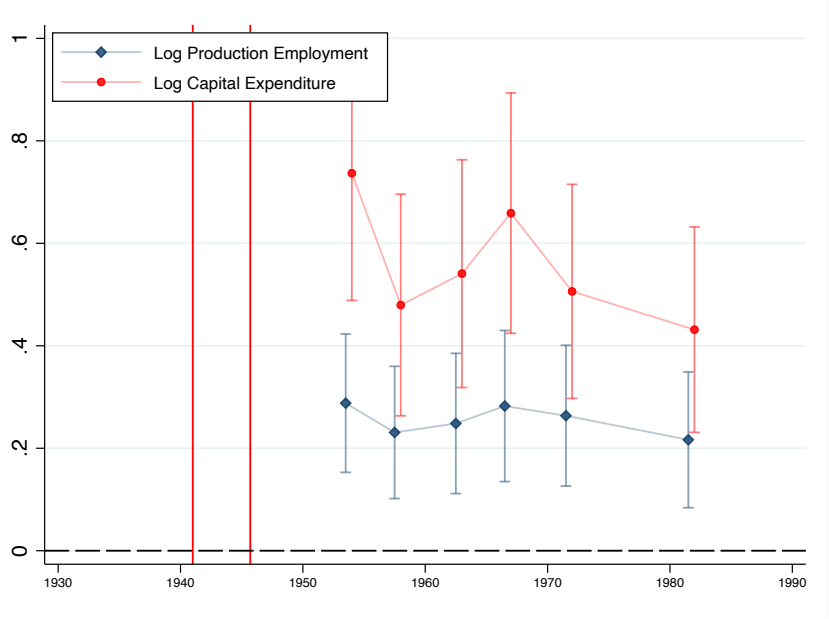
(b) Effect on Manufacturing Employment



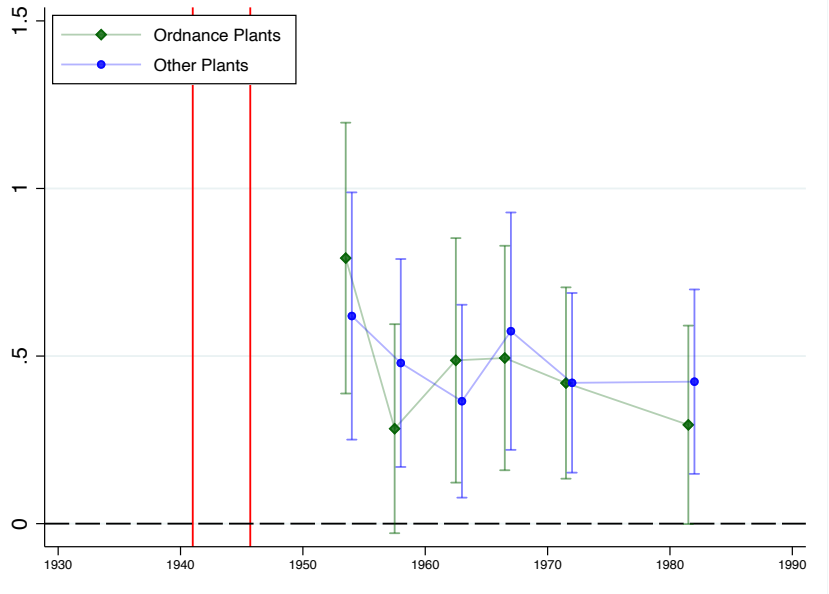
Notes: Figure presents estimation of specification identical to that in Panel A of 4, but binary treatment defined by presence of large, new plant with at least some private financing, using fully analogous definition, and propensity score is re-estimated accordingly. Treatment counties from baseline 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

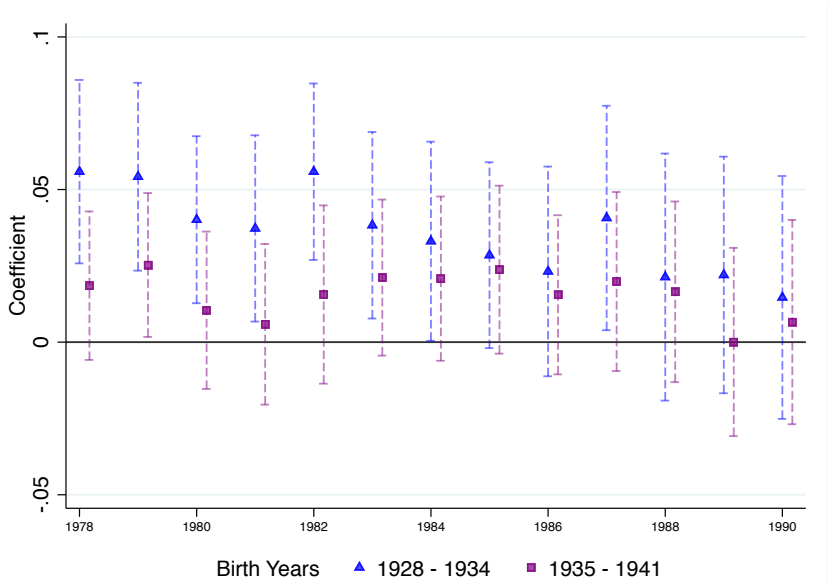


(b) By Plant Type



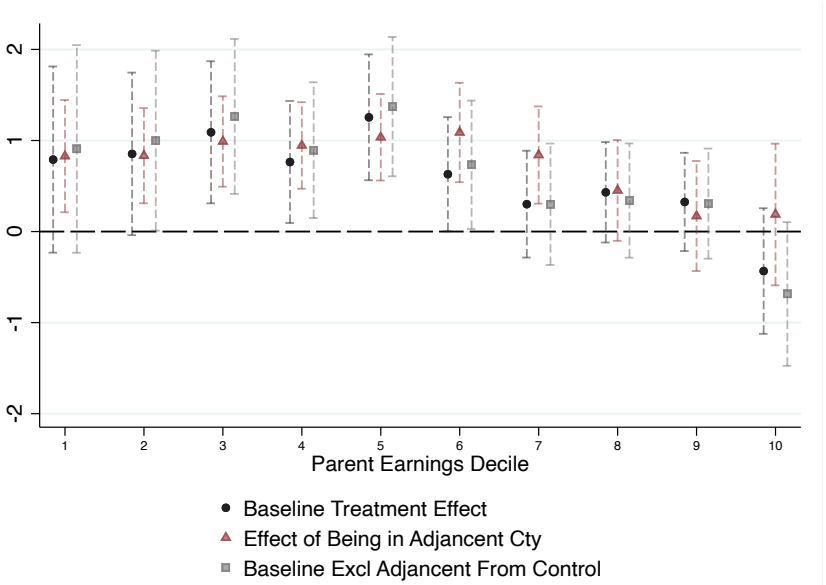
Notes: Figure displays estimates of ATET effects of the binary treatment on levels (not changes) of outcome variables. All other details are as in Figure 7.

Figure A.4: Effects on Adult Earnings, by Cohort Group



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. Wages are defined for individuals with annual wage/salary earnings exceeding equivalent of 2,000 hours work at federal minimum wage in outcome year. Average wages are taken over years in which wages exceed this threshold. Annual earnings are winsorized at the 99th percentile. 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 12: Effects on Manufacturing Outcomes: Alternative Estimates

	PS Re-Weighting				OLS		PS Match	
	Binary Treatment Effect		*Excluding Adjacent Counties*		*Binary Treatment Effect*		*Binary Treatment Effect*	
	1963 Effect	1982 Effect	1963 Effect	1982 Effect	1963 Effect	1982 Effect	1963 Effect	1982 Effect
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Outcome:								
Log Mfg Value Added	0.360**	0.282**	0.369**	0.286**	0.373**	0.293**	0.389**	0.454**
	(0.082)	(0.081)	(0.093)	(0.092)	(0.080)	(0.076)	(0.066)	(0.132)
Log Mfg Esablishments	0.059	0.111*	0.068	0.124*	0.051	0.086*	0.081**	0.103
	(0.038)	(0.059)	(0.044)	(0.067)	(0.035)	(0.052)	(0.040)	(0.071)
Log Mfg Production Payroll	0.327**	0.296**	0.334**	0.291**	0.332**	0.290**	0.288**	0.397**
	(0.072)	(0.076)	(0.081)	(0.085)	(0.072)	(0.075)	(0.076)	(0.118)
Log Mfg Production Employment	0.238**	0.189**	0.247**	0.180**	0.232**	0.164**	0.284**	0.176
	(0.064)	(0.069)	(0.072)	(0.077)	(0.058)	(0.063)	(0.093)	(0.134)
Log Mfg Production Avg Wage	0.086**	0.103**	0.083**	0.103**	0.097**	0.117**	0.062**	0.129**
	(0.023)	(0.027)	(0.025)	(0.029)	(0.026)	(0.031)	(0.031)	(0.038)
Log Mfg VA Per Prod. Employee	0.120**	0.089**	0.118**	0.098**	0.138**	0.120**	0.164**	0.185**
	(0.035)	(0.039)	(0.039)	(0.044)	(0.038)	(0.042)	(0.045)	(0.062)
Log Mfg Prod Labor Share of VA	-0.034	0.014	-0.034	0.005	-0.041	-0.003	-0.101*	-0.058
	(0.031)	(0.037)	(0.034)	(0.042)	(0.034)	(0.037)	(0.043)	(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 13: Effects on Broader Employment: Alternative Estimates

	PS Re-Weighting				OLS		PS Match	
	Binary Treatment Effect		*Excluding Adjacent Counties*		*Binary Treatment Effect*		*Binary Treatment Effect*	
	1963 Effect	1982 Effect	1963 Effect	1982 Effect	1963 Effect	1982 Effect	1963 Effect	1982 Effect
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Outcome:								
Log Population	0.096**	0.111**	0.099**	0.121**	0.079**	0.078**	0.105**	0.127**
	(0.033)	(0.050)	(0.038)	(0.057)	(0.027)	(0.038)	(0.031)	(0.048)
Log Total Employment	0.084**	0.122**	0.089**	0.133**	0.082**	0.102**	0.104**	0.170**
	(0.032)	(0.052)	(0.037)	(0.059)	(0.026)	(0.040)	(0.033)	(0.053)
Mfg Share of Employment	0.030**	0.030**	0.037**	0.039**	0.025**	0.026**	0.038**	0.044**
	(0.010)	(0.011)	(0.012)	(0.013)	(0.005)	(0.008)	(0.003)	(0.013)
Log Retail Employment	0.078**	0.131**	0.076*	0.134*	0.066*	0.094**	0.052	0.115*
	(0.038)	(0.062)	(0.044)	(0.070)	(0.034)	(0.047)	(0.041)	(0.061)
Log Avg Retail Wage	0.026**	0.022*	0.027**	0.021	0.028**	0.019**	0.023**	0.016
	(0.011)	(0.012)	(0.013)	(0.014)	(0.009)	(0.009)	(0.006)	(0.021)
Log Median Family Income	0.033**	0.040**	0.030**	0.035**	0.033**	0.043**	0.041	0.063**
	(0.013)	(0.014)	(0.015)	(0.016)	(0.012)	(0.014)	(0.026)	(0.031)
Log Median Housing Value	0.042	0.056*	0.044	0.069*	0.031	0.036	-0.007	0.014
	(0.027)	(0.034)	(0.029)	(0.035)	(0.027)	(0.032)	(0.035)	(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 houshold 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 14: Effects on Upward Mobility: Alternative Estimators

	Benchmark		*Ordnance Plant*		*General Manufacturing*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			A. Men			
1940 Parent Income Rank						
Below Median	0.900**	0.943**	1.224**	1.169**	0.354	0.294
	(0.338)	(0.330)	(0.457)	(0.500)	(0.526)	(0.503)
Above Median	0.458**	0.461*	0.438	0.465	0.224	0.192
	(0.231)	(0.236)	(0.371)	(0.377)	(0.362)	(0.332)
			B. Women			
1940 Parent Income Rank						
Below Median	0.745**	0.808**	1.162**	1.158**	0.123	0.073
	(0.354)	(0.342)	(0.480)	(0.526)	(0.518)	(0.491)
Above Median	0.626**	0.591**	0.844**	0.834**	0.260	0.305
	(0.210)	(0.210)	(0.319)	(0.338)	(0.318)	(0.297)
N	1172	1072	715	582	1161	814
Baseline Covariates	x	x	x	x	x	x
1940 Income Covariates		x		x		x

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 CBP 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, the 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 re quests 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 Morganthau'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.