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THE LONG-RUN IMPACTS OF PUBLIC INDUSTRIAL INVESTMENT ON LOCAL DEVELOPMENT AND ECONOMIC MOBILITY: EVIDENCE FROM WORLD WAR II

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ABSTRACT

This paper studies the long-run effects of government-led construction of manufacturing plants on the regions where they were built and on individuals from those regions. Specifically, we examine publicly financed plants built in dispersed locations outside of major urban centers for security reasons during the United States' industrial mobilization for World War II. Wartime plant construction had large and persistent impacts on local development, characterized by an expansion of relatively high-wage manufacturing employment throughout the postwar era. These benefits were shared by incumbent residents; we find men born before WWII in counties where plants were built earned $1,200 (in 2020 dollars) or 2.5 percent more per year in adulthood relative to those born in counterfactual comparison regions, with larger benefits accruing to children of lower-income parents. The balance of evidence suggests that these individuals benefited primarily from the local expansion of higher-wage jobs to which they had access as adults, rather than because of developmental effects from exposure to better environments during childhood.

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1 Introduction

Throughout much of the twentieth century, blue-collar manufacturing jobs were understood to provide an important opportunity for less-educated workers to climb the economic ladder by offering high pay and stable careers. It is well documented that the disappearance of high-wage manufacturing work from regions over the past four decades has had dire impacts on affected workers and local labor markets (Wilson, 1997; Moretti, 2013; Autor et al., 2016; Charles et al., 2018a), and there is strong interest among policymakers and academics in place-based policy interventions that incentivize firms to locate new manufacturing plants in regions with limited opportunity for economic advancement (Austin et al., 2018; Bartik, 2020; Slattery and Zidar, 2020).[1] However, it is unclear how effective such policy efforts are at improving employment opportunities for local workers, particularly in the longer term (see, for example, Ip, 2023; Economist, 2023). In particular, even when an intervention can successfully attract jobs to targeted regions, a key question is whether incumbent residents themselves benefit from those new employment opportunities. While there is abundant evidence about what happens when work disappears from a region, we know rather little about what happens when work *appears*, since suitable natural experiments are rare.

This paper turns to an earlier era in U.S. history to test whether the sudden expansion of high-wage work opportunities in a place translated to improved outcomes for the specific individuals living in that place at the outset. We study idiosyncratic, policy-driven manufacturing expansions stemming from one of the largest government economic interventions in U.S. history: the industrial mobilization for World War II (WWII). In particular, we study the long-run effects of government-led construction of manufacturing plants for war production on regions and on the specific *people* from those regions. Importantly, by drawing on a rich array of longitudinally-linked administrative and survey data sources, we are able to identify where children lived prior to the war and observe their earnings and other outcomes as adults, regardless of where they wound up. We combine these data with rich information from the full-count 1940 Census on the prewar socioeconomic status of their parents to study what types of individuals benefited and why.

Our analysis focuses on the construction of a set of large new government-financed manufacturing plants that were strategically sited across the United States. While the majority of production for the war was carried out at preexisting plant sites that were converted and expanded, it was often necessary to build entirely new, large-scale plants to meet production

[1] Recently, legislation enacted by the 117th Congress authorized $80 billion in spending on place-based industrial investment initiatives in the U.S. (Muro, 2023). In addition, Bartik (2020) estimates that state and local governments spend approximately $50 billion per year on tax incentives and other programs to attract manufacturing plants and other large employers to their jurisdiction.

goals for key goods like airplanes, steel, aluminum, synthetic rubber, and ordnance. Due to strategic considerations, such as concerns over supply-chain security and production bottlenecks, the military insisted that many of these new plants be built in dispersed locations outside of the major manufacturing hubs where most production was already slated to take place. While private firms often made their own investments (usually with heavy subsidies) into both new plants and expansions of existing plants involved in war production in cases where they saw long-run value, businesses balked at the prospect of investing in large plants in scattered locations with unclear postwar value, requiring the government to pay for their construction. Plant siting decisions for these publicly financed plants were determined by the military and largely driven by security concerns and short-run expedience—among regions with sufficient populations, idiosyncratic factors dominated site selection in practice. Most such plants continued to operate after the war. Typically, they were either sold to private firms at a small fraction of their original construction cost or used as government-owned, contractor-operated defense production facilities throughout the Cold War era.

In our analysis, we compare counties where these plants were built to other similarly-populated counties outside of major manufacturing centers. Taken together, the historical record and the data support the view that the government-led construction of these plants was the closest the United States has ever come to a random "helicopter drop" of large factories. There is no systematic association between publicly-funded wartime plant construction and county-level economic or demographic characteristics in 1940, whether conditioning only on population alone or additionally conditioning on other basic geographic factors like market access. "Treatment" regions where these plants were built and counterfactual regions with comparable population sizes not only experienced parallel trends in outcomes prior to 1940, but furthermore had no differences in outcome levels in the decades leading up to WWII. At the individual level, we find no differences in parental incomes across children growing up in affected and comparison regions before the war and, moreover, we observe similar rates of upward mobility in the *prior* generation growing up in the region in the 1910 Census.

We present three main sets of results. First, we establish that wartime plant construction had large and persistent impacts on the labor markets in affected counties. After evolving similarly through the start of WWII, output, employment, and average pay in the manufacturing sector in treated counties increased dramatically relative to comparison counties, and remained elevated for most of the remainder of the twentieth century. In the first decades after the war, the increase in manufacturing employment appears to have been driven predominantly by the large new plant itself. While manufacturing employment expanded immediately after the war, population levels grew more gradually as additional people subsequently moved to the region. As a consequence, the manufacturing employment share rose in treatment counties in the initial postwar decades before it eventually returned to parity with the comparison group by the end of the century. In the 40 years after the war, treated counties saw family earnings increase by 5–10%, reflecting higher average wages and employment shares in manufacturing establishments and semi-skilled blue-collar occupations (with only limited average wage growth in non-manufacturing establishments, in white-collar occupations, and for women). In the longer term towards the end of the twentieth century, treated counties remained permanently larger than comparison counties, but with a similar composition of employment across sectors.

Second, we find that wartime plant construction substantially increased the long-run earnings of men who resided in treatment regions as children prior to the start of the war. On average, men born in treatment counties in the 18 years before the war had $1,200 (2020 dollars) more in annual wage earnings reported on tax returns between 1969 and 1984 than those born in comparison counties. These effects were largest for those from families with lower prewar earnings: children of families in lowest deciles of the 1940 earnings distribution saw long-run earnings increase by approximately $1,800 dollars per year in adulthood, amounting to a 3–4 percent increase and a one percentile improvement in the national earnings distribution. By contrast, there were no effects on children born to the highest-earning parents. We find larger effects for Black men; though, in contrast to White men, we observe the largest impacts among Black men from higher-earning families. In all demographic categories, we find comparable effects for men and women in the Form 1040 income tax return data, which is reported at the tax-unit level (roughly akin to a household). However, when we look at individual-level earnings on W-2 returns we find this is entirely driven by increased earnings of men; we find *no* positive effect on women's individual earnings.[2]

Third, turning to mechanisms, the balance of evidence suggests that the children born in treatment counties benefited from local plant construction primarily because of increased availability of higher-wage jobs in adulthood. We find limited scope for developmental effects that accumulated through longer exposure to better environments during childhood as in Chetty and Hendren (2018b). While war plants modestly increased educational attainment, particularly for children of families with the lowest prewar earnings, these observed increases

[2] This evidence contrasts with findings from earlier work linking women's wartime work to higher postwar labor supply using variation in wartime labor demand arising from differences in men's military mobilization rates across regions (Goldin, 1991; Acemoglu et al., 2004; Goldin and Olivetti, 2013). However, our findings are consistent with recent work by Rose (2018) that finds a strong relationship between local war production and female wartime work, but not postwar female labor supply, reflecting the displacement of most women from manufacturing jobs after the war.

in education are not large enough to account for the increase in earnings in adulthood. We do not find larger effects for children who were younger at the start of the war, indicating that the salutary effects of plant construction did not increase with duration of childhood exposure to improved regions. Instead, treated individuals were more likely as adults to live in places with higher median earnings and higher wages for workers in blue-collar semi-skilled occupations. Effects on adult earnings are entirely accounted for by location in adulthood—and are driven by those who remain in their birth county. Further, using individual-level W-2 data, we directly document that children born in treatment counties are themselves more likely to work in industries paying higher wage premiums as adults. While these findings do not preclude the possibility that plant construction led to unmeasured increases in human capital development, the results point to earnings impacts driven primarily by better opportunities for economic advancement in adulthood.

We interpret our findings as evidence that attracting high-wage manufacturing jobs to a region can substantially increase upward economic mobility for local residents. However, our results also suggest that the expansion of manufacturing work *per se* was not what mattered so much as the persistent expansion of employment in jobs that offered improved opportunities for economic advancement to local workers from disadvantaged backgrounds, which would not have been available to them otherwise. When considering implications for future policy, it is therefore important to observe that the expansion in *higher-wage* work opportunities we observe in our setting is not a necessary consequence of policies that attract local investment in general. The plants built during WWII went on to operate in an economic environment characterized by high global demand for U.S. manufactured goods with limited international competition, production technology that was relatively intensive in semi-skilled labor, and substantial worker bargaining power coordinated by strong unions Goldin and Margo (1992); Farber et al. (2021). In contemporary settings where production is highly automated, competition from overseas producers is high, and collective bargaining institutions are weaker, attracting new manufacturing investment to a region may fail to increase the high-wage work opportunities that can drive increased upward mobility. Indeed, places with greater manufacturing employment density no longer have higher rates of upward mobility today Chetty et al. (2014), and, reexamining "million-dollar plant" openings in the late twentieth century from Greenstone et al. (2010), we find that more recent plant openings have no such impact on the local wage structure.

This research contributes to a growing literature examining how place influences long-run economic mobility. A large body of recent work finds that the place where one grows up causally influences adult incomes (Chetty et al., 2016; Chyn, 2018; Chetty and Hendren, 2018b,a; Chyn and Katz, 2021).[3] An important objective for ongoing research is to better understand what makes some places better for upward mobility and which policies can potentially make a difference. The channels by which wartime plant construction influenced adult outcomes in our setting are quite distinct from Chetty et al. (2016) and Chetty and Hendren (2018b), which find that neighborhoods specifically influence long-run earnings through developmental exposure effects that scale with the time spent living in a place during childhood, whether or not one remains in that place during adulthood. By contrast, our evidence suggests that children benefited from plant construction because of improved employment opportunities in the region that they could access as adults irrespective of the number of years they were exposed to the improved labor market during childhood.[4]

Our work also relates to an extensive literature on regional development and the long-run effects of place-based economic interventions (Kline and Moretti, 2014; Neumark and Simpson, 2015; Austin et al., 2018; Bartik, 2019; Slattery and Zidar, 2020). We provide new evidence that short-run policy interventions can have permanent impacts on the geographic distribution of economic activity. The permanent effects on region size we observe, which persist even without continued federal spending and after regions cease to be more manufacturing-intensive than comparison regions, are consistent with models featuring multiple equilibria and path dependence (Murphy et al., 1989; Redding et al., 2011; Bleakley and Lin, 2012; Nunn, 2014; Hanlon, 2017).[5] Our work extends this literature by studying the long-run impacts of a major place-based policy intervention not only on the *places* but on the specific *people* who lived in those places at the outset. Our study most directly complements contemporaneous work by Mitrunen (2021), which examines the long-run impacts of post-WWII industrialization in Finland spurred by the need to make war reparations to the Soviet Union. That study finds qualitatively similar long-run impacts on both regional development and upward mobility for local residents despite the differences in the settings.[6]

[3]Much of the recent literature estimates place effects by studying individuals who move neighborhoods during childhood; our analysis is different in its examination of a place-based shock on incumbent residents, and is more similar to Stuart (2022) in this regard.

[4]Both Tan (2019) and Rothstein (2019) have previously emphasized adult labor market opportunities as a driver of regional variation in upward mobility in both historical and contemporary settings.

[5]A related literature examines the impacts of industrial policies on aggregate development (Fan and Zou, 2018; Criscuolo et al., 2019; Lu et al., 2019; Lane, 2019; Kantor and Whalley, 2023). However, as highlighted by Kline and Moretti (2014), Hornbeck and Rotemberg (2019), and Kantor and Whalley (2023), among others, persistent effects on certain regions and their inhabitants like those we find do not necessarily imply aggregate gains if the benefits accruing in one affected region would have counterfactually occurred elsewhere. We discuss aggregate implications of our results in Section 8.3, but our setting does not allow us to draw decisive conclusions about aggregate effects.

[6]The setting in Mitrunen (2021) differs from ours in key regards. While our analysis focuses on less-developed regions outside of existing major manufacturing hubs in the U.S., the WWII-era Finnish setting in Mitrunen (2021) is one where the *entire* country was at an earlier stage of industrial development. Further, the regional assignment mechanism is quite different in the two settings—whereas our design examines *de*

Finally, our paper contributes to the economic history literature examining the impacts of World War II on the postwar economy. Recent work has documented lasting effects of other WWII programs such as management training (Bianchi and Giorcelli, 2019) and research initiatives (Gross and Sampat, 2023). Our study is among the first to find a direct association between WWII government spending in specific regions and postwar average incomes. Earlier work by Fishback and Cullen (2013) found that regions with more *supply contract spending* per-capita on war contracts had higher population growth but not increased economic activity per capita.[7] In comparison, our work examines variation in *government plant investment* and finds significant effects on average incomes. Our focus on new plant construction is most directly related to a contemporaneous study by Jaworski (2017), which examines county-level variation in logged World War II capital expenditures among counties within the U.S. South and finds no longer-term effects on long-run manufacturing expansion.[8] More broadly, our work complements studies examining how events during WWII impacted the postwar wage structure and cross-group earnings differences (Goldin and Margo, 1992; Ferrara, 2018; Aizer et al., 2020).

2 Historical Background

The industrial mobilization for WWII was the largest government-driven economic expansion in United States history. Between 1940 and 1944, increases in government spending drove Gross National Product to increase by over 70 percent (Tassava, 2008). Several manufacturing industries involved in war production expanded dramatically during this period: employment in the chemical- and metal-working sectors nearly tripled from about three million to nearly eight million, while the relatively nascent aircraft industry increased its employment fourteen-fold (Craven and Cate, 1955). The vast majority of the production for the war was done by private firms under contract. Due to the urgency of the crisis, contracts

novo construction of large plants operating in nascent industries that had minimal prior presence in treated regions, Mitrunen (2021) employs a "shift-share" design in which the underlying demand shock is at the industry level and so regions most affected by reparations demands were those where the relevant industries were already most prevalent at the outset of WWII. Despite these differences, Mitrunen (2021) finds similar effects on regional development to those documented here. That paper also finds average individual-level effects on educational attainment and adult earnings ranks in Finland that are comparable to the effects we find for children of parents with low prewar earnings, suggesting that the mid-century impacts of local industrialization on individuals were not particular to just one specific setting.

[7] A long literature debates whether the WWII industrial mobilization increased growth or welfare in the aggregate (Higgs, 1992; Mulligan, 1998; Higgs, 2004; Field, 2022). While we find that wartime plant investment had long-run effects in certain regions, our results do not enable us to draw clear conclusions about the contribution to aggregate growth.

[8] In our analysis focusing on large publicly-financed plants, we find comparable effects to our main estimates when restricting our analysis to the U.S. South.

were not allocated competitively; instead, "cost-plus-a-fixed-fee" contracts were directly negotiated by a wide array of government military procurement agencies with manufacturing firms. To improve coordination across procurement agencies and thereby eliminate bottlenecks and resource misallocation, the U.S. government established the War Production Board (WPB) and its predecessors, the National Defense Advisory Committee (NDAC) and the Office of Production Management (OPM), to supervise the production effort (White, 1980). A key role for the WPB was overseeing the expansion in industrial facilities required to meet production goals. A large share of production was carried out at existing plants, many of which were significantly expanded or converted from their peacetime use; for instance, many automobile plants pivoted to producing airplanes after civilian automobile production was restricted by the government. Yet, even with large-scale conversion of existing establishments, entirely new plants were required in order to meet the dramatic production goals for key products such as airplanes, ships, ordnance, explosives, and the required chemicals and metals needed to make these products.

For military planners considering potential locations for new plants, strategic considerations were paramount. While private firms pushed to build new plants in major cities near their established operations, officials were highly concerned that excess concentration of production in congested industrial hubs would generate major vulnerabilities in the supply chain and therefore sought to locate new plants in dispersed locations away from highly industrialized areas (McGrane, 1946).[9] However, although firms were willing to raise capital to expand existing facilities, they had little interest in financing the construction of massive new plants in relatively isolated locations with highly uncertain postwar value. As Air Force historians observed after the war, "The industrialists' reluctance to invest in dispersed plant facilities was at odds with the government's hope that private capital could finance new inland construction. Hence, the War Department could carry out its policy only to the extent that the government was willing to put up the money" (Craven and Cate, 1955). Accordingly, the U.S. government directly financed the construction of many such plants. During the war, these plants functioned as government-owned and contractor-operated (GOCO) establishments, which were built and operated by private firms under contract but owned by government agencies, typically the newly formed Defense Plant Corporation (White, 1980).

Plant site decisions for these new, government-financed facilities were made by procurement officers working across a host of military and civilian agencies. Although the staffs of

[9]According to a postwar report by the successor agency to the NDAC, OPM, and WPB, "The Plant Site Board [of the OPM] did endeavor to locate new facilities away from highly industrialized areas. In part the location of new facilities was determined by strategic reasons... According to [OPM Director Donald] Nelson, supply contracts followed the location of industry; but new facilities were planned to follow at least partial decentralization" (McGrane, 1946).

NDAC, OPM, and WPB worked to propose strategic plant sites and coordinate the actions of various procurement offices during the war, there was never a centralized mechanism within the defense apparatus for plant site selection. Much to their chagrin, members of Congress had little ability to influence the siting process beyond recommending suitable parcels in their jurisdictions.[10] At the end of the war, Donald Nelson, the director of the OPM and WPB, recalled: "Platoons of Senators and Representatives, stimulated by their constituents, descended upon us. Hundreds of briefs were submitted by towns all over the United States, and, since we were thinking about defense only, I suppose that our selection of sites pleased nobody."(Nelson, 1946)

Siting decisions were driven by a combination of strategic considerations and short-run expediency. Plants generally needed to be located in areas with sufficient population, power, water, and transportation connections to begin immediate operation, although the specific needs differed depending on the product (McGrane, 1946). In practice, specific sites were chosen based on the immediate availability of suitable parcels that came to the attention of procurement officials. An illustrative example is the Willow Run bomber plant built by Ford in Ypsilanti, Michigan, one of the largest plants built during the war at a cost of $95 million ($1.5 billion in 2020 dollars). With Ford under scrutiny due to concerns over Henry Ford's sympathies to Germany at the start of WWII, his son Edsel offered the land from a charitable farm camp Henry had established in Ypsilanti as a site for a new bomber plant with an adjacent airfield. Ford built and operated the bomber plant at that location during WWII but declined to repurchase the massive facility at the end of the war. It was instead eventually bought by General Motors, which continued to operate the plant through 2010 (Baime, 2014). In other cases, localities that likely would not have been otherwise able to attract private plant investment were able to make appealing propositions to military officials or to leverage local politicians' personal connections to military procurement officers to be considered as plant sites.[11] In general, the historical evidence suggests that siting decisions

[10]In *Destructive Creation: American Business and the Winning of World War II*, Mark Wilson observes:

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

Likewise, in their empirical analysis, Rhode et al. (2018) find little evidence that federal politics drove the geographic distribution of wartime spending.

[11]We discuss additional evidence in Appendix D.

were largely driven by idiosyncratic factors specific to particular circumstances of the war emergency that would not have been relevant for private sector location choices had the war not occurred.

By the end of the war, the government had spent $15.9 billion ($255 billion in 2021 dollars) on expanding the plant stock, $7 billion of which was for the construction of new GOCO plants. Importantly, most of these government-financed plants continued to operate after the war. Plants that could be converted to civilian production—for example, those that had produced planes, tanks, steel, rubber, and machinery during the war—were sold to private industry after the war. Because of concerns among business leaders about potential postwar competition from government-owned facilities, the authorizing legislation included clauses requiring plants to be privatized after the war when possible (Craven and Cate, 1955). Plants were generally sold at a small fraction of their construction costs, in part because they required substantial modifications and were built at a larger scale than needed in peacetime.[12] Meanwhile, plants producing specialized military products like ordnance and ammunition that that could not practically be converted to civilian use typically remained under government ownership after the end of the war and continued to supply the military as GOCO plants throughout the Cold War.

3 Data

3.1 World War II Investment Data

Our data on plant-level capital expenditures come from the 1945 War Production Board data book *War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator* (United States War Production Board, 1945b), which reports all industrial investments that were authorized by the government to support a war supply contract.[13] The book reports information about each plant, its products, the firm operating it, and a breakdown of capital expenditures into privately-financed and government-financed portions. These are further subdivided into expenditures on structures, equipment, and land. We

[12]Although the process of declaring plants surplus and selling them often took years, we observe sales completed by early 1947 recorded in the War Assets Administration Directory of Surplus Real Property (Administration, 1947). Across the 18 large public plant in the directory that were sold by 1947, plants typically sold for about 40 percent of their construction cost on (cost-weighted) average. In many cases, plants were sold to different firms than the ones that had built them. As a prominent example, Alcoa was deemed to have excessive market power over aluminum production after the war and so federal officials required that many of the plants it operated during the war be sold to competitors or new firms (White, 1980).

[13]Investment authorizations through October 1944 account for 90% of all wartime capital authorizations and virtually all public plant construction.

describe these data in additional detail in Appendix B.

We use information in the data book to identify new, publicly-financed plants. In most cases, newly-constructed plants are explicitly labelled as such. However, in some cases, plants that we know were newly-built from external historical sources are missing these flags.[14] We therefore additionally categorize plants as "new" in our baseline analysis if over 40 percent of the total spending was on structures rather than equipment, but also show robustness to keeping only new plants with explicit labels.[15] Second, we categorize plants as "publicly-financed" investments if all expenditures on structures came from public agencies; we classify remaining new plants (partially) "privately-financed".[16] In our analysis, we focus on the impacts of the largest plants with costs exceeding $10 million in contemporary dollars (approximately $150 million in 2020 dollars), both because strategic considerations were most salient for the siting of these very large plants and because these plants had the most potential to have large postwar impacts. The 353 plants that fit this definition account for 70 percent of all spending on new plants during the war (Appendix Table A.1). Nonetheless, we also consider the construction of smaller plants costing between 1 and 10 million dollars.[17]

3.2 County-Level Panel Data

We build our county-level panel using tabulations of Economic Census and the Decennial Population Census data compiled in the County Data books and in work by Haines (2005), which cover the years 1900 through 2000. We supplement these data with additional county-level Census tabulations obtained from the National Historical Geographic Information System (Minnesota Population Center, 2011). Some variables are only tabulated for select years. The county median family income concept available for the 1950–1990 Censuses is not available in published tabulations from the 1940 Census. We therefore proxy for 1939 median family income by calculating median combined household 1939 employment earnings in each county using the full-count 1940 Census microdata from IPUMS. We also use data from

[14]To be conservative, when plants marked as new are in counties where firms have other establishments that are not marked as new but which received investments amounting to 5 percent of total firm spending in that county or more, we relabel all of the firms plants in that county as not new.

[15]A high-profile example of a major new plant missing any explicit label is the Geneva Steel plant built by Columbia Steel in Utah. We chose the 40 percent threshold rule based on the share of expenditures on structures in this case, and hand-checked that the rule appeared to work well in practice for high-cost plants.

[16]Since firms sometimes make minuscule private investments in otherwise publicly-financed plants, we classify plants as public if less than 1 percent of expenditure on structures was privately financed in our baseline analysis, but show robustness to excluding these cases. We only use expenditure on structures for this determination since they are the fundamentally immobile part of the investment.

[17]We omit facilities costing below $1 million from our main treatment definitions as these tend to be warehouses and auxiliary facilities that appear to have supported other larger plant operations; however, we include such facilities in the continuous measures of spending on new, publicly-financed plants described below.

Ferrara (2018) on WWII draft, enlistment, and casualty rates as well as data from Fishback et al. (2005) that account for geographical features, the severity of the Great Depression, and federal New Deal spending. We construct 1940 market access using bilateral travel distances from Jaworski et al. (2023). In supplemental analyses, we use county-level variables obtained from additional sources. We describe all variable sources in Appendix B.

Sample Restrictions We make two initial restrictions on the counties included in our baseline county dataset. First, we exclude counties in Virginia due to significant changes in county definitions during the observation period and we omit Alaska and Hawaii, which did not become states until after WWII[18]. Second, we exclude the least populated counties with no records of wages paid to manufacturing production workers between 1920 and 1939 or with fewer than 10 such workers in 1939. This leaves us with an initial set of 2,026 counties, containing 88.5 percent of the population of all 3,179 counties in the U.S. in 1940.

3.3 Individual-Level Earnings and Income Data

To study individuals over time, we draw on several data sources linked using Protected Identification Keys (PIK) assigned by the Census Bureau (Wagner and Lane, 2014).[19] We begin with the universe of individuals in the Social Security Administration (SSA) Numerical Identification System (NUMIDENT) file, which reports the date and place of birth provided at the time of application for a Social Security Number (SSN). We then use the crosswalk from Taylor et al. (2016) to match string data on places of birth to the corresponding counties. We restrict to individuals with valid PIKs who were 18 years or under in 1940 (birth years 1922–1940). We then merge these records to the following data whenever PIKs are available, each of which are described in detail in Appendix B.

IRS 1040 Individual Income Tax Returns, 1969, 1974, 1979 and 1984. Our primary outcomes are drawn from 1040 individual income tax returns collected by the U.S. Internal Revenue Service. These returns are filed at the "tax-unit" level, which can either be a single individual or a married couple. For tax year 1969, a PIK is available only for the individual listed as the primary tax filer—for the vast majority of married couples, this is a male (Lin et al., 2023). PIKs are available for both individuals in married couples for tax years 1974, 1979, and 1984. The main items we draw from these returns are wage earnings and adjusted gross income (which includes business, investment, and other income sources), both of which are reported at the combined tax unit level rather than the individual level. In our analysis, we code individuals without a tax return in any year as having zero wages and AGI in that

[18] We also exclude the District of Columbia and overseas territories

[19] Each PIK corresponds to a single Social Security Number. After PIK assignment, all other personally identifiable information, such as name or Social Security Number, is removed from the file.

year and focus on average amounts across available tax years to better proxy for long-run earnings.[20] We also assign percentile ranks within the NUMIDENT sample based on those average earnings.[21]

Full-count 1940 Census. We obtain information about demographics and parent characteristics by matching individuals to records in the full-count 1940 Census which could successfully be assigned PIKs. 61 percent of individuals in the NUMIDENT sample match to a 1940 Census record. For these individuals, we are able to observe parent characteristics such as occupation, education, and age. We observe total wage earnings for each parent and a flag for whether they had other income sources totalling at least $50. For children with parental earnings, we construct ranks based on the combined earnings of parents in the household; we separately examine children in the 1940 Census with no parental wage earnings as well of those not linked to the 1940 Census. We also explore occupation-based rankings constructed following the approach in Collins and Wanamaker (2022). Black children and children of low-income parents are less likely to be assigned a PIK in the 1940 Census, and are slightly under-represented in the matched NUMIDENT-Census sample (Appendix Table A.2).

2000 Census Long-Form Sample. We obtain information on educational attainment from PIKed records from the 2000 census long form survey, which was administered to approximately 1 in 6 households.

CPS-SSA-LBD Data. For a limited subset of individuals, we obtain *individual-level* earnings histories the SSA Detailed Earnings Record (DER) file. The DER is a panel of job-level earnings records from IRS W-2 forms from all years beginning in 1978. DER information is available only for individuals in our sample who were surveyed in the Annual Social and Economic Supplement (ASEC) to the Current Population Survey (CPS) in the years 1991, 1994, and 1996-2017. While we do not observe W-2 earnings for individuals surveyed by the CPS before 1991, we observe full W-2 earnings histories beginning in 1978 for all PIKed individuals in the aforementioned CPS ASEC waves. In each earnings year, we link the Employer Identification Number (EIN) of workers' W-2 with the highest earnings to establishment-level data from the Business Register to obtain characteristics for that em-

[20] In some cases, individuals with positive earnings may not file a 1040 return, either because they have sufficiently low earnings such that no taxes are owed and are therefore not required to file, or because they do not comply with tax filing requirements. In practice, filing rates in our sample are very high—80 percent of men in our full sample and 87 percent of men matching to the 1940 Census filed a 1040 return in 1979. Moreover, we find nearly identical effects on male 1040 wage and salary earnings and third-party-reported W-2 wage and salary earnings (reported whether or not the individual files a 1040 return) in the linked SSA-CPS sub-sample in which we observe both types of returns for common years; this suggests that our coding provides a reasonable approximation.

[21] This income rank is similar to that used by Chetty et al. (2014) and subsequent work.

ployer in the specified year. We collapse establishment data to construct firm-level variables including payroll, employment, and industry (using Standard Industry Classification codes, or SIC).

4 Empirical Strategy

The historical evidence indicates that while government-financed plants constructed outside of major industrial centers typically needed to be located near sufficient basic resources and population, location decisions were driven primarily by idiosyncratic considerations that would not have been otherwise relevant. Accordingly, our empirical approach is to compare counties that were not major manufacturing regions where such war plants were built to other such counties with similar populations and access to basic resources.

We implement this comparison as follows: We define counties with large new government-funded plants that cost $10 million or more as our primary treatment group. We separately classify counties as partially treated in cases where smaller government-funded plants costing between $1 and $10 million were built.[22] In our baseline analysis, we limit our focus to treatment and control counties outside of major manufacturing centers by excluding the 100 counties with the greatest 1939 manufacturing production employment levels from our sample—together, these 100 counties alone accounted for over 60 percent of nationwide production employment in 1939.[23] We examine alternative restrictions based on 1940 metropolitan area status, modern commuting zone boundaries, and other criteria in the analysis below. As the impacts of plant construction may spill over across county borders, we omit counties adjacent to those where large plants were built from our comparison group and separately examine spillover effects on such regions in supplemental analyses.[24]

In the resulting analysis sample, 90 counties are classified into the main treatment group with plant spending totalling at least $10 million, 57 counties are partially-treated with spending between $1 and $10 million, and the 1,400 untreated counties in the remaining sample (subject to the restrictions described above) are included in the comparison group.

[22] We exclude partially-treated counties from the comparison group when examining effects of the primary treatment, and vice-versa. However, we do not remove counties with new plants totalling less that $1 million from the comparison groups. In our analysis below we show that small plants costing $1 and $10 million have insignificant impacts, so any plausible effects of even smaller investments would be negligible.

[23] Appendix Table A.3 shows that while the top 100 manufacturing counties received the vast majority of private investment and government spending on expansions and modifications of private plants, a large majority of spending on new GOCO plants occurred outside those counties.

[24] When constructing the comparison group, we also omit counties adjacent to counties with large public plants that were either dropped because of our initial sample restrictions in Section 3 or are in the top 100 manufacturing counties. We do not omit counties adjacent to smaller public plants from the comparison group except when explicitly examining the effects of smaller plant construction.

The 90 treatment counties account for the majority of funds spent on new fully-public plants nationwide and spending on those plants far exceeded all other wartime investments in those regions (Appendix Table A.3). While over two-thirds of spending on fully-public plants occurred outside of the 100 counties with the most manufacturing employment before the war, public spending on expansions of existing facilities and private investment were predominantly concentrated in those pre-existing manufacturing regions. Figure 1 maps the locations of the treatment counties, which are largely dispersed through the interior of the country.[25]

We compare counties where plants were built with similarly-populated counties using regressions with controls for 1940 population size.[26] In our most parsimonious specification, we adjust for the size of the local labor force by controlling only for 1940 log population and the share of residents living on farms as well as the squares of those variables. As basic geography and infrastructure might further affect the ability of a region to host a plant, we also estimate specifications that additionally control for 1940 market access, land area, the share of households with electricity, New Deal public works spending, and variables indicating the presence of coasts, lakes, rivers, beaches, and bays. In addition, we examine whether results are sensitive to conditioning on a more extensive set of economic and social characteristics.[27] We also obtain county-level estimates of average treatment effects on the treated (ATET) using propensity score reweighting in supplemental analyses; in practice the ATET estimates are nearly identical to the OLS regression estimates.

We estimate impacts at the county level using regressions of the form:

$$Y_c = \alpha + \beta Treat_c + \gamma X_c^{1940} + \epsilon_c \tag{1}$$

In the equation above, Y_c is a county-level outcome, $Treat_c$ is the primary treatment variable (which is set to missing for partially-treated regions in the baseline analysis), and X_c^{1940} is a vector of prewar county characteristics. We use an analogous regression framework to study impacts on incumbent individuals; specifically, we estimate:

[25]To visualize which counties in the comparison group are most comparable to the treatment group based on observable characteristics, we map propensity score weights for control counties across alternative covariate specifications in Appendix Figure C.1.

[26]While the propensity for plants to be sited in underpopulated areas is low, it was not zero; in some instances large plants producing dangerous explosives were built in counties with very low populations. This motivates our use of regression rather than dropping all counties below some population threshold.

[27]The longest covariate set includes all the aforementioned covariates as well as log 1940 median family earnings and median house value; log 1939 manufacturing value added, log establishments, log production employment, and log average manufacturing payroll; 1939 logged employment and average wages in services and retail; the share of adults over 25 with a college or high school degree, respectively; log 1940 employment and the share of employment in manufacturing; and the shares of the 1940 population that are Black and foreign-born, respectively.

$$Y_i = \alpha + \beta Treat_{c_{birth}(i)} + \gamma X^{1940}_{c_{birth}(i)} + \delta Z_i + \epsilon_i \quad (2)$$

Importantly, for our person-level regressions, the treatment status of an individual is determined by *the county they were born in* and not where they live when outcomes are observed.[28] Standard errors are always clustered at the county level. We also examine robustness to controlling for individual and parent characteristics Z_i from the 1940 Census for the subset of individuals who are matched across data sources; specifically, these specifications include controls for child race and age, parent maximum years of education, indicators for immigrant parents, parent average age, and number of children in the household. In addition, we also estimate versions of 1 and 2 that use a continuous measure of total county-level spending on all new government-funded plants (irrespective of size) per 1940 resident as the main exploratory variable.

These regression estimators identify the causal effects of plant construction under a conditional independence assumption—specifically that, among regions with observably similar population sizes, plant location decisions were driven by idiosyncratic factors that were independent of potential postwar outcomes of counties and their prewar inhabitants.[29] The purpose of conditioning on observables is *not* to fully parameterize a siting decision process that depended on many factors, but rather to account for the constraints on the otherwise-idiosyncratic assignment process arising from the infeasibility of building many plants in exceedingly underpopulated regions. The conditional independence assumption as stated is a stronger identification assumption than necessary for our county-level analysis; since we observe pre-period realizations of most outcome variables, we could leverage the panel structure of the data to implement a difference-in-differences design that only requires a weaker parallel trends assumption. However, conditional independence is required for our analysis of long-run individual outcomes since they are only observed in the post-period. Accordingly, both equations 1 and 2 are specified as cross-sectional regressions where outcomes are defined for a given period. As specified, identification requires not only parallel trends in potential outcomes but rather complete balance of potential outcomes in all years across

[28] We assign treatment to birth counties in our baseline analysis because birth counties are observed for all individuals in the sample whether or not they are matched to the 1940 census. We also examine specifications restricting to the linked sub-sample where we assign treatment status based on 1940 county of residence.

[29] This assumption further requires a stable unit treatment value assumption (SUTVA) that excludes unmodelled spillover effects across counties, which motivates the exclusion of places adjacent to treatment counties from the control set described above. Nonetheless, there is potential for non-localized general equilibrium spillovers if public investment crowded out private plant construction that would have occurred elsewhere in a no-war counterfactual scenario. Our conjecture is that most counterfactual plant construction would have occurred in the large manufacturing hubs excluded from the analysis sample and that any crowd-out in the comparison regions used in our analysis would have been negligible.

treatment conditions, conditional on the specified observable.

To assess the plausibility of the identification assumptions, we conduct balance tests by estimating Equation (1) using standardized pre-war economic and county characteristics as outcomes. Figure 2 Panel A plots raw differences in treatment and control county means along with differences adjusting for 1940 population and farm share and differences adjusting for additional geographic and infrastructure controls. As expected, treatment counties are substantially larger than the 1,400 comparison counties on average. However, after simply controlling for population size, treatment counties are similar to comparably-sized control regions with regard to earnings levels, demographics, manufacturing development, and industrial composition; we obtain a p-value of 0.30 in a joint test that all differences are zero. Conditioning on geographic and infrastructure characteristics of regions that might affect the suitability of a county for plant siting leads to slightly tighter but otherwise similar balance estimates (joint p-value = 0.36). Further, Figure 2 shows that places where plants were built did not have different enlistment, draft, or casualty rates among prime-aged males during WWII. We find counties are balanced on the continuous measure of spending intensity as well (Appendix Figure C.2). In addition to these balance tests, we test for balance on all outcomes across decades prior to WWII in our county-level analysis in the following section. We additionally test for balance on match rates to the 1940 Census and on parent earnings at the individual level in Appendix Table A.4 and find balance in all cases.

While we cannot directly test for pre-trends in individual earnings outcomes, we are able test for differences in prewar upward mobility in the sample of male children in the full-count 1910 Census who can be matched to the full-count 1940 Census using the crosswalk created by the Census Linking Project (Abramitzky et al., 2022). We assign men in the linked sample who were aged 18 or under in the 1910 Census to their 1910 county and rank them according to their fathers' occupational earnings score calculated following Collins and Wanamaker (2022).[30] We then estimate effects of living in a treatment county in 1910 on adult wage and salary earnings reported in the 1940 Census using the specification in Equation 2 with baseline region-size controls. Figure 2 Panel B displays effects on earnings in 2020 adjusted dollars (including those with no earnings as zeros) and on logged earnings (defined only for those with positive earnings) by 1910 father rank. Consistent with our identification assumptions, we find no evidence of differential economic outcomes or higher rates of upward mobility in treatment counties relative to comparison counties conditional on basic region size.[31]

[30] See Appendix B for additional details about this sample.

[31] In Figure 2 Panel B, we smooth estimates across father ranks using a Kernel smoother: for each percentile level $p \in 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100$ we estimate a separate regression where individuals are weighted by their distance of their father's rank r_i to p using a triangular kernel with a bandwidth of 25

5 County-Level Effects of Wartime Plant Construction

To compare treatment and control counties over time, we estimate Equation 3 separately for each year that county-level outcomes are observed under alternative covariate specifications. The results plotted in Figure 3 show that government-funded construction of large plants had dramatic and persistent effects on local manufacturing employment and on regional development more broadly. By 1970, counties with large public plants had 30 percent higher manufacturing employment, 20 percent larger populations, and 7–8 percent higher median family income than comparison counties. Moreover, consistent with our identification assumptions, we find no differences in outcomes prior to the war conditional on basic measures of region size—treatment and control counties were similar not only in terms of trends but also in outcome *levels* in each year.[32] The estimates are not sensitive to specification and remain virtually unchanged when we condition on geography or additional covariates, including prewar outcome levels. Each panel of Figure 3 also displays the effects of smaller plant openings estimated using Equation 3 with the full covariate set. For these smaller-plant "partial treatments," we do not find any effects comparable to those of the larger plants, though we cannot rule out the presence small effects.[33]

We further explore the dynamics of regional adjustment in Figure 4, in which outcomes are differenced relative to 1940 levels in order to highlight their relative evolution over time.[34] Panel A highlights that, in the immediate aftermath of the war, manufacturing employment in treatment counties expanded dramatically and in far greater proportion than overall population growth. Consequently, manufacturing employment increased as a share of overall employment and remained elevated through the 1970s. However, population growth in treated counties continued to outpace comparison regions in the decades after the war, eventually stabilizing at a new, permanently higher level about 20 percent above that of comparison regions. While the effects on manufacturing employment persisted throughout the 20th century, those effects converged in proportion to the effects on population in the longer term. By

(weight $\omega_i = max(0, 1 - |r_i - p|/25)$). Appendix Figure C.3 shows results within discrete decile bins are similar but noisier. Appendix Figure C.3 further shows that balance is similar across levels of the continuous investment intensity measure.

[32] While 1940 population in Panel A is mechanically balanced across treatment statuses because of the inclusion of this variable baseline controls, there is no such mechanical balance for the outcomes in Panels B and C under the baseline specification.

[33] When estimating the effects of smaller plant openings, we exclude counties in the primary treatment group and immediately adjacent counties from the comparison group, and we additionally drop any remaining counties that are adjacent to the partially-treated counties with smaller plants as well.

[34] All specifications in Figure 4 condition only on the basic region size controls. We present estimates for all outcomes in levels under alternative specifications in Appendix Figure A.1

1990, treatment counties were larger than comparison counties overall but employment was not particularly skewed towards manufacturing. Panel B shows that broader employment, including employment in nontradable sectors like retail, rose in proportion to population; by 1990 the employment effects in other sectors matched the effect in manufacturing. Notably, Panel B also shows that the number of manufacturing establishments did not expand significantly in the short run, but rather expanded gradually in proportion to broader employment. These results suggest that the initial postwar increase in manufacturing employment was primarily a reflection of the large government-funded plant on its own and not broader entry of new manufacturing employers, while the longer-term effects on manufacturing employment largely reflect broader regional expansion. In Appendix Figure A.2 we show that while there were no effects on employment or production in manufacturing employment in adjacent counties, we nonetheless find that population increased in surrounding areas and that residents of surrounding areas were more likely to have manufacturing jobs and higher earnings, consistent with commuting spillovers.

The results in Figure 4 Panel C show that plant construction led not only to an increase in manufacturing employment but also an increase in average production worker pay within the manufacturing sector, which in turn remained elevated for the rest of the 20th century. Average production pay rose by approximately 10 percent, closely tracking similar increases in average labor productivity measured as value-added per worker. Notably, the observed impacts on average manufacturing wages are not reflective of an across-the-board rise in local wages. We also plot effects on average retail wages in Figure 4 Panel C and find effects that are only one-third of the increase in manufacturing wages and are not statistically significant in most years.

Figure 5 provides a broader overview of how wartime plant construction affected the structure of the labor market in the medium-run. The expansion of manufacturing employment as a share of all employment in the 1960s and 1970s is reflected in higher predicted rates of union coverage and a greater share of men in semi-skilled production occupations in treated counties, as evident in Panel B.[35] Most of the reallocation of men towards semi-skilled jobs reflects a movement away from farm work, though we also find that treatment regions had higher male labor force participation rates as well. As employment expanded in semi-skilled production occupations, average male wages within those occupations rose significantly as well—Panel B shows that average wages in craftsperson and operator jobs rose by 8 percent, more than double the increase in high-skill professional and managerial

[35] We predict county-level union density using 1953 estimates of industry union density from Troy (1957) and the industry employment distribution within each county from the 1964 County Business Patterns (CBP) data assembled by Eckert et al. (2022). We use CBP data from 1964 because it is the earliest year with subindustry detail for a sufficient number of counties.

occupations. On the whole, treated counties had 8 percent higher median male individual incomes by 1979, reflecting both the shift towards higher-wage occupations and increases in average pay within occupations. The rise in median family income is primarily a reflection of increased male earnings. We find minimal impacts on female median individual income and no effects on female labor force participation rates, consistent with the findings in Rose (2018).

Our results suggest that the observed effects in county-level average wages are likely driven by high-wage employment at the newly-constructed treatment plants themselves.[36] These plants were concentrated in industries like metal products, chemicals, and transportation equipment that involved more skill and typically paid higher wages than other manufacturing industries like textiles and food processing (Krueger and Summers, 1988). One potential explanation for higher wages in such plants is a rise in production rents as the U.S. established global dominance in these industries after WWII (Field, 2022) along with increases in worker bargaining power over the same period. Increased bargaining power came from greater union representation at treatment plants, most of which were unionized after the war.[37] While the available data do not enable us to test whether unionization at war plants drove higher wages, in Figure 5 we find that industry composition shifts in treated counties predict increased unionization rates.[38] Even without formal unions, workers in the increased number of semi-skilled positions—often requiring expertise in the use of complex, specialized machinery—may have had more *de facto* hold-up power to leverage in wage negotiations.

However, these wage estimates do not hold worker characteristics fixed. Increases in county-level average wages and productivity levels might reflect compositional changes in

[36]The short-run expansion of county level manufacturing employment despite no comparable increase in establishment creation and the limitation of large wage increases to manufacturing jobs implies that all increases in manufacturing wages were driven by either the new entrant firm or other incumbent plants. Since comprehensive establishment-level data is not available for the decades following WWII (as far as we are aware), we cannot directly differentiate the contribution of the new plant itself from changes at other incumbent establishments.

[37]During the war, the National War Labor Board (NWLB) gave unions wide latitude to organize war plants in exchange for pledges not to strike in response to the imposition of wage ceilings (Goldin and Margo, 1992; Wilson, 2016). By 1946, over 80 percent of productions workers in war-related industries—including automobile, aircraft, shipbuilding, steel, and aluminum manufacturing—were covered by union contracts (BLS, 1947). The appendix to Farber et al. (2021) presents evidence that greater war production led to greater unionization rates at the state level.

[38]If unions were an important driver of wage increases, one might expect to find smaller effects on wages in states with early right-to-work laws. However, we find similar wage effects across states with and without such laws in place by 1960 in Appendix Figure A.3 and in Panel A of Appendix Table A.5. We also find similar individual-level earnings effects when restricting the sample to right-to-work states in Appendix Table C.3. It is possible, though, that early right-to-work laws would not have impacted plants that unionized during the war. More generally, the evidence on the effects of early right-to-work laws on unionization is mixed (Farber et al., 2021).

the manufacturing workforce rather than changes in compensation practices. New positions requiring more advanced skills may have been filled by recruits from other counties bringing higher levels of prior training to the job at higher market wages. In that case, average wages in treatment counties might rise without any improvement in market opportunities for incumbent residents. In the next section, we draw on longitudinal data to directly test whether long-run earnings increased for *individuals* growing up in treatment regions prior to the war.

We present additional robustness tests in Appendix C.2. The results are stable across alternative covariate sets and we find that ATET estimates obtained using propensity score reweighting are nearly identical to the baseline OLS estimate. We find the same pattern of effects when regressing on a continuous measure of investment in new publicly-funded plants per 1940 resident instead of a binary indicator. Moreover, the results are similar whether using our baseline restriction excluding the 100 counties with the most manufacturing employment in 1939 or using alternative restrictions to focus on variation outside of the largest prewar urban agglomerations. We find consistent manufacturing expansions and increases in earnings when restricting to non-metropolitan counties, when excluding counties in the largest 50 commuting zones (based on 2000 delineations and 1940 populations), or when additionally dropping state capitals from our baseline sample.[39] Likewise, the results are robust to dropping plants that had small but non-zero private spending on structure or those without explicit "new plant" labels in the WPA data book from the treatment set.

6 Long-Run Earning Effects on Individuals

Did the large and persistent expansions of high-wage manufacturing employment in places where war plants were built translate to improved long-run outcomes for pre-war residents? We estimate the effect of wartime plant construction on the adult earnings of children born between 1922–1940 using the person-level regression specification in Equation 2, in which treatment and comparison county status is assigned by one's county of birth rather than adult location. We begin by presenting effects for men in Table 1. The main outcome is average real wage and salary earnings reported on 1040 individual income tax returns in 1969, 1974, 1979, and 1984, where missing values are coded as zeros.[40] Table 1 displays effects

[39] While we find nearly identical effects on manufacturing wages, manufacturing employment as a share of total employment, and median incomes across specifications, we find somewhat smaller effects on broader population growth in samples that restrict the analysis to relatively more isolated regions using the IPUMS 1940 METAREA indicator (Manson et al., 2019), see notes to Appendix Figure C.6.

[40] We use wage and salary earnings as our primary outcome rather than AGI because AGI is a noisier measure of labor income since it includes capital income and business earnings, which can be highly volatile across years and take on negative values (unlike wage earnings, which are always non-negative). We find

estimated using all men in the NUMIDENT and the subset who are matched to the 1940 Census. In all cases, we present results using each of our three primary control specifications, and we additionally estimate specifications with controls for individual demographics and parent characteristics in the samples matched to the 1940 Census.

The results in Table 1 show government-funded plant construction in one's birth county substantially increased adult earnings two to four decades after the war, particularly for individuals from lower-income backgrounds. On average, men born in treatment counties had approximately \$1,200–\$1,300 more in wage earnings (2020 adjusted dollars) on their tax returns as adults, an increase of 2.5–3 percent and equivalent to a roughly a one-percentile rise in the adult earnings distribution. While the confidence intervals in the most parsimonious specification are relatively wide, the inclusion of additional controls increases the precision of the estimates, which remain stable in magnitude across specifications. Moreover, we find that large plant openings led to similar earnings increases for children born in adjacent counties (Appendix Table A.7). As in the county-level analysis, we find that the construction of smaller plants had no detectable effect on long-run outcomes (Appendix Table A.7). In robustness tests in Appendix C.3, we find consistent results using more conservative definitions of the treatment groups, alternative criteria for excluding the largest prewar manufacturing regions, the continuous measure of investment intensity instead of a binary treatment indicator, or individual treatment assignments based on one's 1940 Census county (when available) rather than their birth county.

One might expect expanding high-wage manufacturing work opportunities to primarily benefit children of lower-income parents, both because those jobs might have increased their parents' earnings the most during childhood and because they themselves might have benefited the most from access to those jobs in adulthood. Accordingly, we examine effect heterogeneity by pre-war parent earnings in Panel A of Figure 6, which presents estimates by 1939 combined parent wage earnings rank. We smooth estimates across father earnings as follows: for each percentile level $p \in 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100$ we estimate a separate regression where individuals are weighted by the distance of their father's rank r_i to p using a triangular kernel with a bandwidth of 25 (weight $\omega_i = max(0, 1 - |r_i - p|/25)$). Figure 6 plots estimates on the y-axis against percentiles p on the x-axis. Appendix Figure C.12. We separately plot effects for individuals linked to parents with missing earnings and examine this group in greater detail in Appendix Table A.9.

Across all specifications in Figure 6, we find the largest effects for children of parents at the bottom of the 1939 earnings distribution. As in Table 1, adding controls beyond basic county

nearly identical effects sizes on AGI in Appendix Table ??, indicating that any effects on non-wage earnings are second-order.

size increases the precision of the estimates, but effect sizes are stable across specifications. For children of the lowest-income parents, wartime plant construction increased adult wage earnings reported on 1040 returns by roughly $2,000 per year or about 4 percent. By contrast, we find no effects on children of parents with the highest 1939 earnings, with effect sizes declining linearly by parent rank. Following the terminology in Chetty et al. (2014), plant construction therefore increased upward mobility for children of low-earning parents in both an *absolute* sense, in that they had higher lifetime earnings relative to similar children in comparison regions, and in a *relative* sense, in that their outcomes converged towards those of children from the most affluent backgrounds. We find that effects on children with parents with no wages are most similar to the effects on children of the lowest-earning parents. In our setting, the former group includes many parents with non-wage farm income who are likely best thought of as low-income families.[41] In Appendix C.3, we find similar patterns of results when estimating effects withing discrete decile bins, when examining effects on AGI, and when ranking fathers by their occupational status following Collins and Wanamaker (2022).[42]

Our results are evidence that wartime construction of large manufacturing plants increased long-run upward economic mobility among men in affected regions, with the largest gains accruing to those from the most disadvantaged backgrounds.[43] One possible interpretation of the results in Figure 6 Panel A is that plant openings always benefit children from lower-income backgrounds more; however, an alternative possibility is that children with parents at the bottom of the national earnings distribution are concentrated in a subset of smaller, more rural regions where new plants had larger impacts on local work opportunities for *all* residents. To distinguish between these channels, Panel B replicates the analysis in

[41] Appendix Table A.9 shows that effects for children without 1939 wage earnings are of comparable magnitude whether or not parents income indicate the presence of non-wage 1939 income in the 1940 census. Many parents with non-wage earnings are farmers and we observe large effects among children of fathers with agricultural occupations irrespective of their incomes. We also find similarly large effects among children who do not match to the 1940 census at all, which potentially reflects lower match rates among children of lower socioeconomic status as documented by Rothbaum and Massey (2021).

[42] We find that slope of effect sizes with respect to ranks is muted when using occupation-based bins, which suggests that occupation scores from a single year might measure true earnings ranks with substantial error, as recently highlighted by Ward (2021).

[43] While our results are indicative of increased upward economic mobility, with increased convergence of outcome of children from the lowest-earning backgrounds towards those of children from affluent backgrounds, these results do not necessarily imply an increase in *intergenerational* mobility since medium-term effects of plant openings on parent's postwar careers might mediate children's long-run outcomes. That is, children might benefit in part *because* of improvements in parent outcomes, not *despite* persistently low parent earnings. A growing literature finds that increasing the incomes of poorer parents can have large effects on children's log-run earnings in a wide array of settings (Hoynes et al., 2016; Aizer et al., 2016; Bastian and Michelmore, 2018; Barr et al., 2021) and parents often transmit specific jobs and employers to the next generation (Staiger, 2022). We consider potential mechanisms in detail in the next section.

Panel A by re-weighting observations so that the total weight in each county is constant (and equal to one) across all regressions—this eliminates the differential representation of different counties across earnings levels.[44] When we impose equal county weighting, we find effects sizes that are constant throughout bottom six deciles of the parent earnings distribution but then drop off steeply at the very top of the earnings distribution.[45] Thus, children of low- and middle-income parents living in the same place as one another were similarly impacted by plant openings, while there was no impact on children of the most affluent parents. We find a similar pattern of effects to Panel B when regressing on the continuous measure of investment per 1940 resident (Appendix Table C.1), which confirms that the smaller treatment effects at the top of the parent earnings distribution are not simply a reflection of exposure to relatively smaller plant openings among these individuals.

Effects By Race. We next examine whether wartime plant construction differentially impacted Black and White men. It is well-documented that Black-White earnings gaps declined around WWII (Maloney, 1994; Margo, 1995) and there is evidence that regional labor shortages (Ferrara, 2018), increased production demand (Aizer et al., 2020), and federal anti-discrimination policy during WWII (Collins, 2001) created new opportunities for occupational advancement by Black men during the war. These gains persisted into the postwar era and in turn contributed to a decline in the racial wage gap.[46] In our setting, it is plausible that shrinking racial gaps account for the increases in upward mobility documented in Figure 6.

Table 2 presents estimates of Equation 2 separately for Black and White male children, using race reported in the 1940 Census. We find that, on average, wartime plant construction increased adult annual earnings of Black children by 4–6 percent (1,500–2,500 2020 Dollars) and of White children by 2–3 percent (1,000–1,500 2020 Dollars). Across specifications, our estimates imply that the the racial earnings gap shrunk about 2 percent in the treatment group. However, declining racial gaps cannot explain the heterogeneity by parent earnings documented above. As in the full population, we find larger effects on White children of parents with earnings below the pooled-race national median and no significant effect on White children of above-median parents. By contrast, we find that Black children of parents

[44] Formally, denoting the kernel weight on individual i born in county c in a given regression by ω_{ic}, we take the sum of weights of all individuals born in each county W_c^{Total} and re-normalize the kernel weights by the county totals such that the new weights $\tilde{\omega}_i \equiv \omega_{ic}/W_c^{Total}$ total to one within each county.

[45] Imposing equal weighting also improves precision across all specifications. This is because the individual analysis effectively gives disproportionate weight to larger counties (even after trimming the very largest); the individual analysis thus effectively down-weights the identifying variation coming from the majority of treatment counties with smaller populations.

[46] During the war, federal regulations under the Fair Employment Practice Committee explicitly attempted to curb racial discrimination among firms with military production contracts

with earnings above the pooled-race national median experienced the largest earnings effects (an increase of 7–10 percent or 4,000-5,000 2020 Dollars) whereas effects for lower-income Black children were 4–6.5 percent and were only 3–4 percent among Black children of parents with no wage earnings. Thus, while Black-White earnings gaps appear to have shrunk across the board, plants construction led to the largest declines in racial earnings gaps among children of parents with earnings higher in the national distribution.

Effects By Gender. Table 3 presents result by gender. When examining *tax filing unit level* outcomes from 1040 income tax returns in Columns 1–6, we find comparable effects on wage earnings for both men and women. However, for married individuals, 1040 wages are the sum of one's own wages and the wages of one's spouse. Since a majority of male and female individuals in our sample are married, one should expect to find similar effects across genders. While we do not observe individual earnings in the population 1040 return data, we are able to observe *individual-level* wage earnings from W-2 returns beginning in 1978 for the smaller SSA-CPS sample.

In contrast to the tax-unit-level results, when we test for effects on individuals W-2 wage earnings within this sample in Columns 7–10, we find stark differences between men and women. We find the effects on men's W-2 wages is nearly identical to the effect on their 1040 wage earnings in the same year, which we estimate for the SSA-CPS sample and present in Columns 11–12. Meanwhile, we find *no* effect on women's individual W-2 earnings, even though we continue to find effects on Form 1040 wages within the SSA-CPS sample that are roughly equal to the effects for men. These results imply that the effects on 1040 wage earnings for women are driven entirely by increases in the earnings of their husbands if married (as over 80 percent of women in our sample are). Likewise, the increases in 1040 wages for men appear to directly reflect increases in their individual earnings. These results are consistent with the county-level results presented above and with recent work by Rose (2018) that finds that although war production led to an increase in female manufacturing employment during WWII, women were almost universally excluded from manufacturing jobs after the conclusion of the war. Interestingly, when we examine heterogeneity by 1940 parent earnings in Appendix Figure A.4, we find the same pattern of effects on 1040 wage earnings for women as for men—the effects on household incomes are largest for women born to the lowest-earning parents, who tended to marry men from similar economic backgrounds.

7 Drivers of Upward Mobility: Access to High Wage Jobs or Developmental Human Capital Effects?

The results in the previous section establish that the government-led construction of manufacturing plants increased the lifetime earnings of children in the region, particularly sons from lower-income families. However, the broader policy implications depend on *why* earnings rose for these individuals. One possibility is that places where plants were built became *better places to grow up*, such that children exposed to those improved places experienced greater human capital development that increased their long-run earnings potential (Topa and Zenou, 2015; Chetty and Hendren, 2018b; Chyn and Katz, 2021).[47] In that case, greater human capital development in affected regions might promote self-sustaining growth and generate benefits that outlast the manufacturing facility itself. Alternatively, plant construction may not have generated any significant development effects on local children, whose earnings might have increased simply by virtue of improved access to better-paying manufacturing jobs in adulthood or from better job opportunities available at the start of their careers as in Kahn (2010) and Oreopoulos et al. (2012). In this section, we examine the available evidence to shed light on the mechanisms at play. The balance of evidence suggests that affected individuals benefited primarily from the local expansion of higher-wage jobs to which they had access as adults, rather than because of developmental effects from exposure to better environments during childhood.

7.1 Developmental Effects During Childhood

To examine whether plants may have led to improved human capital development, we begin by testing for impacts on formal educational attainment. We draw on educational outcomes reported in late adulthood in the 2000 Census long-form survey, which are available for 11.4 percent of men and 13 percent of women in the main NUMIDENT sample.[48] Figure 7 displays effects by 1939 combined parent wage earnings rank obtained following the specification used in Figure 6 above; we display estimates for the full sample and subsamples not matched to parents in the 1940 Census, with estimates in Appendix Table A.10.

[47] In this setting, developmental gains could in principle stem either from improved family resources or from pure place effects as in Topa and Zenou (2015).

[48] The 2000 long form was administered to one in six individuals alive at the time, but our match rates are lower than one-sixth both because not all individuals in our sample survived to 2000 and because some individuals in the 2000 data cannot be linked to a valid PIK. We test for differential match rates by treatment status in Appendix Table A.11. In contrast to the 1940 Census, for which we find no significant association between treatment and match rates, we find some evidence for lower match rates to the 2000 long form survey (approximately one-half of a percentage point less) in the treatment group in some specifications.

We find evidence of modest but statistically significant and robust impacts on male educational attainment.[49] In Panels A and B of Figure 7, we find the largest increases in total years of schooling obtained and high-school completion concentrated among individuals with the lowest-earning parents; these children completed approximately one-quarter of a year more schooling and were 3 percentage points more likely to graduate high school. These effects fade out quickly as one looks higher in the parent earning distribution. In contrast, while we find some evidence of small increases in college attendance and completion, we find the largest effects—about a 2 percentage points on both margins— on children of parents in the middle of the income distribution, with smaller or no effects for children of the highest- and lowest-earning parents. We find similar effects for both Black and White men in Appendix Table A.12. However, we find zero effect on female educational attainment across the parent income distribution (Panels D and E of Appendix Figure A.4).

Our results suggest that increased education among children from the lowest-income backgrounds contributed in part to the effects on economic mobility documented above. However, the magnitudes of the educational attainment effects are too small to fully account for the effects on adult earnings. To compare the effects on education and earnings, we estimate simple Mincer regressions of 1979 log wages reported on Form 1040 on years of schooling, a quadratic in 1979 experience, and indicators for high school and college degree completion for men in the sample who were born in control regions. We then use the resulting estimates to predict 1979 wages of all men in the sample matched to the 2000 census long form based only on educational attainment. In Appendix Table A.10, we find that the effect on predicted earnings is statistically significant, but can explain less than one-half of the actual effect on 1979 wages we estimate in the 2000 long form linked sample.[50]

Children growing up in affected counties may have experienced other developmental benefits that are not reflected in formal education. A growing literature finds that, while growing up in certain places can causally improve long-run outcomes, those developmental impacts typically take the form of *exposure effects* that scale with the amount of time spent in the region (Chetty et al., 2016; Chetty and Hendren, 2018b; Chyn and Katz, 2021). If developmental effects of this type are an important driver of our estimated earning effects, one would expect the effects to be largest for the youngest cohorts who had the most potential exposure to treatment counties after the plants had been constructed. However, we find no evidence of heterogeneity in earnings effects across birth cohorts in Appendix Table A.13.

[49] We also find evidence of effects on local government spending per capita on education in the late 1950s in Appendix Figure A.5, though the effect disappears by the early 1970s.

[50] Estimates of the returns to a year of schooling in the middle of the twentieth century typically range from 4 to 9 percent (Goldin and Katz, 2008; Feigenbaum and Tan, 2020)—even at the high end of this range, the effects on years of schooling in Figure 7 could not account for the effect sizes in Figure 6.

Another possibility is that as migration into treatment counties increased after the war, local residents benefited from proximity to higher-skilled individuals moving in nearby. However, in practice, we find no evidence that postwar migrants into treatment regions were from better-educated or higher income families or had more education themselves compared to migrants into comparison regions (Appendix Table A.14).[51]

7.2 Contemporaneous Effects On Adult Labor Market Opportunities

Even if wartime plant construction did not make treated counties better places to grow up in the sense of Chetty and Hendren (2018b), the expansion of high-wage jobs open to lower-skilled individuals in treatment counties may have contemporaneously increased earnings of incumbent children when they reached adulthood. To explore that possibility, we first examine whether children born in treated counties go on to live in labor markets with higher-wage blue-collar jobs as adults. We estimate the effect of being born in a treatment county on the characteristics of one's 1979 county of residence in Figure 8.[52] We find that, in adulthood, individuals born in treatment counties lived in counties with higher median male earnings, higher manufacturing wages, and higher wages in semi-skilled professions–but not higher wages in highly-skilled professions. The magnitude of these effects—3 to 4 percent higher male wages, driven by increased wages in blue-collar jobs—is consistent with the individual earnings effect sizes in Figure 6.

These effects on adult county characteristics appear to be primarily a reflection of the labor market improvements in workers' birth counties. The directions of the effects on adult location characteristics are consistent with the impacts on individuals' birth counties (also displayed in Figure 8). However, the effects on adult characteristics are notably *smaller* than the effects on birth county characteristics, indicating that individuals from both the treatment and control group who move out of their birth counties wind up in relatively similar labor markets. Meanwhile, we find that plant construction has no significant impact on long-run geographic mobility, though the the sign on the point estimates indicates that treated

[51]The analysis in Appendix Table A.14 examines the characteristics of individuals in the 1922–1940 birth cohort NUMIDENT data who were born in a different county than where they reported living in 1979. The coefficients on in-migrant education are similar in magnitude to the effects on incumbent children, but the effects are never statistically significant. We do find evidence that in-migrants in treatment regions had substantially higher earnings than those in comparison regions, but this result is also consistent with their improved access to higher-paying jobs in adulthood, as discussed below.

[52]Counties on 1979 returns are obtained by linking reported 1040 addresses to an extract of the Census Bureau's Master Address File (MAF). The MAF includes all current residential addresses, requiring the 1979 addresses still exist by at least 2008 (the initial available MAF year). Importantly, we find no effect of treatment on the presence of a linked 1040-MAF county in our sample.

individuals are slightly more likely to remain in their birth counties through adulthood.

Appendix Figure A.6 presents several additional results that suggest that treated individuals primarily benefit from wartime plant construction because of access to improved labor market opportunities in their birth counties if they remain there through adulthood. In Column A, we present results separately for individuals who remain in their birth county and those observed to have relocated by 1979. Strikingly, we find large differences in earnings between stayers in treatment and control counties—which, for children of low-income parents, are similar in magnitude to the county-level manufacturing wage effects—but *no* difference in outcomes for treated movers and control movers. While suggestive, this result conditions on adult location choice (an endogenous outcome) and therefore cannot be directly interpreted as a causal effects on earnings. In principle, the results in Column A could also arise if plant construction selectively induced higher-ability individuals to remain in their birth counties. However, if this were the case, we would expect to observe similar differences in observable measures of skill or ability like educational attainment. In practice, we do not find larger treatment-control differences in educational attainment at the bottom of the parent earnings distribution for stayers compared to what we find for movers.

To further probe whether adult location might mediate the earnings effect, we estimate specifications that limit the comparison to treatment and control individuals living in the same counties as one another in adulthood by including 1979 location fixed effects. The results in Panel B of Figure A.6 find there is *no* effect on earnings after conditioning on adult location, such that adult location *entirely* explains the increased earnings in the treatment group. This result is consistent with wartime plant construction increasing postwar market wages for all blue-collar workers in the regions regardless of one's birth county, with children born nearby benefiting more than those born elsewhere simply by virtue of being more likely to live in the area as adults in the presence of migration frictions. Again, this analysis conditions on an endogenous outcome (adult location choice) and therefore could reflect either a causal effect of adult location or sorting of individuals with the same endogenous earning potentials into the same locations. Nonetheless, the descriptive results in Figure A.6 limit the number of stories consistent with our findings: either treatment effects are causally mediated by contemporaneous place effects in adulthood, or plant construction had positive developmental effects and induced selective "brain-gain" migration where the positive causal effects and negative selection of out-migrants exactly cancel out.

Although we do not observe employer information in the Form 1040 data, we are able to more directly examine effects on adult job characteristics in the smaller SSA-CPS sample for which we observe job-level annual compensation information with firm tax identifiers from W-2 information returns. We use these identifiers to match firms to data on all of their constituent establishments in each year contained in in the Business Register (BR), which reports annual employment, pay, and industry classifications for the universe of establishments. This in turn allows us to match firms to their industry's 1984 wage premium from Krueger and Summers (1988), which we use to measure whether jobs are in high-paying industries.[53] We then estimate the effects of plant construction in one's birth county on the characteristics of full-time minimum wage equivalent jobs held in adulthood, pooling data on all jobs held during the decade 1978–1987 (the first decade for which data is available). Specifically, we estimate regressions pooling observations across years using the following specification:

$$\ln Y_{i,t} = \alpha_t + \beta Treat_{c_{birth}(i)} + \gamma X^{1940}_{c_{birth}(i)} + \delta Z_i + \epsilon_{i,t} \tag{3}$$

This is equivalent to the cross-sectional specification in Equation 2, but is estimated on a panel of outcomes.[54]

We find in Table C.7 that men born in counties where war plants were built worked at jobs in higher-paying industries as adults, and that the effects on the wage premium of one's adult employer were largest for children of low-income parents. These effects are sizable: just using information about the coarse industry category of one's adult employer alone is sufficient to account for approximately one-third of the realized increase in log wages.[55] Any wage increases from shifts towards higher-paying firms or narrow industries within these coarse industry groupings would be supplemental to the estimated effects. While we find some evidence that individuals born in treatment counties were more likely to work for firms with at least one manufacturing plant, the effects are not significant, suggesting that the effects on industry premiums largely reflect shifts towards higher-wage industries within the

[53]A single firm tax identifier (Employer Identification Number, or EIN) may have several establishments in different industries; accordingly, we match the wage premium estimates from the published Table A1 in Krueger and Summers (1988) at the two- or three-digit SIC code level (as made available) to each establishment-year observation in the BR based on the reported SIC code. We then calculate the employee-weighted average premium at the EIN-year level, which are then linked to SSA-CPS job observations.

[54]Importantly, $Treat_{c_{birth}(i)}$, $X^{1940}_{c_{birth}(i)}$, and Z_i are time-invariant. We include controls Z_i for birth year and race reported in the CPS ASEC in all specifications to increase power. We define full-time minimum wage jobs as those with earnings exceeding the amount that would be earned from 2000 hour of work at the prevailing federal minimum wage in each year. We limit our focus in this analysis to full-time minimum wage jobs with non-missing industry information for comparability to the wage premium estimates in Krueger and Summers (1988), though this limits comparability of the wage estimates to those in other analyses. In Appendix Tables C.7 and C.8 (respectively) we examine the sensitivity of our wage estimates to conditioning on employment and test for treatment effects on employment status.

[55]The effects on full-time minimum wage earnings for men in the SSA-CPS sample reported in Table C.7 are larger than the effects in the 1040 return data reported in Table 1. This largely reflects the composition of CPS-SSA sample; when we restrict the analysis to individuals in the smaller SSA-CPS census in Table 3 above, we find larger effects on 1040-reported wage earnings that are comparable in magnitude with effects on W-2 wages for that sample. In Appendix Table C.7, we show the effect sizes increase further when additionally restricting to full-time minimum wage jobs in the SSA-CPS data as in Table C.7.

manufacturing sector rather than shifts into manufacturing.

Taken together, these results suggest that a key channel through which wartime plant construction benefited individuals growing up nearby was the local expansion of higher-wage employment that was available to those individuals in adulthood. Whereas geographically-mobile individuals wound up in similar places and experienced similar outcomes in adulthood regardless of their treatment status, it appears that treated individuals who remained in their birth counties benefited contemporaneously from improved labor market opportunities in the region. On one hand, these improved blue-collar work opportunities might be a reason the observed effects on educational attainment were not larger, as higher opportunity costs led individuals to pursue less schooling.[56] At the same time, it is also possible that the increases in educational attainment we do observe are a *result* of workers pursuing expanded opportunities in semi-skilled occupations requiring high school level education.[57]

8 Discussion and Policy Implications

8.1 Manufacturing Investment and Upward Mobility

Our results indicate that government-funded plant construction during WWII generated large lifelong benefits for children growing up nearby. We estimate that having a plant built in one's birth county led to an unconditional increase in men's average wage earnings of $1,200 (2020 dollars) in each year over two decades. From the perspective of the localities where plants were built, a $24,000 increase in earnings over twenty years for each of the nearly 1.2 million men born 1922–1940 in treated counties implies a combined earnings increase of approximately $29 billion dollars over that period. This figure is likely a significant understatement of the total gains accruing the prewar population of treated counties, as it does not reflect any earnings gains for these cohorts over the first two decades after WWII, nor does it include any postwar earnings gains for either older cohorts of incumbent residents born before 1922 or younger cohorts born after 1940.[58] Even after accounting for those additional years and cohorts, the resulting figure would still not reflect earnings gains for those born in adjacent counties, which our estimates suggest could be of similar magnitude

[56] Prior research finds evidence linking increased work opportunity in manufacturing and other blue collar jobs to lower levels of educational attainment across a wide array of settings (Goldin and Katz, 2008, 2009; Atkin, 2016; Charles et al., 2018b).

[57] Similarly, if children benefited from connections to local high-wage jobs at firms where their parents worked as in Staiger (2022), such connections would have only mattered in the case that those firms continued to offer employment opportunities when they reach adulthood.

[58] We only include men in this calculation to avoid double-counting, since Form 1040 wages are reported jointly for married couples and we find no evidence of increases in female individual earnings.

to those in the treatment counties themselves.[59] Although our data do not allow us to give a full accounting of income gains to local residents from 1945 onwards, our estimates suggest it is highly plausible that the net present value of the earnings gains for incumbent residents exceeded the initial $60 billion in government investment, a portion of which was recovered through post-war sales of the plants.[60] Thus, setting aside any military value, wartime plant construction appears to have been an effective means of raising local incomes, providing what could be called place-based "predistribution."

Yet, while our findings offer evidence that policies that attract manufacturing investment into a region can succeed at increasing upward mobility in the local population, the results also highlight potential limitations of such policy efforts. In particular, our analysis suggests that a key mechanism by which plant construction benefited local residents was the persistent expansion of employment in jobs that offered higher-wage opportunities to workers from the region than would have been available to them otherwise. Importantly, such increases in high-wage work are not a necessary consequence of policies that attract local investment; for instance, in simple spatial equilibrium models with perfect mobility across regions and fully competitive labor markets (Roback, 1982), one would expect no change in local wages within skill groups. In our setting, the observed increases in wages for semi-skilled blue collar jobs are likely at least partly a reflection of the specific institutional context of the war emergency and the postwar era. The urgency of the wartime industrial mobilization led simultaneously to the adoption of production techniques that were intensive in the untrained labor at hand, changes in norms surrounding equity and fairness, and increased institutional bargaining power of unions in sectors engaged in war production, while American manufacturing firms established global economic dominance in the wake of WWII in part due to the widespread destruction in major European and Asian economies—these all likely contributed to conditions that empowered semi-skilled manufacturing workers to secure significant wage gains after the war. In addition, the expansion of semi-skilled work opportunities in less-industrialized regions of the U.S. created new opportunities for occupational and skill upgrading, which may have driven some of the observed increases in educational attainment among children from low-income families.

As a result, there is reason to think it might be challenging for modern policy to replicate the impacts of plant construction on local upward mobility observed during WWII. Whereas

[59] We find the impacts on children born in treatment counties and the impacts on children in adjacent were roughly equal in magnitude. Given that the combined population in the the 363 adjacent counties and the 90 treatment counties is 2.8 times the population in the treatment counties alone.

[60] The government typically recovered 40 percent of construction cost when selling plants to the public sector, based on 18 large plants sold by early 1947 that were reported by the War Assets Administration in Administration (1947).

the the WWII era witnessed the expansion of production methods intensive in less-skilled labor, expanded unionization, and limited international competition, the past half century has been characterized by the ongoing automation of routine semi-skilled tasks, declining institutional power of unions, and increased manufacturing competition from lower-wage economies. As a result, the manufacturing jobs accessible to individuals from economically-disadvantaged backgrounds today may no longer offer the same economic advancement opportunities they once did (Acemoglu and Autor, 2011; Wilson, 1997). This shift is evident in the data—Appendix Figure A.8 shows that while there was a clear relationship between manufacturing employment density and upward mobility in 1950, that association entirely vanished by the end of the 20th century. The same shift is also apparent in evidence from the more more recent "million-dollar plant" (MDP) openings in the 1980s and 1990s first studied by Greenstone et al. (2010). In Appendix Figure A.7, we follow the event-study specification from Monte et al. (2018) to estimate effects of MDP openings on county-level average manufacturing pay. In sharp contrast to our finding of large increases in average manufacturing wages after government-funded plant construction during WWII, we find that MDP openings expanded manufacturing employment (as documented previously) but had *no* impact on average wages, suggesting little increase in opportunities for economic advancement.[61] Similarly, Slattery and Zidar (2020) find that plant openings since 2000 have no impacts on county-level incomes. Thus, there is reason to think that modern-day place-based industrial interventions may do little to promote upward mobility in target regions unless explicit efforts are made to create higher-wage worker opportunities for local residents.

8.2 Persistence and Path Dependence

In considering broader policy implications of our findings, another key question is whether wartime plant construction is correctly thought of as a one-time intervention rather than as the initial selection of a location that was supported by continued public spending after the war. Indeed, many treatment plants—particularly those producing ordnance and ammunition—went on to operate as government-owned, contractor-operated (GOCO) military production facilities throughout the Cold War era. This raises the possibility that the persistence in regional effects was largely driven by continued military spending directed at regions where plants were built during WWII.

To test whether the observed persistence was driven by Cold War military spending, Appendix E separately examines the impacts of constructing ordnance and ammunition facilities, which typically became GOCO facilities or Air Force Bases after WWII, and the

[61] Patrick (2016) also re-examines the million-dollar plant data and finds similar results.

impacts of constructing other types of plants that overwhelmingly transitioned to privatized production for civilian markets.[62] We first measure Vietnam War era defense spending using information from DD350 forms in the Military Prime Contract File databases from 1966 to 1975 and verify that counties with ordnance and ammunition facilities received disproportionately more defense spending during the Cold War while counties with other plants did not. However, turning to long-run outcomes, we find the regional effects on manufacturing employment were similarly persistent in both cases, and that both types of plants had comparable impacts on the long-run earnings of local children. Moreover, *only* general manufacturing plants—which did not lead to increased postwar military spending—had large permanent effects on overall population growth. Thus, the persistence of effects appears to not have been driven by postwar military spending.

A simple explanation for the medium-term persistence of the effects documented in Section 5 was the persistence of the publicly-financed plant itself. Even if the majority of the initial wartime investment in structures and equipment at these plants depreciated quickly during the transition to peacetime, the sunk costs in surveying and securing a suitable parcel for a plant, obtaining local approval for its operation, and establishing connections to transportation and power infrastructure would have made it worthwhile to continue to re-invest in updated facilities at those existing sites rather than establish new sites *de novo* in other locations.[63] The persistent value of the plant sites themselves may have been enough to generate highly persistent effects on local manufacturing even without broader agglomeration effects at the region level. Consistent with this explanation, Appendix E presents evidence that public plant construction in a county led to persistently higher levels of private manufacturing investment in the region—likely in the original plant itself, given the absence of a similar effect on the number of establishments in Section 5. Case study evidence about specific plants presented in Appendix D confirms that many of the plants built during WWII plants continued to operate through 1980 and beyond.

However, the longer-term persistence we document in Section 5—in particular the seemingly permanent effect on log population—is indicative of path-dependence in regional dynamics. Control counties *never* converge to treatment counties in population size, even through the end of the 20th century, despite the fact that the initial plants began to shutter and the manufacturing employment shares in treatment counties fell to parity with shares

[62] Using descriptions in the WPB data, we classify the largest new public plant in each of the main treatment counties as either an ordnance/ammunition plant or a general manufacturing plant. We include the U.S. Government Aircraft Modification Centers and Assembly Plants in the category of ordnance and ammunition plants, which were typically built with large airfields and converted to Air Force Bases after the war.

[63] Redding et al. (2011) explicitly model how sunk costs give rise to spatial persistence in the case of airports.

in control counties. More generally, there are no observable long-run differences in the *composition* of the production structures of treatment and control counties—all long-run effects on quantities are in exact proportion to the population effect. These results suggest that the persistent population differentials are self-sustaining and not driven by the continued operation of the initial plants, consistent with path-dependence and multiple equilibria as previously documented in Bleakley and Lin (2012) and Hanlon (2017). As in Ehrlich and Seidel (2018), subsequent development of public infrastructure may have served to reinforce the larger size of treatment regions; for instance, we find positive effects on local government spending on utilities capital outlays per capita in Appendix Figure A.5 and additionally find in Appendix E that treatment counties were more likely to be connected to the interstate highway system in subsequent decades.

With multiple equilibria, there may be threshold effects such that sufficiently large interventions ("big pushes") might shift regions past tipping points into new self-sustaining equilibria while smaller interventions fail to have any lasting effect Murphy et al. (1989); Kline and Moretti (2014).[64] This is one possible explanation for the absence of any detectable impact of smaller plant openings in our analysis. To further probe whether there are nonlinear returns to investments that are larger relative to baseline size of the local labor market, we sort the 147 counties with any larger or smaller public plant construction into quartile bins according to the level of investment per 1940 resident and test how effects differ across these bins in Appendix Figure A.9 and plot effect sizes against the level of spending intensity. While the effects are by far the largest in the top intensity bin, the average spending per capita in this bin is dramatically higher in this top bin than in the other bins (in 2020 dollars, $16,700 per resident in the top bin, $4,100 in the second, $1,200 in the third, and $300 in the fourth). Accordingly, and given the wide confidence intervals on the the bin-specific estimates, we cannot rule out the possibility that effects are linear in investment intensity.

8.3 Aggregate Impacts

Government-funded plant construction during WWII appears to have driven upward mobility among children in the surrounding area—it is important to note, however, that this does not necessarily imply that construction of treatment plants drove the broader increase in U.S. upward mobility in the *aggregate* (Jacome et al., 2021; Ward, 2021; Davis and Mazumder, 2022) or contributed to the postwar "great compression" in the national wage distribution (Goldin and Margo, 1992). An explicit goal of the U.S. government's WWII industrial

[64]It is possible to write models in which temporary demand shocks have permanent effects without any tipping points, see for example Blanchard and Katz (1992).

facilities program was to build plants in dispersed locations outside of preexisting manufacturing hubs where firms were concentrating their investments. It is possible that if the U.S. government had not paid for the construction of those plants and sold them at low prices to private firms, those firms would have built additional factories in major cities after the war, which might have counterfactually increased opportunities for economic advancement in those cities. While this sort of general equilibrium externality may be less relevant for local governments competing to attract new plants, they are crucial considerations for national governments seeking to promote upward mobility in the full population.

Two types of general equilibrium spillovers are particularly relevant. First, as suggested above, publicly-financed plant construction in certain locations may have crowded out private investment in new plants in other locations, as firms may have opted to purchase war plants after the war in lieu of building new ones.[65] The aggregate labor market impacts of public plant construction in turn hinges on how much government intervention increased postwar manufacturing capital and employment in the aggregate—in the extreme, public plant construction may have had had *no* causal impact on the total plant stock, thereby resulting in a geographic reallocation of manufacturing activity without any net increase.[66] Given the lack of a clean counterfactual for the aggregate U.S. economy, it is difficult to determine the causal impact on aggregate postwar production and the question remains an active topic of debate among economic historians (Field, 2022).

Second, to the extent plant construction did drive a reallocation of manufacturing investment across regions, such reallocation would only lead to aggregate gains if the impacts in the selected locations were larger than the impacts would have been in counterfactual locations.[67] In particular, if building a plant in a relatively isolated, less-developed, or economically depressed region yields greater benefits than building the same plant in a denser or more affluent region, this would create a rationale for national governments to pursue place-base industrial policies Austin et al. (2018). While it is highly plausible that plant sitings had disproportionately larger impacts on the dispersed locations that were chosen

[65]Such spillovers do not necessarily imply any violation of the SUTVA, which only requires that public war plant construction did not crowd-out plant construction in *comparison regions*. Rather, we expect the potential for crowd-out was largest in major manufacturing locations that are excluded from the analysis sample.

[66]Even in the case of perfect crowd-out, government funded plant construction may have promoted aggregate upward mobility if the government-funded plants were higher-wage plants than those that would have been built in the counterfactual due to increased unionization or greater intensity in less-skilled labor.

[67]This point is made explicitly in the context of a spatial equilibrium model in Kline and Moretti (2014). In their model, reallocation of labor from one region to another only increases aggregate productivity (and thus income) only if the *agglomeration elasticity*—the elasticity of local productivity to the size of the labor force—is larger in the receiving region than in the sending region. In their structural estimation of the model, they find evidence of such nonlinearities in the agglomeration elasticity.

for public investment than they would have had in larger, denser manufacturing hubs, our empirical design only allows us to directly study the former group. We can nonetheless examine whether effects differ across treatment counties in our sample at different stages of development. In Appendix Table A.5, we present evidence that plant openings in regions with lower prewar manufacturing employment shares and lower initial market access led to larger postwar manufacturing expansions, but find mixed evidence about differences in earnings effects. This analysis raises the possibility of gains from reallocation, but there is insufficient variation across treatment counties to draw strong conclusions.

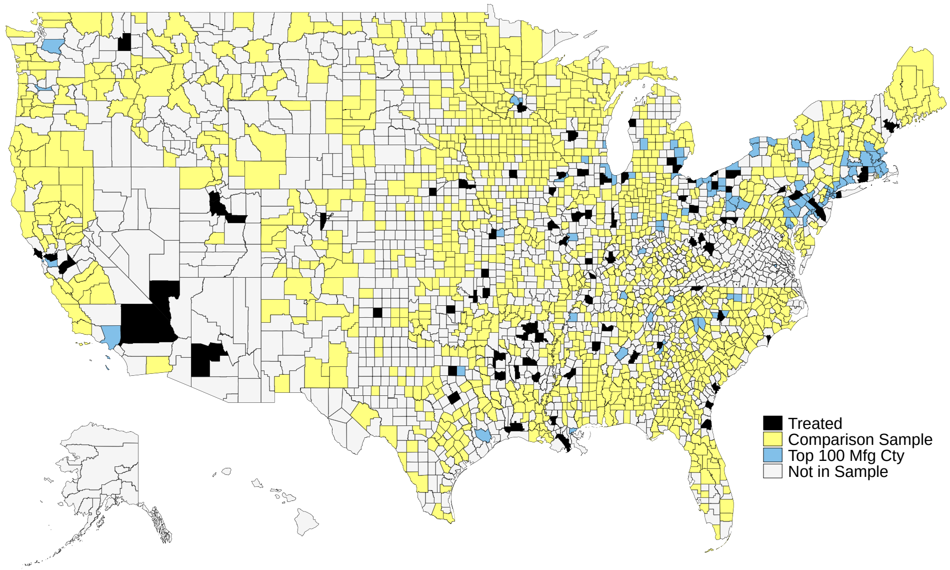
9 Conclusion

This paper finds that government-funded construction of manufacturing plants during WWII in dispersed locations outside of major urban centers region had large and persistent effects on both the development of affected regions and on the lifetime earnings of children born in those regions before the war. Taken together, our results indicate that local children in treated areas benefited from that the persistent expansion of high-wage blue collar jobs to which they had access as adults. The impacts were largest for children of parents with the lowest prewar earnings, consistent with views that local manufacturing jobs acted as a "ladder to the middle class" for economically-disadvantaged residents in the middle of the twentieth century.

Our findings highlight the potential for place-based economic policies to expand opportunities for economic advancement among residents of target regions. Yet our analysis also gives reason to think that the success of any proposed place-based intervention will depend crucially on the details, as previously highlighted by Bartik (2020). Policymakers aiming to promote upward mobility should carefully consider whether an intervention will generate paths to higher-wage employment for the people already living in a target area. These goals may not always align with priorities of the firms that policies aim to attract, and may be more difficult to sustain in the twenty-first century global economy than in the postwar era. Those goals also may not align with other objectives of industrial policy such as the reshoring of strategic sectors or the development of nascent industry clusters that might contribute to productivity growth. Ironically, WWII plant construction may have been particularly effective at improving outcomes for local residents in part because officials were acting to meet the needs of a short-term defense crisis and *not* pursuing longer-term economic efficiency. In order for new plants in peripheral regions to quickly reach output goals for new products, every available worker in the community had to be put to work as effectively as possible, fueling employment in semi-skilled occupations that offered better pay for workers with less formal education. That operating model stands in stark contrast to that of modern production facilities and distribution centers which are highly automated and rely increasingly heavily on highly-interchangeable workers from staffing agencies (Dey et al., 2012). A key question for future research is whether the types of contemporary incentives to attract jobs to distressed regions discussed by Bartik (2020) and Slattery and Zidar (2020) can in practice replicate the impacts found here and in Mitrunen (2021) in the case of war-driven industrial expansions.

Figures

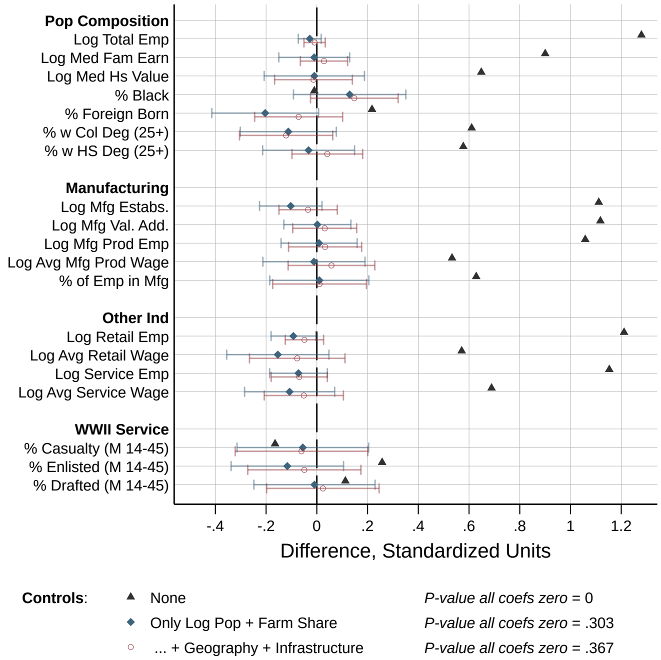
Figure 1: Locations of Treatment and Comparison Counties



Notes: Map displays 90 counties in primary treatment group shaded in black. Counties with top 100 1939 manufacturing employment excluded from the analysis are displayed in blue. The 1400 counties included in the comparison sample are shown in yellow; we display propensity score weights for comparison counties corresponding to alternative covariate specifications in Appendix Figure C.1.

Figure 2: Prewar Balance

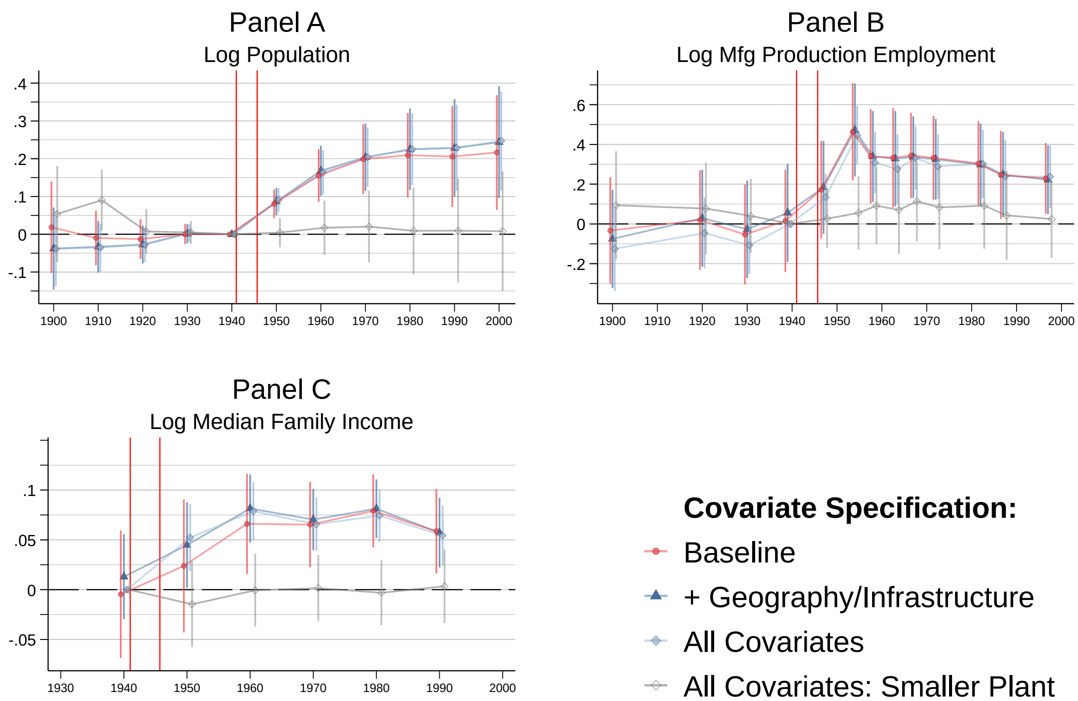
(a) County-Level Covariate Balance



(b) Placebo: 1940 Earnings of 1910 Child Residents, by Father Occupation Rank

Notes: Panel A displays estimates from regressions of pre-war county characteristics on the main treatment indicator following the specification in Equation 1 using alternative sets of included covariates, along with 95 percent confidence intervals. See Appendix B for variable sources. Sample includes 90 treatment counties and 1400 comparison counties. Confidence intervals are not displayed for uncontrolled mean differences. P-values from F-test that all coefficients are jointly zero are displayed at bottom. Panel B displays estimates of equation 2, including controls for 1940 log population and farm share only, estimated in the sample of male children aged 18 or under in the 1910 Census who are matched to adult outcomes in the 1940 Census using the crosswalk from Abramitzky et al. (2022), where individuals are assigned treatment status and county covariates based on their county in the 1910 Census. 1910 father ranks are assigned using the algorithm in Collins and Wanamaker (2022). For each percentile level $p \in 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100$ we estimate a separate regression where individuals are weighted by their distance of their father's rank r_i to p using a triangular kernel with a bandwidth of 25 (weight $\omega_i = max(0, 1 - |r_i - p|/25)$). Appendix Figure C.3 shows results within discrete decile bins. See Appendix B for additional details about the sample.

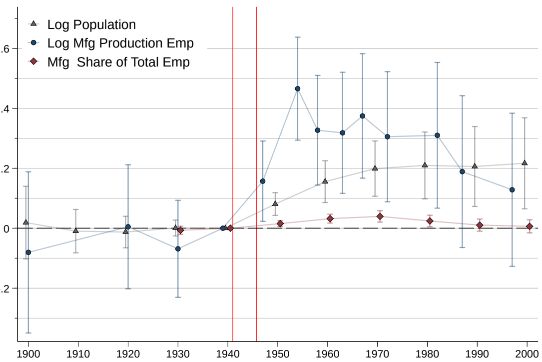
Figure 3: Treatment-Control County Differences over Time



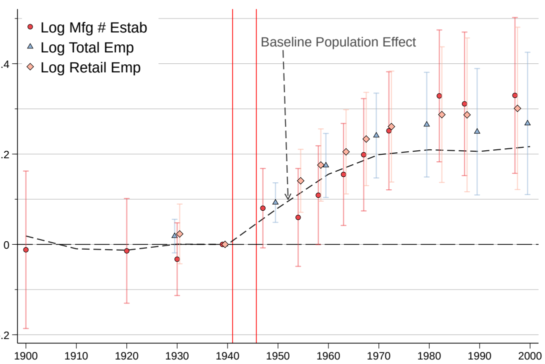
Notes: Figure displays OLS estimates of equation 1 under alternative covariate specifications described in the main text. Each estimate and the associated 95% confidence interval is from a separate regression of the outcome measured in the year specified in the x-axis on the treatment indicator or small plant ("partial-treatment") indicator when specified. When estimating effects of main treatment, the estimation sample includes 90 treatment counties and 1400 comparison counties; when estimating effects of smaller plants the sample includes 57 counties with smaller plants and 1,233 comparison counties. Outcomes in Panel A and Panel C are county-level tabulations from the Decennial Censuses except 1940 median family income which is tabulated based just on wage earnings from the 1940 full-count Census microdata (Manson et al., 2019); manufacturing production employment in Panel B is from county-level tabulations of the Census of Manufactures—see Appendix B for sources. Dependent variable means are displayed in Appendix Figure C.8. In panel A, 1940 estimates are mechanically zero because of control for 1940 log population; likewise, in "all covariate" specifications 1940 estimates are mechanically zero because of controls for 1940 values. Red lines denote beginning and end of U.S. involvement in WWII, during which time outcomes are not observed. When estimating impacts of "Smaller plants", treatment plants are excluded from the control group and vice versa.

Figure 4: County-Level Adjustment Dynamics

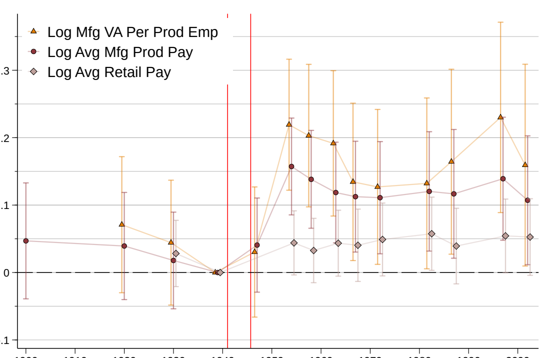
(a) In Short Run, Manufacturing Employment Grows in Greater Proportion than Population



(b) Other Outcomes Evolve in Proportion with Population



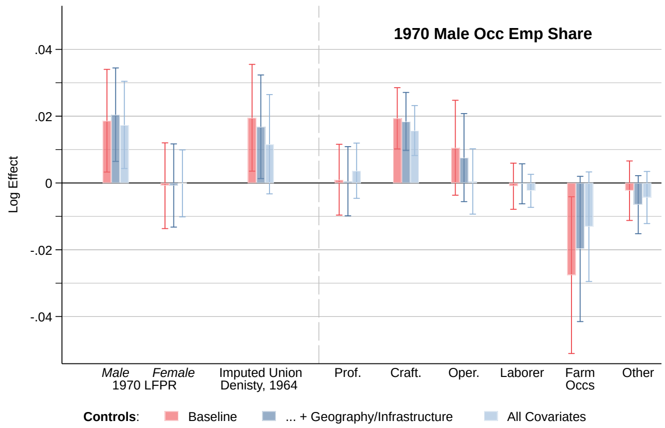
(c) Manufacturing Wages Rise with Productivity, Not with Wages in Other Sectors



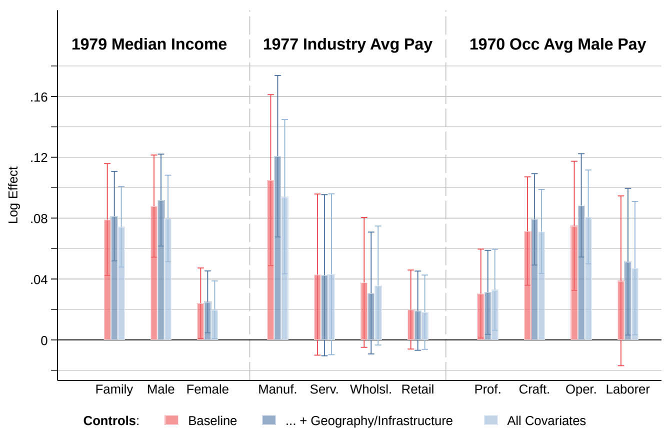
Notes: Figure displays OLS estimates of equation 1 under the baseline specification with controls only for 1940 log population and farm share. Sample includes 90 treatment counties and 1400 comparison counties. Confidence intervals are not displayed for uncontrolled mean differences. All outcomes are differences relative to 1940 outcome levels (or 1939 as available) to compare differential increases in outcomes, 1940 effects are zero by construction. Each estimate and the associated 95% confidence interval is from a separate regression of the differenced outcome measured in the year specified in the x-axis on the treatment indicator. Outcomes observed in decennial years are tabulations from Decennial Censuses, all other outcomes are tabulations from Economic Censuses—see Appendix B for sources. In Panel B, estimates of effects on log population from Panel A are reproduced using dotted line (without confidence intervals) for comparison. Red lines denote beginning and end of U.S. involvement in WWII, during which time outcomes are not observed.

Figure 5: Medium-Run Effects on Labor Market Composition

(a) Effects on Employment Composition



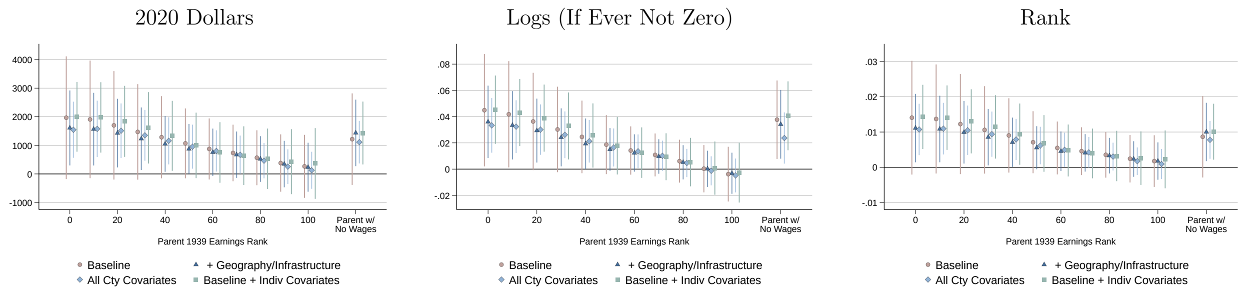
(b) Effects on Wage Structure



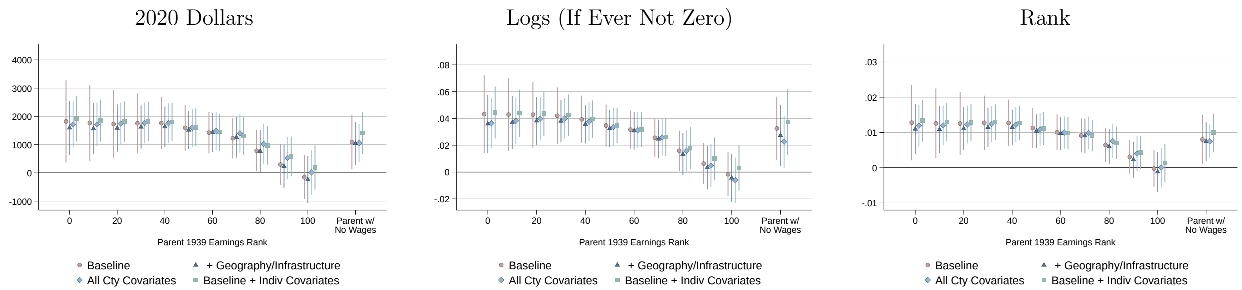
Notes: Figure displays OLS estimates of Equation 1 under alternative covariate specifications described in the main text. Each estimate and the associated 95% confidence interval is from a separate regression of the specified outcome on the treatment indicator. The estimation sample includes 90 treatment counties and 1400 comparison counties. 1970 labor force participation rates and employment and average wages for men by occupation are tabulations from the 1970 Decennial Census and 1979 median incomes are from tabulations of 1980 Decennial Census; years are chosen based on availability of tabulated data from NHGIS (Minnesota Population Center, 2011). 1977 industry average pay are from tabulations of Economic Census data. "Imputed Union Density" is calculated using 1953 estimates of industry union density from Troy (1957) and the industry employment distribution within each county from the 1964 County Business Patterns data (the earliest year with industry detail for a sufficient number of counties) assembled by Eckert et al. (2022). See Appendix B for sources.

Figure 6: Effects on Male 1969–1984 Average Wage Earnings Reported on 1040 Returns

A. Individual-Level Regression Estimates, Unweighted



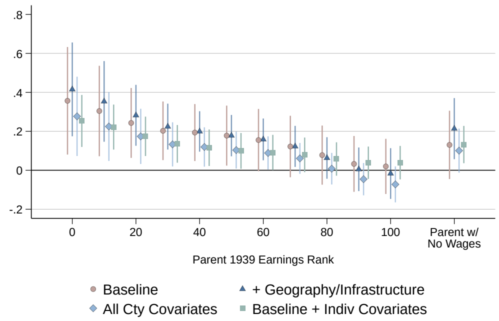
B. Equalized County Weighting



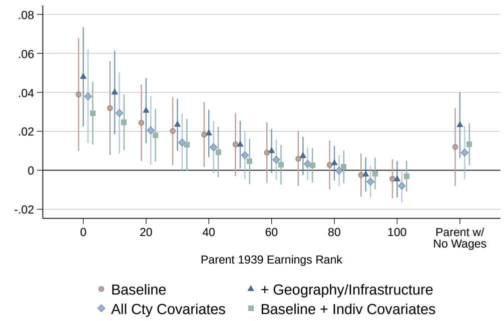
Notes: Figure displays estimates of Equation 2 under alternative covariate specifications described in the main text. Each estimate and the associated 95% confidence interval is from a separate regression, where standard errors are clustered at the county level. The sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties who are matched to parents in the 1940 Census. For each percentile level $p \in 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100$ we estimate a separate regression where individuals are weighted by their distance of their father's rank r_i to p using a triangular kernel with a bandwidth of 25 (weight $\omega_i = max(0, 1 - |r_i - p|/25)$); we then plot estimates on the y-axis against the corresponding percentiles p on the x-axis. We separately report estimates for the sample of male children matched to parents in the 1940 Census with no wage earnings. Appendix Figure C.12 presents alternative estimates across discrete decile bins. Apart from weighting, specification details in Panel A are the same as in Table 1. In Panel B, we further adjust individual weights so that so that the total sum of weights within a county is one in each regression; specifically, in each regression sample, we calculate the total weight $W_c = \sum_c \omega_{ic}$ for each county and weight individuals by $\tilde{\omega_{ic}} = \omega_{ic}/W_c$ so that relative weights of individuals within counties are preserved but counties receive equal total weight.

Figure 7: Effects on Male Educational Attainment Reported in 2000 Census

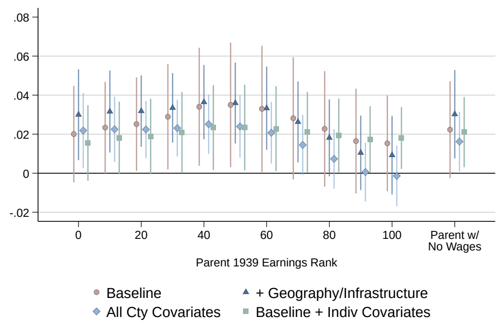
(a) Years of Schooling



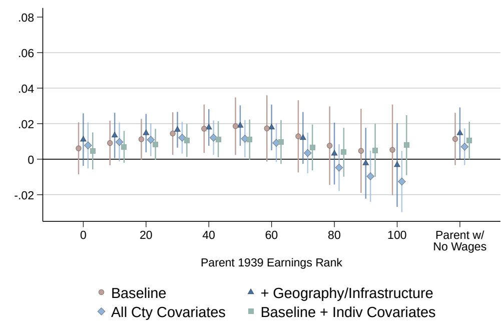
(b) HS Degree



(c) Any College

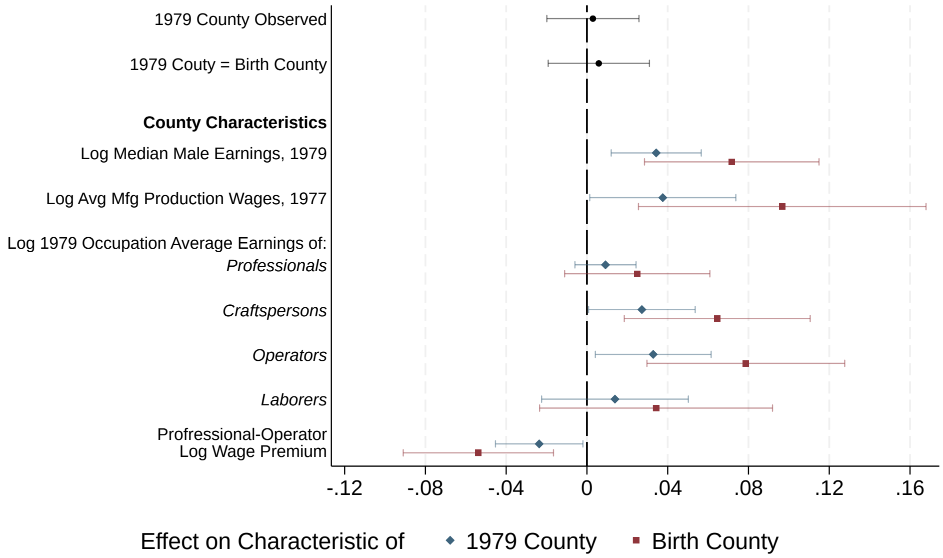


(d) College Degree



Notes: Figure displays estimates of Equation 2 under alternative covariate specifications described in the main text. Each estimate and the associated 95% confidence interval is from a separate regression, where standard errors are clustered at the county level. The sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties who are matched to parents in the 1940 Census and are further matched to the 2000 Census long form data. For each percentile level $p \in 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100$ we estimate a separate regression where individuals are weighted by their distance of their father's rank r_i to p using a triangular kernel with a bandwidth of 25 (weight $\omega_i = max(0, 1 - |r_i - p|/25)$); we then plot estimates on the y-axis against the corresponding percentiles p on the x-axis. We separately report estimates for the sample of male children matched to parents in the 1940 Census with no wage earnings.

Figure 8: Effects on Adult Location Characteristics



Notes: Figure displays estimates of Equation 2 where the outcomes are characteristics of the county individuals reported living in on their 1979 Form 1040 tax return or were born in, as specified. All regression include only the baseline contols for log 1940 population and farm share in one's birth county. Each estimate and the associated 95% confidence interval is from a separate regression, where standard errors are clustered at the county level. The baseline sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties. The indicator for remaining in one's birth county "1979 County = Birth County" and all 1979 county characteristics are only defined for individuals with valid counties on a 1979 1040 return. County characteristics are measured using the outcome variables from Figure 5. The estimated effects on birth characteristics are not identical to the estimates in Figure 5 because they are estimated at the individual level; however, the birth-county results are identical to those in Figure 5 once we re-weight individuals so that the total sum of weights in each county is equal to one in Appendix Figure C.13.

Tables

Table 1: Effects on Men's Wage Earnings Reported on 1040 Tax Returns 1969–1984

	Average Wage Earnings on 1040 Tax Return, 1969–1984						
	Full Sample			**Linked to Parents in 1940 Census**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adjusted 2020 Dollars	1,480	1,318*	1,172**	1,259	1,308**	1,155***	1,305**
	(1,240)	(695)	(459)	(850)	(532)	(346)	(638)
N	8,246,000	8,246,000	7,848,000	4,943,000	4,943,000	4,697,000	4,845,000
Dependent Variable Mean		49,580			54,510		
Logs	0.032*	0.030***	0.023***	0.026*	0.026***	0.020***	0.027**
	(0.019)	(0.012)	(0.008)	(0.015)	(0.010)	(0.007)	(0.013)
N	7,068,000	7,068,000	6,727,000	4,520,000	4,520,000	4,295,000	4,432,000
Dependent Variable Mean		10.67			10.71		
Rank (0 to 1)	0.0111	0.0096*	0.0085**	0.0087	0.0089**	0.0079***	0.0090**
	(0.0095)	(0.0053)	(0.0035)	(0.0061)	(0.0037)	(0.0024)	(0.0045)
N	7,772,000	7,772,000	7,397,000	4,691,000	4,691,000	4,458,000	4,599,000
Dependent Variable Mean		0.535			0.571		
Included Covariates							
Baseline County Size	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓	
Extended County Covariates			✓			✓	
Individual + Parent Characteristics							✓

Notes: Table displays OLS estimates of equation 2 under alternative covariate specifications described in the main text. Standard errors clustered at the county level are displayed in parenthesis. The sample in Columns 1–3 is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties; the sample in Columns 4–7 is the subset who match to parents in the 1940 Census (whether or not that parent has any reported earnings). In Column 7, we include the following controls from the 1940 Census: child race and age, parent maximum years of education, indicators for immigrant parents, parent average age, and number of children in the household. Outcomes are tax-unit-level wages reported on form 1040 averaged across years 1969, 1974, 1979, and 1984, with missing values in each year treated as zeros so that average earnings in dollars are never missing and are only zero if missing or zero in all years (in which case the logarithm is not defined). We assign percentile ranks scaled from 0 to 1 based on average earnings for all individuals in entire United States born 1922–1940 who survive through 1984. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table 2: Effects on Men's Wages Reported 1040 Returns, by Race

A. White Men

	Average Wage Earnings on 1040 Tax Return, 1969–1984											
	Earings, Adjusted 2020 Dollars				**Logged Average Earnings**				**Rank (0 to 1)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Linked to Parents in 1940 Census	1,652**	1,614**	942**	1,272*	0.032**	0.031***	0.015**	0.026**	0.0111**	0.0063**	0.0087*	0.0087*
	(819)	(639)	(393)	(671)	(0.015)	(0.012)	(0.007)	(0.013)	(0.0044)	(0.0028)	(0.0048)	(0.0048)
N	4,442,000	4,442,000	4,218,000	4,359,000	4,108,000	4,108,000	3,901,000	4,032,000	4,228,000	4,228,000	4,015,000	4,149,000
Dependent Variable Mean	56,490				10.74				0.585			
Parents w/ 1939 Earnings < Median	1,986**	1,694**	1,188**	1,561**	0.066***	0.007	0.022**	0.032**	0.0119**	0.0119**	0.0083**	0.0111**
	(937)	(694)	(541)	(712)	(0.015)	(0.007)	(0.010)	(0.015)	(0.0050)	(0.0050)	(0.0039)	(0.0051)
N	1,579,000	1,579,000	1,507,000	1,569,000	1,469,000	1,469,000	1,402,000	1,459,000	1,502,000	1,502,000	1,433,000	1,492,000
Dependent Variable Mean	54,250				10.73				0.569			
Parents w/ 1939 Earnings > Median	714	613	461	572	0.102***	0.007	0.005	0.006	0.0037	0.0037	0.0028	0.0034
	(499)	(424)	(315)	(537)	(0.025)	(0.007)	(0.005)	(0.009)	(0.0027)	(0.0027)	(0.0020)	(0.0036)
N	1,183,000	1,183,000	1,119,000	1,176,000	1,119,000	1,119,000	1,058,000	1,112,000	1,129,000	1,129,000	1,067,000	1,122,000
Dependent Variable Mean	66,490				10.96				0.653			
Parents w/ No 1939 Wage Earnings	1,787**	1,791***	973**	1,495**	0.046***	0.040***	0.019*	0.042***	0.0128**	0.0126***	0.0067**	0.0105**
	(816)	(690)	(419)	(618)	(0.016)	(0.015)	(0.010)	(0.015)	(0.0058)	(0.0048)	(0.0030)	(0.0044)
N	1,679,000	1,679,000	1,593,000	1,614,000	1,521,000	1,521,000	1,442,000	1,460,000	1,598,000	1,598,000	1,515,000	1,535,000
Dependent Variable Mean	51,560				10.60				0.551			
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓			✓	✓	
Extended County Covariates			✓				✓				✓	
Individual + Parent Characteristics				✓				✓				✓

B. Black Men

	Average Wage Earnings on 1040 Tax Return, 1969–1984											
	Earings, Adjusted 2020 Dollars				**Logged Average Earnings**				**Rank (0 to 1)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Linked to Parents in 1940 Census	2,524***	2,495***	1,428***	1,896***	0.058***	0.058***	0.058***	0.043***	0.0180***	0.0175***	0.0097***	0.0137***
	(709)	(526)	(487)	(552)	(0.014)	(0.012)	(0.012)	(0.011)	(0.0054)	(0.0039)	(0.0036)	(0.0043)
N	482,000	482,000	460,000	468,000	396,000	396,000	378,000	385,000	445,000	445,000	426,000	433,000
Dependent Variable Mean	36,760				10.38				0.443			
Parents w/ 1939 Earnings < Median	2,810***	2,640***	1,412***	2,181***	0.066***	0.063***	0.063***	0.052***	0.0199***	0.0183***	0.0091**	0.0158***
	(730)	(580)	(504)	(555)	(0.015)	(0.014)	(0.014)	(0.011)	(0.0055)	(0.0043)	(0.0037)	(0.0042)
N	248,000	248,000	237,000	246,000	203,000	203,000	194,000	202,000	229,000	229,000	219,000	227,000
Dependent Variable Mean	37,070				10.38				0.444			
Parents w/ 1939 Earnings > Median	5,149***	4,783***	4,241***	3,868***	0.102***	0.098***	0.098***	0.072***	0.0384***	0.0344***	0.0316***	0.0292***
	(1,394)	(1,027)	(1,109)	(1,127)	(0.025)	(0.024)	(0.024)	(0.025)	(0.0095)	(0.0072)	(0.0078)	(0.0083)
N	16,500	16,500	15,000	16,500	14,000	14,000	13,000	14,000	15,000	15,000	14,000	15,000
Dependent Variable Mean	47,720				10.63				0.526			
Parents w/ No 1939 Wage Earnings	1,852**	1,862***	1,058*	1,293*	0.041**	0.042***	0.042***	0.027*	0.0131**	0.0131***	0.0074	0.0092*
	(754)	(629)	(627)	(685)	(0.017)	(0.015)	(0.015)	(0.016)	(0.0059)	(0.0048)	(0.0048)	(0.0053)
N	217,000	217,000	209,000	206,000	178,000	178,000	171,000	169,000	201,000	201,000	193,000	190,000
Dependent Variable Mean	35,570				10.35				0.435			
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓			✓	✓	
Extended County Covariates			✓				✓				✓	
Individual + Parent Characteristics				✓				✓				✓

Notes: Table extends Table 1 to examine results within subsamples of individuals matched to the 1940 Census split by race and whether their parents had no reported 1939 wage earnings or 1939 earnings above or below the national median across *all* children in the same cohorts (conditional on having nonmissing wage earnings). Standard errors clustered at the county level are displayed in parenthesis. See notes to Table 1 for additional details. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table 3: Effects on Tax-Unit and Individual Wage Earnings, by Gender

	Full NUMIDENT-IRS Sample						CPS-SSA Sample					
	Filing-Unit Wages on 1040 Return				Has Spouse on		Individual Wages on W-2 Return				1979 Filing-Unit Wages	
	1979		1974–1984 Average		1979 1040 Return		1979		1978-1987 Average		on 1040 Return	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Male	1,867	1,527***	1,575	1,196**	-0.0023	0.0008	3,724***	2,543***	2,898***	2,053***	3,840***	2,352**
	(1,360)	(543)	(1,270)	(477)	(0.0040)	(0.0018)	(1,038)	(890)	(903)	(736)	(1,328)	(1,074)
N	8,246,000		8,246,000		6,572,000		31,500		31,500		31,500	
Dependent Variable Mean	53,830		50,690		0.86		47,720		44,540		64,000	
Female	973	1,052**	754	782*	-0.0057	0.0017	44	-246	-379	-631	3,554***	2,500**
	(1,276)	(472)	(1,189)	(414)	(0.0051)	(0.0020)	(446)	(441)	(459)	(408)	(1,317)	(1,004)
N	8,137,000		8,137,000		6,413,000		38,000		38,000		38,000	
Dependent Variable Mean	47,920		44,590		0.81		16,020		16,270		55,250	
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓		✓

Notes: Table displays OLS estimates of equation 2 separately for men and women under alternative covariate specifications described in the main text. The samples in Columns 1–6 are all men and women in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties (separated by gender reported in the NUMIDENT), whether or not they match to the 1940 Census. Outcomes in Columns 1–4 are tax-unit-level wages reported on form 1040 either from 1979 alone or averaged across years 1974, 1979, and 1984, with missing values in each year treated as zeros; 1969 is excluded because identifiers for the secondary filer on tax returns are not in the data. Indicator for has spouse on 1040 return is coded as zero if no return is present in the data. The samples in Columns 7–12 are all men and women in the CPS-SSA data born 1922–1940 in one of the 90 treatment or 1,400 comparison counties (separated by gender reported in the NUMIDENT). Outcomes in Columns 7–10 are individual wages reported on W-2 returns from 1979 alone or averaged across 1978–1987 (the first decade of available data), with missing values in each year treated as zeros. We merge individuals in this SSA-CPS sample to their 1979 form 1040 tax returns to compare effects on 1040-reported earnings and W-2 reported earnings for a consistent sample in a single year; estimates are reported in Columns 11–12. Standard errors clustered at the county level are displayed in parenthesis. See notes to Table 1 for additional details. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table 4: Effects on Male 1978–1987 W-2 Earnings and Job Characteristics at Full-Time Minumum Wage Jobs

	Log W-2 Wage at Main Job			Krueger-Summers Industry Wage Premium			Firm Has Any Manufacturing Etablishments		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full Sample	0.050***	0.050***	0.046***	0.016***	0.016***	0.017***	0.017	0.016	0.023*
	(0.017)	(0.016)	(0.014)	(0.004)	(0.004)	(0.004)	(0.015)	(0.014)	(0.012)
N		125,000			125,000			125,000	
Linked to Parents in 1940 Census	0.045***	0.046***	0.046***	0.015***	0.015***	0.018***	0.021	0.015	0.025
	(0.016)	(0.015)	(0.015)	(0.005)	(0.005)	(0.005)	(0.016)	(0.016)	(0.015)
N		81,500			81,500			81,500	
Parents w/ 1939 Earnings < Median	0.061***	0.059***	0.055**	0.017**	0.017**	0.018**	0.027	0.018	0.020
	(0.022)	(0.021)	(0.023)	(0.007)	(0.007)	(0.008)	(0.025)	(0.025)	(0.025)
N		31,500			31,500			31,500	
Parents w/ 1939 Earnings > Median	0.028	0.024	0.024	0.008	0.006	0.014	0.035	0.029	0.043
	(0.022)	(0.022)	(0.022)	(0.008)	(0.008)	(0.009)	(0.027)	(0.028)	(0.029)
N		20,500			20,500			20,500	
Included Covariates									
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓
CPS ASEC Race Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Table reports OLS estimates of the pooled-year specification in Equation 3, estimated on the panel of 1978–1987 W-2 earnings at *full-time minimum wage main jobs with non-missing industry identifiers* observed in the SSA-CPS data. Table displays estimates of Equation 3 under alternative covariate specifications and for subsamples split by whether the individual is matched to parents in the 1940 Census and the combined earning status of their parents. Sample includes the 31,500 men in the CPS-SSA data who were born 1922–1940 in one of the 90 treatment or 1,400 comparison counties. All specifications control for race listed in the CPS. Analysis only include annual W-2 observations that i) are the individual's primary job in that year, 2) pay more than the equivalent of 2,000 hours at the federal minimum wage in that year, and 3) have an EIN that matches to at least one establishment in the Longitudinal Business Database with a valid industry code. We match estimates of 1984 wage premiums from Table A.1 Krueger and Summers (1988) to LBD establishments by SIC code and calculate a job's expected industry premium as the employment-weighted average of all establishments within an EIN in the observation year. "Firm has any manufacturing establishment" is an indicator denoting that the EIN on the W-2 is associated with at least one manufacturing establishment in the LBD in that same year. Wage effect estimates are not directly comparable to estimates in other tables due to the exclusion of jobs with pay below the full-time minimum wage and years with zero earnings; we examine sensitivity of results to alternative restrictions in Appendix Table C.7 and estimate effects on employment status in Appendix Table C.8. Standard errors clustered at the county level are displayed in parenthesis. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

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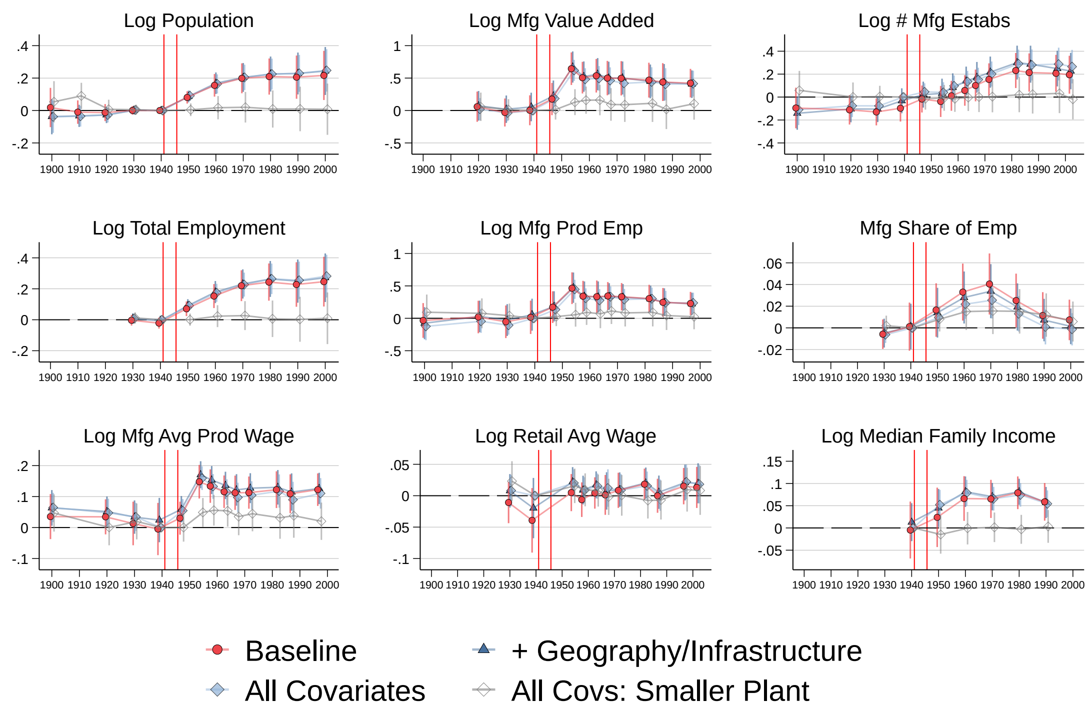
Appendix

A Supplemental Tables and Figures

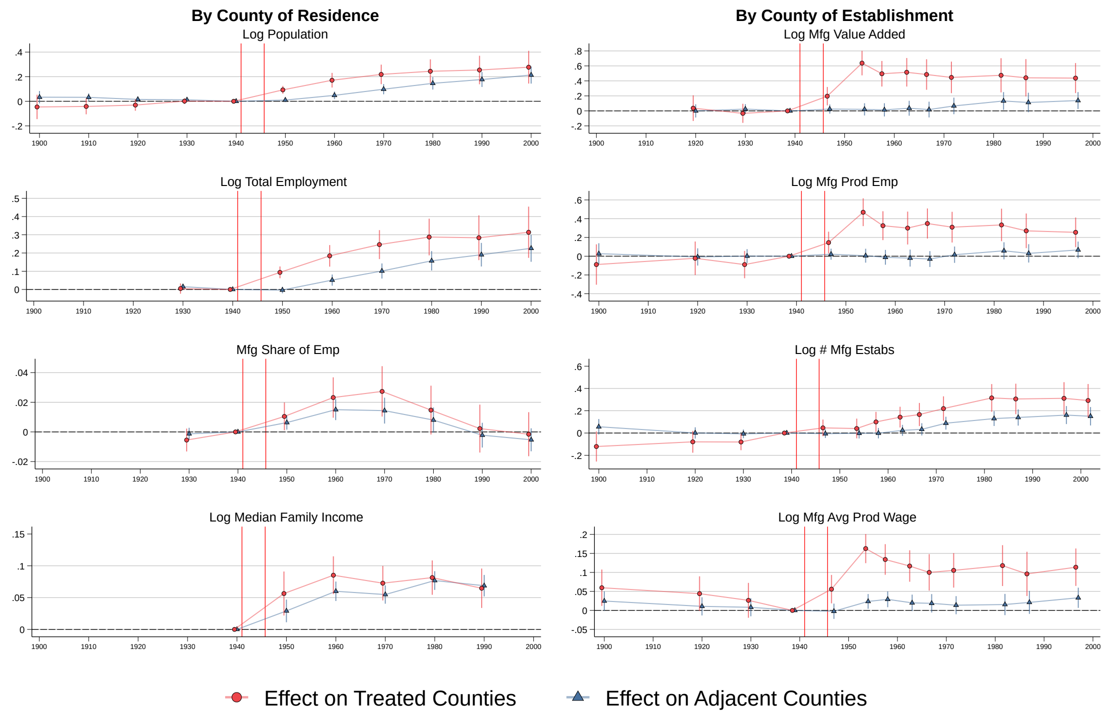
A.1 Figures

Figure A.1: Treatment-Control County Adjusted Differences over Time, Additional Outcomes



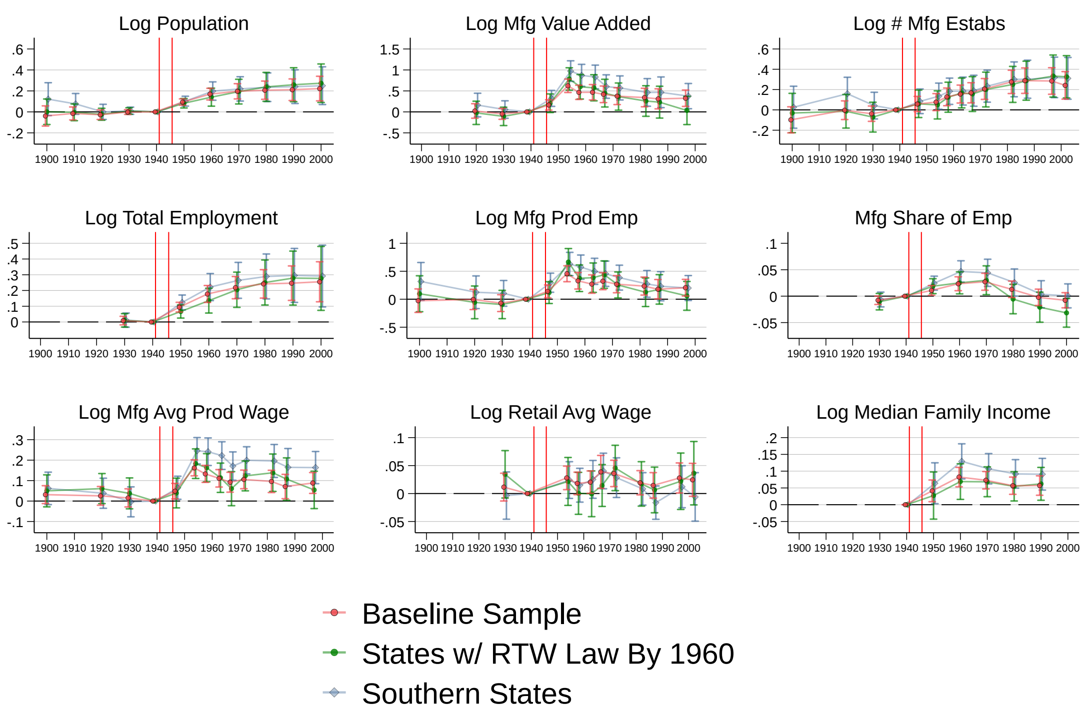
Notes: Figure presents additional outcomes following the specification in Figure 3, see figure notes for details and Appendix B for variable sources. Dependent variable means are displayed in Appendix Figure C.8.

Figure A.2: Impacts on Adjacent Counties



Notes: Figure displays OLS estimates of equation 1 with controls for extended county covariates as described in the text. Outcomes are in levels. Each estimate and the associated 95% confidence interval is from a separate regression of the outcome measured in the year specified in the x-axis on either the main treatment indicator or on an indicator denoting counties adjacency to a county with a large, publicly-financed plant. Effects of main treatment replicate results from Appendix Figure A.1. In regressions on adjacency indicator, estimation sample restricts to counties outside of the 100 largest manufacturing counties and omits counties with larger or smaller public plants from control group; we include counties adjacent to counties with large public plants even when the adjacent county with the plant is a top 100 manufacturing employment county in 1940 or a county dropped after our initial sample restrictions so long as the adjacent county itself is not dropped and not a top 100 manufacturing county. Red lines denote beginning and end of U.S. involvement in WWII, during which time outcomes are not observed.

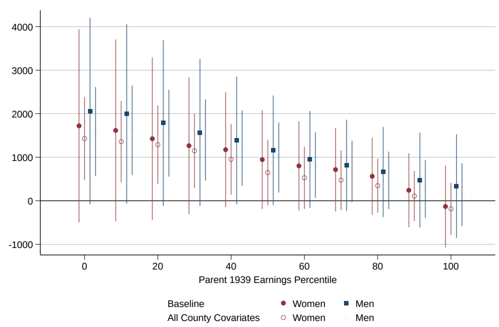
Figure A.3: Effects in Right-to-Work and Southern States



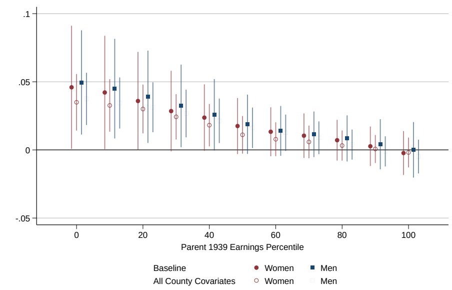
Notes: Figure replicates Figure A.1 further restricting the sample to only include counties either in states with right-to-work laws in place by 1960 or Southern states (based on Census region) as specified. Specifications include controls for extended county covariates as described in the text in order to increase precision. See figure notes to Figure A.1 for further details.

Figure A.4: Long-Run Effects, by Gender

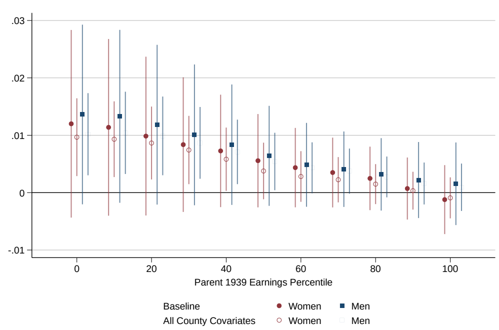
(a) 74–84 Wages, 2020 Dollars



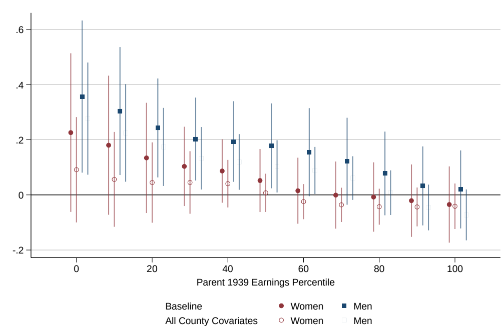
(b) 74–84 Wages, Logs



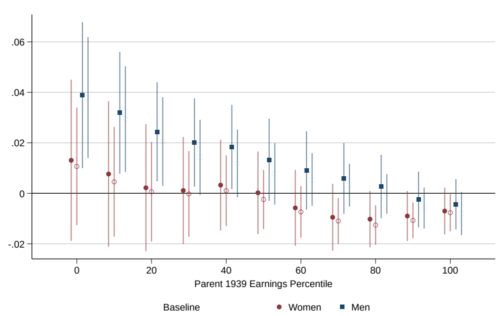
(c) 74–84 Wages, Rank



(d) Years of Schooling

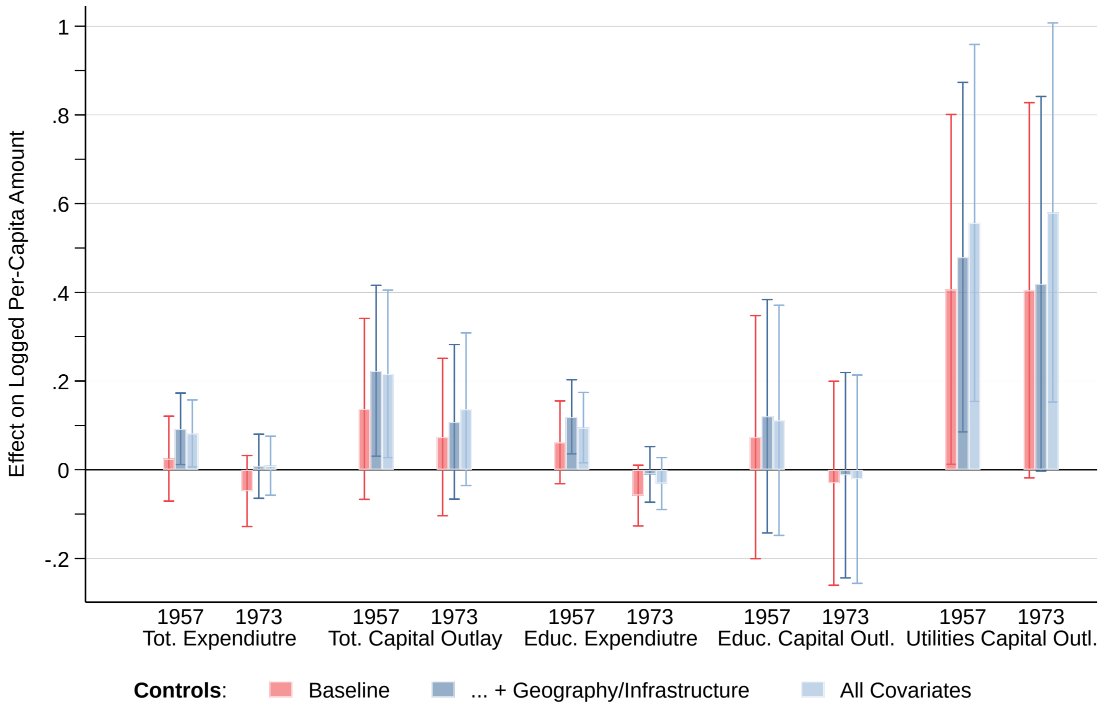


(e) HS Degree



Notes: Figure replicates select estimates from 6 and 7 under baseline covariate set including only 1949log population and and additionally presents estimates from identical specifications estimated using the sample of all women in NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties who are matched to parents in the 1940 Census. See notes to Figures 6 and 7 for additional details.

Figure A.5: County-Level Effects on Local Government Expenditures

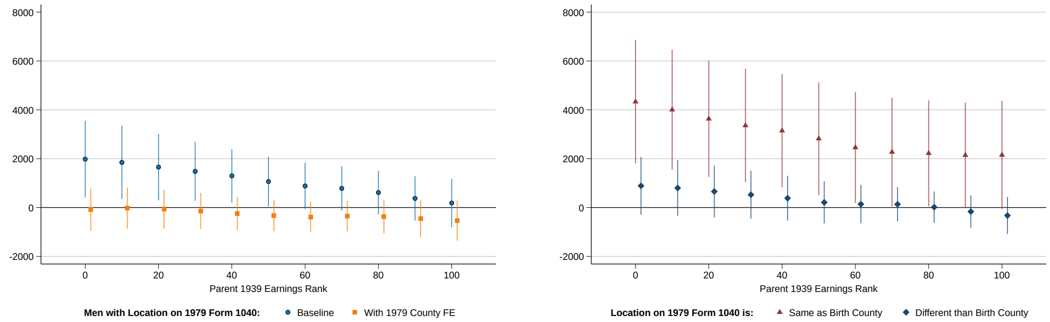


Notes: Figure displays OLS estimates of Equation 1 under alternative covariate specifications described in the main text. Each estimate and the associated 95% confidence interval is from a separate regression of the specified outcome on the treatment indicator. The estimation sample includes 90 treatment counties and 1400 comparison counties. All outcomes are local government expenditures and capital outlays within counties from the 1957 and 1972 Censuses of Government. Outcome values for 1957 and 1972 converted to per-capita amounts using county population from the 1960 and 1970 Decennial Censuses, respectively; per-capita amounts are then logged.

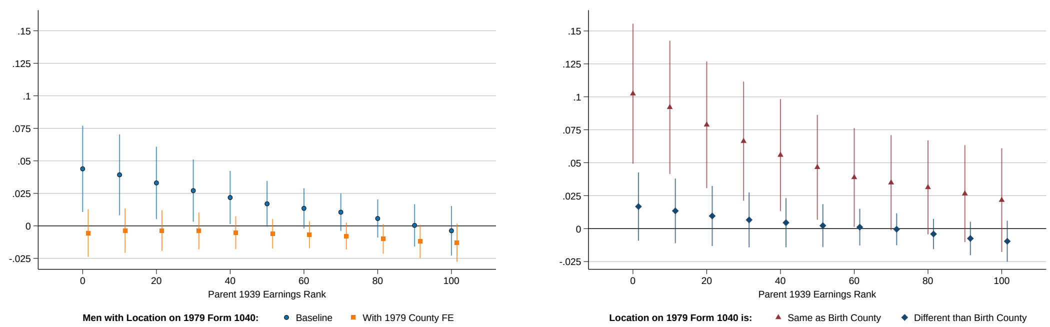
Figure A.6: Effects by Adult Location

Column A. 1979 Location FE **Column B. By 1979 Location**

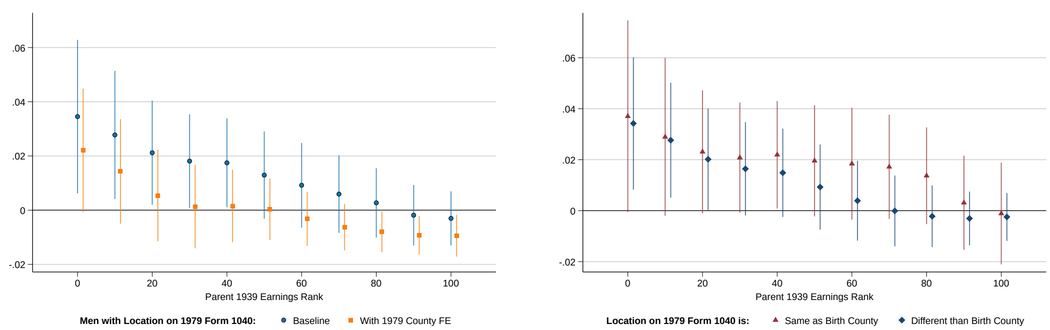
Outcome: 1969–1984 Average 1040 Wages, 2020 Dollars



Outcome: 1969–1984 Average 1040 Wages, Logs



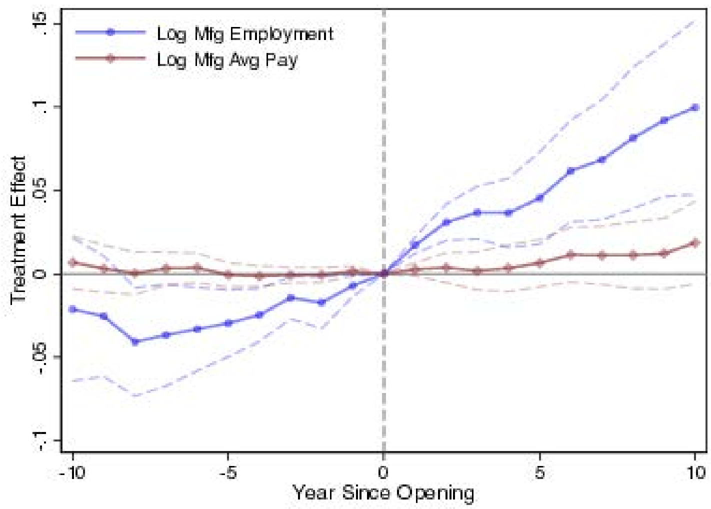
Outcome: High School Graduate, 2000 Census



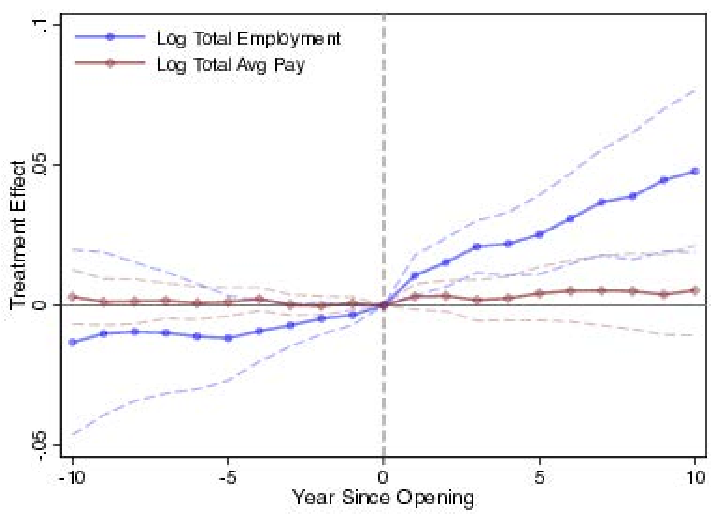
Notes: Figure replicates select estimates from 6 and 7 for men with valid county locations reported on a 1979 Form 1040 return under the baseline specification and alternative specifications conditioning on adult location. All specifications include only baseline controls for 1940 log population and farm share. In Column A, specifications are displayed that include fixed effects for the county reported on one's 1979 Form 1040 return. In Column B, the baseline specification is estimated on subsamples split by whether the county reported one's 1979 Form 1040 return is the same as their birth county in the NUMIDENT. See notes to Figures 6 and 7 for additional details.

Figure A.7: County-Level Effects of Greenstone et al. (2010) "Million Dollar" Manufacturing Plant Announcements 1982–1993

(a) Manufacturing Employment

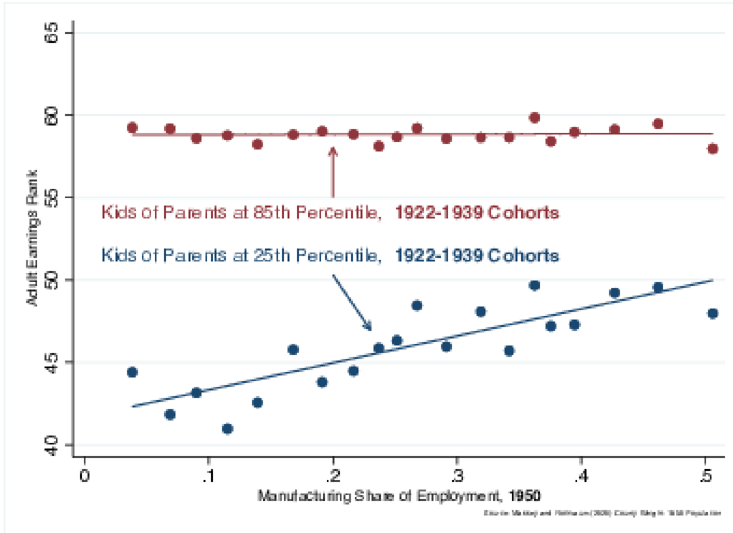


(b) All Employment

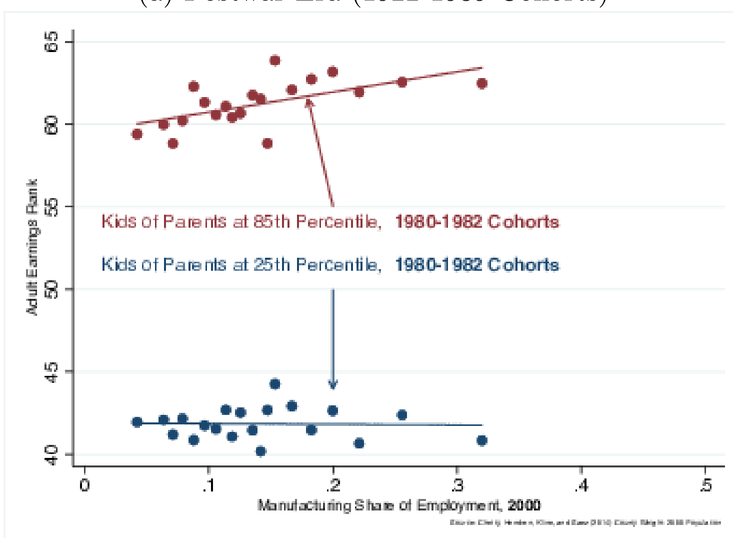


Notes: Panel A replicates Figure 4 of Monte et al. (2018) using their replication code and data, which in turn examine the plant openings documented by Greenstone et al. (2010). We limit our sample to the 63 manufacturing plant sitings out of the 82 total plant sitings in the full sample. Panel B replicates the same specification for the 63 manufacturing plant but uses county-level manufacturing employment and average annual earnings we collect ourselves from the QCEW. Each outcome is plotted on a different scale.

Figure A.8: County-Level Manufacturing Shares and Intergenerational Mobility



(a) Postwar Era (1922-1939 Cohorts)

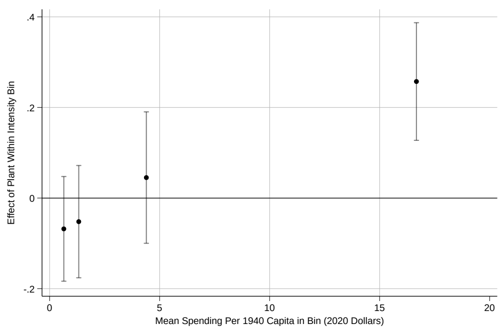


(b) Modern Era (1980-1982 Cohorts)

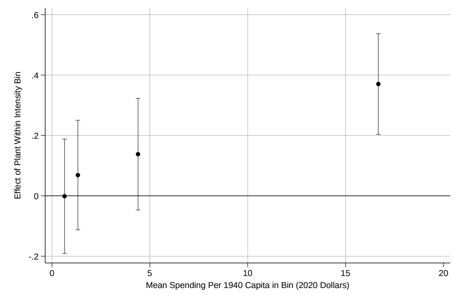
Notes: Plots are binned scatter points with OLS best-fit lines superimposed; each dot represents the average value of the y-axis variable for counties in each of 20 equally-sized vingtile bins. Data on mobility of 1980–1982 cohorts by county is from Chetty et al. (2014); outcomes for 25th and 85th percentiles of 1996 national parent income distribution are obtained from county-level tabulations of 25th percentile outcomes and county-specific rank-rank slopes. Data on mobility of 1922-1939 cohorts by county and 1940 parent earnings decile are from Rothbaum and Massey (2021); outcomes for 25th and 85th percentiles are average outcomes children in the third and eight deciles in each county, respectively.

Figure A.9: Examining Nonlinear Effects by Investment Intensity

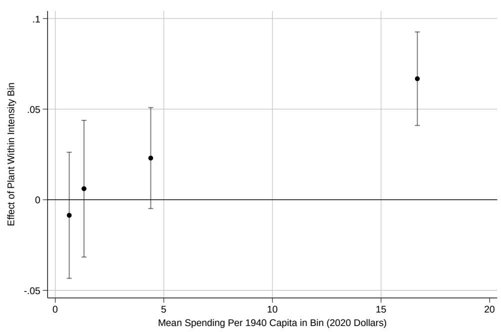
(a) 1980 Log County Population



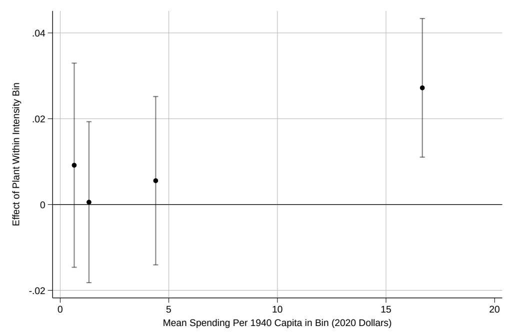
(b) 1977 Log County Manuf Empl.



(c) 1980 County Med. Family Income



(d) 1969–1984 Log Wages, Microdata



Notes: Figure presents estimates of modified versions of Equation 1 (panels a-c) and 2 (panel d) that include four indicators denoting plant in different intensity bins instead of a single treatment indicator. To construct the bins, we sort all 147 counties in our main sample (excluding the top 100 manufacturing counties in the US) where a larger *or* smaller "partial-treatment" public plant (costing over $1 million in nominal dollars) was built into quartile bins according to the investment spending per 1940 resident. Each specification includes the 147 counties with treatment or partial-treatment and the 1400 comparison counties in the main tables and figures. In each panel, we plot the effect for each bin indicator on the y-axis against the average spending per capita in the same bin in 2020 dollars on the x-axis. To improve precision, we use the extended county covariates specification described in the main text in all regressions. The county-level outcomes in panels a-c are as in Figure 3. The sample in panel d is all men in the NUMIDENT born 1922–1940 in one of the 147 counties treatment/partial-treatment counties or 1,400 comparison counties regardless of match to the 1940 Census; standard errors are clustered at the county level.

A.2 Tables

Table A.1: Summary of World War II Plant Investment

	Publicly-Financed Plants			Plants With >1% Private Financing for Structure		
	New Plants			New Plants		
	Based on Data Book Flag	Based on Structure Spending	Existing Plants	Based on Data Book Flag	Based on Structure Spending	Existing Plants
	(1)	(2)	(3)	(4)	(5)	(6)
Investments Over $1M)						
N Plants	257	96	127	153	203	972
Total Cost ($Millions)	5,591	1,458	1,592	949	1,395	6,161
% of Cost in Structures	55	59	23	33	55	20
% of Structure $ from Gov't	100	100	100	72	75	52
Investments Below $1M						
N Plants	101	112	142	2,051	3,023	5,523
Total Cost ($Millions)	28	35	49	185	385	1,078
% of Cost in Structures	47	63	23	32	66	15
% of Structure $ from Gov't	100	100	100	6	4	5

Notes: All values are in unadjusted nominal dollars. Table summarizes all plants in all 48 states in the U.S. as of 1945 in the War Production Board data book (United States War Production Board, 1945b), including those that could not be matched to counties based on place names.

Table A.2: Summary Statistics, Microdata Samples

A. Full Sample Statistics

	Full Numident/ IRS File	Full Count 1940 Census	Linked Numident- Census Sample	SSA-CPS Sample	Linked SSA-CPS- Census Sample
	(1)	(2)	(3)	(4)	(5)
Individual Characterstics, Means					
N Individuals	41,590,000	40,600,000	25,380,000	170,000	109,000
Female	0.495	0.491	0.490	0.547	0.540
Age in 1940	9.16	9.18	9.30	7.86	8.21
In Treatment County	0.058	0.055	0.058	0.055	0.056
In Sample County	0.537	0.534	0.527	0.547	0.546
1940 Parent Chatacteristics, Means					
N Individuals Matched to Parents		40,600,000	25,380,000		109,000
On Farm		0.283	0.262		0.288
Highest Grade obtained (both parents)		8.75	9.09		9.30
Any wage and salary earnings		0.948	0.953		0.954
Any nonwage earnings		0.384	0.381		0.393
1939 Parent Wage/Salary Earnings, Means (SD)					
N Individuals Matched to Parents w/ Earnings		38,470,000	24,190,000		104,000
Combined Parent Earnings, Nominal $		812	879		856
SD in parenthesis		(976)	(1,001)		(1,019)
Parent Earnings Rank Relative to All Children with Parent Earnings in Numident Sample			50.21		49.34
SD in parenthesis			(28.84)		(28.79)
Parent Earnings Rank Relative to All Children of Same Age in 1940 Census		50.00	52.51		51.97
SD in parenthesis		(28.86)	(28.55)		(28.59)
Father Occupation Score Rank Relative to All Children of Same Age in 1940 Census		50.00	52.50		52.28
SD in parenthesis		(28.87)	(28.27)		(28.15)

B. Statistics by Race

	White Men		Black Men	
	Full Count 1940 Census	Linked Numident-Census Sample	Full Count 1940 Census	Linked Numident-Census Sample
	(1)	(2)	(3)	(4)
Individual Characterstics, Means				
N Individuals	36,090,000	23,240,000	4,299,000	2,043,000
Female	0.489	0.489	0.501	0.502
Age in 1940	9.22	9.33	8.82	8.88
In Treatment County	0.055	0.058	0.054	0.059
In Sample County	0.526	0.520	0.600	0.602
1940 Parent Chatacteristics, Means				
N Individuals Matched to Parents	36,090,000	23,240,000	4,299,000	2,043,000
On Farm	0.264	0.248	0.438	0.411
Highest Grade obtained (both parents)	9.06	9.32	6.27	6.61
Any wage and salary earnings	0.950	0.954	0.927	0.935
Any nonwage earnings	0.384	0.380	0.383	0.382
1939 Parent Wage/Salary Earnings, Means (SD)				
N Individuals Matched to Parents w/ Earnings	34,280,000	22,180,000	3,984,000	1,911,000
Combined Parent Earnings, Nominal $	872	926	315	353
SD in parenthesis	(1,005)	(1,023)	(452)	(472)
Parent Earnings Rank Relative to All Children with Parent Earnings in Numident Sample		52.26		24.43
SD in parenthesis		(28.44)		(20.17)
Parent Earnings Rank Relative to All Children of Same Age in 1940 Census	52.66	54.55	24.69	26.69
SD in parenthesis		(28.09)	(20.17)	(20.77)
Father Occupation Score Rank Relative to All Children of Same Age in 1940 Census	54.01	55.67	10.76	11.89
SD in parenthesis	(26.99)	26.62	(12.79)	(13.58)

Notes: Panel A describes all individuals born 1922–1940 in the component microdata source files and the matched samples for all individuals born in the United States with nonmissing birth locations in the NUMIDENT, except in Column 2 where we include all US-born individuals aged 18 or under in the 1940 Census. Panel B presents tabulations by race as reported in the 1940 Census. Tabulations of age in 1940 and geographic location are based on NUMIDENT birthdate and place of birth in Panel A columns 1, 3, 4, and 5 and Panel B columns 2 and 4, and are based on 1940 census age and location in Panel A column 2 and Panel B columns 2 and 4.

Table A.3: County Level Plant Spending Distribution

	Number of Counties	Public Investment in: New Publicly-Funded Plants	New Plants with Private Financing	Existing Plants	Private Investment
	(1)	(2)	(3)	(4)	(5)
Main Treatment Group	90	3,938	126	351	286
Partially Treated (Smaller Plants)	57	262	122	157	191
Top 100 Mfg Counties	95	2,286	1,017	3,639	2,813
Comparison Counties	1,400	14	198	439	550
Counties Adjacent to Treatment	363	6	153	168	277

Notes: Table breaks down all investment in counties in baseline county sample after applying preliminary restrictions described in Section 3. All investment amounts are in millions of nominal dollars. The main treatment definition is counties with at least one "New Publicly-Funded" plant that cost $10 million or more in nominal dollars. 5 of the top 100 counties by 1939 manufacturing employment in the U.S. are dropped by our preliminary restrictions. Importantly, though we classify counties across rows based on our "large plant" treatment definition, the total investment amounts displayed in each column include all investment in each plant category *regardless* of plant size or expense amount. For example, in Column (2), "public investment in new publicly-funded plants" includes all such plants inclusive of plants costing less than $1 million.

Table A.4: Balance on 1940 Parent Match and Earning

	Coefficient
All Men in Numident Sample	
(N=8,246,000)	
Not Matched to Parent in 1940 Census	0.0023
	(0.0146)
Matched to Parent w/ No Wage Income	-0.0019
	(0.0058)
Matched to Parent w/ Wages in Q1	-0.0006
	(0.0049)
Matched to Parent w/ Wages in Q2	-0.0075
	(0.0048)
Matched to Parent w/ Wages in Q3	-0.0021
	(0.0071)
Matched to Parent w/ Wages in Q4	0.0097
	(0.0073)
Men Matched to Parents w/ Wage Earnings	
(N=3,037,000)	
Log Parent Wage Earnings	0.0045
	(0.0345)
Parent Earninks Rank (0-100)	0.63
	(1.16)

Notes: Table presents estimates of equation 2 on men in baseline NUMIDENT sample (whether or not they match to the 1940 census) controlling only for 1940 population and farm share on parent match characteristics. Standard errors clustered at the county level are displayed in parenthesis. The first six outcomes are mutually exclusive indicators and all coefficients sum to zero by construction. The final two outcomes condition on matching to parents with nonmissing combined wage earnings greater than zero.

Table A.5: County-Level Effects: Heterogeneity

Right-to-Work and Southern States

	Log 1970 Population			**Log 1972 Avg Mfg Prof Wage**			**Log 1972 Manuf Prod Emp**			**1970 Med Fam Income**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Interaction with Indicator for Right-to-Work Law by 1960												
Treat	0.203***	0.205***	0.208***	0.098**	0.094**	0.107***	0.290***	0.228**	0.276***	0.063***	0.053**	0.066***
	(0.047)	(0.045)	(0.040)	(0.042)	(0.041)	(0.023)	(0.111)	(0.112)	(0.083)	(0.023)	(0.022)	(0.014)
Interaction	0.038	0.036	0.049	-0.028	-0.017	0.003	0.080	0.097	0.058	-0.035	-0.029	0.009
	(0.045)	(0.043)	(0.037)	(0.040)	(0.038)	(0.021)	(0.104)	(0.104)	(0.077)	(0.022)	(0.020)	(0.013)
Panel B: Interaction with Indicator for South												
Treat	0.194***	0.191***	0.200***	0.095**	0.093**	0.107***	0.299***	0.233**	0.283***	0.062***	0.057***	0.068***
	(0.047)	(0.044)	(0.040)	(0.041)	(0.040)	(0.023)	(0.111)	(0.112)	(0.083)	(0.022)	(0.022)	(0.013)
Interaction	-0.078*	-0.094**	-0.076**	-0.012	0.007	0.059***	-0.163	-0.149	-0.083	-0.018	-0.001	0.021*
	(0.043)	(0.040)	(0.037)	(0.038)	(0.037)	(0.021)	(0.102)	(0.102)	(0.077)	(0.021)	(0.020)	(0.013)
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Interaction with Prewar Market Access and Manufacturing Density

	Log 1970 Population			Log 1972 Avg Mfg Prof Wage			1970 Manufacturing Emp Shr			1970 Med Fam Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel C: Interaction with 1940 Log Market Access (Standardized and De-Meaned)												
Treat	0.236***	0.243***	0.239***	0.375***	0.300***	0.331***	0.041***	0.040***	0.031***	0.074***	0.064***	0.072***
	(0.048)	(0.046)	(0.041)	(0.113)	(0.114)	(0.085)	(0.009)	(0.009)	(0.009)	(0.024)	(0.022)	(0.014)
Interaction	-0.122***	-0.147***	-0.125***	-0.247**	-0.233**	-0.153**	-0.033***	-0.030***	-0.020**	-0.032	-0.024	-0.021*
	(0.042)	(0.040)	(0.035)	(0.097)	(0.098)	(0.073)	(0.008)	(0.008)	(0.008)	(0.021)	(0.019)	(0.012)
Panel D: Interaction with 1940 Manufacturing Employment Share (Standardized and De-Meaned)												
Treat	0.206***	0.216***	0.207***	0.346***	0.305***	0.386***	0.057***	0.054***	0.043***	0.083***	0.067***	0.078***
	(0.052)	(0.050)	(0.044)	(0.114)	(0.114)	(0.092)	(0.011)	(0.010)	(0.009)	(0.026)	(0.024)	(0.015)
Interaction	-0.016	-0.026	-0.005	-0.058	-0.088	-0.191**	-0.037***	-0.045***	-0.036***	-0.023	-0.018	-0.026**
	(0.045)	(0.043)	(0.038)	(0.099)	(0.099)	(0.079)	(0.009)	(0.009)	(0.008)	(0.023)	(0.021)	(0.013)
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Notes: Table presents estimates of a modified version of Equation 1 that includes an interaction of the treatment of the indicator with the specified county characteristic as well as controls for the main effect of that characteristic along with the specified covariates. Outcomes are differenced relative to 1939/1940 levels; see Appendix B for further details. Both 1940 log market access and 1940 manufacturing employment share are standardized and de-meaned within the analysis sample. 1970 outcomes are county-level tabulations from the Decennial Census; 1972 outcomes are county-level tabulations from the Census of Manufactures. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table A.6: Effects on Male Wages and AGI Reported on 1040 Returns

(a) Form 1040 Wage Outcomes

	Form 1040 Wages, 1969–1984							
	2020 Dollars		**Logged**		**Rank (0 to 1)**		**Nonzero Indicator**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	1,480	1,172**	0.032*	0.023***	0.0111	0.0085**	0.0037	0.0039
	(1,240)	(459)	(0.019)	(0.008)	(0.0095)	(0.0035)	(0.0092)	(0.0036)
N	8,246,000	7,848,000	7,068,000	6,727,000	7,772,000	7,397,000	8,246,000	7,848,000
Dependent Variable Mean	49,580		10.67		0.535		0.8572	
Linked to Parents in 1940 Census	1,259	1,155***	0.026*	0.020***	0.0087	0.0079***	0.0014	0.0027*
	(850)	(346)	(0.015)	(0.007)	(0.0061)	(0.0024)	(0.0038)	(0.0014)
N	4,943,000	4,697,000	4,520,000	4,295,000	4,691,000	4,458,000	4,943,000	4,697,000
Dependent Variable Mean	54,510		10.61		0.571		0.9145	
Parents w/ 1939 Earnings < Median	1,468	1,308***	0.031*	0.026***	0.0105	0.0091***	0.0021	0.0039**
	(960)	(453)	(0.019)	(0.009)	(0.0071)	(0.0033)	(0.0051)	(0.0019)
N	1,836,000	1,752,000	1,679,000	1,603,000	1,739,000	1,659,000	1,836,000	1,752,000
Dependent Variable Mean	51,850		10.69		0.552		0.9149	
Parents w/ 1939 Earnings > Median	724	556*	0.008	0.006	0.0045	0.0035*	0.0001	0.0013
	(504)	(318)	(0.008)	(0.005)	(0.0034)	(0.0021)	(0.0018)	(0.0009)
N	1,202,000	1,136,000	1,135,000	1,072,000	1,146,000	1,083,000	1,202,000	1,136,000
Dependent Variable Mean	66,220		10.95		0.651		0.9442	
Included Covariates								
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓

(b) Form 1040 AGI Outcomes

	AGI, 1969–1984							
	2020 Dollars		**Logged**		**Rank (0 to 1)**		**Nonzero Indicator**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample	1,490	1,366**	0.027	0.027***	0.0108	0.0096**	0.0025	0.0042
	(1,430)	(563)	(0.022)	(0.009)	(0.0102)	(0.0040)	(0.0093)	(0.0037)
N	8,246,000	7,848,000	7,221,000	6,873,000	7,772,000	7,397,000	8,246,000	7,848,000
Dependent Variable Mean	55,910		10.85		0.537			
Linked to Parents in 1940 Census	1,306	1,449***	0.022	0.026***	0.0088	0.0097***	0.0006	0.0034***
	(994)	(425)	(0.017)	(0.008)	(0.0067)	(0.0029)	(0.0037)	(0.0013)
N	4,943,000	4,697,000	4,626,000	4,396,000	4,691,000	4,458,000	4,943,000	4,697,000
Dependent Variable Mean	61,990		10.90		0.577			
Parents w/ 1939 Earnings < Median	1,467	1,411***	0.027	0.027**	0.0103	0.0095**	0.0018	0.0038*
	(1,096)	(544)	(0.021)	(0.011)	(0.0077)	(0.0038)	(0.0051)	(0.0020)
N	1,836,000	1,752,000	1,704,000	1,626,000	1,739,000	1,659,000	1,836,000	1,752,000
Dependent Variable Mean	56,670		10.81		0.543			
Parents w/ 1939 Earnings > Median	1,171*	787*	0.017*	0.012**	0.0073*	0.0051**	0.0009	0.0020**
	(598)	(409)	(0.009)	(0.006)	(0.0038)	(0.0026)	(0.0015)	(0.0008)
N	1,202,000	1,136,000	1,153,000	1,089,000	1,146,000	1,083,000	1,202,000	1,136,000
Dependent Variable Mean	74,620		11.12		0.659			
Included Covariates								
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓

Notes: Table extends Table 1 to examine additional earnings outcomes and results within subsamples of individuals whose parents had nonmissing combined earnings in the 1940 Census, split by whether their parents had earnings above or below the national median across all children in the same cohorts. Standard errors clustered at the county level are displayed in parenthesis. Panel A presents the same Form 1040 wage earnings outcomes as in Table 1 with the addition of an indicator for average earnings equal zero as an outcome. Panel B presents outcomes constructed analogously using Form 1040 adjusted gross income instead of wages. See notes to Table 1 for additional details. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.7: Effects on Men Born in Adjacent Counties

	Adjusted 2020 Dollars			Logged		
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample	2,120.0***	1,235.0***	1,082.0***	0.0467***	0.0348***	0.0290***
	(478.7)	(311.0)	(242.7)	(0.0075)	(0.0054)	(0.0044)
N	9,140,000			7,862,000		
Linked to Parents in 1940 Census	1,604.0***	1,211.0***	1,121.0***	0.0417***	0.0344***	0.0284***
	(339.0)	(239.7)	(196.6)	(0.0063)	(0.0050)	(0.0043)
N	5,508,000			5,041,000		
Parents w/ 1939 Earnings < Median	1,970.0***	1,255.0***	1,165.0***	0.0441***	0.0296***	0.0266***
	(380.2)	(265.3)	(234.5)	(0.0077)	(0.0055)	(0.0049)
N	2,070,000			1,898,000		
Parents w/ 1939 Earnings > Median	-74.5	-183.5	54.5	0.0003	-0.0015	-0.0001
	(299.6)	(267.6)	(215.7)	(0.0051)	(0.0044)	(0.0037)
N	1,288,000			1,217,000		
Included Covariates						
Baseline County Size	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓
Extended County Covariates			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 in which outcomes are regressed on an indicator for whether the individual was born in a county adjacent to a county where a large public plant was built. Standard errors clustered at the county level are displayed in parenthesis. Regressions are estimated on the specified sample using alternative control sets described in the main text. Standard errors clustered at the county level are displayed in parentheses. As in the baseline sample, the estimation sample restricts to children born counties outside of the 100 largest manufacturing counties and omits children born in counties with larger or smaller public plants from control group; we do include children born in counties adjacent to counties with large public plants even when the adjacent county with the plant is a top 100 manufacturing employment county in 1940 or a county dropped after our initial sample restrictions so long as the adjacent birth county itself is not dropped and not a top 100 manufacturing county. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.8: Effects on Men Born in Counties With Smaller Plants

	Adjusted 2020 Dollars			Logged		
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample	1,930.0	148.0	56.2	0.0337	0.0073	0.0072
	(1,707.0)	(969.7)	(678.0)	(0.0237)	(0.0136)	(0.0092)
N	6,872,000			5,860,000		
Linked to Parents in 1940 Census	1,376.0	252.5	254.8	0.0246	0.0053	0.0062
	(1,009.0)	(630.5)	(428.4)	(0.0172)	(0.0109)	(0.0073)
N	4,087,000			3,727,000		
Parents w/ 1939 Earnings < Median	1,668.0	-15.0	-7.4	0.0342	-0.0001	0.0012
	(1,297.0)	(729.7)	(615.6)	(0.0243)	(0.0135)	(0.0114)
N	1,532,000			1,398,000		
Parents w/ 1939 Earnings > Median	534.7	50.1	-81.5	0.0077	-0.0012	-0.0029
	(512.6)	(447.3)	(342.2)	(0.0085)	(0.0069)	(0.0054)
N	938,000			885,000		
Included Covariates						
Baseline County Size	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓
Extended County Covariates			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 in which outcomes are regressed on an indicator for whether the individual was born in a county where a smaller plant was built ("partially-treated" counties) indicator when specified. Standard errors clustered at the county level are displayed in parenthesis. Regressions are estimated on the specified sample using alternative control sets described in the main text. Standard errors clustered at the county level are displayed in parentheses. The sample includes children born in 57 counties with smaller plants and 1,233 comparison counties. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.9: Effects on Men, Additional Parent Earnings and Occupation Breakdowns

(a) Effects by Parent Earnings Status

	Form 1040 Wages, 1969–1984									
	2020 Dollars		**Logged**		**Rank (0 to 1)**		**Yrs Schooling**		**HS Grad**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Not Matched	1,751	924	0.043*	0.027***	0.0141	0.0069	0.207	0.027	0.0193	0.0053
	(1,388)	(568)	(0.023)	(0.009)	(0.0112)	(0.0046)	(0.131)	(0.075)	(0.0122)	(0.0077)
N	3,303,000	3,151,000	2,548,000	2,432,000	3,081,000	2,939,000	316,000	302,000	316,000	302,000
Matched Parent with:										
Nonmissing Wages Above Median	724	556*	0.008	0.006	0.0045	0.0035*	0.121	0.033	0.0045	0.0004
	(504)	(318)	(0.008)	(0.005)	(0.0034)	(0.0021)	(0.084)	(0.040)	(0.0068)	(0.0042)
N	1,202,000	1,136,000	1,135,000	1,072,000	1,146,000	1,083,000	258,000	245,000	145,000	138,000
Nonmissing Wages Below Median	1,468	1,308***	0.031*	0.026***	0.0105	0.0091***	0.230***	0.160**	0.0235**	0.0195**
	(960)	(453)	(0.019)	(0.009)	(0.0071)	(0.0033)	(0.089)	(0.067)	(0.0099)	(0.0083)
N	1,836,000	1,752,000	1,679,000	1,603,000	1,739,000	1,659,000	221,000	211,000	221,000	211,000
No Wages	1,216	1,111***	0.038**	0.024**	0.0087	0.0078***	0.130	0.101*	0.0120	0.0090
	(817)	(380)	(0.015)	(0.010)	(0.0059)	(0.0027)	(0.089)	(0.058)	(0.0102)	(0.0070)
N	1,906,000	1,810,000	1,706,000	1,620,000	1,807,000	1,716,000	145,000	138,000	258,000	245,000
No Wages and Non-Wage Income > $50	959	1,136***	0.034**	0.024**	0.0069	0.0080***	0.166*	0.144**	0.0149	0.0123*
	(698)	(357)	(0.014)	(0.010)	(0.0051)	(0.0026)	(0.086)	(0.061)	(0.0102)	(0.0071)
N	1,408,000	1,339,000	1,260,000	1,199,000	1,336,000	1,271,000	195,000	186,000	195,000	186,000
No Wages and No Non-Wage Income > $50	1,736	836	0.043**	0.018	0.0122	0.0058	0.040	-0.026	0.0043	0.0005
	(1,205)	(587)	(0.022)	(0.013)	(0.0085)	(0.0041)	(0.139)	(0.090)	(0.0154)	(0.0119)
N	498,000	471,000	446,000	421,000	471,000	445,000	62,500	59,000	62,500	59,000
Included Covariates										
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓

(b) Effects by Father Occupation Status

	Form 1040 Wages, 1969–1984									
	2020 Dollars		**Logged**		**Rank (0 to 1)**		**Yrs Schooling**		**HS Grad**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Father Occupation										
Professional	509	140	0.003	-0.003	0.0035	0.0009	0.089	0.011	0.0009	-0.0044
	(715)	(367)	(0.012)	(0.006)	(0.0049)	(0.0024)	(0.070)	(0.054)	(0.0054)	(0.0051)
N	468,000	443,000	436,000	412,000	447,000	423,000	58,500	55,500	58,500	55,500
Clerical	1,374*	654*	0.020	0.007	0.0089	0.0038	0.125*	-0.039	0.0080	-0.0035
	(789)	(390)	(0.014)	(0.007)	(0.0056)	(0.0026)	(0.070)	(0.060)	(0.0065)	(0.0060)
N	372,000	346,000	350,000	326,000	356,000	331,000	46,000	43,000	46,000	43,000
Semi-skilled blue collar	1,157	950**	0.019	0.014*	0.0076	0.0062**	0.150*	0.095*	0.0123	0.0074
	(748)	(401)	(0.013)	(0.008)	(0.0053)	(0.0028)	(0.083)	(0.051)	(0.0088)	(0.0067)
N	1,259,000	1,199,000	1,180,000	1,124,000	1,197,000	1,140,000	153,000	146,000	153,000	146,000
Farming	1,696**	1,974***	0.056***	0.048***	0.0123**	0.0141***	0.168	0.221***	0.0180	0.0250***
	(737)	(451)	(0.016)	(0.014)	(0.0054)	(0.0033)	(0.108)	(0.071)	(0.0138)	(0.0090)
N	1,531,000	1,463,000	1,367,000	1,306,000	1,456,000	1,391,000	217,000	207,000	217,000	207,000
Laborer or Domestic	848	1,273**	0.021	0.027**	0.0062	0.0089**	0.133	0.108	0.0119	0.0132
	(1,124)	(521)	(0.022)	(0.011)	(0.0083)	(0.0038)	(0.103)	(0.074)	(0.0116)	(0.0088)
N	795,000	758,000	727,000	693,000	752,000	717,000	95,000	91,000	95,000	91,000
Included Covariates										
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓

Notes: Table displays OLS estimates of equation 2 under alternative covariate specifications described in the main text. Standard errors clustered at the county level are displayed in parenthesis. Panel A presents estimates from subsamples split by whether the individual is matched to parents in the 1940 Census and the combined earning status of their parents. Panel B presents estimates from subsamples split by the occupation of fathers for individuals matched to the 1940 Census; occupational categories can be reported even for fathers with no wage earnings (for example, self-employed farm workers) but are not exhaustive. See notes to Table 1 for additional details. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.10: Effects on Men's Educational Attainment, by Parent Income Subsamples

	Years of Schooling				HS Graduate				Any College				College Graduate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Full Sample	0.183*	0.219***	0.088		0.016*	0.021***	0.009		0.026**	0.030***	0.016**		0.013	0.015**	0.006	
	(0.097)	(0.080)	(0.056)		(0.009)	(0.007)	(0.006)		(0.013)	(0.011)	(0.006)		(0.008)	(0.007)	(0.004)	
N	940,000	940,000	896,000		940,000	940,000	896,000		940,000	940,000	896,000		940,000	940,000	896,000	
Dependent Variable Mean	12.09				0.70				0.37				0.18			
Linked to Parents in 1940 Census	0.175**	0.221***	0.110**	0.124***	0.015*	0.021***	0.010*	0.010**	0.028**	0.033***	0.019***	0.022**	0.015*	0.017**	0.008**	0.010**
	(0.084)	(0.068)	(0.047)	(0.040)	(0.008)	(0.006)	(0.005)	(0.004)	(0.013)	(0.011)	(0.006)	(0.009)	(0.008)	(0.007)	(0.004)	(0.005)
N	624,000	624,000	594,000	612,000	624,000	624,000	594,000	612,000	624,000	624,000	594,000	612,000	624,000	624,000	594,000	612,000
Dependent Variable Mean	12.24				0.72				0.38				0.19			
No 1940 Match	0.207	0.213**	0.027		0.019	0.022**	0.005		0.021	0.024**	0.008		0.009	0.011	0.001	
	(0.131)	(0.106)	(0.075)		(0.012)	(0.009)	(0.008)		(0.013)	(0.011)	(0.007)		(0.009)	(0.008)	(0.006)	
N	316,000	316,000	302,000		316,000	316,000	302,000		316,000	316,000	302,000		316,000	316,000	302,000	
Dependent Variable Mean	11.78				0.66				0.35				0.16			
1940 Match and Parents with:																
No 1939 Earnings	0.130	0.214***	0.101*	0.132***	0.012	0.023***	0.009	0.013**	0.022*	0.030***	0.016**	0.021**	0.011	0.015**	0.007	0.011**
	(0.089)	(0.080)	(0.058)	(0.049)	(0.010)	(0.009)	(0.007)	(0.006)	(0.013)	(0.012)	(0.008)	(0.009)	(0.008)	(0.007)	(0.005)	(0.005)
N	258,000	258,000	245,000	248,000	258,000	258,000	245,000	248,000	258,000	258,000	245,000	248,000	258,000	258,000	245,000	248,000
Dependent Variable Mean	11.86				0.68				0.33				0.16			
1939 Earnings < Median	0.230***	0.267***	0.160**	0.162***	0.023**	0.029***	0.020**	0.017***	0.027**	0.033***	0.023***	0.020**	0.012**	0.015***	0.011**	0.009**
	(0.089)	(0.073)	(0.067)	(0.048)	(0.010)	(0.008)	(0.008)	(0.006)	(0.013)	(0.009)	(0.007)	(0.010)	(0.006)	(0.005)	(0.005)	(0.004)
N	221,000	221,000	211,000	220,000	221,000	221,000	211,000	220,000	221,000	221,000	211,000	220,000	221,000	221,000	211,000	220,000
Dependent Variable Mean	11.75				0.67				0.32				0.13			
1939 Earnings > Median	0.121	0.104*	0.033	0.073*	0.004	0.005	Z	0.001	0.029*	0.025**	0.013	0.021**	0.015	0.011	0.001	0.008
	(0.084)	(0.062)	(0.040)	(0.043)	(0.007)	(0.005)	(0.004)	(0.004)	(0.016)	(0.011)	(0.008)	(0.010)	(0.012)	(0.009)	(0.006)	(0.007)
N	145,000	145,000	138,000	145,000	145,000	145,000	138,000	145,000	145,000	145,000	138,000	145,000	145,000	145,000	138,000	145,000
Dependent Variable Mean	13.67				0.86				0.57				0.33			
Included Covariates																
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓			✓	✓			✓	✓	
Extended County Covariates			✓				✓				✓				✓	
Individual + Parent Characteristics				✓				✓				✓				✓

Notes: Table displays estimates of Equation 2 under alternative covariate specifications described in the main text. The full sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties who are matched to the 2000 Census long form data. We estimate effects separately within specified subsamples based on whether individuals are matched to parents in the 1940 census, whether their parents had nonmissing combined earnings in the 1940 Census, and split by whether their parents had earnings above or below the national median across all children in the same cohorts. Standard errors clustered at the county level are displayed in parenthesis. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table A.11: Additional Outcomes, 2000 Census Long Form Matched Sample

	2000 Census Match				1979 Wages on 1040 if 2000 Match				Mincer Predicted 1979 Wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	-0.0056*	-0.0041*	-0.0016		0.0417***	0.0379***	0.0245***		0.0106*	0.0127***	0.0051	
	(0.0029)	(0.0022)	(0.0020)		(0.0121)	(0.0112)	(0.0078)		(0.0057)	(0.0048)	(0.0032)	
N	8,246,000	8,246,000	7,848,000		796,000	796,000	758,000		940,000	940,000	896,000	
Dependent Variable Mean	0.114				10.99				10.97			
Linked to Parents in 1940 Census	-0.0070***	-0.0043**	-0.0017	-0.0057***	0.0384***	0.0350***	0.0220**	0.0351***	0.0104**	0.0130***	0.0064**	0.0073***
	(0.0025)	(0.0021)	(0.0021)	(0.0020)	(0.0117)	(0.0111)	(0.0086)	(0.0115)	(0.0051)	(0.0042)	(0.0028)	(0.0025)
N	4,943,000	4,943,000	4,697,000	4,845,000	528,000	528,000	503,000	518,000	624,000	624,000	594,000	612,000
Dependent Variable Mean	0.126				10.99				10.98			
No 1940 Match	-0.0032	-0.0036*	-0.0020		0.0476***	0.0419***	0.0284***		0.0114	0.0119*	0.0015	
	(0.0030)	(0.0022)	(0.0018)		(0.0143)	(0.0128)	(0.0090)		(0.0075)	(0.0061)	(0.0043)	
N	3,303,000	3,303,000	3,151,000		267,000	267,000	255,000		316,000	316,000	302,000	
Dependent Variable Mean	0.096				10.97				10.95			
1940 Match and Parents with:												
No 1939 Earnings	-0.0104***	-0.0048	-0.0026	-0.0082***	0.0504***	0.0408**	0.0264**	0.0486***	0.0078	0.0125***	0.0058*	0.0079***
	(0.0033)	(0.0029)	(0.0026)	(0.0026)	(0.0175)	(0.0173)	(0.0132)	(0.0174)	(0.0053)	(0.0048)	(0.0034)	(0.0029)
N	1,906,000	1,906,000	1,810,000	1,828,000	208,000	208,000	198,000	200,000	258,000	258,000	245,000	248,000
Dependent Variable Mean	0.135				10.86				10.95			
1939 Earnings < Median	-0.0066**	-0.0043*	-0.0025	-0.0051**	0.0382***	0.0306***	0.0247**	0.0356***	0.0128**	0.0151***	0.0094**	0.0091***
	(0.0030)	(0.0025)	(0.0021)	(0.0023)	(0.0127)	(0.0110)	(0.0099)	(0.0121)	(0.0050)	(0.0042)	(0.0039)	(0.0028)
N	1,836,000	1,836,000	1,752,000	1,824,000	191,000	191,000	183,000	190,000	221,000	221,000	211,000	220,000
Dependent Variable Mean	0.120				10.99				10.95			
1939 Earnings > Median	-0.0051***	-0.0035**	-0.0021	-0.0049***	0.0248**	0.0225**	0.0173**	0.0227**	0.0076	0.0063	0.0016	0.0043
	(0.0018)	(0.0015)	(0.0016)	(0.0018)	(0.0097)	(0.0094)	(0.0084)	(0.0102)	(0.0055)	(0.0041)	(0.0027)	(0.0029)
N	1,202,000	1,202,000	1,136,000	1,194,000	129,000	129,000	122,000	128,000	145,000	145,000	138,000	145,000
Dependent Variable Mean	0.121				11.20				11.07			
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓			✓	✓	
Extended County Covariates			✓				✓				✓	
Individual + Parent Characteristics				✓				✓				✓

Notes: Table displays estimates of Equation 2 under alternative covariate specifications described in the main text. The full sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties. We estimate effects separately within specified subsamples based on whether individuals are matched to parents in the 1940 census, whether their parents had nonmissing combined earnings in the 1940 Census, and split by whether their parents had earnings above or below the national median across all children in the same cohorts. The sample in Columns 1–4 includes all individuals in specified sample regardless of whether they match to the 2000 Census long form data; the samples in all other columns are limited to individuals with educational attainment information in the 2000 Census long form. Mincer predicted wages as follows: we estimate simple regressions of 1979 log wage Form 1040 on years of schooling, a quadratic in 1979 experience, and indicators for high school and college degree completion from for all men in the sample who were born in *control* regions and use this to predict 1979 1040 wages of *all* men in the sample matched to the 2000 Census Long Form based only on educational attainment. The outcome in Columns 5–8 is 1979 Form 1040 wages and only men with nonmissing Mincer predicted wages are included in the estimation samples; the outcome in Columns 9–12 is the Mincer predicted 1979 Form 1040 wages. Standard errors clustered at the county level are displayed in parenthesis. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table A.12: Effects on Male Educational Attainment, by Race

	Years of Schooling							
	White Men				Black Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Linked to Parents in 1940 Census	0.201**	0.241***	0.089*	0.122***	0.244*	0.313***	0.200*	0.138
	(0.088)	(0.077)	(0.049)	(0.041)	(0.143)	(0.113)	(0.115)	(0.115)
N	4,228,000	4,228,000	4,015,000	4,149,000	39,000	39,000	37,000	37,500
Dependent Variable Mean	12.4				10.5			
Parents w/ 1939 Earnings < Median	0.240**	0.276***	0.138*	0.151***	0.311**	0.357***	0.200*	0.201*
	(0.095)	(0.081)	(0.072)	(0.050)	(0.146)	(0.132)	(0.120)	(0.107)
N	1,502,000	1,502,000	1,433,000	1,492,000	20,000	20,000	19,000	19,500
Dependent Variable Mean	11.8				10.9			
Parents w/ 1939 Earnings > Median	0.121	0.104*	0.028	0.073*	0.309	0.316	0.280	0.198
	(0.085)	(0.062)	(0.040)	(0.043)	(0.228)	(0.231)	(0.264)	(0.240)
N	1,598,000	1,598,000	1,515,000	1,535,000	1,400	1,400	1,300	1,300
Dependent Variable Mean	13.7				12.8			
Parents w/ No 1939 Wage Earnings	0.179*	0.245***	0.088	0.134***	0.178	0.241	0.207	0.085
	(0.092)	(0.088)	(0.061)	(0.050)	(0.228)	(0.178)	(0.192)	(0.209)
N	1,129,000	1,129,000	1,067,000	1,122,000	17,500	17,500	17,000	17,000
Dependent Variable Mean	12.0				9.9			
Included Covariates								
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓	
Extended County Covariates			✓				✓	
Individual + Parent Characteristics				✓				✓

Notes: Table displays estimates of Equation 2 for Black and white men under alternative covariate specifications described in the main text. The full sample is all men in the NUMIDENT born 1922–1940 in one of the 90 treatment or 1,400 comparison counties who are matched to both the 1940 Census (where race is observed) and 2000 Census long form data. For each group, we estimate effects separately within specified subsamples based on whether individuals are matched to parents in the 1940 census, whether their parents had nonmissing combined earnings in the 1940 Census, and split by whether their parents had earnings above or below the national median across all children in the same cohorts. Standard errors clustered at the county level are displayed in parenthesis. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.13: Effects on Men, Older Versus Younger Cohorts

(a) Older Male Cohorts Born 1922–1930

	A. Older Male Cohorts Born 1922–1930									
	Wages, 1969–1984									
	2020 Dollars		Logged		Rank		Years Schooling		HS Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full Sample	1,439	1,217**	0.037**	0.026***	0.011	0.009**	0.183*	0.065	0.018*	0.007
	(1,276)	(475)	(0.019)	(0.008)	(0.010)	(0.004)	(0.110)	(0.065)	(0.011)	(0.007)
N	3,954,000	3,759,000	3,209,000	3,050,000	3,624,000	3,446,000	376,000	357,000	376,000	357,000
Linked to Parents in 1940 Census	1,411*	1,318***	0.032**	0.023***	0.010	0.009***	0.155*	0.082	0.014	0.007
	(844)	(344)	(0.014)	(0.007)	(0.006)	(0.002)	(0.092)	(0.053)	(0.010)	(0.006)
N	2,352,000	2,233,000	2,092,000	1,984,000	2,184,000	2,073,000	257,000	245,000	257,000	245,000
Parents w/ 1939 Earnings < Median	1,557	1,349***	0.034*	0.027***	0.011	0.009***	0.216**	0.139*	0.025**	0.023**
	(1,014)	(458)	(0.018)	(0.009)	(0.008)	(0.003)	(0.101)	(0.079)	(0.011)	(0.010)
N	774,000	737,000	685,000	653,000	713,000	680,000	77,500	74,000	77,500	74,000
Parents w/ 1939 Earnings > Median	963*	868***	0.013	0.010*	0.006*	0.006**	0.074	-0.017	0.001	-0.006
	(533)	(336)	(0.009)	(0.006)	(0.004)	(0.002)	(0.089)	(0.048)	(0.009)	(0.006)
N	583,000	551,000	540,000	510,000	544,000	514,000	61,000	58,000	61,000	58,000
Included Covariates										
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓

(b) Younger Male Cohorts Born 1931–1940

	B. Younger Male Cohorts Born 1931–1940									
	Wages, 1969–1984									
	2020 Dollars		Logged		Rank		Years Schooling		HS Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full Sample	1,590	1,194**	0.029	0.022***	0.012	0.009**	0.187**	0.114**	0.015*	0.012*
	(1,244)	(485)	(0.020)	(0.008)	(0.009)	(0.004)	(0.090)	(0.055)	(0.009)	(0.006)
N	4,291,000	4,089,000	3,859,000	3,677,000	4,148,000	3,952,000	564,000	539,000	564,000	539,000
Linked to Parents in 1940 Census	1,145	1,040***	0.022	0.018**	0.008	0.007***	0.191**	0.138***	0.015*	0.014**
	(886)	(378)	(0.016)	(0.007)	(0.006)	(0.003)	(0.081)	(0.050)	(0.008)	(0.006)
N	2,590,000	2,465,000	2,428,000	2,310,000	2,507,000	2,386,000	367,000	350,000	367,000	350,000
Parents w/ 1939 Earnings < Median	1,433	1,289***	0.029	0.026***	0.010	0.009**	0.233***	0.171**	0.022**	0.017*
	(955)	(488)	(0.019)	(0.010)	(0.007)	(0.003)	(0.088)	(0.070)	(0.011)	(0.009)
N	1,062,000	1,015,000	994,000	950,000	1,025,000	980,000	143,000	137,000	143,000	137,000
Parents w/ 1939 Earnings > Median	519	280	0.004	0.002	0.003	0.002	0.159*	0.077*	0.008	0.006
	(510)	(335)	(0.009)	(0.006)	(0.004)	(0.002)	(0.085)	(0.045)	(0.006)	(0.004)
N	618,000	585,000	594,000	562,000	602,000	569,000	84,500	80,000	84,500	80,000
Included Covariates										
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓

(c) Male Cohorts Under 18 at *End* of War (Born 1928–1940)

	C. Male Cohorts Under 18 at End of War (Born 1928–1930)									
	Wages, 1969–1984									
	2020 Dollars		Logged		Rank		Years Schooling		HS Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Full Sample	1,592	1,194**	0.031	0.023***	0.012	0.008**	0.178*	0.093*	0.015	0.010
	(1,264)	(480)	(0.019)	(0.008)	(0.009)	(0.004)	(0.092)	(0.055)	(0.009)	(0.006)
N	5,576,000	5,309,000	4,939,000	4,703,000	5,353,000	5,097,000	705,000	672,000	705,000	672,000
Linked to Parents in 1940 Census	1,175	1,086***	0.024	0.019***	0.008	0.007***	0.172**	0.116**	0.014*	0.011**
	(882)	(366)	(0.016)	(0.007)	(0.006)	(0.003)	(0.081)	(0.048)	(0.008)	(0.006)
N	3,379,000	3,213,000	3,144,000	2,990,000	3,253,000	3,093,000	464,000	442,000	464,000	442,000
Parents w/ 1939 Earnings < Median	1,445	1,292***	0.030	0.026***	0.010	0.009***	0.213**	0.148**	0.022**	0.018*
	(970)	(476)	(0.019)	(0.010)	(0.007)	(0.003)	(0.086)	(0.068)	(0.010)	(0.009)
N	1,337,000	1,277,000	1,243,000	1,187,000	1,284,000	1,226,000	175,000	167,000	175,000	167,000
Parents w/ 1939 Earnings > Median	613	438	0.006	0.004	0.004	0.003	0.138*	0.051	0.005	0.002
	(507)	(327)	(0.008)	(0.006)	(0.003)	(0.002)	(0.084)	(0.042)	(0.006)	(0.004)
N	818,000	773,000	782,000	739,000	791,000	748,000	108,000	102,000	108,000	102,000
Included Covariates										
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓		✓

Notes: Table displays estimates of Equation 2 for specified cohort groups under alternative covariate specifications described in the main text. In Panels A, B, and C, the full samples include all men in the NUMIDENT born in one of the 90 treatment or 1,400 comparison counties in the 1922–1930 birth cohorts, 1931-1940 birth cohorts, and 1928–1940 cohorts, respectively. We estimate effects separately within specified subsamples based on whether individuals are matched to parents in the 1940 census, whether their parents had nonmissing combined earnings in the 1940 Census, and split by whether their parents had earnings above or below the national median across all children in the same cohorts. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table A.14: Effects on Composition of In-Migrants

	Individual-Level Unweighted		Equalized 1979 County Weights		N In-Migrants
	(1)	(2)	(3)	(4)	(5)
Parent Characteristics					
Parent Earnings Rank (1-100)	-0.049	0.488	-1.225	-0.620	1,991,000
	(1.356)	(0.723)	(0.780)	(0.494)	
Parent Max Yrs Education	0.172	0.076	-0.016	0.044	2,963,000
	(0.118)	(0.072)	(0.082)	(0.058)	
Education (2000 Census)					
Years of Schooling	0.124	0.070	0.047	0.081	592,000
	(0.136)	(0.086)	(0.087)	(0.068)	
High School Graduate	0.020	0.012	0.011	0.013*	592,000
	(0.012)	(0.009)	(0.008)	(0.007)	
College Graduate	0.00	-0.003	-0.008	-0.004	592,000
	(0.019)	(0.011)	(0.010)	(0.007)	
Adult Wage Earnings (69–84)					
Logs	0.089***	0.047***	0.086***	0.069***	4,506,000
	(0.031)	(0.017)	(0.019)	(0.017)	
Rank (0-1)	0.029**	0.017***	0.026***	0.022***	4,436,000
	(0.012)	(0.006)	(0.006)	(0.005)	
Baseline County Size	✓	✓	✓	✓	
County Geography/Infrastructure		✓		✓	
Extended County Covariates		✓		✓	

Notes: Table displays estimates of Equation 2 on the sample of men in the NUMIDENT born 1922–1940 who reported on their 1979 Form 1040 return that they lived in one of the 90 treatment or 1,400 comparison counties *and* whose 1979 county is different than their NUMIDENT birth county (but not missing). Standard errors clustered at the county level are displayed in parenthesis. In Columns 3 and 5, we apply alternative sample weights constructed so that all individuals in the regression sample living in a county *in 1979* are equally weighted by $1/N_c$, where N_c are the number of individuals *in the regression sample* in county c such that the sum of weights within a *destination* county is one. For "Parent Characteristics" outcomes, the dependent variables are only defined for individuals who match to parents in the 1940 Census, and parent earnings rank is only defined for children whose parents have nonmissing 1939 earnings. "Education" outcomes are only defined for individuals who are matched to the 2000 Census Long Form data. Adult wage earnings are drawn from the Form 1040 tax return data and outcomes are constructed as in Table 1. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

B Data Appendix

B.1 Plant Data

We use plant-level data on wartime capital expenditures from the 1945 War Production Board data book *War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator* (United States War Production Board, 1945b). We originally accessed the book at the Harvard University Library where we had all pages photographed. The photographs were then digitized by the authors. The raw data report firm name, division, and plant location; the war products and production capacity resulting from the specific investment as well as the date the capacity became available; the sponsor agency; total cost, with breakdowns by "Publicly Financed; Total," "Publicly Financed; Structure," "Publicly Financed; Equipment," "Privately Financed; Total," "Privately Financed; Structure," "Privately Financed; Equipment," and "Privately Financed; Other."

We collapse all expenditure information to the plant level. The raw data report place names and states of each plants; we match place name strings to county identifiers using as crosswalk file generously provided by James Lee (Lee, 2016). We examined the list of unmatched place names by hand and updated place names that have changed since 1945 whenever possible.

We identify newly-constructed plants using two criteria. First, several plants in the data have a "New Plant" label under their location, though there is no documentation describing the assignment of this label. We checked these "New Plant" labels by hand against several case studies of plants we know were newly-constructed based on other historical texts—while informative, these labels are incomplete. For example, one of the largest new steel mills constructed for the war effort, the Columbia Steel Geneva plant in Utah, was not labelled as a "New Plant." A common feature of the plants explicitly marked as new is that the amount of expenditure on structures is very large relative to the expenditure on equipment. Accordingly, we used a second criterion to identify new plants: we marked plants as new if over 40 percent of the total cost was spent on structures rather than equipment. One potential concern is that seemingly new plants—either those marked as new in the books or those with high amounts of investment in structures—may actually be additions to larger, pre-existing industrial complexes run by the operating firm. To mitigate this possibility, we exclude plants from being classified as "new" if their operating firm made private investments in any other plant that had *not* been marked as new in our data by either criteria. (One exception is if a new public plant has other *entirely* public-funded plants nearby that are equipment intensive but not explicitly marked as new, it is likely a new operation with supporting facilities, and we continue to classify it as "new.") We researched the histories of a sample of plants categorized as new under our scheme and found in all cases that the plants were, in fact, newly constructed.

We identify new plants as "publicly-financed" if virtually all expenditures on structures came were from public agencies; we classify remaining new plants as (partially) "privately-financed." Since firms sometimes make minuscule private investments in otherwise publicly-financed plants, we classify plants as public if less than 1 percent of expenditure on structures was privately financed. 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. We exclude these "partially privately-financed" plants from our definition of publicly-financed plants, and restrict our focus to publicly-financed plants that had virtually no private investment in any structure at the facility.

The plant-level data are summarized in Appendix Table A.1. In total, our classification identifies only 366 out of 12,766 plants in the data book as being new, publicly-financed plants that cost over $1 million in contemporaneous dollars. Despite comprising just 3 percent of all plants in the data, this subset of plants accounts for 39 percent of all industrial investment listed in the data book ($7.3 billion out of $18.9 billion total).

B.2 County-Level Data

To avoid issues with changes in county borders, we identify counties that split or merged with other counties after 1940 and drop observations in years after the split or merger. Due to significant changes in county definitions during the observation period, we exclude all counties and county-equivalent regions in Virginia. We omit Alaska and Hawaii, which did not become States until after WWII.

We use county-level variables covering the period 1900–2012 drawn from several sources. We obtain economic and demographic tabulations of records from the Economic Censuses and the Decennial Population Censuses that have been compiled in several data archives, in particular the Census Bureau County and City Data Books (CCDB) (Census, 1984a,b, 1989), the IPUMS National Historical Geographic Information System (NHGIS) (Minnesota Population Center, 2011), and by (Haines, 2005). We obtained data from 2000 onwards from County Business Patterns, American Fact Finder, and other searchable databases on the Census Bureau website. We are grateful to Andreas Ferrara for sharing data on WWII mobilization and casualty rates from Ferrara (2018). We additionally 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. This information allows us to account for prewar policy interventions in our analysis.

In some specifications in our county-level analysis, we difference outcomes over their 1939 or 1940 values, depending on what is available. The variables derived from economic censuses (all manufacturing outcomes except for the share of residential employment in manufacturing, and all other sector-specific outcome) and median family income from the Decennial Census are all differenced relative to 1939 outcome levels; all other variables from the Decennial Censuses are differenced reltive to 1940 levels. In all cases but one, manufacturing outcomes are derived from the Census of Manufactures; the one exception is the manufacturing share of civilian employment which is derived from the Censuses of Population and refers to individuals who reside in the specified county (rather than individuals employed at establishments in the specified county as in the Economic Censuses).

Definitions of Constructed Variables

- **Manufacturing/Retail/Wholesale Average Wage**: In all years, average wages are calculated as total payroll divided by total number of employees of all establishments in the county. For manufacturing, payroll and employment are restricted to production workers.

- **Median Family Income** County-level tabulations are available for the 1950–1990 Censuses. For 1940, we directly calculate median family pre-tax wage and salary income for married-couple family households in each county using the full-count Census data available through IPUMS.

- **1940 Market Access** We construct 1940 county-level market access using data on 1940 travel times between county pairs from Jaworski et al. (2023). We then construct market access using these travel times as $MA_{c;1940} = \sum_{c' \neq c} L_{c';1940} time_{c,c';1940}^{-1.5}$, where $time_{c,c';1940}$ is the travel time between county c and county c', and $L_{c;1940}$ is the 1940 population of county c. We take the elasticity of -1.5 from Herzog (2021)—in Herzog (2021), this elasticity is the *product* of the elasticity of iceberg travel costs to travel time and the structural trade elasticity.

- **1963 Predicted Union Density** We predict county-level union density using 1953 estimates of industry union density from Troy (1957) and the industry employment distribution within each county from the 1964 County Business Patterns data assembled by Eckert et al. (2022). We use CBP data from 1964 because it is the earliest year with subindustry detail for a sufficient number of counties.

B.3 Individual-Level Microdata Construction

NUMIDENT

In this section we describe the “Numerical Identification System” (or “NUMIDENT”), created by the Social Security Administration in 1972. The NUMIDENT is a database with applicant information supplied to Form SS-5, an application for a U.S. social security number. It reports the date and place of birth, name of the applicant, and other information such as citizenship, race, sex, and death date. As with all data sets on individuals we use in this paper, the Social Security Numbers (SSNs) on the Census Bureau’s NUMIDENT file have been replaced with anonymized Protected Identification Keys (PIKs), which we can use to link individuals across files. Each SSN is associated with a single PIK.

The NUMIDENT contains a 12-character string for the birth city and two letter state or county abbreviation. Using the crosswalk created by Taylor et al. (2016), we map those birth locations to counties. Stuart (2022) has a publicly-accessible description of the birthplace to county crosswalk in an online appendix. One challenge in creating the crosswalk is that not all city strings map to a single county in a state. For example, Stuart (2022) notes that three different Populated Places in North Carolina have names that begin with “Bells Crossroads” and each is located in a different county. However, he finds that 94 percent of the individuals in his sample (from linked ACS respondents) could be assigned a unique county of birth. We only include birthplace information for individual’s whose birth location can be mapped to a single county. Throughout our discussion, we use the phrase ”full NUMIDENT sample” to refer to individuals in the NUMIDENT with valid birth county information.

We subset to individuals born in 1922 - 1940 and drop those who are foreign-born, missing birth county, or labeled as not a “good match” under the place matching algorithm of Taylor et al. (2016).

IRS Form 1040 Return Data

We obtain the IRS Form 1040 Return data for all individuals in 1969, 1974, 1979, 1984, 1989 with the PIK available for primary filers in all years but secondary filers in 1974 onwards. The dataset contains information on annual adjusted gross income (AGI) and wage and salary earnings and county identifiers at the tax unit level. We subset to primary filers that could be matched to the NUMIDENT.

We inflation-adjust the Adjusted Gross Income (AGI) and wage and salary earnings variables to 2020 dollars using the CPI-U-RS, which has been used to adjust income for official Census Bureau publications. If an individual is not listed as a filer on Form 1040 in a given year, we code their AGI and earnings as zero. Some individuals who actually have positive but low levels of earnings may not file a 1040 tax return if they have no positive tax liability after credits, exemptions, and deductions and are not required to file or if they do not comply with filing requirements—our method codes earnings in these cases as zeros. In practice, filing rates in our sample are very high—80 percent of men in our full sample and 87 percent of men matching to the 1940 Census filed a 1040 return in 1979. Moreover, we find nearly identical effects on male 1040 wage and salary earnings and third-party-reported W-2 wage and salary earnings (reported whether or not the individual files a 1040 return) in the linked SSA-CPS sub-sample in which we observe both types of returns for common years; this suggests that our coding provides a reasonable approximation. We calculate the average wage and salary earnings and AGI pooled across years, treating missing values as zero. For the average AGI we use the original variables, keeping negative values as is.

We construct average income ranks using AGI and wage and salary earnings separately using pooled real income across specified years, ranking children against others in the same birth cohort who survived until the end of the pooling period. Children with the exact same income are assigned the average rank. Lastly, we winsorize annual and average income variables (AGI and wage and salary earnings) at the 95th percentile among those with non missing and non negative values and take logs.

Record Linkage

In this section, we describe the data linkage process used to construct the data in this paper. As described in Ferrie et al. (2021), the Census Bureau uses the Person Identification Validation System (PVS) to assign a PIK to individuals in a "match" file, such as survey respondents and individuals in administrative records. These individuals are assigned a PIK if their characteristics match sufficiently well to a reference record associated with that PIK. The reference file is created from the Social Security Administration's NUMIDENT file as well as other federal administrative data sources. Each of the over 500 million Social Security Numbers in the reference file corresponds to a PIK. The reference file also contains information on date of birth, name, location of birth, name changes (for example, at marriage), and locations over time (from other federal administrative data).

The PVS generally follows several steps to link individuals to PIKs: 1) preprocessing, 2) sorting into blocks, 3) identifying potential matches, and 4) resolving multiple matches. In preprocessing, the match file records are standardize the input fields that will be matched to the reference file. In the sorting into blocks step, the records are sorted and grouped into "blocks" to be fed into the matching system. These blocks define the groups that will be matched to corresponding blocks on the reference file. For example, a file may be divided by state and the records in each state will be matched to the records from that state on the reference file. This reduces the search space when attempting to find matches in the reference file Michelson and Knoblock (2006). There are various blocking strategies (called modules), such as the first three digits of SSNs, various definitions of geography (such as zip code), first and last initials, date of birth, etc.

Within each block, PVS creates the Cartesian product of the records to be linked and the reference file within each black. Each potential match is scored based on the similarity of the information on the match and reference files. For example, similarity could determined using a string comparator program to measure the Jaro-Winkler distances between names Winkler (1995). These distances measure the extent to which two names match and make it possible to match names in the presence of misspellings or other errors in the data. Inexact matches are also permitted for variables such as year of birth. The final score for each potential match is determined from the information in each variable used Fellegi and Sunter (1969). Matches are kept if the score exceeds sum pre-specified cutoff. At the end of a module (which may include multiple passes), records with multiple possible matches are compared with a pre-defined decision rule about which, if any, of the matches will be kept. Finally, the matched records are removed from the file and the unmatched records proceed to the next module.

1940 Decennial Census

In this section we describe the 1940 Decennial Census, which provides information for the parents. The 1940 Census file contains demographic and economic information, such as race, education, wage and salary earnings, and other income, for the children's households. We include only those who were children aged 0-18 in 1939 and their parents. The 1940 Census only includes reported wage earnings in 1939 with a separate flag for non wage earnings of at least 50 dollars. The wage income is top coded by Census at 5,001 dollars and we do not winsorize any further.

We use two income measures to construct the ranks for parents: 1) the fathers' occupational scores assigned based on Census region, landownership status (if farmer), occupation in 1950 basis, and race following the method in Collins and Wanamaker (2022) and 2) the sum of both parents' reported earned income in 1939. Using the income scores, we construct national ranks for fathers against other fathers in the subset matched to the NUMIDENT by PIK (62.51% of children in Census with valid birth county information can be matched to NUMIDENT and assigned a birth county). Similarly, using both parents' earned income, we construct national ranks for parents against other parents among those matched to NUMIDENT with reported wage from at least one parent. We scale both ranks to 0-100. We use the matched NUMIDENT sample as the basis for comparison because we define the sample based on NUMIDENT age, which does not always match 1940 Census age. For comparison, we also calculate ranks relative to all children of the same age in the 1940 Census and display summary statistics in Appendix Table A.2.

2000 Decennial Census Long Form

Here we describe the 2000 Decennial Census Long Form, which we additionally link to NUMIDENT to measure educational attainment for a larger sample than SSA-CPS. The long form includes additional questions on housing and population questions and was administered to 1 in 6 households. 11.6% of those in the full NUMIDENT/IRS and 12.5% of those in linked 1940 Census-NUMIDENT can be matched to 2000 long form. We generate indicators for degree completed from the years of schooling variable on the long form ("College grad" if years of schooling is at least 16, "Any college" if least 13, and "High school grad" if at least 12).

SSA-CPS Data

In this section, we describe in more detail the construction of the SSA-CPS data used in the paper. For the CPS ASEC, we use 1991, 1994, and 1996-2017, standardizing variables names and codings that changed over time, particularly education, race and Hispanic-origin.[68] As the CPS ASEC is a panel where housing units are in sample in two consecutive years, many individuals are in the survey more than once. We keep responses for the first year an individual appears in the sample. Only individuals with a PIK are in our analysis sample, as only these individuals can be linked to the birth information we need to assign treatment status.

From the Detailed Earnings Record (DER), our measure of earnings includes Box 1 wage and salary earnings from W-2 returns plus deferred compensation reported in W-2 Box 12.[69] We created two summary files of earnings from the DER for PIKs in our CPS ASEC file: 1) a person-year file of total wage and salary earnings and 2) person-job-year file of wage and salary earnings in each job. The second file contains the Employee Identification Number (EIN) of each firm the individual received a W-2 return in each year. We inflation adjust the DER earnings using the CPI-U-RS, as has been used in official Census Bureau income publications. Individuals that were assigned a PIK but do not have earnings in a given year in the DER did not receive a W-2 return in that year are assigned wage and salary earnings

[68] We use these years, as they were the only ones with PIKs assigned to survey respondents and linkable to the data described below at the start of the project.

[69] The deferred compensation includes individual, but not employer, contributions to 401(k), 403(b), 408(k), 457(b), and 501(c) retirement plans.

of zero. As noted above, we get birthplace information of all individuals in our CPS ASEC PIK file from the NUMIDENT.

We limit our sample to individuals born between 1922-1940, dropping those whose ages from NUMIDENT and CPS ASEC differ by more than 3 years (due to misreporting, errors, imputations etc). We also drop those with missing counties. Linkage rates in the CPS ASEC have varied over time. Ziliak et al. (2020) report linkage rates from 62 to 79 percent from 1996 to 2005, a period in which respondents had to opt in to linkage as part of the survey. In 2006, the Census Bureau changed the opt-in policy to one in which respondents could opt out of linkage by mail or through the Bureau's website. Subsequently, linkage rates increased to 84 percent or higher in each year from 2006 on.

1940 Decennial Census

We also use the 1940 decennial census for information on the pre-war characteristics of individuals. The 1940 census file contains information on the parent's race, education, wage and salary earnings, and other income. Massey et al. (2018) detail the linkage process for the 1940 decennial census, which was unique for two reasons. First, the reference file discussed above does not contain information on location of residence as far back as 1940, making linkage more difficult. Second, the 1940 census contains information that can aid in linkage that is not traditionally used in the PVS process. In particular, Massey et al. (2018) use the parents' names for children co-residing with their parents in 1940. As a result, despite an overall linkage rate of 40 percent, 70 percent of children were assigned a PIK.

As noted in Rothbaum and Massey (2021), there are several reasons children may not receive a PIK in the 1940 census. Their information may not be sufficiently unique (name, age, birth location, etc.) to cross the pre-defined linkage threshold in the PVS process used by Massey et al. (2018). Additionally, it is possible that unmatched children never received an SSN. Until relatively recently, SSNs were not assigned at birth. Instead, workers received SSNs when they started a job that was covered by the Social Security system. This excluded large classes of jobs until the late 1970s. Furthermore, the NUMIDENT, which forms the backbone of the linkage reference file, was not created until 1972.[70]

Matching DER to Firm data

We construct our firm data from the Standard Statistical Establishment List (SSEL) files created from the Census Bureau's Business Register (BR). The SSEL contains information on establishments by firm and year, including location, employment, and payroll. We use

[70]See **?** for more information on the history of SSNs and the NUMIDENT.

the data from 1978-1990. We create two summary files from the SSEL.

The first file summarizes the total employment and payroll by Standard Industry Classification (SIC) code for each firm Employee Identification Number (EIN) at the national level. The SSEL is an establishment-level file, with industry classifications assigned to each establishment. Multi-unit firms can therefore have multiple industry codes. For each EIN, we assign the industry with the largest share of a firm's employment. We use this file to determine average co-worker pay (total payroll minus the individual's DER earnings divided by employment minus one) and firms' core industries.

The second file which tabulates firm employment and payroll by SIC code and county. As with the national file, we assign each EIN-county pair the SIC code with the largest share of firm employment (in each county). We use this second file to calculate the Krueger and Summers (1988) industry premium of each firm. We match the industry premium in Appendix Table 1 in Krueger and Summers (1988) to establishments using the most disaggregated SIC code available and calculate an employment weighted average premium across all establishments within the firm. We also use this second file to create an indicator denoting whether the firm issuing the largest W-2 return to an individual in a year in DER has any establishments in that individual's birth county.

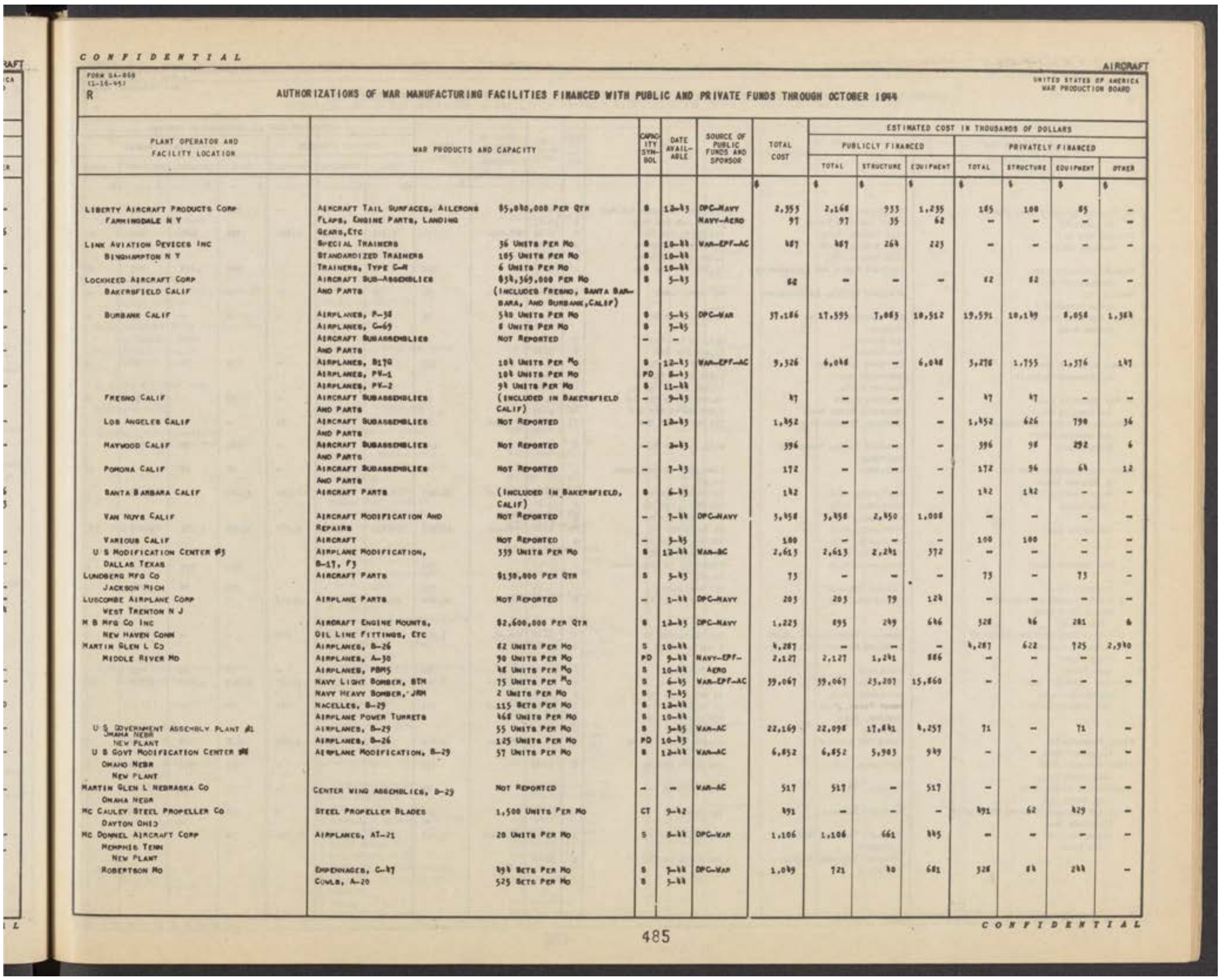
1910-1940 Linked Census Data

For our analysis of the 1910-1940 Linked Censuses, we construct our sample as follows: We first merge the full count 1910 Census with 1940 Census using the crosswalk provided by The Census Linking Project (Abramitzky et al., 2022), keeping only observations matched using the exact name procedure (.12% of observations in 1910 did not find matches). We then identify fathers from the 1910 Census for the native-born black or white sons aged 18 years or younger. Approximately 8.6% did not have a father in the household.

Since the 1910 Census does not have information for income, we follow the methodology from Collins and Wanamaker (2022) and calculate occupational-income scores and assign them based on Census region, landownership status (if farmer), occupation in 1950 basis, and race, to sons in 1940 and fathers in 1910. The score is missing if occ1950 is "not yet classified" (code 979), "occupation is missing/unknown" (code 997), and "N/A(blank)" (code 999).

We follow Collins-Wannamaker's procedure and drop all sons without fathers in the household. To construct the ranks, we first round the income scores to nearest .1, and calculate the national ranking corresponding to each income score in fathers' and sons' generation (1910 and 1940, respectively). Specifically, we assign income scores to the full count censuses and calculate the average percentile ranks of each score. Then we merge the percentile ranks to the matched fathers and sons sample based on income scores and identify the national ranking of fathers and sons in their respective generations.

Figure B.1: Example Page, WWII Plant Data



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Form SA-868 (1-16-45) R

AUTHORIZATIONS OF WAR MANUFACTURING FACILITIES FINANCED WITH PUBLIC AND PRIVATE FUNDS THROUGH OCTOBER 1944

United States of America War Production Board

Plant Operator and Facility Location	War Products and Capacity		Capacity Symbol	Date Available	Source of Public Funds and Sponsor	Total Cost	Estimated Cost in Thousands of Dollars: Publicly Financed: Total	Publicly Financed: Structure	Publicly Financed: Equipment	Privately Financed: Total	Privately Financed: Structure	Privately Financed: Equipment	Privately Financed: Other
						$	$	$	$	$	$	$	$
Liberty Aircraft Products Corp Farmingdale N Y	Aircraft Tail Surfaces, Ailerons Flaps, Engine Parts, Landing Gears, Etc	$5,040,000 Per Qtr	S	12-43	DPC-Navy	2,353	2,168	933	1,235	185	100	85	–
					Navy-Aero	97	97	35	62	–	–	–	–
Link Aviation Devices Inc Binghampton N Y	Special Trainers	36 Units Per Mo	S	10-44	War-EPF-AC	487	487	264	223	–	–	–	–
	Standardized Trainers	105 Units Per Mo	S	10-44									
	Trainers, Type C-8	6 Units Per Mo	S	10-44									
Lockheed Aircraft Corp Bakersfield Calif	Aircraft Sub-Assemblies And Parts	$34,369,000 Per Mo (Included Fresno, Santa Barbara, And Burbank, Calif)	S	5-43		82	–	–	–	82	82	–	–
Burbank Calif	Airplanes, P-38	540 Units Per Mo	S	5-45	DPC-War	37,186	17,595	7,083	10,512	19,591	10,149	8,058	1,384
	Airplanes, C-69	8 Units Per Mo	S	7-45									
	Aircraft Subassemblies And Parts	Not Reported	–	–									
	Airplanes, B17G	104 Units Per Mo	S	12-43	War-EPF-AC	9,326	6,048	–	6,048	3,278	1,755	1,376	147
	Airplanes, PV-1	104 Units Per Mo	PD	8-43									
	Airplanes, PV-2	94 Units Per Mo	S	11-44									
Fresno Calif	Aircraft Subassemblies And Parts	(Included in Bakersfield Calif)	–	9-43		47	–	–	–	47	47	–	–
Los Angeles Calif	Aircraft Subassemblies And Parts	Not Reported	–	12-43		1,452	–	–	–	1,452	626	790	36
Maywood Calif	Aircraft Subassemblies And Parts	Not Reported	–	2-43		396	–	–	–	396	98	292	6
Pomona Calif	Aircraft Subassemblies And Parts	Not Reported	–	7-43		172	–	–	–	172	96	64	12
Santa Barbara Calif	Aircraft Parts	(Included In Bakersfield, Calif)	S	6-43		142	–	–	–	142	142	–	–
Van Nuys Calif	Aircraft Modification And Repairs	Not Reported	–	7-44	DPC-Navy	3,458	3,458	2,450	1,008	–	–	–	–
Various Calif	Aircraft	Not Reported	–	3-45		100	–	–	–	100	100	–	–
U S Modification Center #3 Dallas Texas	Airplane Modification, B-17, F3	339 Units Per Mo	S	12-44	War-AC	2,613	2,613	2,241	372	–	–	–	–
Lundberg Mfg Co Jackson Mich	Aircraft Parts	$130,000 Per Qtr	S	3-43		73	–	–	–	73	–	73	–
Luscombe Airplane Corp West Trenton N J	Airplane Parts	Not Reported	–	1-44	DPC-Navy	203	203	79	124	–	–	–	–
M B Mfg Co Inc New Haven Conn	Aircraft Engine Mounts, Oil Line Fittings, Etc	$2,600,000 Per Qtr	S	12-43	DPC-Navy	1,225	895	249	646	328	46	281	1
Martin Glen L Co Middle River Md	Airplanes, B-26	82 Units Per Mo	S	10-44		4,287	–	–	–	4,287	622	725	2,940
	Airplanes, A-30	90 Units Per Mo	PD	9-44	Navy-EPF-Aero	2,127	2,127	1,241	886	–	–	–	–
	Airplanes, PBM5	48 Units Per Mo	S	10-44									
	Navy Light Bomber, BTM	75 Units Per Mo	S	6-45	War-EPF-AC	39,067	39,067	23,207	15,860	–	–	–	–
	Navy Heavy Bomber, JRM	2 Units Per Mo	S	7-45									
	Nacelles, B-29	115 Sets Per Mo	S	12-44									
	Airplane Power Turrets	468 Units Per Mo	S	10-44									
U S Government Assembly Plant #1 Omaha Nebr New Plant	Airplanes, B-29	55 Units Per Mo	S	3-45	War-AC	22,169	22,098	17,841	4,257	71	–	71	–
	Airplanes, B-26	125 Units Per Mo	PD	10-43									
U S Govt Modification Center #4 Omaha Nebr New Plant	Airplane Modification, B-29	57 Units Per Mo	S	12-44	War-AC	6,852	6,852	5,903	949	–	–	–	–
Martin Glen L Nebraska Co Omaha Nebr	Center Wing Assemblies, B-29	Not Reported	–	–	War-AC	517	517	–	517	–	–	–	–
Mc Cauley Steel Propeller Co Dayton Ohio	Steel Propeller Blades	1,500 Units Per Mo	CT	9-42		491	–	–	–	491	62	429	–
Mc Donnel Aircraft Corp Memphis Tenn New Plant	Airplanes, AT-21	20 Units Per Mo	S	8-44	DPC-War	1,106	1,106	661	445	–	–	–	–
Robertson Mo	Empennages, C-47	494 Sets Per Mo	S	3-44	DPC-War	1,049	721	40	681	328	84	244	–
	Cowls, A-20	525 Sets Per Mo	S	3-44									

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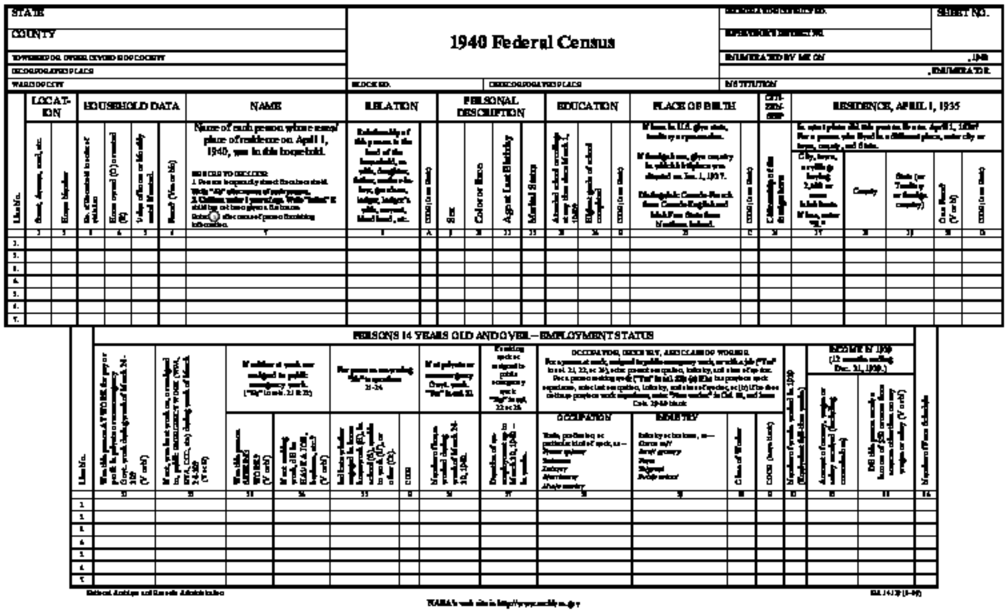
Source: War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator (United States War Production Board, 1945b)

Figure B.2: 1940 Census Enumeration Form


1940 Federal Census

STATE

COUNTY

LOCATION | HOUSEHOLD DATA | NAME | RELATION | PERSONAL DESCRIPTION | EDUCATION | PLACE OF BIRTH | RESIDENCE, APRIL 1, 1935

PERSONS 14 YEARS OLD AND OVER—EMPLOYMENT STATUS

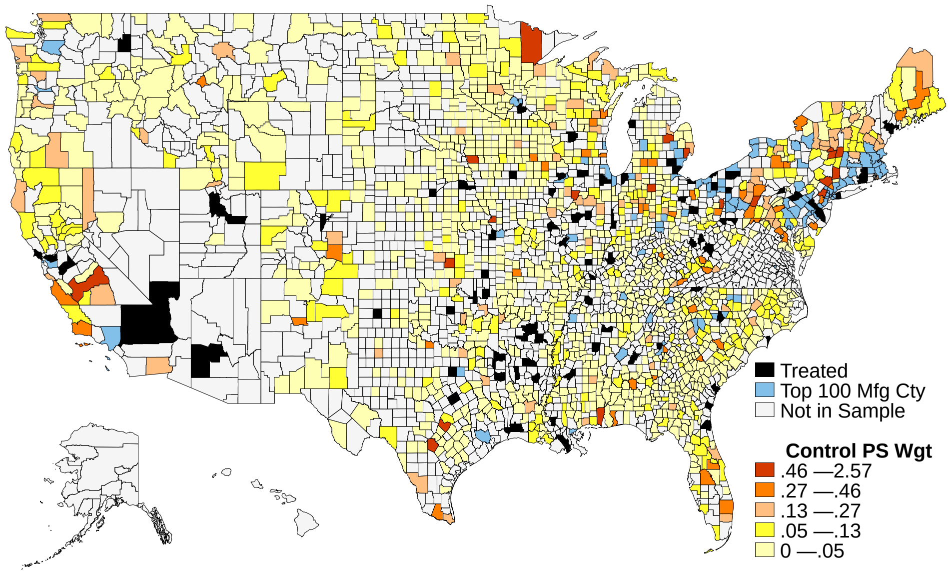
Source: National Archives https://www.archives.gov/files/research/census/1940/1940.pdf, accessed 5/15/19.

C Additional Robustness

C.1 Additional Balance Tests

Figure C.1: Control County Weighting Under Alternative Covariate Specifications

(a) Basline (Only Population and Farm Share)



(b) Including Geography and Infrastructure Controls

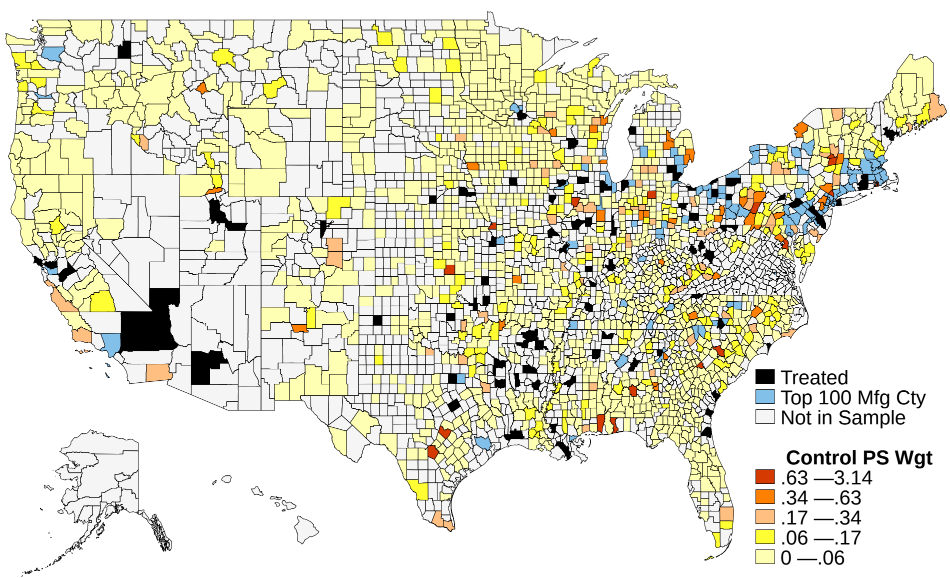
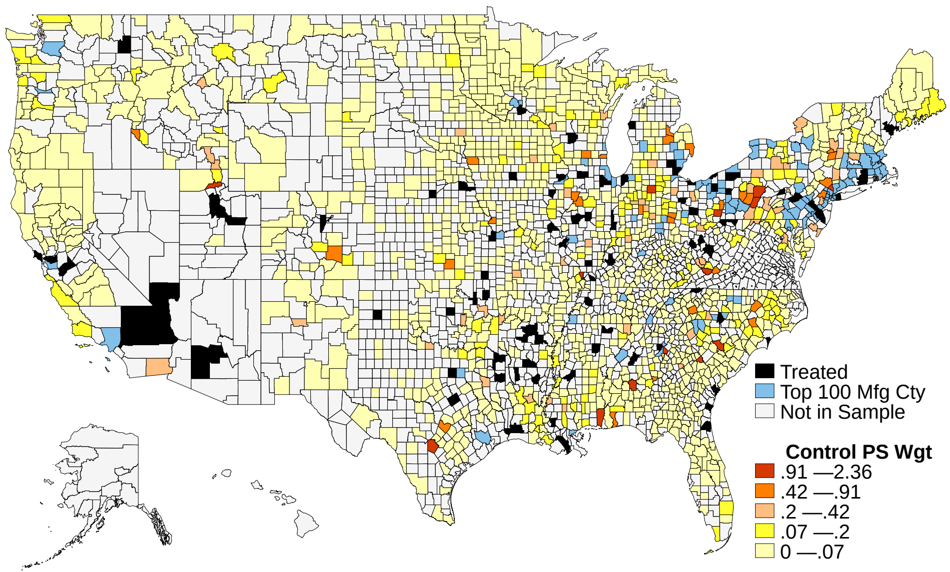


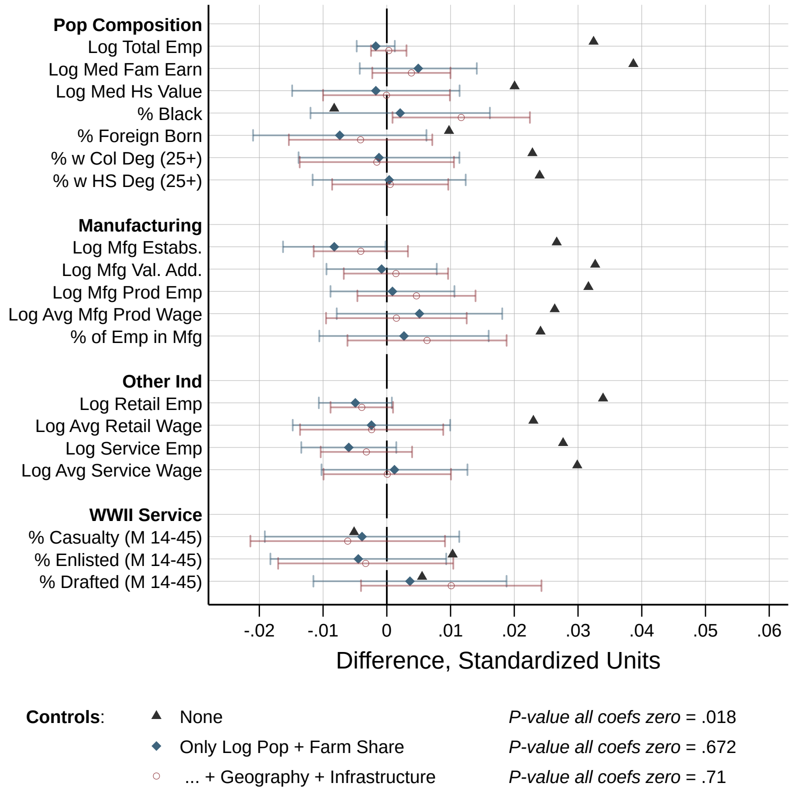
Figure C.1: Control County Weighting, Continued

(c) Full County Controls



Notes: Map displays 90 counties in primary treatment group shaded in black. Counties with top 100 1939 manufacturing employment excluded from the analysis are displayed in blue. The 1400 counties included in the comparison group are shown in other colors. In each panel, we estimate a probit model regressing treatment on the specified covariate specification (just 1940 population and farm share in Panel A, additional geographic and infrastructure controls in Panel B, and the extensive control set if Panel C) and shade comparison counties by their implied ATET propensity score weights $\hat{p}(X_i)/(1-\hat{p}(X_i))$. Counties are assigned to shades so that the counties of each shade have equal combined weight (20 percent of the total control group weight).

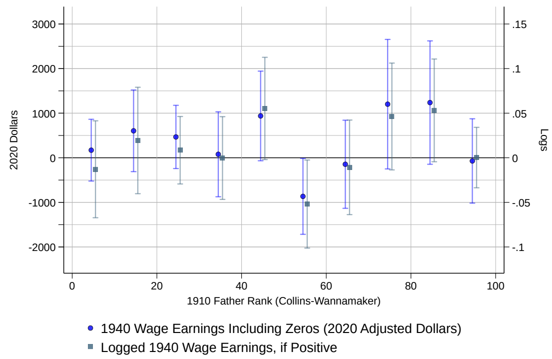
Figure C.2: County-Level Covariate Balance, Continuous Investment Intensity



Notes: Replicates Panel A of figure 2 replacing binary treatment with continuous measure of spending on new public plants (of any size) per capita. N = 1,931 counties, we include all counties except those in the top 100 counties by 1940 manufacturing employment and those dropped due our preliminary sample restrictions.

Figure C.3: 1940 Earnings of 1910 Child Residents by Father Occupation Rank. Alternative Specifications

(a) Main Treatment, Discrete Decile Bins

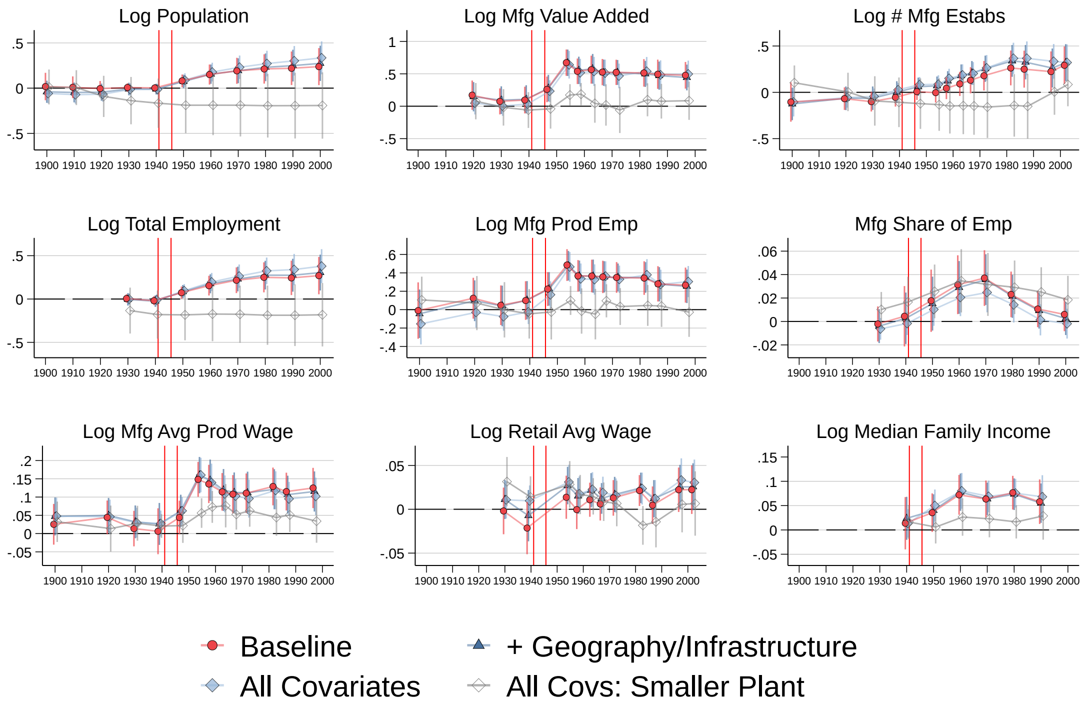


(b) Continuous Investment Intensity, Smoothed Bins

Notes: Replicates Panel B of Figure 2. The first panel splits the sample into 10 non-overlapping decile bins. The second uses the same kernel smoother as in Figure 2 replacing binary treatment with continuous measure of spending on new public plants (of any size) per capita.

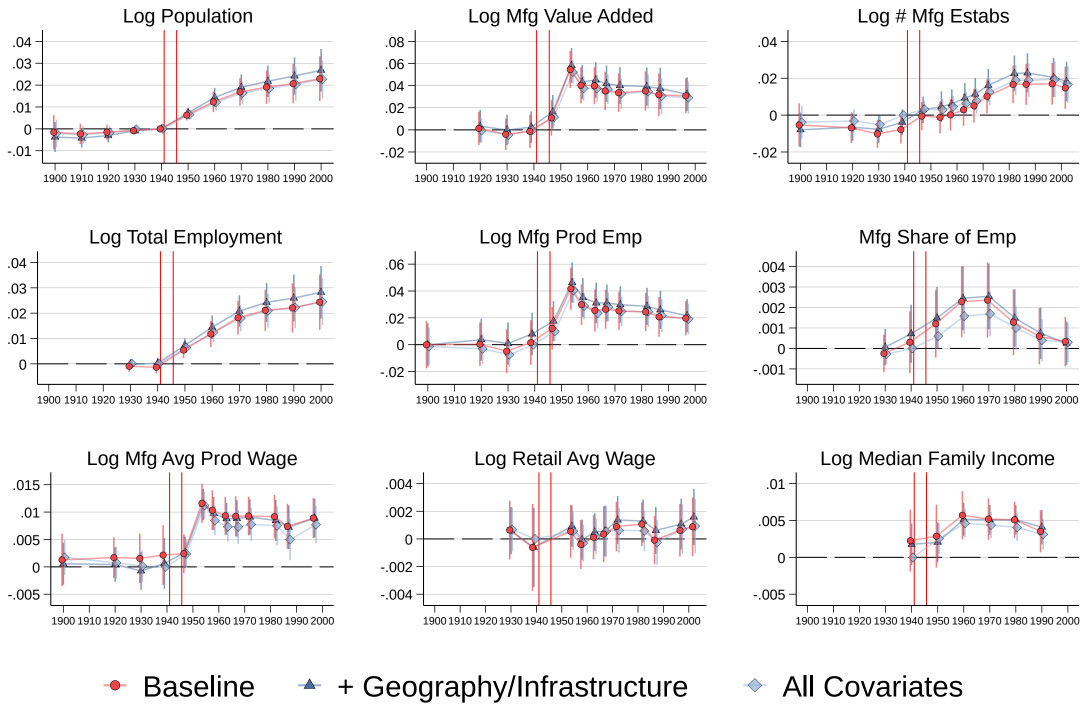
C.2 Additional County-Level Robustness

Figure C.4: County-Level Impacts, Propensity Score Re-weighting



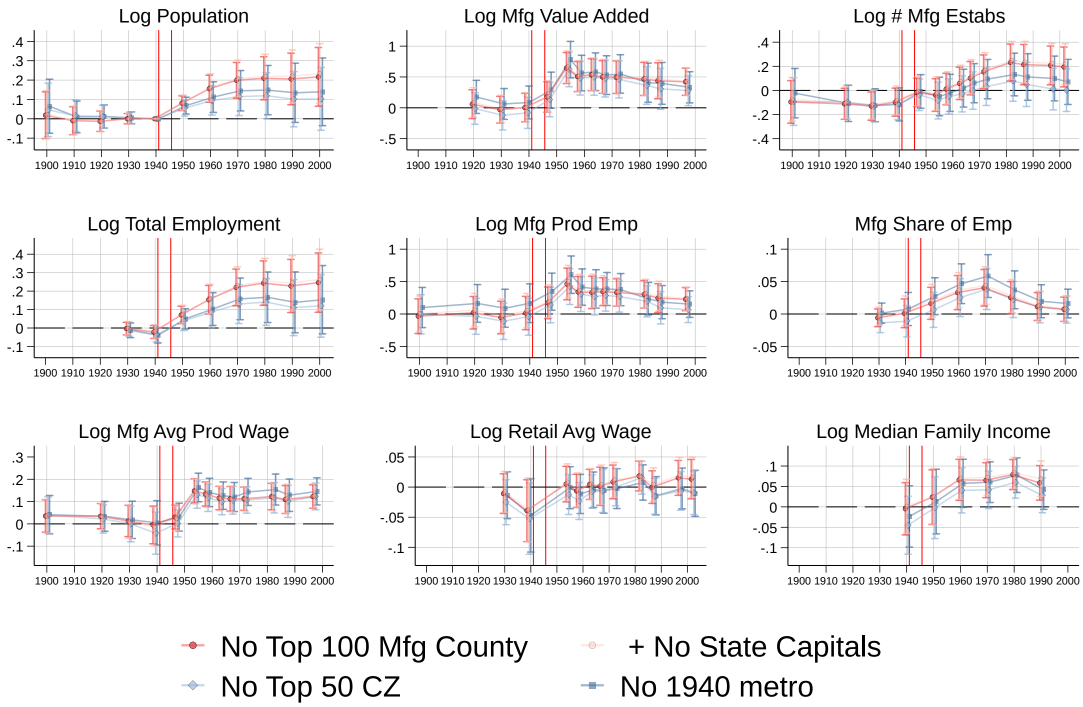
Notes: Figure replicates Figure A.1 using propensity score re-weighting estimator instead of OLS estimator. Propensity scores are estimated using a probit regression of treatment status (or the smaller plant indicator) on the specified covariates within the analysis sample described in the text using the specified covariate set. We trim the sample so that the propensity score ranges of the treatment and control group overlap and estimate the average treatment on treated counties using the following estimator: $\hat{ATET}_t = \frac{1}{N_{Treat}} \sum_{i:Treat_i=1} Y_{it} - \frac{\sum_{i:Treat_i=0} Y_{it} W_i}{\sum_{i:Treat_i=0} W_i}$. Each point estimate is obtained separately; propensity score weighted specifications are jointly estimated with the propensity score after initially trimming the overlap sample.

Figure C.5: County-Level Impacts, Per Capita Investment Intensity



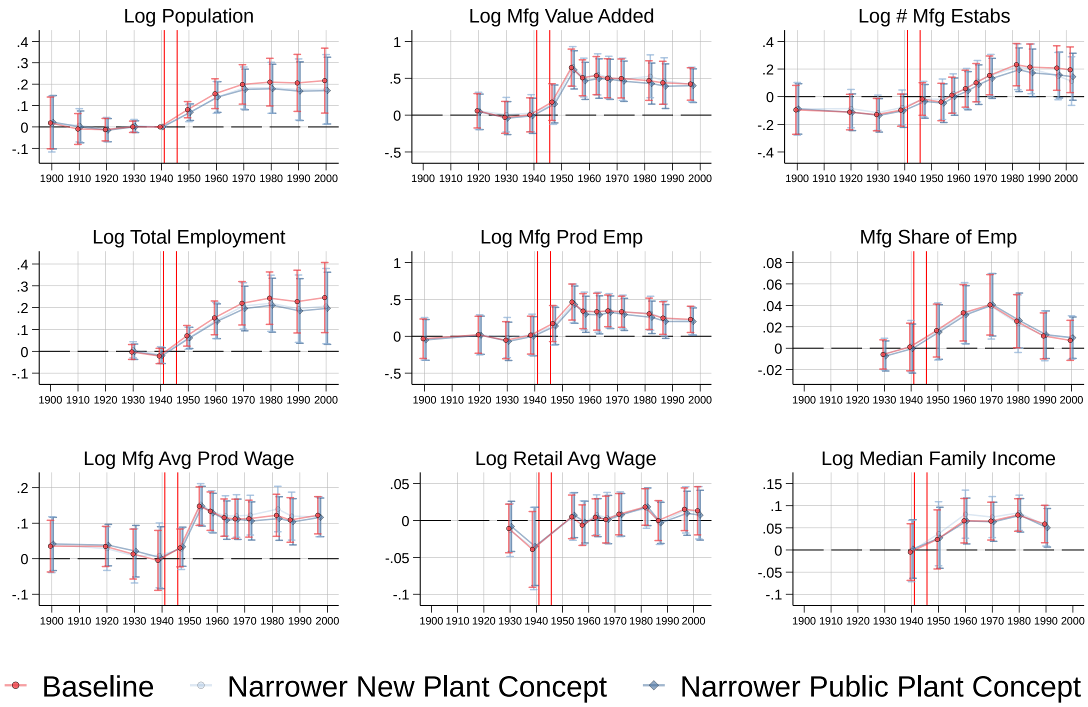
Notes: Figure replicates Figure A.1 replacing binary treatment with continuous measure of spending on new public plants (of any size) per capita.

Figure C.6: County-Level Impacts, Alternative Sample Restrictions



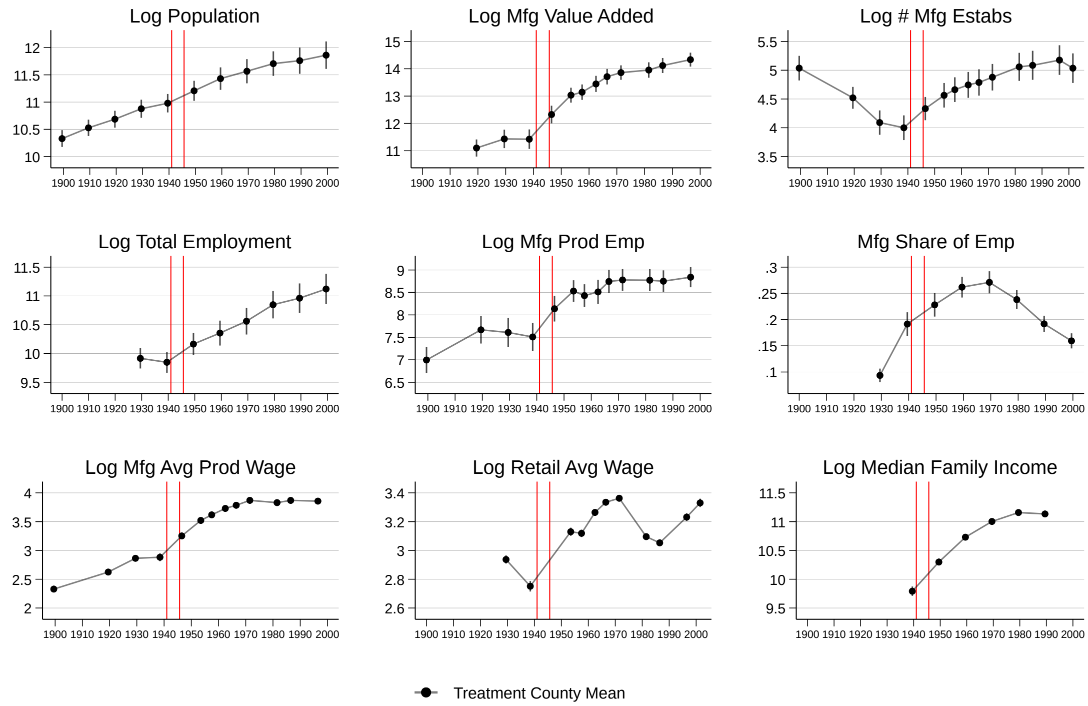
Notes: Figure replicates estimates from Figure A.1 using the baseline covariate set with only 1940 log population and farm share and applying alternative sample restrictions. We directly replicate the results from A.1 which exclude counties from the top 100 1940 manufacturing employment counties from the sample in dark red. The other series are estimates of the same specification in samples where we do one of the following instead of dropping the top 100 manufacturing counites: 1) drop the top 100 manufacturing counties as well as counties with state capitals, 2) drop all counties in the 50 commuting zones (2000 definition) with the largest 1940 combined population, or 3) drop all counties in 1940 metropolitan areas defined by the IPUMS METAREA variable (Manson et al., 2019), which applies the official 1950 definition to 1940 counties.

Figure C.7: County-Level Impacts, Alternative Treatment Constructions



Notes: Figure replicates estimates from Figure A.1 using the baseline covariate set with only 1940 log population and farm share and applying alternative treatment definitions. "Narrower New Plant Concept" specification excludes plants that are not explicitly marked as new in the data book from the construction of treatment regions and "Narrower New Plant Concept" specification excludes plants with any nonzero private investment, no matter how small, from the construction regions. Counties in the baseline treatment group that are dropped in these alternative constructions are also omitted from the control group.

Figure C.8: Treatment Group, Raw Outcome Means



Notes: Figure displays raw means of outcomes from Appendix Figure A.1 for counties in the main treatment group and are provided to aid in benchmarking effect sizes. The displayed quantities are not treatment effect estimates; rather, one can obtain counterfactual outcomes by subtracting the treatment effects in Appendix Figure A.1 from the actualized treatment group outcomes displayed here.

C.3 Additional Individual-Level Robustness

Table C.1: Effects on Men's Earnings, Per Capita Investment Intensity

	Adjusted 2020 Dollars			Logged		
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample	110.0**	67.9**	61.1**	0.0025***	0.0018***	0.0013**
	(53.3)	(32.7)	(30.7)	(0.0008)	(0.0006)	(0.0006)
N						
Linked to Parents in 1940 Census	105.3**	76.9***	69.6**	0.0023***	0.0018***	0.0012**
	(42.1)	(28.8)	(27.7)	(0.0007)	(0.0006)	(0.0006)
N						
Parents w/ 1939 Earnings < Median	117.4**	68.3**	80.9**	0.0025***	0.0015**	0.0016**
	(47.9)	(33.9)	(32.3)	(0.0009)	(0.0007)	(0.0007)
N						
Parents w/ 1939 Earnings > Median	74.3*	58.2*	51.0*	0.0011*	0.0008	0.0007
	(38.4)	(33.9)	(28.2)	(0.0006)	(0.0005)	(0.0005)
N						
Included Covariates						
Baseline County Size	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓
Extended County Covariates			✓			✓

Notes: Table estimates the same specifications as in Columns 1–4 of Table but replacing binary treatment with continuous measure of spending on new public plants (of any size) per capita. Sample includes men in NUMIDENT sample born in 1,931 counties as specified in notes to Table C.5. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table C.2: Effects on Men's Earnings, Alternative Sample Restrictions

	Counties Outside 1940 IPUMS Metropolitan Areas						Counties Outside 50 CZs (2000 Definition) with Largest 1940 Population					
	Wages, 2020 Dollars			Wages, Logs			Wages, 2020 Dollars			Wages, Logs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	2,294**	1,691***	2,015***	0.055***	0.041***	0.041***	285	1,327**	1,065**	0.013	0.027**	0.022***
	(993)	(506)	(442)	(0.015)	(0.009)	(0.008)	(1,137)	(639)	(442)	(0.017)	(0.011)	(0.008)
N	6,883,000	6,883,000	6,647,000	5,895,000	5,895,000	5,695,000	7,113,000	7,113,000	6,899,000	6,070,000	6,070,000	5,889,000
Linked to Parents in 1940 Census	2,090***	1,821***	2,035***	0.051***	0.040***	0.039***	122	1,023**	952***	0.008	0.019**	0.018**
	(715)	(437)	(395)	(0.013)	(0.009)	(0.008)	(782)	(474)	(357)	(0.014)	(0.010)	(0.008)
N	4,094,000	4,094,000	3,956,000	3,735,000	3,735,000	3,610,000	4,220,000	4,220,000	4,094,000	3,848,000	3,848,000	3,734,000
Parents w/ 1939 Earnings < Median	2,629***	2,162***	2,332***	0.061***	0.048***	0.048***	602	1,315**	1,065**	0.017	0.029***	0.024**
	(835)	(527)	(477)	(0.017)	(0.011)	(0.010)	(909)	(537)	(435)	(0.017)	(0.011)	(0.009)
N	1,556,000	1,556,000	1,502,000	1,424,000	1,424,000	1,374,000	1,582,000	1,582,000	1,533,000	1,444,000	1,444,000	1,399,000
Parents w/ 1939 Earnings > Median	1,515***	1,457***	1,504***	0.026***	0.023***	0.021***	62	275	284	-0.002	0.002	0.002
	(535)	(483)	(387)	(0.009)	(0.008)	(0.007)	(516)	(385)	(309)	(0.009)	(0.007)	(0.005)
N	850,000	850,000	832,000	801,000	801,000	785,000	960,000	960,000	943,000	906,000	906,000	890,000
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Table C.2: Effects on Men's Earnings, Alternative Sample Restrictions, Ct'd

	Counties With 1940 Population < 100,000						Baseline Restriction, Dropping Counties with State Capitals					
	Wages, 2020 Dollars			Wages, Logs			Wages, 2020 Dollars			Wages, Logs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	563	1,025*	1,508***	0.032**	0.032***	0.036***	1,565	1,105*	1,141***	0.035*	0.028**	0.024***
	(941)	(550)	(445)	(0.015)	(0.011)	(0.009)	(1,275)	(638)	(430)	(0.020)	(0.011)	(0.007)
N	5,664,000	5,664,000	5,424,000	4,848,000	4,848,000	4,644,000	7,835,000	7,835,000	7,437,000	6,720,000	6,720,000	6,379,000
Linked to Parents in 1940 Census	901	1,285***	1,658***	0.033**	0.031***	0.035***	1,326	1,248**	1,149***	0.030*	0.026**	0.021***
	(711)	(440)	(397)	(0.013)	(0.010)	(0.009)	(900)	(540)	(338)	(0.016)	(0.010)	(0.007)
N	3,370,000	3,370,000	3,229,000	3,068,000	3,068,000	2,940,000	4,703,000	4,703,000	4,458,000	4,301,000	4,301,000	4,075,000
Parents w/ 1939 Earnings < Median	1,385*	1,633***	1,862***	0.037**	0.037***	0.038***	1,479	1,111**	1,278***	0.032*	0.023**	0.026***
	(839)	(533)	(489)	(0.016)	(0.011)	(0.010)	(988)	(560)	(452)	(0.019)	(0.011)	(0.009)
N	1,280,000	1,280,000	1,225,000	1,170,000	1,170,000	1,120,000	1,752,000	1,752,000	1,668,000	1,604,000	1,604,000	1,527,000
Parents w/ 1939 Earnings > Median	771*	863**	1,190***	0.016*	0.015*	0.019**	748	654	532	0.010	0.008	0.006
	(454)	(423)	(421)	(0.008)	(0.008)	(0.008)	(553)	(434)	(327)	(0.009)	(0.007)	(0.006)
N	625,000	625,000	607,000	589,000	589,000	571,000	1,111,000	1,111,000	1,045,000	1,049,000	1,049,000	987,000
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 applying alternative sample restrictions instead of dropping top 100 1940 manufacturing employment counties. We apply the following sample restrictions: 1) drop all counties in 1940 metropolitan areas defined by the IPUMS METAREA variable (Manson et al., 2019), which applies the official 1950 definition to 1940 counties, 2) drop all counties in the 50 commuting zones (2000 definition) with the largest 1940 combined population, 3) drop all counties with 1940 populations exceedung 100,000, and 4) drop the top 100 manufacturing counties as well as counties with state capitals. Regressions are estimated on the specified sample using alternative control sets described in the main text. Outcomes are as defined in Table 1. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table C.3: Effects on Men's Earnings, Right-to-Work and Southern States

	Right-to-Work States						South Only					
	Wages, 2020 Dollars			Wages, Logs			Wages, 2020 Dollars			Wages, Logs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	2,998*	2,988***	1,956***	0.070***	0.070***	0.058***	3,897***	3,089***	1,324***	0.076***	0.063***	0.034***
	(1,722)	(902)	(553)	(0.025)	(0.016)	(0.012)	(960)	(640)	(435)	(0.015)	(0.011)	(0.009)
N	3,107,000	3,107,000	2,973,000	2,578,000	2,578,000	2,464,000	4,025,000	4,025,000	3,892,000	3,296,000	3,296,000	3,186,000
Linked to Parents in 1940 Census	3,048**	3,063***	2,602***	0.069***	0.068***	0.064***	3,288***	2,908***	1,662***	0.069***	0.060***	0.039***
	(1,253)	(668)	(518)	(0.021)	(0.015)	(0.013)	(655)	(486)	(396)	(0.012)	(0.010)	(0.009)
N	1,698,000	1,698,000	1,620,000	1,521,000	1,521,000	1,450,000	2,060,000	2,060,000	1,992,000	1,839,000	1,839,000	1,778,000
Parents w/ 1939 Earnings $<$ Median	3,690***	3,349***	2,377***	0.083***	0.075***	0.058***	3,234***	2,618***	1,503***	0.071***	0.059***	0.038***
	(1,424)	(807)	(558)	(0.027)	(0.016)	(0.013)	(707)	(542)	(453)	(0.014)	(0.012)	(0.011)
N	618,000	618,000	589,000	553,000	553,000	527,000	856,000	856,000	828,000	761,000	761,000	736,000
Parents w/ 1939 Earnings $>$ Median	1,239*	1,177**	1,752***	0.024**	0.023**	0.033***	1,837***	1,897***	983**	0.029***	0.028***	0.017**
	(682)	(520)	(415)	(0.011)	(0.010)	(0.009)	(522)	(476)	(422)	(0.009)	(0.009)	(0.008)
N	271,000	271,000	262,000	254,000	254,000	246,000	332,000	332,000	325,000	309,000	309,000	302,000
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 further restricting the analysis sample to men born in counties in states with right-to-work laws in place by 1960 or Southern states (based on Census regions) as specified. Regressions are estimated on the specified sample using alternative control sets described in the main text. Outcomes are as defined in Table 1. Standard errors clustered at the county level are displayed in parentheses. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table C.4: Effects on Men's Earnings, Alternative Treatment Constructions

	Only Include Plants Explicitly Marked as "New" in Book						**Only Include Plants with Exactly $0 Private Structure Spending**					
	Adjusted 2020 Dollars			**Logged**			**Adjusted 2020 Dollars**			**Logged**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	1,689.0	1,526.0**	1,212.0**	0.0363*	0.0334***	0.0238***	1,536.0	1,257.0*	1,131.0**	0.0326*	0.0281**	0.0216***
	(1,350.0)	(762.2)	(484.8)	(0.0206)	(0.0126)	(0.0080)	(1,292.0)	(726.8)	(484.6)	(0.0197)	(0.0121)	(0.0079)
N	8,078,000			6,919,000			8,153,000			6,988,000		
Linked to Parents in 1940 Census	1,589.0*	1,563.0***	1,242.0***	0.0314*	0.0291***	0.0203***	1,296.0	1,255.0**	1,083.0***	0.0264*	0.0235**	0.0176**
	(924.3)	(579.8)	(368.1)	(0.0163)	(0.0107)	(0.0074)	(886.3)	(556.1)	(359.5)	(0.0157)	(0.0104)	(0.0072)
N	4,837,000			4,421,000			4,887,000			4,469,000		
Parents w/ 1939 Earnings < Median	1,593.0	1,293.0**	1,300.0***	0.0335*	0.0256**	0.0259***	1,507.0	1,153.0*	1,210.0**	0.0316	0.0230*	0.0237**
	(1,035.0)	(626.0)	(473.6)	(0.0199)	(0.0123)	(0.0094)	(993.7)	(600.0)	(474.1)	(0.0193)	(0.0120)	(0.0094)
N	1,794,000			1,640,000			1,817,000			1,662,000		
Parents w/ 1939 Earnings > Median	1,054.0**	834.6*	609.5*	0.0127	0.0088	0.0055	775.2	631.4	516.2	0.0095	0.0068	0.0054
	(533.9)	(451.6)	(346.0)	(0.0090)	(0.0072)	(0.0060)	(536.8)	(436.6)	(330.2)	(0.0089)	(0.0069)	(0.0056)
N	1,165,000			1,100,000			1,180,000			1,114,000		
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 applying alternative treatment definitions. "Narrower New Plant Concept" specification excludes plants that are not explicitly marked as new in the data book from the construction of treatment regions and "Narrower New Plant Concept" specification excludes plants with any nonzero private investment, no matter how small, from the construction regions. Individuals born in counties that are in the baseline treatment group but are excluded in these alternative constructions are omitted from the control group. Regressions are estimated on the specified sample using alternative control sets described in the main text. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table C.5: Effects on Men's Earnings, Equal County Weighting

	Average Wage Earnings on 1040 Tax Return, 1969–1984											
	Earings, Adjusted 2020 Dollar				**Logged Average Earnings**				**Rank (0 to 1)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	1,124*	948**	1,106***		0.035***	0.029***	0.027***		0.009*	0.007**	0.008***	
	(675)	(415)	(333)		(0.011)	(0.008)	(0.007)		(0.005)	(0.003)	(0.002)	
N	8,246,000	8,246,000	7,848,000		7,068,000	7,068,000	6,727,000		7,772,000	7,772,000	7,397,000	
Dependent Variable Mean	49,580				10.67				0.53			
Linked to Parents in 1940 Census	1,438***	1,349***	1,376***	1,589***	0.034***	0.030***	0.027***	0.037***	0.010***	0.009***	0.010***	0.011***
	(509)	(348)	(304)	(343)	(0.010)	(0.008)	(0.007)	(0.009)	(0.004)	(0.002)	(0.002)	(0.002)
N	4,943,000	4,943,000	4,697,000	4,845,000	4,520,000	4,520,000	4,295,000	4,432,000	4,691,000	4,691,000	4,458,000	4,599,000
Dependent Variable Mean	54,510				10.71				0.57			
Parents w/ 1939 Earnings < Median	1,593***	1,451***	1,586***	1,719***	0.039***	0.034***	0.036***	0.040***	0.012***	0.010***	0.011***	0.012***
	(607)	(402)	(363)	(355)	(0.012)	(0.008)	(0.008)	(0.008)	(0.004)	(0.003)	(0.003)	(0.003)
N	1,836,000	1,836,000	1,752,000	1,824,000	1,679,000	1,679,000	1,603,000	1,669,000	1,739,000	1,739,000	1,659,000	1,727,000
Dependent Variable Mean	51,850				10.69				0.55			
Parents w/ 1939 Earnings > Median	987***	990***	1,148***	1,097***	0.020***	0.019***	0.020***	0.022***	0.007***	0.007***	0.008***	0.008***
	(346)	(351)	(330)	(324)	(0.007)	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)	(0.002)	(0.002)
N	1,202,000	1,202,000	1,136,000	1,194,000	1,135,000	1,135,000	1,072,000	1,128,000	1,146,000	1,146,000	1,083,000	1,139,000
Dependent Variable Mean	66,220				10.95				0.65			
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓			✓	✓			✓	✓	
Extended County Covariates			✓				✓				✓	
Individual + Parent Characteristics				✓				✓				✓

Notes: Table replicates Table 1 under alternative sample weights constructed so that all individuals born in a county are equally weighted by $1/N_c$, where N_c are the number of individuals in the regression sample born in county c such that the sum of weights within a county is one. See notes to Table 1 for additional details. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

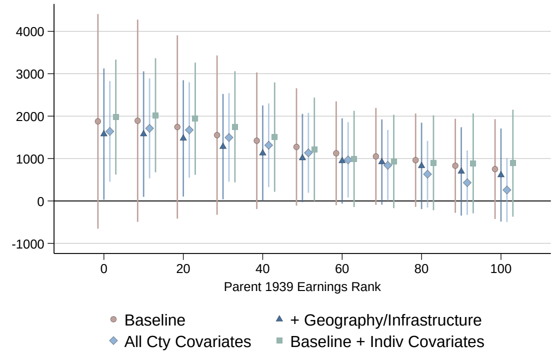
Table C.6: Effects on Men, Treatment Assigned Based on 1940 Census Location

	Average Wage Earnings on 1040 Tax Return, 1969–1984							
	2020 Dollars		Logs		Rank (0–1)		Years Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Linked to Parents in 1940 Census	1,358	1,069***	0.027*	0.017**	0.0093	0.0074***	0.196**	0.097**
	(830)	(376)	(0.015)	(0.008)	(0.0059)	(0.0026)	(0.085)	(0.047)
N	4,866,000	4,684,000	4,449,000	4,282,000	4,621,000	4,447,000	617,000	593,000
Parents w/ 1939 Earnings < Median	1,531*	1,296***	0.030*	0.024***	0.0112*	0.0094***	0.211**	0.116
	(918)	(473)	(0.018)	(0.009)	(0.0068)	(0.0035)	(0.091)	(0.072)
N	1,807,000	1,744,000	1,653,000	1,596,000	1,712,000	1,653,000	219,000	211,000
Parents w/ 1939 Earnings > Median	674	412	0.006	0.002	0.0041	0.0026	0.164**	0.041
	(491)	(321)	(0.008)	(0.005)	(0.0033)	(0.0021)	(0.082)	(0.043)
N	1,156,000	1,117,000	1,092,000	1,055,000	1,104,000	1,067,000	141,000	136,000
Parents w/ No 1939 Wage Earnings	1,334*	1,193***	0.041***	0.028**	0.0092*	0.0083***	0.160*	0.129**
	(777)	(432)	(0.015)	(0.011)	(0.0055)	(0.0031)	(0.092)	(0.059)
N	1,903,000	1,822,000	1,704,000	1,631,000	1,805,000	1,728,000	257,000	246,000
Included Covariates								
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓		✓		✓		✓
Extended County Covariates		✓		✓		✓		✓

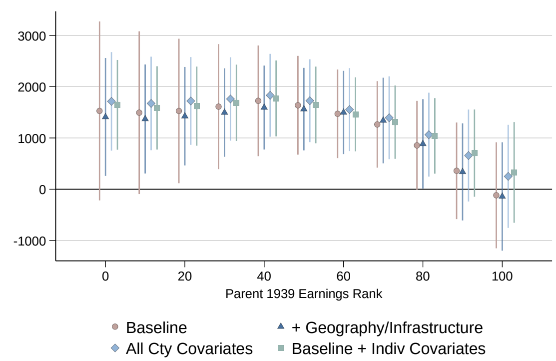
Notes: Table displays OLS estimates of a modified version of equation 2 in which treatment is assigned based on individual's county in the 1940 Census instead of their birth county. Sample only includes individuals matched to the 1940 census. Regressions are estimated on the specified sample using alternative control sets described in the main text. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Figure C.9: Effects on 1969–1984 Male Average AGI Reported on 1040 Returns

(a) Individual-Level Regression Estimates



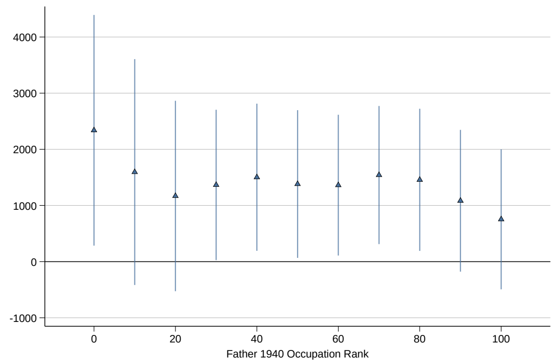
(b) Equalized County Weighting



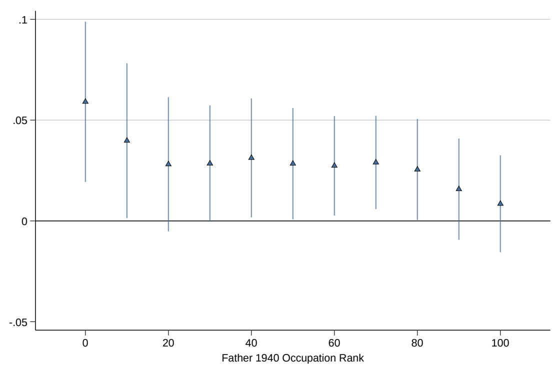
Notes: Figure replicates Figure 6 using outcomes based on Form 1040 adjusted gross income instead of Form 1040 wages.

Figure C.10: Effects on Wage Earnings, by Father Occupation Rank

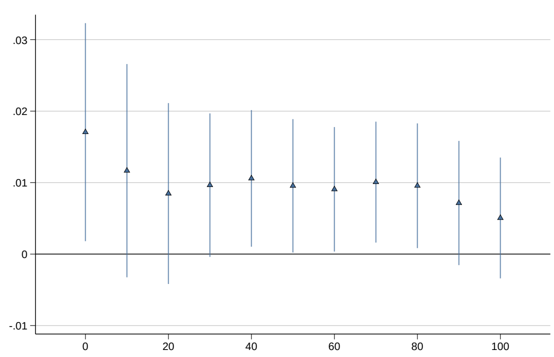
(a) 2020 Dollars



(b) Logs



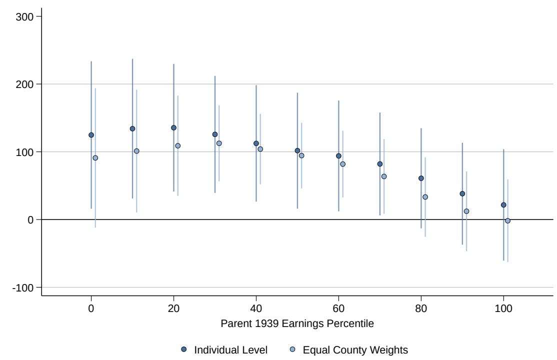
(c) Rank (0-1)



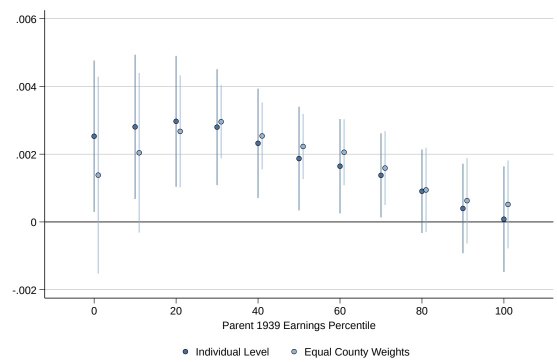
Notes: Figure replicates estimates from Figure 6 Panel A under the baseline specification with only 1940 log population and farm share controls, but with parent ranks assigned based on fathers' 1940 occupation score following Collins and Wanamaker (2022) as in Panel B of 2.

Figure C.11: Effects on Wage Earnings, Per Capita Investment Intensity

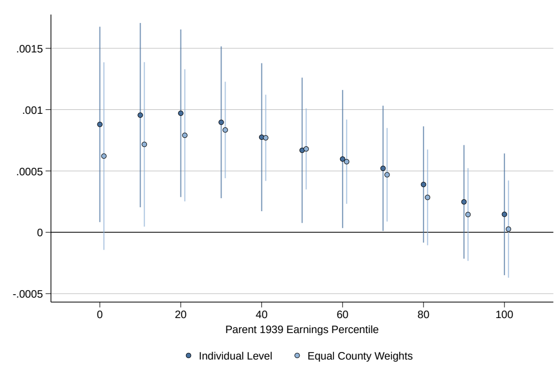
(a) 2020 Dollars



(b) Logs



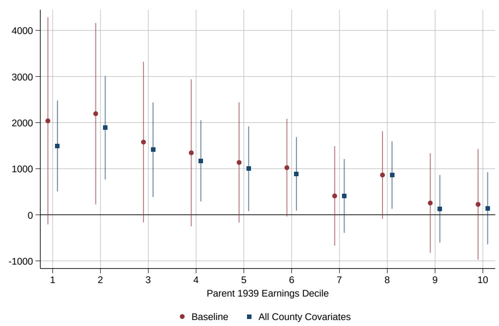
(c) Rank



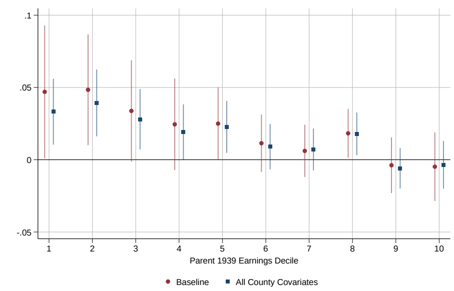
Notes: Figure replicates estimates from Figure 6 under the baseline covariate specification with only 1940 log population and farm share controls, but replacing binary treatment with continuous measure of spending on new public plants (of any size) per capita. Sample includes men born in 1,931 counties as specified in notes to Table C.5. "Individual-Level" and "Equal County Weights" estimates use the weighting schemes from Panels A and B of Figure 6, respectively.

Figure C.12: Effects by Parent Earnings, Discrete Decile Bins

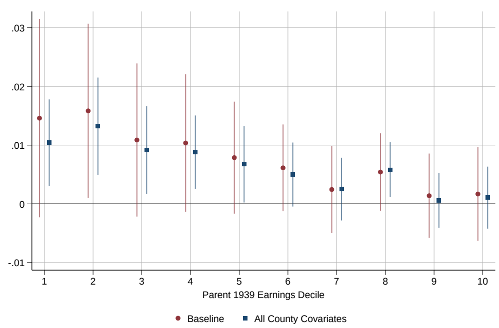
(a) 69–84 Wages, 2020 Dollars



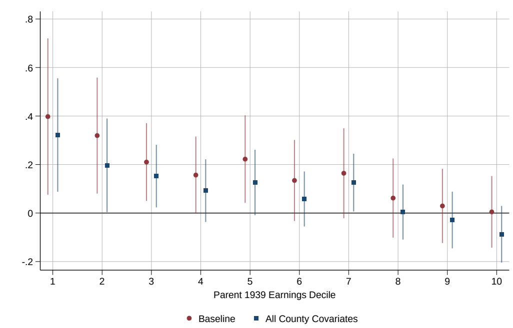
(b) 69–84 Wages, Logs



(c) 69–84 Wages, Rank



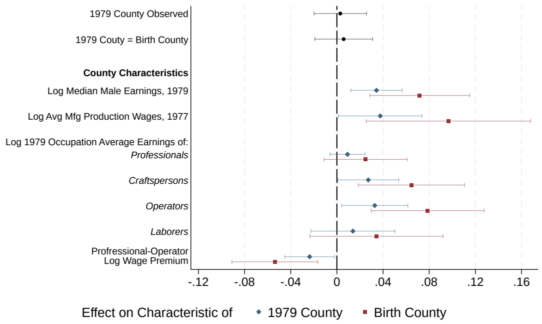
(d) Years of Schooling



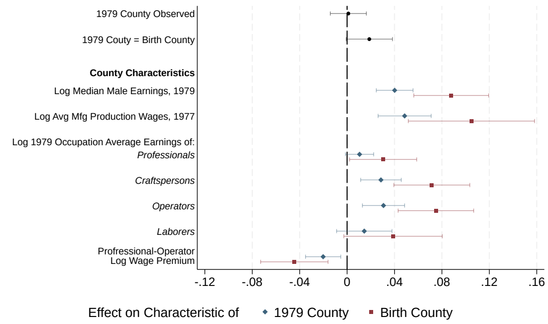
Notes: Figure replicates estimates using specified covariate sets from Figure 6, but splitting sample into parent earnings decile bins with no overlapping observations across bins instead of using kernel weights to smooth across parent ranks.

Figure C.13: Effects on Adult Location, Alternative Weightings/Samples

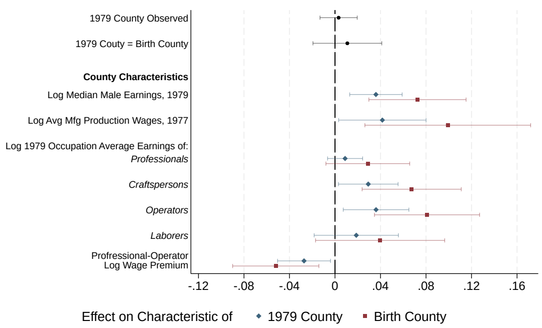
(a) All Men



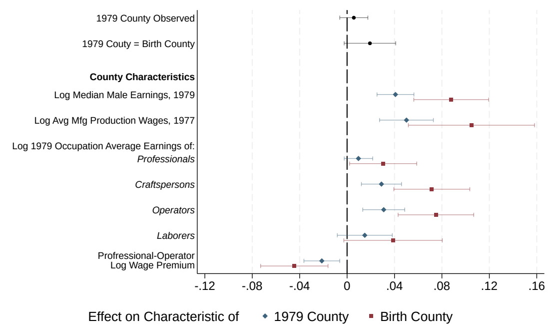
(b) ... Equal County Weights



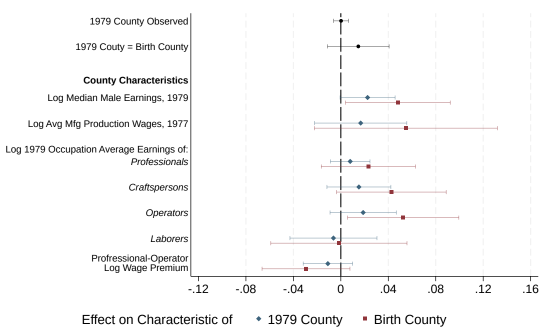
(c) Men, Parent Earnings < Median



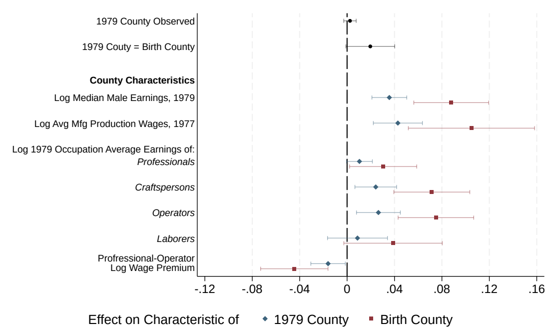
(d) ... Equal County Weights



(e) Men, Parent Earnings > Median



(f) ... Equal County Weights



Notes: Panel A replicates analysis in Figure 8. Panel B replicates analysis in Figure 8 under alternative sample weights constructed such that all individuals born in a county are equally weighted by $1/N_c$, where N_c are the number of individuals in the regression sample born in county c so that the sum of weights within a county is one. Panels C and E report results for the subset of men matched to parents in the 1940 Census with non-missing earnings below or above the national median, respectively, and Panels D and F display corresponding estimates re-weighting observations so that the total weight on each birth county equals one. In Panels B, D, and E, the birth county effects are the same as the effects in Figure 5 by construction.

Table C.7: Effects on 1978–1987 W-2 Earnings, Sensitivity to Conditioning on Employment

	W-2 Wages at Main Job, 2020 Dollars								
	All Observations Incl. 0s			FTMW Obs Only			FTMW + Ind Obs Only		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full Sample	2,898***	2,774***	2,053***	2,776***	2,617***	2,082***	3,058***	3,055***	2,854***
	(903)	(823)	(735)	(964)	(888)	(753)	(1,092)	(1,023)	(926)
N		313,000			213,000			125,000	
Dependent Variable Mean		44,540			64,560			67,170	
Linked to Parents in 1940 Census	2,758***	2,709***	2,107***	2,356***	2,169***	1,758**	2,747***	2,811***	2,883***
	(863)	(811)	(787)	(883)	(835)	(803)	(1,044)	(1,007)	(997)
N		203,000			138,000			81,500	
Dependent Variable Mean		45,170			65,610			68,240	
Parents w/ 1939 Earnings < Median	4,251***	3,670***	3,245**	3,604***	3,410***	3,045**	3,667**	3,528**	3,394**
	(1,225)	(1,243)	(1,270)	(1,219)	(1,165)	(1,200)	(1,430)	(1,381)	(1,454)
N		75,000			53,000			31,500	
Dependent Variable Mean		44,520			61,970			64,390	
Parents w/ 1939 Earnings > Median	1,720	1,887	1,592	703	229	-208	1,614	1,415	1,290
	(1,420)	(1,351)	(1,343)	(1,336)	(1,363)	(1,329)	(1,560)	(1,546)	(1,524)
N		47,000			35,000			20,500	
Dependent Variable Mean		55,440			73,920			76,440	
Included Covariates									
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓
CPS ASEC Race Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Table reports OLS estimates of the pooled-year specification in Equation 3, estimated on the panel of male 1978–1987 W-2 earnings observed in the SSA-CPS, first under the restriction to full-time minimum wage main jobs with non-missing industry identifiers applied in Table 4 and then under weaker restrictions. See notes to Table 4 for specification and sample details. Standard errors clustered at the county level are displayed in parenthesis. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .

Table C.8: Effects on 1978–1987 Employment Status, Men in SSA-CPS Data

	Has FTMW Job			Has FTMW Job w/ Ind Info		
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample	0.014*	0.013*	0.008	0.031***	0.028***	0.027***
	(0.008)	(0.008)	(0.008)	(0.012)	(0.011)	(0.010)
N		313,000			313,000	
Dependent Variable Mean		0.68			0.40	
Linked to Parents in 1940 Census	0.016*	0.017**	0.013	0.026*	0.025**	0.024**
	(0.009)	(0.009)	(0.009)	(0.013)	(0.012)	(0.011)
N		203,000			203,000	
Dependent Variable Mean		0.68			0.40	
Parents w/ 1939 Earnings $<$ Median	0.027**	0.020	0.018	0.056***	0.053***	0.050***
	(0.013)	(0.014)	(0.014)	(0.018)	(0.018)	(0.018)
N		75,000			75,000	
Dependent Variable Mean		0.71			0.42	
Parents w/ 1939 Earnings $>$ Median	0.016	0.023*	0.024*	-0.007	-0.005	0.005
	(0.014)	(0.013)	(0.014)	(0.019)	(0.018)	(0.018)
N		47,000			47,000	
Dependent Variable Mean		0.74			0.44	
Included Covariates						
Baseline County Size	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓
Extended County Covariates			✓			✓
CPS ASEC Race Controls	✓	✓	✓	✓	✓	✓

Notes: Table reports OLS estimates of the pooled-year specification in Equation 3, estimated on the panel of 1978–1987 W-2 earnings observed in the SSA-CPS data. Outcomes are indicators for full-time minimum wage employment in each year with and without a further condition requiring non-missing industry codes from the LBD; outcomes are definied in all years. See notes to Table 4 for specification and sample details. Standard errors clustered at the county level are displayed in parenthesis. *** indicates p $<$ .01, ** indicates p $<$ .05, * indicates p $<$ .10 .

D Historical Appendix

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

D.1 Oversight of Plant Construction During WWII

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

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

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

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

> The Plant Site Board cooperated with similar boards set up by the War and Navy Departments in the review of locations for defense plants, and did not hesitate to withhold its approval where sites were deemed unsatisfactory. The Plant Site Board had its own research staff, which analyzed all proposals in the light of availability of labor, transportation, housing, power, raw materials, supply and destination of product, and other relevant factors. So far as possible,

> an attempt was made to locate plants away from highly industrialized areas. Other agencies of the Government, such as the Federal Power Commission, the Coordinator of Defense Housing, the Bureau of Labor Statistics, and the National Resources Planning Board were consulted for factual information, as were the various divisions and branches of OPM. ...
>
> The Plant Site Board actually exercised a species of over-all planning function, although it was done in negative terms. The Board could not initiate anything, but by rejecting proposals offered, asking reexamination, and recommending specific changes, it did exercise a guiding influence in plant location, which prevented many bottlenecks and much undue concentration of industry. (Fesler, 1947)

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

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

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

D.2 Plant Siting Considerations of Coordinating Bodies

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

> Insofar as it was consistent with the primary objective of expediting the national defense program and with due regard to appropriate military factors, the Committee was to be guided in approving plant site locations by a policy of wide geographic decentralization of defense industries and full employment of all available labor. In other words, the Plant Site Committee was to review all facilities

> projects financed by the Government with two objectives in mind: (1) No new facilities should be created as long as alternate capable facilities were available; (2) no facilities should be located in inappropriate spots relative to the supply of labor, power, utilities or housing. (Fesler, 1947)

The push for dispersion arose from concerns about supply chain security. If production facilities were excessively concentrated, localized attacks or blackouts could severely disrupt the war effort. A December 1941 letter by Major T.A. Sims, Assistant Technical Executive and later Deputy Chief of Staff in the Army Air Force Material Command (which oversaw aircraft procurement), suggested that aircraft producers that had factories along the coasts should construct new modification centers in the interior to ensure continuous operations:

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

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

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

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

> *to avoid, if possible, the building of plants in already highly industrialized and congested areas.* (McGrane, 1946)

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

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

> The OPM was deluged with requests from Congressmen and Senators from various parts of the country suggesting the location of defense plants in their respective Districts and States. Such re quests were received from members of Congress from Wisconsin, Arkansas, Louisiana, Montana, Kansas, Indiana and Connecticut. Senator Arthur Capper of Kansas stressed the importance of locating plants in the Middle West. He asserted the Middle West possessed the following advantages for national defense: (1) The greatest safety from foreign invasion and sabotage; (2) a large number of vacant housing facilities; (3) many idle schools, churches, stores, public utilities; (h) excellent transportation facilities; (5) abundant fuel; (6) low living costs; (7) good native American labor; and (8) a great supply of easily accessible raw materials. Residents of Kansas and Nebraska complained that their region did not receive its share of defense plants; yet, as a matter of fact, the Government spent large sums in the expansion of aircraft assembly plants at Wichita, Kansas City, and Omaha. Likewise, representatives from the South protested that the OPM had established a policy that no defense industries should be located within a 200 mile zone of the coast line of the Gulf of Mexico. There was no such fixed policy, for the Government financed the expansion of shipbuilding, ship repair, and magnesium facilities in Louisiana and Texas along the coasts. (McGrane, 1946)

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

> [Senators] Stefan, Truman, and many of their peers remained dissatisfied and critical of the distribution of war work because their own influence was limited. The location of new plants was influenced less by the pull of congressmen and governors than by the calculations of military and civilian officials in the executive branch. Those officials often did favor the South and West because they endorsed a policy of decentralization, for strategic as well as political reasons. However,

> even this spreading of the work failed to placate many congressmen because, in most cases, it was the military and its contractors who selected sites using calculations of available transport, power, water, and local labor supply. Internal Navy correspondence from early 1941 shows that the Navy believed that it, and not Congress or even civilian mobilization officials, controlled the choice of plant sites. Under these conditions, even the most powerful congressmen might be stymied. (Wilson, 2016)

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

D.3 Plant Siting in Practice

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

> The War Department had decided that new defense plants should be built in the interior of the country at least 200 miles from the borders, and the Air Corps selected Omaha and Tulsa as the sites for the two new plants. But the hard facts of the nation's economic structure made the policy difficult to follow. The greater part of American industry was concentrated along the Atlantic and Pacific coasts or in the Great Lakes region. Manufacturers in general resisted proposals for a transfer of their operations to areas remotely situated from established centers of labor and technical skills, and not without reason. As Knudsen once explained to General Marshall: "We can't move Detroit." *The industrialists' reluctance to invest in dispersed plant facilities was at odds with the government's hope that private capital could finance new inland construction; hence, the War Department could carry out its policy only to the extent that the government was willing to put up the money.* (Craven and Cate, 1955; emphasis added)

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

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

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

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

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

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

Outside of major manufacturing hubs, siting decisions were driven by fairly idiosyncratic factors so long as locations were deemed to have sufficient access to labor, housing, transportation, and power. As an example, consider the Geneva steel mill built in Utah (near Provo), which opened in 1942 and was the largest steel plant ever built west of the Rockies. Whetten (2011) notes that while private financiers had seen little prospect in such a large steel plant in Utah, the federal government stepped in for reasons of short-run necessity:

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

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

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

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

Connections of local officials to key military officials became particularly valuable during the rapid industrial mobilization for WWII, given the considerable influence procurement officers had over contract assignment. For example, when General Lucius Clay, the son of senator Alexander Stephens Clay of Marietta, Georgia, became the director of materiel for the U.S. War Department with considerable sway over procurement decisions, local officials from Marietta were able to successfully win Clay's backing for a war plant in his home town. The resulting bomber plant built by Bell Aircraft remains in operation today as Lockheed Martin aircraft plant that is government owned and contractor operated. (Scott, 2020)

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

> When the frost was out of the ground in the spring of 1938 , 65 boys pitched a row of tents on a 320 - acre tract of land near Dearborn, Michigan, and went to work . That was the origin of Camp Legion. "All of the boys had been unemployed. Many were sons of dead or disabled war veterans . Some were homeless. Most of them were undernourished. [...] The experiment was so successful that, the following year, Mr. Ford arranged for a second camp, just like the first, near Ypsilanti, Michigan. In 1939, Camp Legion and Camp Willow Run wrought the same change in 130 more boys. One boy gained 21 pounds. Together , they gained 1140 pounds more than half a ton of hard flesh and muscle. And again they earned more than their keep. (Company., 1940)

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

> Beset by Henry Morganthau's treasury department sleuths investigating fmc ties to Ford of France and Germany- was fmc cooperating willfully with the Nazis?- and by the Truman committee, and the FBI (what were Henry Ford's and test pilot- consultant Charles Lindbergh's loyalties?), Edsel persevered to turn Henry's farm camp for disadvantaged boys into the largest aircraft factory in the world— Willow Run. To please father, the plant was configured to stay within Washtenaw county which had voted Republican in 1940. ...
>
> Like other war factories built in rural areas, Willow Run had no housing and workers could not commute to work from Detroit. Perhaps because of the importance of the b-24, the government agreed to release materials to build housing—"Bomber City." Needing ten thousand workers, fmc turned to recruiting and training southern whites and blacks—a hypergolic racial mix. They hired a very large number of women, again against social norms. (Baime, 2014)

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

E Results by Plant Type and Implications for Persistence

To assess whether continued post-WWII military spending drives the persistence in our main results, we separately examine the effects of the construction of ordnance and ammunition plants and the effects of other general manufacturing plants. While factories that produced planes, tanks, steel, rubber, and machinery during the war could easily be repurposed for civilian production after the war, the ordnance works and ammunition plants built during WWII were highly specialized facilities that could not practically be converted to civilian use. As a result, most of this latter group of facilities remained under government ownership after the end of the war and continued to supply the military as GOCO plants throughout the Cold War. In contrast, there was intense political opposition to postwar government ownership of plants that could potentially compete with civilian private industry and, as a result, most general manufacturing facilities were sold to the private sector in the years following the end of the war(Craven and Cate, 1955). Accordingly, this latter group of plants is less likely to have supported by military spending after WWII. Using descriptions in the WPB data, we classify the largest new public plant in each of the main treatment counties as either an ordnance/ammunition plant or a general manufacturing plant and estimate Equation 1 using modified treatment that only includes one type of plant or the other in the treatment definition.[71] We then estimate effects on adult earnings using these same sets of counties following Equation 2.

We directly test whether either kind of plant siting led to increased Cold War defense contract spending in the top row of Appendix Figure E.1. To measure county-level defense spending during the Cold War, we use information from all DD350 forms in the Military Prime Contract File databases from 1966 to 1975, covering the peak of the Vietnam War. These forms report the value of each military contract during that period as well as the name and location of the prime contractor, which we aggregate to the county level.[72] There are clear differences in the effects of each of the two types of plants on postwar defense spending displayed in Appendix Figure E.1. The construction of an ordnance facility in a county during WWII resulted in a roughly 125 log-point increase in defense spending and a 80 log-point increase in the ratio of defense spending to all production output. In

[71] The counties in the estimation sample are the same as in Figure 3 except that when studying the effects of one type of plant we drop treatment counties with the other type of plant from the sample entirely. We include the U.S. Government Aircraft Modification Centers and Assembly Plants in the category of ordnance and ammunition plants, which were typically built with large airfields and converted to Air Force Bases after the war. There are 48 treatment plants coded as ordnance facilities and 42 coded as general manufacturing plants, and 1400 comparison counties in all specification.

[72] See Nakamura and Steinsson (2014) for a detailed discussion of the DD350 data.

contrast, the construction of a general manufacturing facility in a county during WWII had no robust effect on the level of postwar defense spending, and what increase is observed is what would be expected by as a result of random allocation of contracts to private sector firms given the growth of the local manufaturing sector—we find *no* effect on the ratio of defense spending to manufacturing production. These results are consistent with the narrative that ordnance plants largely continued to produce for the military after WWII while other general manufacturing plants by and large converted to civil production.

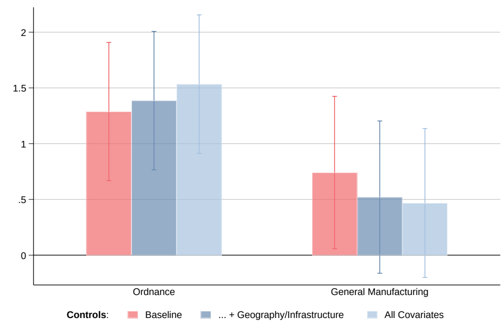
Nonetheless, the estimates in Appendix Figure E.2 show that *both* kinds of plants had persistent effects on manufacturing employment and wages of comparable magnitudes, even though postwar defense spending was only a major driver of manufacturing demand in counties where ordnance plants were built. Likewise, both types of plants had similar impacts on the broader wage and earnings structure in affected counties. In turn, we find that both types of plants had similar effects on the long-run earnings of children born in the same county before WWII. The results we find for the general ordnance plants are unlikely to have been directly driven by continued military spending.

To understand the drivers of persistence Appendix Figure E.1 examines effects on postwar infrastructure development and private investment. In the second row Appendix Figure E.1, we find that the construction of a general manufacturing plant in a county made it more likely to be connected to the interstate highway system in subsequent decades. The increased population and production activity resulting from plant construction may have increased the priority of these regions in the design of the interstate highways, which could have re-inforced a larger region size in the long run. At a more basic level, sunk investment of building a plant—not just the structure, but the preparation if of a suitable parcel– made it more attractive to continue to re-invest in those facilities rather than build new factories elsewhere. The results in the third row of Appendix Figure E.1 show that both types of plant sitings resulted in elevated private capital expenditures in subsequent decades.

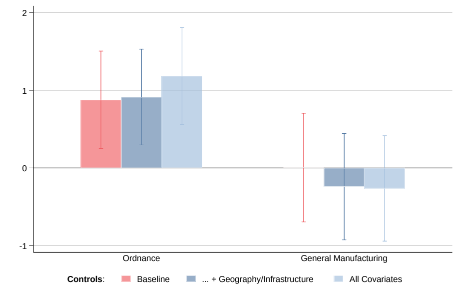
Figure E.1: Effects on Mid-cenutry Defense Spending and Investment

Vietnam War Defense Spending, DD-350 Data

(a) Log Spending

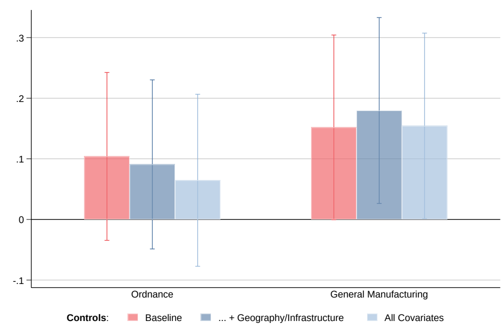


(b) Log (Spending/ Mfg VA)

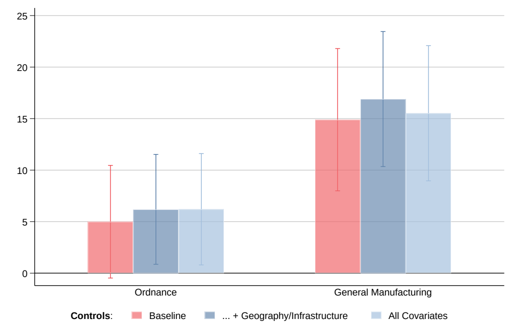


Interstate Highway Construction by 2000

(c) Any Connection

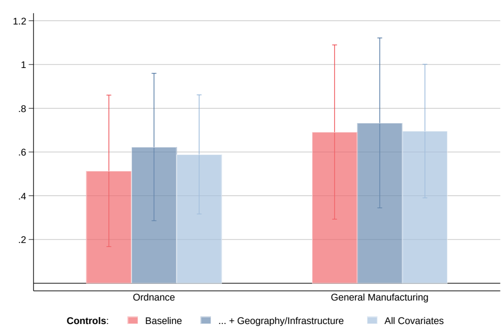


(d) Miles

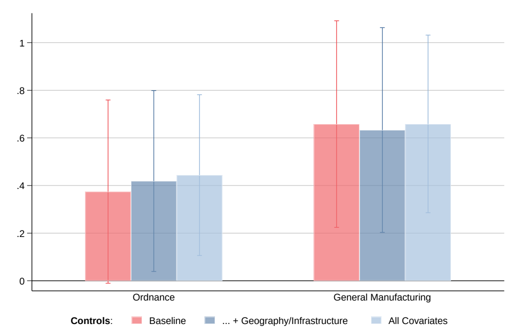


Private Manufacturing Capital Expenditures

(e) Log, 1963

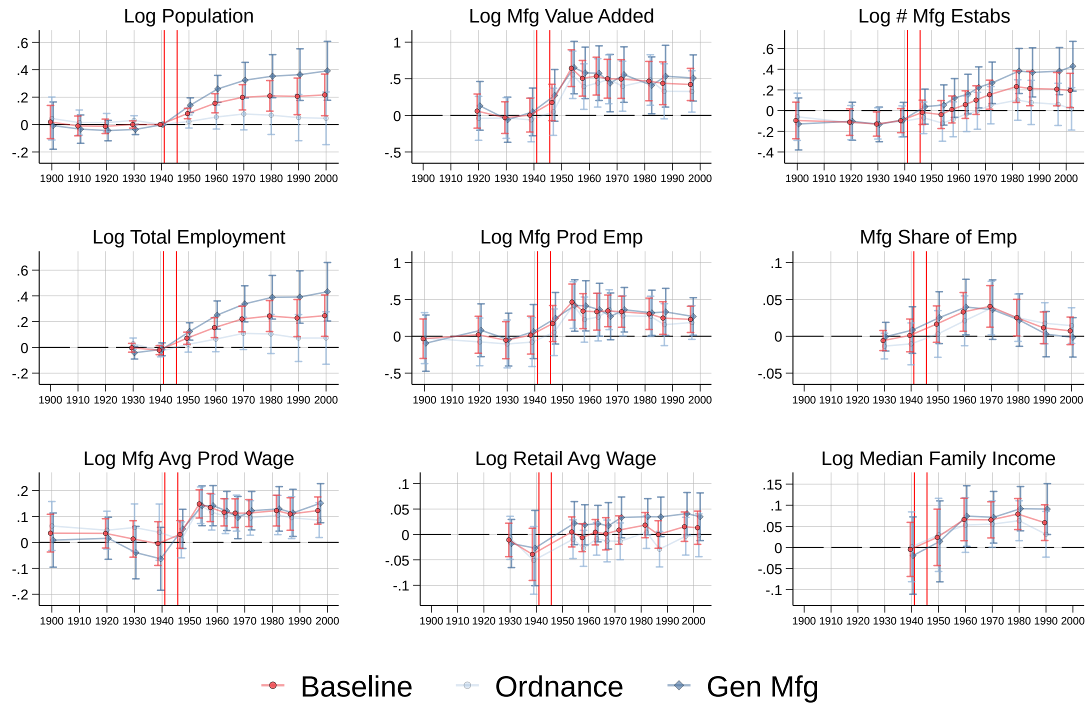


(f) Log, 1982



Notes: Figure displays OLS estimates of Equation 1 under alternative covariate specifications described in the main text including only treatment counties with ordnance plants or treatment counties with other general manufacturing plants in the treatment definition. Each estimate and the associated 95% confidence interval is from a separate regression of the specified outcome on the treatment indicator. When studying ordnance plants, there are 48 treatment counties and the same 1400 control counties in Figure 3, and when studying other general manufacturing plants there are 42 treatment plants and the same 1400 control counties. "Vietnam War Defense Spending" is tabulated at the county level using all contracts reported on DD350 forms in the Military Prime Contract File databases from 1966 to 1975, described in Nakamura and Steinsson (2014)." Interstate highway measures constructed by authors using GIS files. County-level manufacturing capital expenditures outcomes are tabulations from the Census of Manufactures.

Figure E.2: County-Level Impacts, By Plant Type



Notes: Figure replicates estimates from Figure A.1 using the baseline covariate set with only 1940 log population and farm share keeping either only 48 treatment counties with ordnance plants or 42 counties with other general manufacturing plants in treatment group. The comparison group is the same 1400 counties as in A.1 across all specifications. Each estimate and the associated 95% confidence interval is from a separate regression of the specified outcome on the treatment indicator.

Table E.1: Effects on Men's Earnings, By Plant Type

	Non-ordinance						**Ordinance**					
	Adjusted 2020 Dollars			**Logged**			**Adjusted 2020 Dollars**			**Logged**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full Sample	1,883.0	1,507.0*	1,153.0*	0.0399	0.0378**	0.0227**	1,448.0	1,566.0*	1,337.0**	0.0269	0.0262*	0.0235**
	(1,833.0)	(875.2)	(600.7)	(0.0284)	(0.0153)	(0.0108)	(1,232.0)	(867.3)	(616.7)	(0.0184)	(0.0139)	(0.0101)
N	7,709,000			6,606,000			7,563,000			6,476,000		
Linked to Parents in 1940 Census	1,565.0	1,635.0**	1,141.0**	0.0335	0.0358**	0.0208**	1,093.0	1,209.0**	1,169.0**	0.0181	0.0162	0.0170*
	(1,303.0)	(759.1)	(476.7)	(0.0229)	(0.0140)	(0.0105)	(829.7)	(606.1)	(460.2)	(0.0147)	(0.0115)	(0.0091)
N	4,619,000			4,222,000			4,519,000			4,126,000		
Parents w/ 1939 Earnings < Median	1,456.0	1,244.0*	1,200.0**	0.0329	0.0291*	0.0270**	1,619.0*	1,398.0**	1,437.0**	0.0296	0.0224*	0.0248**
	(1,457.0)	(756.1)	(587.3)	(0.0278)	(0.0150)	(0.0119)	(925.8)	(673.1)	(590.2)	(0.0182)	(0.0134)	(0.0119)
N	1,720,000			1,573,000			1,693,000			1,548,000		
Parents w/ 1939 Earnings > Median	737.6	754.1	549.0	0.0083	0.0100	0.0064	769.8	537.5	530.5	0.0060	0.0017	0.0044
	(773.4)	(587.6)	(431.6)	(0.0127)	(0.0091)	(0.0075)	(485.7)	(476.1)	(440.9)	(0.0086)	(0.0082)	(0.0074)
N	1,091,000			1,031,000			1,029,000			970,000		
Included Covariates												
Baseline County Size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Geography/Infrastructure		✓	✓		✓	✓		✓	✓		✓	✓
Extended County Covariates			✓			✓			✓			✓

Notes: Table displays OLS estimates of a modified version of equation 2 including only treatment counties with ordnance plants or treatment counties with other general manufacturing plants in the treatment definition. When studying ordnance plants, there are 48 treatment counties and the same 1400 control counties in Table 1, and when studying other general manufacturing plants there are 42 treatment plants and the same 1400 control counties. Individuals born in counties that are in the baseline treatment group but are excluded in these alternative constructions are omitted from the control group. Regressions are estimated on the specified sample using alternative control sets described in the main text. Standard errors clustered at the county level are displayed in parentheses. *** indicates p < .01, ** indicates p < .05, * indicates p < .10 .