Was the Arsenal of Democracy an Engine of Mobility? The World War II Industrial Expansion 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) both on local labor markets 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 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 therefore test for individual-level effects 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 1940. Our results suggest that wartime policies causally contributed to the midcentury rise in upward mobility.

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1 Introduction

This paper evaluates the long-run effects of the publicly financed construction of large manufacturing facilities during World War II (WWII) on local labor markets and individual earnings mobility. Many have hypothesized that the postwar expansion of high-wage manufacturing employment and associated "Great Compression" in the earnings distribution was a direct result of the industrial mobilization for the war (Goldin and Margo 1992). This paper provides a direct test whether regions that received more wartime investment supported better manufacturing jobs in the long run—and, moreover, whether such manufacturing jobs were an important driver of mid-century economic opportunity. 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 industrial policy is justified in developing regions, whether place-based labor policies can help the economically disadvantaged reach the middle class, and the extent to which infrastructure shapes how urban clusters form and persist. Moreover, as evidence mounts that the places children grow up have causal effects on their adult earnings, it remains an open question what role local economic policy plays in determining those effects. 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.

The industrial expansion for WWII provides a unique opportunity to study this question. Due to the short-run military emergency, political and military leaders demanded that the United States increase its domestic industrial output nearly threefold over the course of only three or four years. This increment to output primarily consisted of airplanes, ships, ordnance (guns and ammunition of all varieties), explosives, and the metals and chemicals used in the production of those various types of matériel. Although the military attempted to incentivize firms to put their own capital on the line and build plants as necessary, for some particularly large plants in secure locations, these incentives were insufficient to attract any private investment, particularly when an expensive plant was built to churn out a product that was insufficiently similar to products more likely to be demanded during peacetime. In these cases, the plants were ordered and owned by the U.S. government. As the private sector was unwilling to finance plant construction at the sites selected by the U.S. government, it is unlikely that similar plants would have been sited in the same locations if not for the war. These large, durable, public plants are more plausibly located for quasi-random short-run reasons than any comparable infrastructure investment in Western history.

To estimate the effects of siting a large plant in a specific locale, we compare counties that received large and completely federally funded plants due to idiosyncratic war-related circumstances to other counties that were observably similar at the dawn of the Great Depression. Our conjecture is that in the absence of a war, neither the control nor the treatment counties would have had such an additional plant open; the only reason potential outcomes differ across treatment and control counties were circumstances created by the war. We first test for county-level effects using a simple difference-in-differences framework that

compares how much more employment and wages rose in "treated" locales compared to similar "control" locales. This approach is supported by the data: all outcomes we observe for treatment and control countries evolved in parallel between 1920 and 1940. By contrast, plants that received substantial *private* funding seem to be systematically located in regions experiencing higher prewar industrial expansion, even conditioning on a wide array of observable characteristics. This supports our focus on government-financed plants. We select control groups using several methods and covariate sets and find the choice of method does not significantly impact the results. In practice, we find that "treatment" counties are observably similar to other counties of similar size in 1930, and therefore difference-in-differences estimates are similar to effects estimated from simple post-war treatment-control comparisons.

We find that the construction of a large industrial plant during WWII had a large and persistent effect on the local labor market, with particularly pronounced effects on the manufacturing wage. After the postwar reconversion, manufacturing employment in counties grew by 30 percent and remained elevated until the 1980s. Moreover, the average wage of local production-line workers rose by 10 percent, and this resulting wage differential persisted beyond even the 1980s. We find that 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. These effects are large, even relevant to the costs of construction: At a 10 percent local income tax rate, local governments would have recovered roughly half of the cost of the plant construction from the payroll increment alone. Moreover, we find that the most persistent effects were caused by the plants most likely to be reconverted for civil purposes—those making planes, tanks, and steel—rather than specialized ordnance plants that tended to cater to military production throughout the Cold War era. This suggests the persistent effects were the result of the initial investments during the war, not continued government or military investment.

While the identification of persistent effects on average wages is of interest, the implications for policy depend on the reason for the wage increases. On one hand, the increase in local average wages may reflect an actual increase in earnings opportunities for certain individuals—those who would have resided near the plant in any circumstance and who might have 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 affecting any single individual's earning opportunities.

We therefore test for individual-level effects by studying the long-term outcomes of workers born in the treatment and control counties or residing in those at the time of the 1940 census. By examining outcomes of individuals who resided in these counties as children before the start of the War, we are able to isolate the causal effects of plant construction on long-run earnings opportunity apart from endogenous mobility during and after the war. We draw on two new data sources for this analysis: estimates of child outcomes by county from Massey and Rothbaum (2019) using data from the 1940 Census linked to IRS tax returns from 1974 and 1979, and the Current Population Surveys from 1990 onwards linked to the full Social Se-

curity earnings histories from 1978 onwards and place of birth for respondents that can be linked to their Social Security Numbers[1]. Though these data do not permit the differences-in-differences employed in the county-level analysis, we verify that remaining covariates are well-balanced in the baseline specification, that county-level differences-in-differences estimates closely match post-period differences, and that results are extremely robust to additional controls. In addition, we verify that linkage rates are similar for children regardless of treatment status.

We find that growing up in a place where a large, "good-paying" plant opened during the war had a significant impact on individual men's earnings as adults in the 1970s. More specifically, these plant openings had impacts on long-run 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—equivalent to being born into a family with a 1939 earnings rank that was four percentiles higher. At the same time, we find little effect on individuals whose families had above-median incomes when they were children in 1939. (Additional results on human capital development and other mechanisms from the CPS-based data are undergoing disclosure review.)

These findings highlight the large role that government-supported manufacturing expansions played in the spread of "good-paying" midcentury manufacturing jobs. Moreover, they provide direct evidence that the industrial expansion during World War II caused an increased rate of upward mobility during the middle of the 20th Century. The effects we identify are large in magnitude relative to the costs: At a 10% local income tax rate, local governments would have recovered roughly half of the cost of the plant construction from the payroll increment alone.

This work adds to a growing literature evaluating the regional effects of place-based policies and large manufacturing investments. The potential for "big push" investments to have spillover effects throughout regions has been highlighted in influential work by Ellison, Glaeser and Kerr (2010) and Murphy, Shleifer and Vishny (1989). A study of "Million Dollar Plants" that opened in the 1980s and 1990s by Greenstone, Hornbeck, and Moretti (2010) found while that new plant sitings increase regional productivity, there were limited effects on local earnings—yet little is known about whether plant sitings had similar effects when manufacturing plants offered better work opportunities midcentury. Work by Moretti and Kline on the Tennessee Valley Authority (2014) found that a major regional development push in the 1930s had long-run impacts, but it is difficult to conclude which aspects of the policy drove the results. A contemporaneous study of World War II plant investments by Jaworsky (2015) found little evidence of long-run effects of total plant spending among counties in the US South, though that analysis does not focus on the quasi-experimental sitings of new public plants. Relatedly, this paper contributes to a growing empirical literature in macroeconomics that exploits variation in military spending over time and place to estimate the

[1]Using personally identifiable information (PII), such as name and date of birth, survey respondents are linked to their Social Security Numbers (SSNs). The SSNs and PII are then removed from the file and replaced with a Protected Identification Key (PIK), which can be used to link individuals with administrative earnings data such as the Detailed Earnings Record (DER) and Numerical Identification System file (Numident). The Numident contains information on place of birth, which was matched by Black et al. (2015) to their county of birth. For more information on the assignment of PIKs, see Wagner and Layne (2014).

short and long-run impacts of different types of government spending on regional economic performance; Barro (1981), Ramey (2011), Nakamura and Steinsson (2014) are key papers in this vein. Related to this paper, Brunet (2018) uses a complementary data set on WWII war supply contracts to study the short-run macroeconomic multiplier effects of war spending on regional economies.

This paper joins a growing literature that explores the long-run effects of World War II on various aspects of the post-war economy. Goldin and Margo (1992) were the first to clearly document that a distinct "great compression" in the wage distribution occurred during the 1940s. Recently, Brunet (2018) has studied the short-run macroeconomic stimulus effects of war spending using detailed data on military expenditures. Angrist and Krueger (1989) looked at the impact of the military service GI bill on post-war education and earnings, finding that most of the effects appeared to be driven by selection. Fishback and Cullen (2013) examine the relationship between aggregate local spending and post-war retail sales and population growth, and appear to find relatively small effects. Higgs (1992, 2004) and Mulligan (1998) present calculations that suggest the effects of the war on the labor force and postwar growth was minimal. Several papers, notably Goldin (1991), Goldin and Olivetti (2013) and Acemoglu, Autor, Lyle (2004) have measured the impacts of the war on female labor force participation in the post war period.

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. In Section 3, 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 4 presents the baseline results pertaining to post-war manufacturing output, employment, and wages. Section 5 expands the analysis to examine effects on broader labor market outcomes. Section 6 discusses what one can and cannot reasonably conclude from these findings, and suggests several tests to empirically distinguish between the potential mechanisms at play. Section 7 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 (USAF 1955, 331). 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.

While the government was the source of demand for materiel, the vast majority of production was done by private firms under contract. Given the urgency of the mobilization and the need to make dramatic changes to product lines, competitive bidding was ruled out from the start. 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).

However, few firms had the capacity to meet demanded output levels of any war good, so the expansion required massive investment in new industrial capacity.[2]Not only was the capital stock insufficient to meet demand, but industry in 1939 was also largely oriented to activities such as agricultural processing, textiles, apparel manufacturing, and wood/paper processing that were difficult to convert to production of wartime goods like airplanes, ordnance, and explosives.[3] Most production contracts therefore required some expansion of firms' productive capacities. In many cases, firms simply enhanced existing plants to increase output of pre-war goods (like canned food, uniforms, or iron) or converted and retool of factories to make different products. However, when a contract called for production of a new type of product on an unprecedented scale, a completely new plant was often required.

By the end of the War, over $20 billion in 1940 dollars—approximately $300 billion in 2015 dollars, over twenty percent of 1939 GNP, and approximately fifteen percent of all war outlays—had been spent on industrial capital investments in support of war supply contracts. [4] This is a particularly massive increase in comparison to the outstanding manufacturing capital stock at the dawn of the war, which was valued at approximately $40 to $60 billion dollars in 1940 currency.[5] Of the over $20 billion spent on plant expansions, the majority was spent on brand new factories, particular on a small number of very large new factories. (WPB 1940A, WPB 1940B). Notably, just three hundred large plants account for ten billion dollars of the wartime plant expansion–approximately half of the value of plant put in place.

[2]American industry had been poorly organized during the First World War, and as a result never truly converted to coordinated production of war material. While manufacturing had boomed during the 1920s, industrial output lagged during Great Depression and many plants had closed.

[3]While the automobile sector had grown since the dawn of the century, it was still dwarfed by these other, more traditional manufacturing sectors; and, while some now-famous aircraft companies had opened doors before the outbreak of the war, aircraft production comprised a trivial part of American manufacturing capacity in 1939.

[4]One should compare this to the recent American Recovery and Reinvestment act, which authorized $111 billion dollars for infrastructure construction. Even after adjusting prices, the real economy has expanded over sevenfold since WWII–thus, $111 billion is a dramatically smaller intervention relative to the size of the economy than the WWII plant expansion. One might also compare this to about $15 billion (1940 dollars) spent on public works during the New Deal from 1933-1939 (under the PWA and WPA).

[5]The Civilian Production Administration (successor to the WPB) estimates place total wartime investment in plant and equipment through the end of 1945 at $23 billion. They estimate of 1939 manufacturing plant excluding land value as $39.5 billion—amounting to an expansion worth 58.4% over the pre-war plant stock, and the federally financed portion worth 43.5% of the total prewar stock (CBP 1946). The same study notes that capital expenditures in facilities in 1939 totaled $1.2 billion, while annual *private* investment in war manufacturing facilities was about $1.8 billion per year on average throughout the war; the publicly financed investment–which was approximately three times the amount of private investment–was entirely supplemental.

2.2 WWII's Big New Plants

While many essential supply contracts required investments in large new plants, contracting firms were generally reluctant to stake their own finances on massive new facilities. The willingness of firms to raise private capital for plant expansions depended on the amount of risk such a plant entailed. Firms were often eager to have the government share the cost of smaller plants with clear long-run value such as machine tool shops in major industrial hubs or petroleum refineries. However, firms and their financiers were much less eager to risk capital on very large industrial plants with highly uncertain post-war value like ordnance factories or bomber assemblies. While the firms might have recouped costs after a long war, an unexpectedly quick end to the war would radically reduce plants' profitability since repurposing for civilian production would be costly.

Compounding this risk, military planners determined that new plants should be sited outside of established manufacturing hubs when possible. 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, concentration of 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. [6] 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 (USAF Historical Division 1955). Further, assembly of airplanes typically required that plants be built along 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. 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" (USAF Historical Division 1955). Nonetheless, 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 US government financed the plants directly. While the contracted firm would

[6] 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." Similar standards were later applied when entirely new aircraft assembly plants were necessitated by the expansion.

typically construct and operate the facility, the facility itself would be fully 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 right of first refusal offered to the operator.[7] Such arrangements allowed the government to assume the full financial risk of these crucial plants. (White 1980)

Outside of major manufacturing hubs, siting decisions were driven by fairly idiosyncratic concerns. Any city with basic transport connectivity, access to electricity and water sources, and sufficient population to staff a plant could be suitable. In practice, the foremost challenge was to find a parcel spanning hundreds of acres that could be purchased or seized with minimal difficulty. Often times, entrepreneurial federal representatives and local officials in remote areas, who typically could not attract private investment outside of wartime, would offer military officials full disposal of local public services and even free land if they located war plants in their jurisdictions.

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" (Whetten 2011). 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.

After the war, most plants built during the war were either converted to civilian production after the war or put to continued use for defense production as the Cold War began. Plants specializing in the production of ammunition and ordnance were often kept under government ownership, but operated by private firms under contract. Most other plants owned by the DPC and other government agencies were sold to private sector firms, generally at a small fraction of their construction price (WAA 1947). While some wartime contractors purchased the plants they had operated, others declined to do so or were prohibited

[7] Business interests and anti-New Dealers were highly concerned about government ownership of productive facilities that might potentially compete with private interests in the post-war. The authorizing legislation therefore required that plants only be operated by private firms after the war, but liquidated in such a way that allowed incumbent firms to get a foothold in the postwar civilian economy first.

from doing so due to antitrust concerns.[8] For example, Ford declined to purchased the massive Willow Run plant it build outside Ypsilanti, Michigan (on farmland personally owned by the Ford family) and it was instead sold to Kaiser Jeep 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

To study large plants, we digitized a War Production Board (WPB) database of all substantial capital expenditures made for the sake of war production. Although procurement contracts were negotiated by more than a dozen distinct government and military offices, all contracts for the production of war goods were reported the War Production Board and all requests for public funds, tax write-offs, and other public resources for plant expansion required WPB approval, so as to ensure efficient use of scarce materials and elimination of redundancies. Hence, the WPB was able to compile administrative databases detailing the placement of each supply contract and the universe of investments with a war rationale.[9] In particular, we draw from a 1945 WPB data book, *War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator* (WPB 1945a).[10] 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. The cost data is subdivided into privately financed and publicly financed amounts, and those amounts 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 case studies of new plants studies identified based on external information. In some cases, plants were explicitly identified in the data book as new plants, in which we classify the plants as new regardless of their investment composition.[11] Next, we identify plants as "publicly-financed" investments if 99 percent or more of capital

[8] 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)

[9] All capital expansions with a war rationale were eligible for tax incentives or public funds, which required WPB approval. All plants involved in war production sought these incentives, and WPB approval was almost never denied, since the requests occurred after military procurement agencies had completed negotiations. One should note that use of scarce construction inputs (steel, iron, concrete, etc.) was highly restricted in the civilian sector during the war, so these plant investments account for the universe of industrial construction during 1941-1945.

[10] Very few new projects were authorized in the final year of the war–these data account for approximately 90% of all authorizations.

[11] 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

expenditures on structures came were from public agencies; we classify remaining new plants (partially) privately financed.[12] 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. However, even within this group the bulk of expenditure occurred in a small subset of very large plants.

To study effects on local labor markets, we use this plant data to define county-level treatment variables, discussed in more depth below. The county-level outcome data are tabulations of records from the Economic Censuses and the Decennial Population Censuses compiled in the County Data books and in work by Haines (2010) covering the years 1920 through 2012. To account for prewar policy interventions, we include data from Fishback et al. (2005) that account for various geographical features and that measure the severity of the Great Depression and local exposure to various New Deal interventions. We also aggregate records of postwar defense contracts reported on DD350 forms the county level based on the locations of the contracting firms. In our analysis, we drop counties that have no records of wages paid to manufacturing production workers in any year between 1920 and 1955 or with fewer than 10 such workers in 1939. We also exclude counties in Alaska, Hawaii, and Virginia due to significant changes in county definitions during the observation period.

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

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").

[12] 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.

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. [13]

4 Research Design

4.1 Empirical Strategy

The goal of this paper is to study how siting a large, publicly-financed plant in a given region during WWII impacted its labor market and the earnings of its pre-War residents in the long run. As noted above, the large facilities paid for by the US government were investments that that no firm would have made in the same location if not for circumstances created by the war. In practice, the location decisions were driven by idiosyncratic short-run strategic considerations, independent of longer-term economic forecasts. Conceptually, our approach is compare two observably similar counties, neither of which would have likely been the location of a large plant opening in the counterfactual world where no war occurred, and only one of which actually received a plant due to short-run strategic war needs. If the war-specific factors drove why one of the two similar counties got a plant, as opposed to longer-term labor market and location characteristics, then the similar county with no wartime plant can be used to infer the counterfactual potential outcomes of the affected county if it had not been "treated."

This local labor-market 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

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

a county received a large, public plant. To increase statistical power, we focus on counties where plants are not only large in absolute terms, but also large relative to the local labor market.[14] Only in such cases could plants plausibly have large effects. Accordingly, we rank the 202 counties with any large, new public plants by the total spending on such plants per capita in 1940 and designate the half of counties with the most intensive spending as the treatment group (101 counties in total). We then omit the remaining 101 counties from the pool of potential comparison counties. Second, to exploit the full variation in investment, we use the total spending on large, new public plants per capita in 1940 as a continuous exposure measure.

We identify the causal impacts of plant openings by comparing these treated counties to comparable control counties, both before and after the war. Since very rural counties could not plausibly have supported a new plants, and since large manufacturing hubs would have likely received major manufacturing expansions in any scenario, we allow for selection into treatment based on basic 1930-1935 county characteristics. Specifically, our baseline set of observable characteristics includes several 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). We explore how sensitive results are to the choice of observables in the analysis below. 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 postwar economic development.

We estimate treatment effects using two different estimators. We use the binary treatment variable to estimate the average treatment effect on the 101 treated counties (ATOT) using a propensity score reweighting estimator. This essentially amounts to creating a synthetic control group based on the selection variables, and nonparametrically estimating the treatment effects. To do this, we first estimate a probit regression of $TREAT_i$ on the same covariate set X_i^{1930}, that is:

$$\Pr(Treat_i = 1 | X_i^{1930}) = \Phi(\beta X_i^{1930}) \tag{1}$$

I estimate propensity scores $\hat{p}_i$ as the the predicted treatment probabilities from the fitted model $\hat{p}_i \equiv \Phi(\hat{\beta} X_i^{1930})$. All observations with propensity scores outside the overlap region (almost the entirety of which are control observations with propensity scores below the range observed in the treatment group) are trimmed from the sample, as they have no comparable counterpart in the opposing sample. 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. Imbens (2004) shows that under the selection on observables assumption, the

[14] While large cities like Detroit and Chicago did receive large new public plants, these investments were minuscule relative to the size of the existing manufacturing base.

propensity score weighting estimator

$$\hat{ATOT}_t = \frac{1}{N_{Treat}} \sum_{i:Treat_i=1} Y_{it} - \frac{\sum_{i:Treat_i=0} Y_{it} \frac{\hat{p}_i}{1-\hat{p}_i}}{\sum_{i:Treat_i=0} \frac{\hat{p}_i}{1-\hat{p}_i}} \tag{2}$$

is a consistent estimator of the average treatment effect on the treated. Here, ΔY_{it} denotes the change in an outcome variable from 1940 (or 1939 for some variables) to some other year t. In the Rosenbaum and Rubin (1983) potential outcomes framework, letting i's potential outcome change if treated be $Y_{1i,t}$ and its potential outcome change if untreated is $\Delta Y_{0i,t}$, the conditional independence assumption can be written as $Y_{0i,t}, Y_{1i,t} \perp TREAT_i | X_i^{1930}$. Written in terms of potential changes, this formalizes the parallel trends assumption necessary for causal inference in difference in differences designs.

To study the effects of a marginal dollar of investment per capita, we also examine effects of the continuous exposure measure using standard OLS regressions. Specifically, we estimate equations of the form

$$\ln Y_{it} = \alpha_t + \beta_t \frac{Expend}{Capita}_i + \gamma_t X_i^{1930} + \epsilon_{it} \tag{3}$$

t is an outcome year, δ_{st} is a state fixed effect for the specific outcome year, X_i^{1930} is the 1930s 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. Under the assumptions of linear effects and selection-on-observables (equivalent to parallel trends in this case), specifically

$$\epsilon_{it} \perp TREAT_i | X_i^{1930} \tag{4}$$

each β_t is identified. We estimate these regressions on the full analysis, including all 202 counties with treatment spending. Rather than define an explicit control group, this approach relies on the linear structure of the model to partial out the effects of confounding covariates.

4.2 Identification in Market-Level and Individual-Level Analyses

In our baseline analysis of market-level effects, we use a panel dataset of county-level outcomes that facilitates a difference-in-differences design. We implement this design by replacing county level outcomes in year t, Y_{it}, with differenced in outcomes relative to 1940 levels, $\Delta Y_{it} = Y_{it} - Y_{i1940}$. Doing this facilitates causal inference under a weaker set of assumptions on the data—rather than assuming that treatment and control outcomes would have been the same in the absence of differential treatment conditional on observed covariates, it only needs to be the case that outcomes would have changed the same between 1940 and the outcome year. The validity of this assumption—the standard "parallel trends" assumption in difference-in-differences designs—can be partially tested by examining whether plant spending is correlated with differential changes in outcomes prior to the War. For this reason, we only use 1930 covariates in

our baseline in order to examine whether outcomes for counties with different levels of public investment trended similarly between 1920 and 1940.

For our individual-level analysis, we cannot apply the same differences-and-differences approach, because we do not observe pre-War outcomes for individuals in our data sources. Our approach, therefore, is to measure post-period differences between individuals who resided as children in counties that received plants compared to those who resided in counties that did not. Making causal inferences based post-period comparisons requires a stronger set of identifying conditions. Specifically, it must be the case that the assignment of public plant investments was as good as random, conditional on observable covariates, so that outcomes would have been identical in the absence of any investment. The discussion in Section 2 suggests that this is a plausible assumption. We examine the validity of this assumption in several ways. First, we test whether a wide set of pre-treatment covariates are well balanced across public investment levels conditional on a basic set of 1930 covariates. Second, we compare our differences-in-differences estimates of market-level effects to estimates based on post-period differences only, which allows us to test whether differencing outcomes affects the results. If both approaches yield the same estimates, this would suggest that differencing is not essential for identification of causal effects. Finally, we test robustness of our results to additional controls for a wide set of county-level and individual-level covariates.

4.3 Sample Characteristics and Covariate Balance

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. The plants largely specialized in production of steel (especially in the West), aluminum, aircraft, chemicals, and explosives (especially in the Midwest). To visually compare the spatial distribution of treatment counties to that of the re-weighted control counties, Figure 3 maps how propensity score weights vary across counties. Along with the 101 treatment counties (each with weight 1), there are 1,117 control counties in the trimmed sample with a weight of sums equal to 97.3. The map divides counties into four differently-shaded bins such that each bin has equal amounts of *weight* (rather than split control counties into weight quartiles with equal *numbers of counties*). Similar to the treatment counties, the highest-weighted control counties tend to be in small or middle sized heartland cities (apparent in the top 10, listed in Appendix Table 12) with a similar geographic distribution (even though no geographic information is used in the calculation of the propensity score).

Under the conditional independence assumption and correct specification of the propensity score, one should observe covariate balance between the treatment and the re-weighted control group—both for conditioning variables the conditioning variables themselves and other pre-treatment variables that are potentially correlated with potential outcomes. Testing for covariate balance can therefore partially validate the identifying assumptions. Table 2 presents tests for covariate balance for the 1930 variables included in estimation of propensity score, for additional 1930 variables excluded from the propensity score, and for

1940 variables that are not used in determination of the weights. We find that treatment counties are different than the rest of the country as a whole, suggesting treatment is not *unconditionally* randomly assigned. However, consistent with the selection-on-observable assumption, there is no evidence of imbalance between the control group and the treatment group once the re-weighting is implemented.

More generally, if the identifying assumption holds, then conditional on 1930 observables there should be no systematic relationship between the treatment and outcomes in 1939 before war broke out. Accordingly, for each outcome, we estimate effects on changes ending in wide range of years both before and after the war. We then plot the effects over time in a event study diagrams to facilitate inspection of any potential pre-period trend differences that might confound causal inference.

5 Long Run Impacts on Local Labor Markets

5.1 Manufacturing Sector Outcomes

I 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.

The baseline results are displayed in Table 3. The left-hand panel reports ATOT effects on changes in the outcome variables from 1939 to either 1963, 1982, or 2002. The right hand panels show OLS estimates of the effects of an addition $1000 (1940$) per capita in public plant spending for the same years and outcomes. Each estimate is from a separate regression. The outcomes are log value added (which is measures the production level as value of outputs produced less the cost of purchased input parts), log average annual earnings of manufacturing workers involved in production (as opposed to clerical and managerial positions), the log employment level of manufacturing production workers, and the log manufacturing establishment count. In each case, we use the baseline set of 1930 observables discussed above as either determinants of the propensity score or as controls, depending on the estimator.

Several findings stand out. The effects of a plant siting on aggregate manufacturing output is large—production increases in treatment counties by approximately 35 percent more than in control counties in 1963. Moreover, these effects are highly persistent. As late as 2002, manufacturing output remains .25 log points higher in treatment counties than in the re-weighted counterfactual group. These increases in production are reflected in a comparable rise in manufacturing payrolls. This increase in payrolls reflects not only a large increase in employment, but also a significant and persistent increase in average production wages.[15]

[15]To interpret the magnitude of these effects, the median treatment county had 5566 production workers employees in 1954, which

Wages rise by an additional 8 percent in treatment counties, and that same difference persists through 2002. 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.

To provide a broader look at the time pattern of these effects, Figure 4 plots the reweighting estimates for each economic Census year from 1920 to 2012. These plots show that the years examined in Table 3 are consistent with the broader pattern. For all outcomes, initial effects peak in 1953. The small effects recorded 1947 are likely reflections of the gradual conversion of plants from defense production to civilian production after the war.[16] Interestingly, although the employment effects decline substantially during the 1970s and 1980s, the wage effects remain consistent during this period. Even as manufacturing jobs became more scarce later in the century, the remaining manufacturing jobs in these locations continued to 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. 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.4 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 identifying causal effects.

In Figure 5, we examine how sensitive the results are both to the choice of variables in the propensity score and the choice of estimator. The solid black line in each panel displays the wage effects for all years. In Panel (a), we compare this to re-weighting estimates where the propensity score is based solely on 1930 log population. We also estimate the effects of the same binary treatment variable on the same analysis sample (trimming the same overlap sample use in the propensity score re-weighting estimation) using simple OLS estimation using the pre-period covariates as linear controls. The results in all years for both methods

accounts for roughly one-fifth of employment in the typical treatment county. 20% fewer production employees in the typical treatment county would amount to over one thousand fewer jobs.

[16] Reconversion was often a lengthy process. Because these plants were owned by the government under military sponsorship, the military had to first deem these plants surplus and no longer necessary for immediate defense purposes before they could be sold or leased and retooled for civilian production. Following the surplus declaration, plants were auctioned off by government bureaucracies designed to assure the public that they were getting a good deal and to assure big business that they were not trying to flood any particular market with cheap capital. Once a deal had been agreed to, the plants needed to be re-equipped for production of civilian goods. As a result, few large public plants plants began civilian production before 1947–and many plants only began civilian operations even later. Thus, few treatment plants would have been operating at full capacity until the very end of the decade. (WAA 1947; White 1980).

are almost identical to the baseline results. In Panel (b) we plot results when we add all the 1930 and 1940 variables examined in Table 2 to the set of conditioning variables—again, the results remain nearly identical in all years. In the background of Panel (b), we display estimates from intermediate specifications that only include 1930 manufacturing variables or include all 1930 covariates in Table 2 but no 1940 covariates; the effect levels and the time patterns are similar in all cases. In Panel (c), we estimate the treatment effects using the same propensity score but implementing a propensity score matching estimator, rather than the reweighting estimator, again finding similar results. Appendix Figure A.3 shows that the Value Added effects display similar robustness.

Though the mere presence of sizable post-war manufacturing effect may not be surprising given the physical presence of a large plant, the persistence of these effects—particularly the wage effects—is of note. The increments to manufacturing employment and activity in treatment counties outlasts any realistic depreciation horizon for the initial war-time infrastructure. Such persistence implies that either the operating firms continued to reinvest and upgrade these former war plants throughout the post-war period, or other firms made additional investments in the locale of the former war plant that outpaced investment in untreated counties. We explore these hypotheses below

5.2 Broader Labor Market Outcomes

I next examine how the effects on manufacturing employment and wages documented above translate into labor-market-wide impacts. Table 4 presents re-weighting estimates of difference in differences effects on a wider set of outcomes, using the same specification as in the left panel of Table 3. Since these outcomes are drawn from several surveys, the observation years differ across outcomes and are noted in the table. The base year also differs slightly across outcomes—whereas 1939 is the base year for all manufacturing and retail outcomes, 1940 is the base year for all other outcomes.

Receiving a publicly financed plant during the war led to a persistent increases in both employment and total population of roughly equal proportion. The full time-path of these effects is displayed in Panel (a) of Figure 6. Treatment plants experience a permanent population boost that outlived the effects on manufacturing employment. In 1960, the effect on employment was 11.6 percent; though smaller than the manufacturing employment effect in log terms, this effect reflects a significantly larger increase in levels. Given that 1960 employment in treatment counties was roughly 63,119, whereas average production employment in 1963 in these same counties was 12,294, the increase in manufacturing jobs explains only about one-third of the total increase in employment. Indeed, we find that retail sector employment persistently increased in proportion to overall population. Although plant sitings shifted the employment distribution towards manufacturing in the short run, we find that the initial increase in manufacturing's share of jobs declines over time.

While the employment effects spread far beyond the manufacturing sector, increases in manufacturing wages were not shared by all other sectors. We find 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). Thus, although the treatment does have a significant effect on median family incomes, the evolution of the effect displayed in Panel (b) of Figure 6 appears to reflect the rise and fall of the manufacturing employment level over time. 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.

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 did 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 Table 5. 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 con-

tinued 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.[17] 5 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.

However, although ordnance plants get substantially more military spending after 1945, these plans do not drive the main results of the paper. Figure 7 presents event studies for several of the main outcomes, estimated separately for ordinance and general manufacturing plants. Both types of plants appear to have been sited for idiosyncratic reasons, as neither treatment is associated with differential trends prior to the War. Yet, not only are the initial effects of the general manufacturing plants larger, they are much more persistent. 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. By contrast, the effects of the general manufacturing plants hardly dissipate at all after the 1950s. Moreover, this latter group of plants has significantly larger effects on the broader labor market, including wages outside of manufacturing. 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.

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 differences-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.5. 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 esti-

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

mated effects on value added, then the implied effects of wartime investment on postwar investment are large. Figure A.5 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 complementaries 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 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 Identifying Effects on People versus Places

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 the same differences-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.[18] Our approach, therefore, is to measure post-period differences between individuals who resided as children in counties that received plants compared to those who resided in counties that did not. 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 6 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

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

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—variables for which we do not have pre-war measurements at the county level. We find the effects on median male wages are similar in magnitude to the effects on average manufacturing wages, while effects on female median wages are positive but significantly smaller in magnitude. 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

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 the their CPS year in the 1990s or later. However, we find no evidence of selective attrition that may bias our estimates—there is no relationship between county-level public investment and the share of county-level birth cohorts that eventually appear in the CPS-DER file. Third, the DER file contains total wage and salary earnings at an annual level, so we cannot differentiate between part-time work and low-wage work. We therefore classify individuals as full-time wage workers in a year if their earnings were at least the level one would earn working 2,000 hours at the Federal minimum wage in that year; we then test separately for earnings affects among full-time workers and for effects on the probability of participating in the full-time workforce on the extensive margin. We winsorize earnings at the 99th percentile.

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

$$\ln Y_{ict} = \alpha_t + \beta_t Treat_c + \gamma_t X_i^{1930} + \sum_{\tau=1928}^{1941} \delta^\tau \mathbf{1}[YOB_i = \tau] + \epsilon_{it} \tag{5}$$

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 7 indicate that the construction of large, high-paying manufac-

turing 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 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 8 show the effects are concentrated among white men. We find no average effect for either black men or women of any race.[19] 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.6 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 9 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 the 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 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 9 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?

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

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 upwards increase in 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 children depending on the 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. Results are presented in Table 10.

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. 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.

Table 11 breaks out the results among further subgroups. In contrast to the results in the CPS-DER, we find similar effects for men and women. This difference is largely an artifact of our use of *individual-level* wage and salary data in the prior analysis but *tax-unit* level AGI in the present analysis. We find little difference in effects across cohorts, in contrast to our results in the CPS-DER. We again find no effect on upwards mobility for black men or women born to parents of below-median income. However, we find dramatic effects on upwards mobility—increases exceeding three percentiles—for black men and women born to parents with incomes above the national median in 1939. While only a relatively small minority of black households in 1939 had above-median incomes, these results suggest only black children from the

best-off families were able to benefit from increased availability of high-wage manufacturing jobs.

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 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 Moretti and Kline (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 plans 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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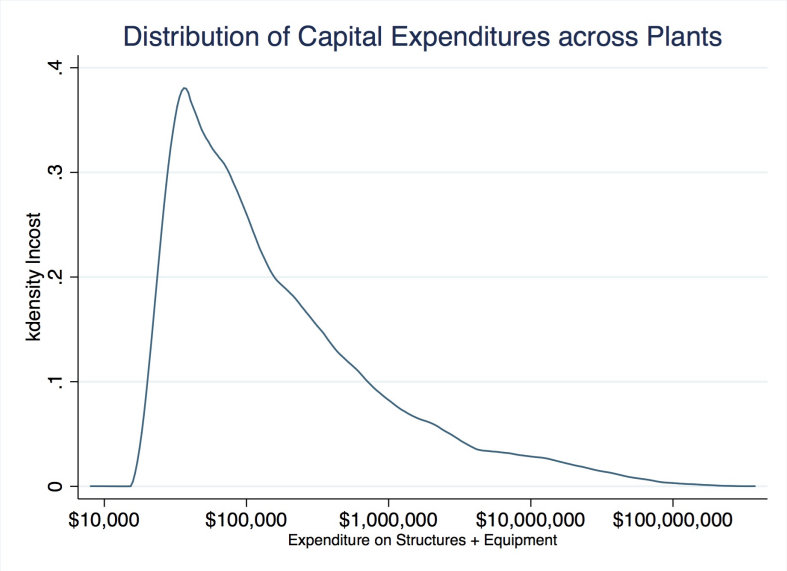
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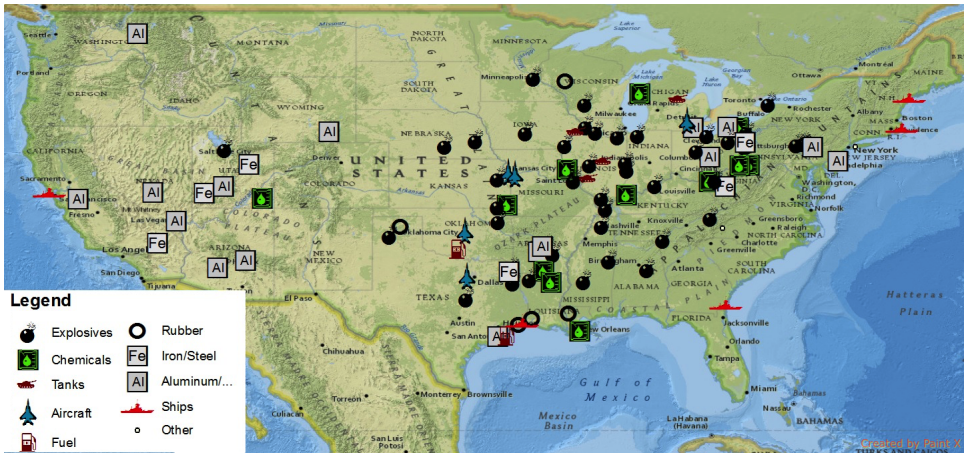
Figures and Tables

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



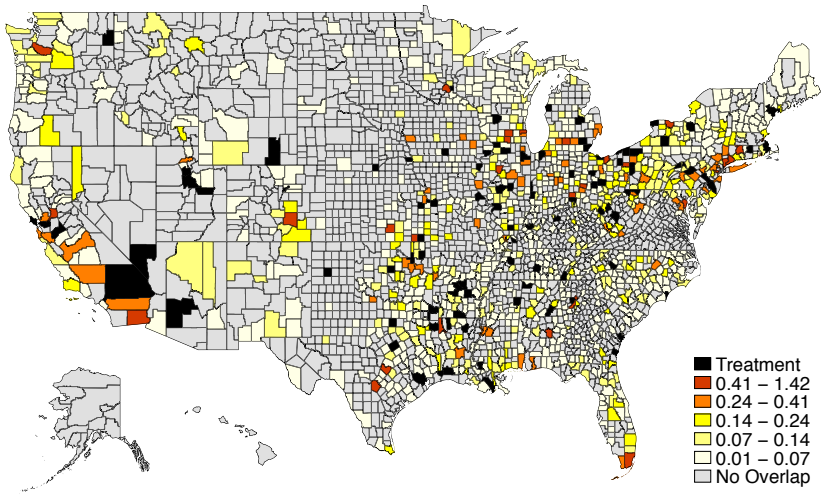
Notes: Figure is kernel density plot of the distribution of wartime all capital expenditures (structures and equipment, in logged 2014 dollars) across all establishments listed in WPB records.

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).

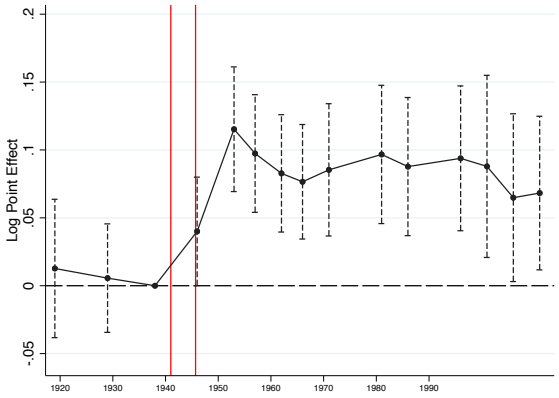
Figure 4: Effects on Manufacturing Outcomes



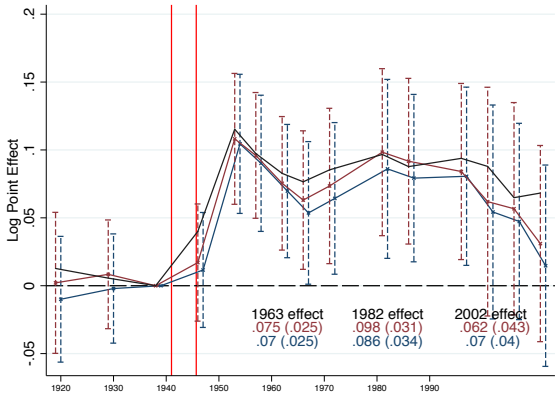
Notes: Panel (a) plots propensity-score reweighting estimates of ATOT effects of the binary treatment on changes in the outcome variables from 1939 to each outcome year. Sample is 101 treatment counties and 1117 control counties in overlap region. Panel (b) plots OLS estimates of the effects of an addition $1000 (1940$) per capita in public plant spending for the same years and outcomes, N=1,981. Each outcome variable is colored according to legend. 95% confidence intervals and select point estimates are displayed for wage effects only. 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). Each point estimate is from a separate regression.

Figure 5: Robustness of Wage Results to Alternative Specifications

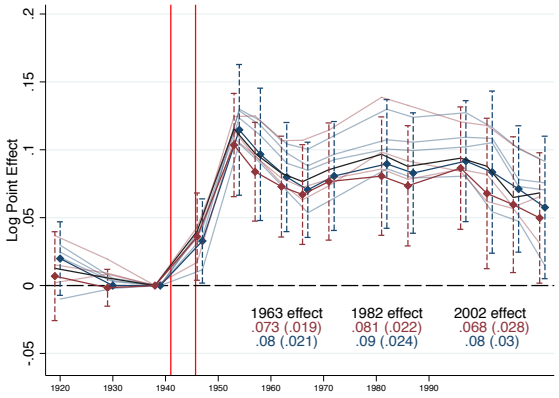
(a) Baseline PS Specification



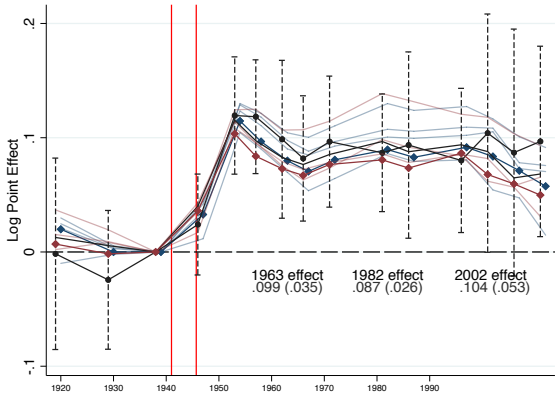
(b) OLS + PS: Only 1930 Ln Population Covariate



(c) OLS + PS: All 1930 Covariates and All 1940 Covariates



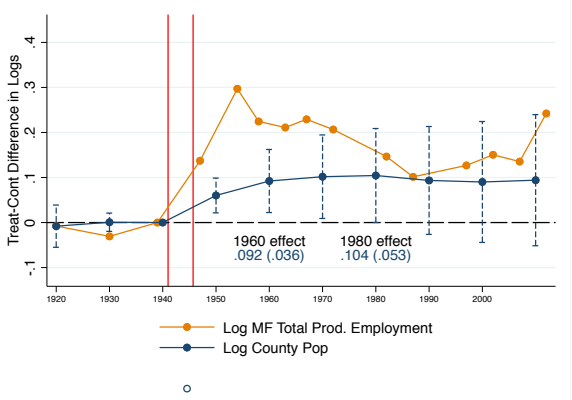
(d) Propensity Score *Matching:* Baseline Covariates



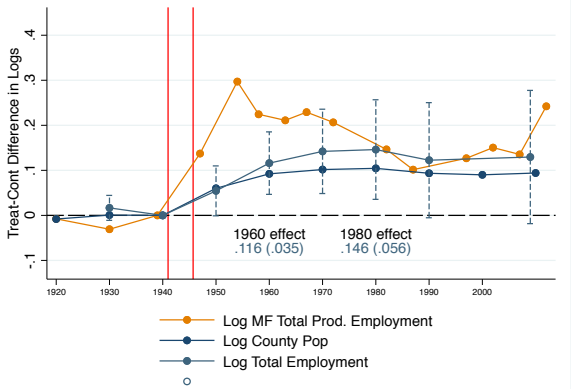
Notes: Figure displays estimates of ATOT effects of the binary treatment on changes in the log production wage from 1939 to each outcome year with different sets of conditioning variables and/or different estimators. The black line in each panel reproduces the propensity score re-weighting (PS) wage effects from Figure 4. Panel A plots PS and OLS estimates of binary treatment effect estimates using only 1930 Log Population as a conditioning variable. Panel B plots PS and OLS estimates of binary treatment effect estimates using all 1930 and 1940 covariates in Table 2 as conditioning variables. Panel B also displays intermediate specifications described in the text in background. Panel C presents propensity score matching estimates using baseline propensity score.

Figure 6: Broader Employment Outcomes: Reweighting Estimates

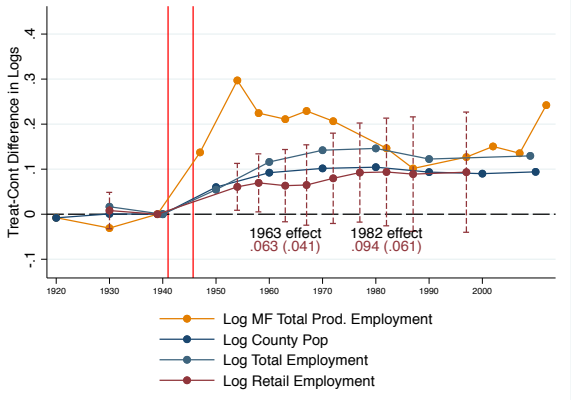
(a) Population



(b) Employment



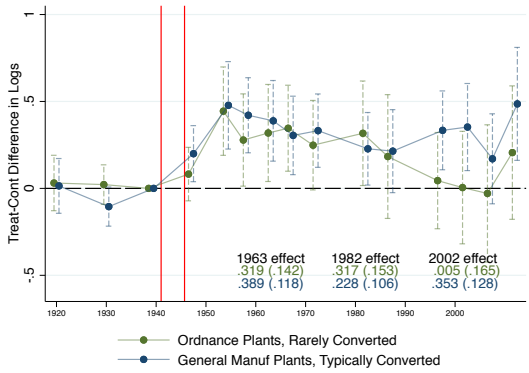
(c) Retail Employment



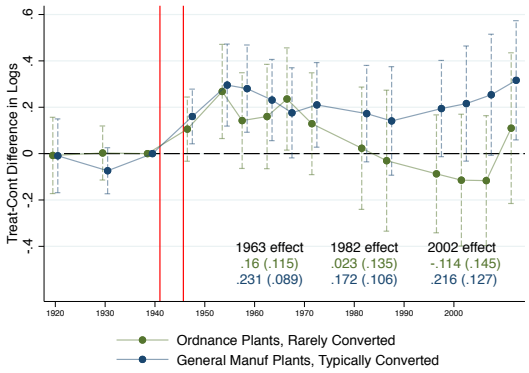
Notes: Panels plot propensity-score reweighting estimates of ATOT effects of the binary treatment on changes in the outcome variables from 1939 to each outcome year. Sample is 101 treatment counties and 1117 control counties in overlap region. Each outcome variable is colored according to legend. 95% confidence intervals and select point estimates are displayed for one outcome in each panel. 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). Each point estimate is from a separate regression.

Figure 7: Heterogeneity by Plant Type

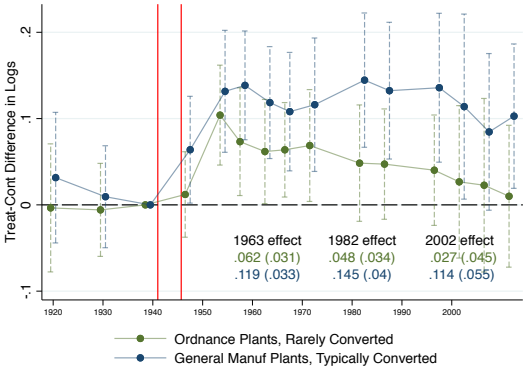
(a) Outcome: Log Mfg Value Added



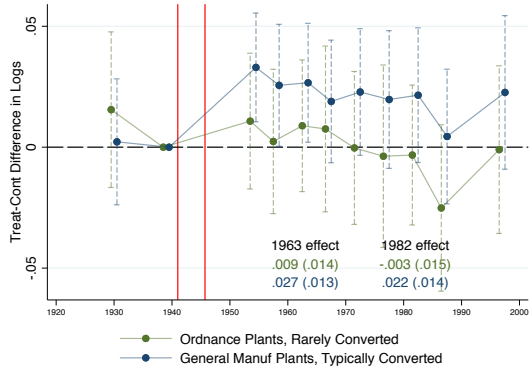
(b) Log Mfg Production Employment



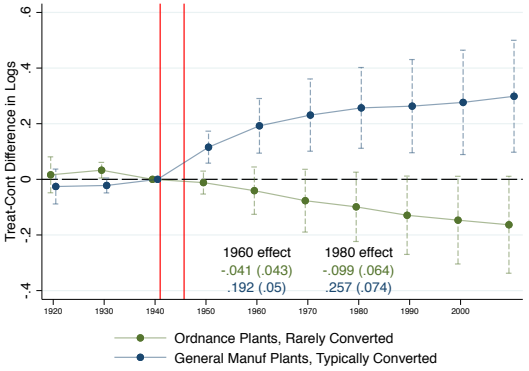
(c) Log Mfg Average Production Wage



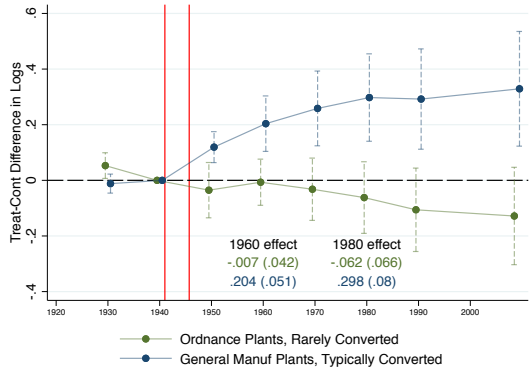
(d) Log Avg Retail Wage



(e) Log Total Employment



(f) Log Total Employment



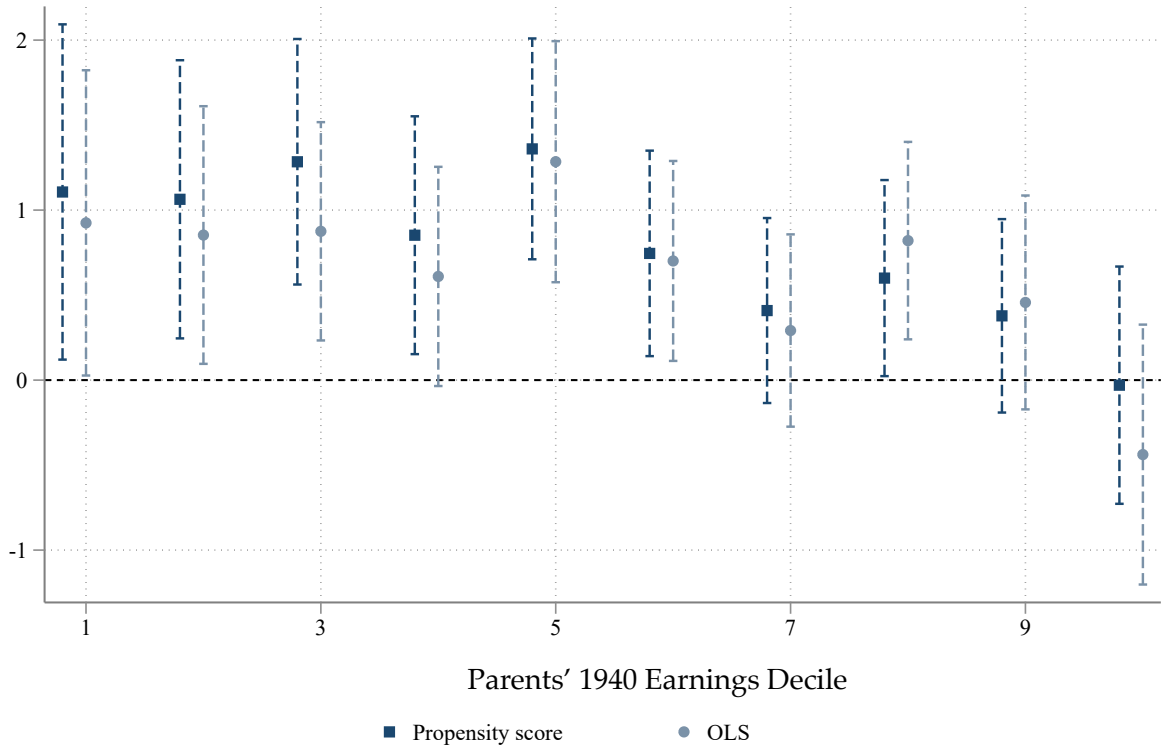
Notes: Panels plot propensity-score reweighting estimates of ATOT effects of one of two binary treatment variables: 1. treated and largest plant is ordnance/ammunition facility, and 2. treated and largest plant is other manufacturing facility. For each of these treatments, separate propensity scores are estimated and reweighting estimates are calculated on corresponding overlap samples. 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 5 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



Notes: Table reports propensity-score reweighting estimates of ATOT effects on levels (not differenced) of the outcome variables, as well as OLS estimates of binary treatment effects conditional on identical covariate sets. 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. Sample is 101 treatment counties and 1117 control counties in overlap region. All specifications use the covariate set from Columns (3) and (4) of Table10. 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

1930s Covariates in Propensity Score					**Covariates Excluded from Propensity Score**				
Variable	Diff´	*t-stat*	RW Diff	*t-stat*	Variable	Diff´	*t-stat*	RW Diff	*t-stat*
Log Population	0.97	***12.37***	0.04	*0.68*	1940 Log Population	0.99	***12.54***	0.04	*0.65*
Log Employment	1.02	***12.53***	0.04	*0.76*	1940 Log Employment	1.03	***12.27***	0.03	*0.45*
Log Mfg Production Emp	1.86	***10.83***	0.09	*0.96*	1940 Log Mfg Production Emp	1.89	***10.64***	0.12	*1.35*
Log Mfg Production Wage	0.19	***5.47***	0.01	*0.61*	1940 Log Mfg Production Wage	0.22	***5.24***	0.01	*0.33*
Log Mfg Value Added	2.05	***11.70***	0.10	*0.97*	1940 Log Mfg Value Added	2.06	***11.31***	0.14	*1.38*
Log Retail Emp	1.02	***11.99***	0.04	*0.61*	1940 Log Retail Emp	1.43	***12.33***	0.06	*0.84*
Log Black Pop	2.01	***6.65***	0.08	*0.73*	1940 Log Black Pop	2.13	***6.81***	0.10	*0.82*
Log Mfg Estabs	1.09	***11.39***	0.06	*0.91*	1930 Log Layoffs	1.52	***10.77***	0.11	*1.47*
Log Urban Pop	3.82	***8.83***	0.05	*0.48*	1930 Wholesale Wage	1.96	***8.15***	0.10	*0.91*
Log Foreign Born Pop	1.66	***7.49***	0.02	*0.16*	1930 Log Retail Wage	1.55	***12.22***	0.07	*0.87*
PC 1930's AAA Grants	-77.67	***-5.82***	-0.88	*-0.32*	1940 Log Retail Wage	0.15	***5.58***	-0.01	*-0.89*
Log 1930s WPC/WPA Pub Wks	0.23	***4.06***	0.01	*0.41*	1940 Retail Sales Per Capita	184.38	***6.93***	5.69	*0.37*
Log 1937 Emergency Workers	1.11	***10.79***	0.08	*1.19*	1940 Log Median Housing Val	0.34	***6.24***	-0.01	*-0.30*
Log Agricultural Land Value	0.26	***2.41***	0.02	*0.27*	1940 Log Median Contract Rent	0.30	***7.55***	0.00	*0.09*
					1940 Log Rural Non Farm Pop	0.66	***6.29***	-0.09	*-1.10*
N treated	101				1940 Log Male Craftsmen	1.48	***13.24***	0.07	*0.99*
N control in overlap sample	1117				1940 Log Female Craftswomen	1.56	***11.66***	0.06	*0.72*
Sum of control weight	97.3				1940 Log Male Operatives	1.49	***12.39***	0.07	*1.06*
					1940 Log Female Operatives	1.72	***10.01***	0.03	*0.35*

Bold t-stats are significant at 10% Level

Notes: Control group are counties with no new large public plants that with propensity scores that overlap with the 101 counties receiving the binary treatment. The propensity score is estimated using the variables in the left-hand column.

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

	PSWT Binary Treatment Effect			*OLS Effect of $1000 (1940$) Per Capita*		
	1963 Effect	1982 Effect	2002 Effect	1963 Effect	1982 Effect	2002 Effect
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Outcome:						
Log Mfg Value Added	0.352**	0.297**	0.267**	0.033**	0.026**	0.026**
	(0.088)	(0.089)	(0.109)	(0.009)	(0.009)	(0.008)
Log Mfg Production Payroll	0.301**	0.257**	0.232**	0.025**	0.022**	0.018**
	(0.078)	(0.087)	(0.100)	(0.009)	(0.008)	(0.007)
Log Mfg Production Employment	0.211**	0.146*	0.150	0.017**	0.013*	0.013*
	(0.069)	(0.080)	(0.093)	(0.007)	(0.008)	(0.007)
Log Mfg Production Avg Wage	0.087**	0.097**	0.080**	0.007**	0.007**	0.050**
	(0.023)	(0.026)	(0.034)	(0.002)	(0.003)	(0.002)

Notes: The left-hand panel reports propensity-score reweighting estimates of ATOT effects of the binary treatment on changes in the outcome variables from 1939 to either 1963, 1982, or 2002. Sample is 101 treatment counties and 1117 control counties in overlap region. The right-hand panels show OLS estimates of the effects of an addition $1000 (1940$) per capita in public plant spending for the same years and outcomes, N=1,981. 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). Each estimate is from a separate regression.

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

	PSWT Binary Treatment Effect			
	1960 Effect	1963 Effect	1980 Effect	1982 Effect
Outcome:	*(1)*	*(2)*	*(3)*	*(4)*
Log Population	0.092**		0.104**	
	(0.036)		*(0.053)*	
Log Total Employment	0.116**		0.146**	
	(0.035)		*(0.056)*	
Mfg Share of Employment	0.114**		0.081*	
	(0.033)		*(0.042)*	
Log Retail Employment		0.063		0.94
		(0.041)		*(0.061)*
Log Avg Retail Wage		0.019		0.012
		(0.01)		*(0.011)*
Log Median Family Income	0.037**		0.025*	
	(0.017)		*(0.016)*	

Notes: Table reports propensity-score reweighting estimates of ATOT effects of the binary treatment on changes in the outcome variables, either from 1939 to either 1963 or 1982, or from 1940 to either 1960 or 1980. Sample is 101 treatment counties and 1117 control counties in overlap region. 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). Each estimate is from a separate regression.

Table 5: Effects on Postwar Defense Contracts

		Reweighting Estimate of Effect on:		
		Ln Defense Contracts		$\ln(\frac{\text{Def Contract 1966-70}}{\text{Mfg V Add 1967}})$
Treatment	Treat *N*	Total 1966-1970	Total 1971-1975	
All Plants	101	0.711	0.652	0.235
		(3.15)	*(3.13)*	*(1.16)*
General Manufacturing	56	0.427	0.363	-0.100
		(1.47)	*(1.47)*	*(-0.46)*
Ordnance Works	44	0.985	0.887	0.674
		(2.94)	*(2.74)*	*(2.03)*

Notes: Table reports propensity-score reweighting estimates of ATOT effects on levels (not differenced) of the outcome variables. Sample is 101 treatment counties and 1117 control counties in overlap region. 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). Each estimate is from a separate regression. For each treatment subgroup, the propensity score is estimated separately; treated counties in any treatment group a never in any control group.

Table 6: Comparison of Difference-in-Differences and Post-Period-Difference Estimates of Earnings Effects

	Estimator					
	PPD	DD	PPD	DD	PPD	DD
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Outcome:						
1982 Production Wage	0.103**	0.092**	0.099**	0.095**	0.094**	0.096**
	(0.021)	(0.027)	(0.021)	(0.026)	(0.021)	(0.026)
1982 Retail Wage	0.001	0.008	-0.001	0.008	-0.000	0.009
	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)	(0.010)
1980 Median Family Income	0.046**	0.011	0.047**	0.021	0.044**	0.037**
	(0.012)	(0.016)	(0.012)	(0.015)	(0.010)	(0.012)
1980 Median Male Wage	0.096**	-	0.093**	-	0.091**	-
	(0.037)		(0.037)		(0.037)	
1980 Female Wage	0.047	-	0.045	-	0.044	-
	(0.035)		(0.036)		(0.037)	
Baseline Covariates, Excl Income	x	x	x	x	x	x
+ Mfg Wages			x	x	x	x
+ Med. Income + Home Val					x	x

Notes: Table reports propensity-score reweighting estimates of ATOT effects on levels (not differenced) of the outcome variables. Sample is 101 treatment counties and 1117 control counties in overlap region. Baseline set of conditioning variables is select logged 1930 variables, excluding information on payrolls, wages, or incomes—population, employment, manufacturing value added and establishments, manufacturing production worker employment, 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). "Mfg Wages" specifications include controls for 1930 average production worker wages, "Med Income + House Val" specifications include controls for 1940 median family income and median house value. Each estimate is from a separate regression.

Table 7: 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 5. 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 8: 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 5. 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 9: 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 5. 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 10: Effects on Upward Mobility

	Estimator			
	PS	OLS	PS	OLS
	(1)	*(2)*	*(3)*	*(4)*
		A. Men		
1940 Parent Income Rank:				
Below Median	1.014**	1.440***	0.829*	0.938**
	(0.381)	(0.322)	(0.379)	(0.305)
Above Median	0.512*	0.784***	0.505*	0.620**
	(0.242)	(0.232)	(0.250)	(0.232)
		B. Women		
1940 Parent Income Rank:				
Below Median	0.852*	1.294***	0.657	0.764*
	(0.402)	(0.345)	(0.395)	(0.315)
Above Median	0.574*	0.769***	0.559*	0.615**
	(0.223)	(0.215)	(0.233)	(0.216)
Baseline Covariates	x	x	x	x
+ Income and Housing Values			x	x

Notes: Table reports propensity-score reweighting estimates of ATOT 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 . Sample is 101 treatment counties and 1117 control counties in overlap region. Baseline set of conditioning variables is select logged 1930 variables, excluding information on payrolls, wages, or incomes—population, employment, manufacturing value added and establishments, manufacturing production worker employment, 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). "Income and Housing Value" specifications include controls for 1930 average production worker wages, 1940 median family income and median house value. Each estimate is from a separate regression.

Table 11: Upward Mobility: Effects by Demographic Group

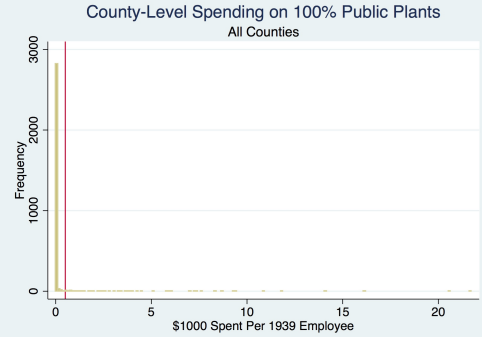
		1940 Age			
	All	0 to 6	7 to 12	White	Black
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
			A. Men		
1940 Parent Income Rank:					
Below Median	0.829**	0.995**	0.766*	0.865**	-0.759
	(0.379)	(0.363)	(0.407)	(0.330)	(1.522)
Above Median	0.505**	0.485*	0.564*	0.547**	3.137**
	(0.250)	(0.261)	(0.292)	(0.253)	(1.426)
			B. Women		
1940 Parent Income Rank:					
Below Median	0.657*	0.624*	0.844*	0.666**	-1.213
	(0.395)	(0.359)	(0.446)	(0.324)	(1.231)
Above Median	0.559**	0.536**	0.443*	0.560**	-0.307
	(0.233)	(0.227)	(0.227)	(0.232)	(1.609)

Notes: Table reports propensity-score reweighting estimates of ATOT 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. Sample is 101 treatment counties and 1117 control counties in overlap region. All specifications use the covariate set from Columns (3) and (4) of Table10.Each estimate is from a separate regression.

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

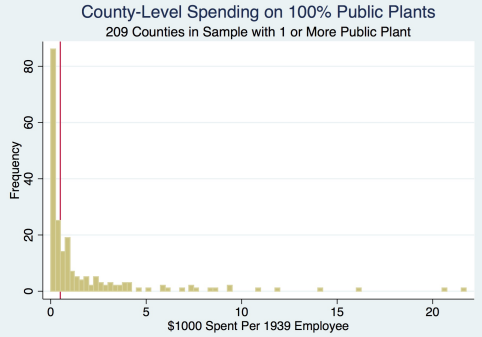


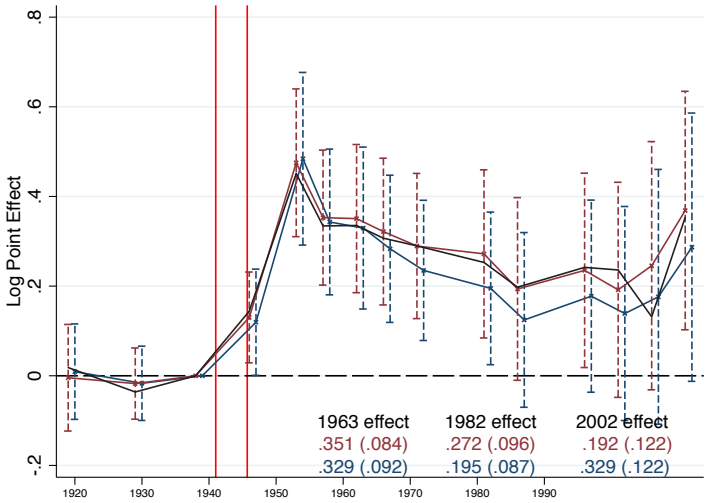
Figure A.2: Intensive Margin Effects on Manufacturing Outcomes



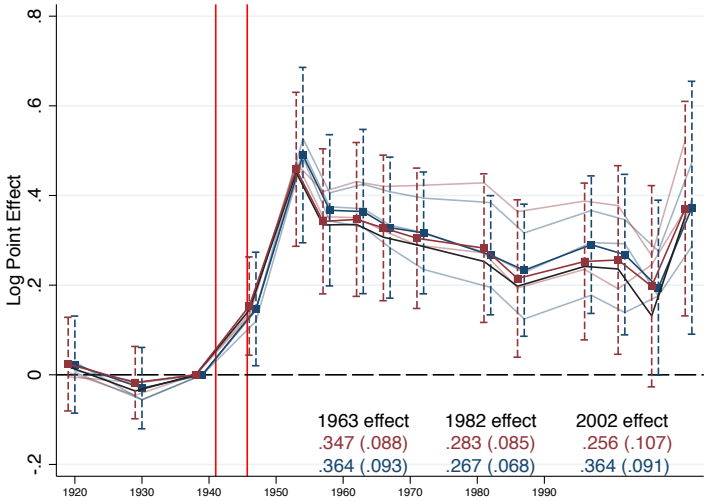
Notes: Panel (a) plots propensity-score reweighting estimates of ATOT effects of the binary treatment on changes in the outcome variables from 1939 to each outcome year. Sample is 101 treatment counties and 1117 control counties in overlap region. Panel (b) plots OLS estimates of the effects of an addition $1000 (1940$) per capita in public plant spending for the same years and outcomes, N=1,981. Each outcome variable is colored according to legend. 95% confidence intervals and select point estimates are displayed for wage effects only. 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). Each point estimate is from a separate regression.

Figure A.3: Robustness of Value Added Results to Alternative Specifications

(a) OLS + PS: Only 1930 Ln Population Covariate



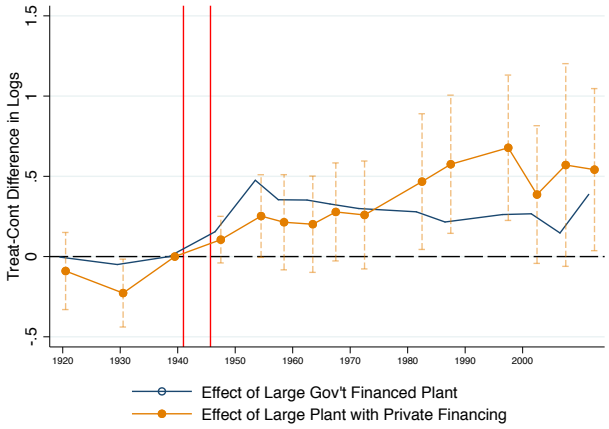
(b) OLS + PS: All 1930 Covariates and All 1940 Covariates



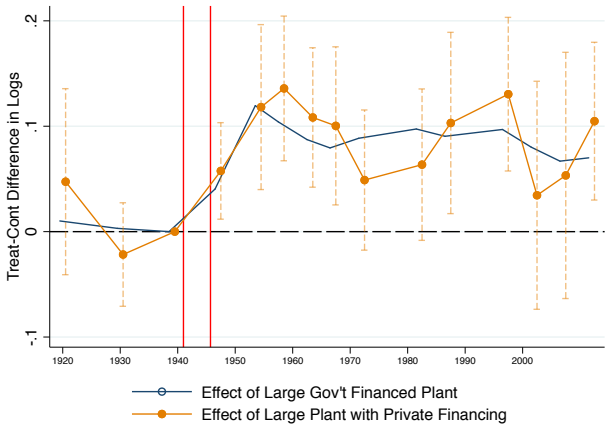
Notes: Figure reproduces Panels A and B of Figure 5 for log manufacturing value added outcome. See Figure 5 for additional details.

Figure A.4: Comparison: Effect of Big New Plants with Private Financing

(a) Effect on Manufacturing Value Added



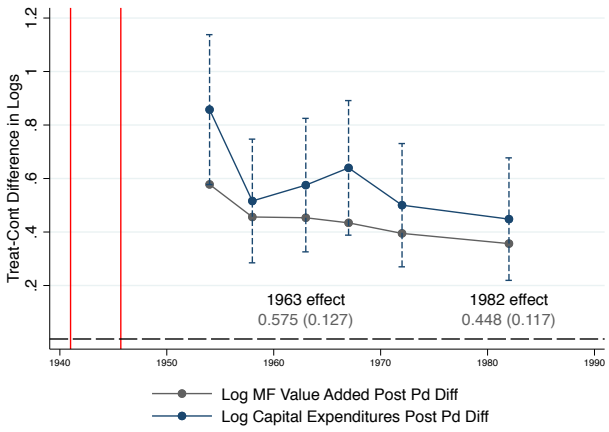
(b) Effect on Production Wages



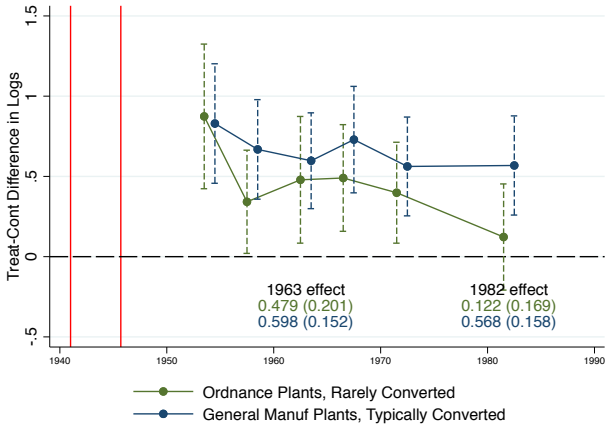
Notes: Figure presents estimation of specification identical to that in Panel A of 4, but binary treatment defined by presence of large, new plant with at least some private financing, using fully analogous definition, and propensity score is re-estimated accordingly. Treatment counties from baseline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

Figure A.5: Effects on Postwar Capital Expenditures

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

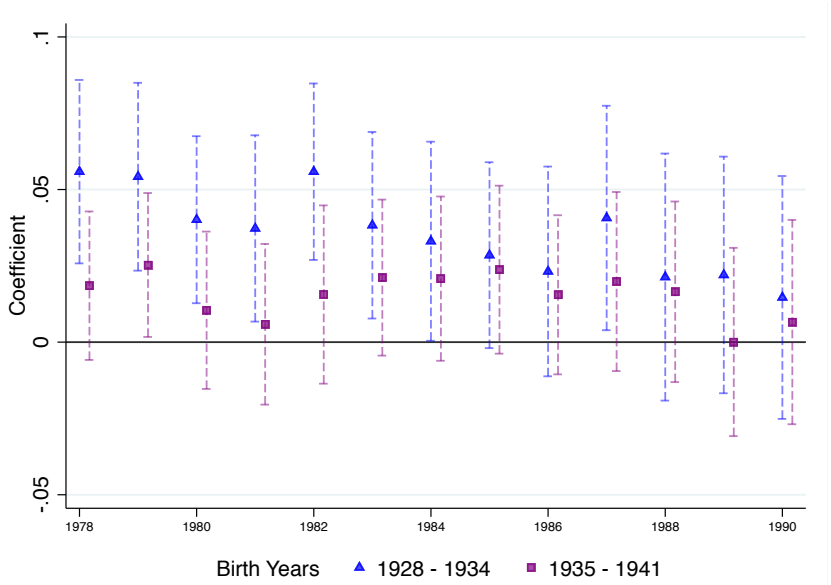


(b) By Plant Type



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

Figure A.6: Effects on Adult Earnings, by Cohort Group



Notes: Figure plots OLS estimates of specification in Equation 5 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.

Table 12: Top-Weighted Counties in Control Group

	County	State	Biggest City	Weight
1	Franklin	OH	Columbus	1.42
2	Westchester	NY	Yonkers	1.05
3	Hennepin	MN	Minneapolis	1.02
4	Fulton	GA	Atlanta	.95
5	Winnebago	IL	Rockford	.87
6	Suffolk	MA	Boston	.85
7	Peoria	IL	Peoria	.83
8	Sedgwick	KS	Wichita	.81
9	Mahoning	OH	Youngstown	.74
10	Bexar	TX	San Antonio	.72

Notes: Control group are counties with no new large public plants that with propensity scores that overlap with the 101 counties receiving the binary treatment. The propensity score is estimated using the variables in the left-hand column.