Public Investment and the Spread of "Good-Paying" Manufacturing Jobs: Evidence from World War II’s Big Plants

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Abstract

This paper studies the long-run local labor market effects of the publicly-financed construction of large manufacturing facilities during World War II. I focus on a subset of large, new plants that the military was not able to incentivize private firms to stake any capital on, and likely would not have been built if not for the war. I compare recipient counties to counties that were similar but for conditions engendered by the war. After establishing an absence of pre-trends across a number of outcomes, I show that recipient counties experienced a large post-reconversion boost in manufacturing employment and wages that persisted for several decades. I show how these effects impact broader labor market outcomes in the post-war period and discuss methods for distinguishing between causal mechanisms using plant-level data.

1 Introduction

Rising income inequality and disappearance of “middle-skill” jobs have been hallmarks of the United States economy for the past half-century. Yet, in the long arc of history, the postwar period appears to have been a period of exceptional opportunity for the middle-class. By the end of World War II the premium for skill had fallen notably, the pre-tax wage distribution had compressed, and the employment level in middle-class manufacturing occupations had reached unprecedented heights. Many influential scholars and commenters (Goldin and Margo 1991, Piketty 2010) have claimed a causal link between the events of the war and the prolonged period of middle-class prosperity. However, only scant evidence exists as to how and why various aspects of the military and industrial mobilization for war impacted postwar labor markets.

This paper conjectures that durable investments in productive capacity made as part of the war mobilization effort had labor market effects that long outlived the war. The industrial mobilization effort entailed a large-scale, rapid and publicly-financed push to construct new manufacturing plants for war-specific purposes (such as bomber assembly). Yet as the war ended in 1945, these plants, built to churn out unprecedented output quantities for a lengthy war, stood intact. While no company was willing to finance a dime of such
a massive plant designed to produce war materiel, most such plants were bought by private firms at large markdowns and repurposed for civilian production after the war. Thus, for reasons that were in large part idiosyncratic and due to the short-run strategic goals of the war, many communities in the United States found themselves with large, durable, state-of-the-art industrial facilities. I exploit idiosyncratic variation in wartime investment to test the extent to which public investment in industrial infrastructure can transform local labor markets so as to boost manufacturing employment and buoy middle-class opportunities in the long-run.

This research speaks directly to a core question for economic policy-makers: can a brief public intervention have beneficial impacts that persist well beyond the period of government intervention? Understanding the answer is vital to understanding whether industrial policy is justified in developing regions, whether place-based labor policies can help the middle class, and the extent to which infrastructure shapes how urban clusters form and persistent. However, opportunities to study this question directly are extremely rare—big "pushes" are few and far between, and when they do occur (in the form of plant openings or major infrastructure works) they are typically systematically targeted at places that are expected to grow or stagnate. Moreover, it is very difficult to determine what the reason for persistence is.

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

To estimate the impacts of siting a large plant in a specific locale, I compare counties that received large and completely federally-funded plants to counties that were observably similar at the dawn of the great depression. My conjecture is that in the absence of a war, neither the control nor the treatment counties would have had such an additional plant open; the only reason potential outcomes differ across treatment and control counties were circumstances created by the war. I hone in on control groups using several methods, although the choice of method does not significantly impact the results. For a given control group definition, I examine event studies using a wide array of outcomes. I find that while there is no difference in trends in the run-up to the war, there is a pronounced difference in outcomes that emerges only at the end of the "reconversion" period that occurred in the immediate aftermath of the war, and then persists throughout
the balance of the century. The across-the-board absence of pre-trends motivates a difference-in-differences estimator to estimate the short-run and long-run impacts of plant openings.

While the identification of persistent effects on local labor markets is of interest, the implications for policy depend on the reason for persistence. Persistent effects may result from dynamics that are entirely internal to the plant—this may simply reflect slow depreciation of durable capital, but it may also arise from dynamic complementarities that incentivize firms to continue to reinvest in the same location (which would give rise to path dependence). Alternatively, persistence may also arise due to external economies—the creation of manufacturing clusters due to productivity spillovers, the formation of a large labor pool, or the establishment of infrastructure with benefits beyond the original plant. The county-level analysis is not particularly conducive to tests of specific mechanisms. However, I discuss methods to distinguish between these mechanisms using restricted-access plant-level data that I am in the process of acquiring.

This paper contributes to several strands in the economics literature. First, it contributes to the literatures on how place-based policies impact economic geography, regional development, and local-labor market. The focus on plant openings is similar to Greenstone, Hornbeck, Moretti (2010), while the focus on persistent effects of place-based policies follows work by Moretti and Kline on the Tennessee Valley Authority (2014). Other notable work on localization economies and “big push” policies are Ellison, Glaeser and Kerr (2010), Lee (2015), and Murphy, Shleifer and Vishny (1989). Second, it contributes to a growing empirical literature in macroeconomics that exploits variation in military spending over time and place to estimate the short and long-run impacts of different types of government spending on regional economic performance; Barro (1981), Ramey (2011), Nakamura and Steinsson (2014) are key papers in this vein. Finally, this paper joins a growing literature that explores the long-run effects of World War II on various aspects of the post-war economy. Goldin and Margo (1992) were the first to clearly document that a distinct “great compression” in the wage distribution occurred during the 1940s. Angrist and Krueger (1989) looked at the impact of the military service GI bill on post-war education and earnings, finding that most of the effects appeared to be driven by selection. Fishback and Cullen (2013) examine the relationship between aggregate local spending and post-war retail sales and population growth, and appear to find relatively small effects. Higgs (1992, 2004) and Mulligan (1998) present calculations that suggest the effects of the war on the labor force and postwar growth was minimal. And several papers, notably Goldin (1991), Goldin and Olivetti (2013) and Acemoglu, Autor, Lyle (2004) have attempted to measure to impacts of the war on female labor force participation in the post war period.

The remained of the paper proceeds as follows. Section 2 provides historical and institutional background on the economic mobilization for WWII places decisions to build plants with public funds in that context. In Section 3, I develop a research design that exploits the institutional context to obtain credibly causal estimates of the impact of wartime plant openings on post-war local economies. Section 4 presents the baseline results pertaining to post-war manufacturing output, employment, and wages. Section 5 expands the analysis to examine effects on broader labor market outcomes. Section 6 discusses what one can and cannot reasonably conclude from these findings, and suggests several tests to empirically distinguish between
the potential mechanisms at play. Section 7 Concludes.

2 Institutional Background: New Plants for War

The industrial mobilization during WWII was by many counts the most dramatic economic expansion in United States history. When war broke out in Europe in 1939, the industrial sectors in the USA were ill-equipped to support a sustained war effort. American industry had been poorly organized during the First World War, and as a result never truly converted to coordinated production of war material. While manufacturing had boomed during the 1920s, industrial output lagged during Great Depression, during which period many firms had closed their doors for good. Moreover, the vast majority of American manufacturing experience in 1939 had been concentrated in sectors that could not easily pivot towards production of metal- and chemical-intensive war goods like airplanes, ordnance, and explosives—primarily agricultural processing, textiles and apparel manufacturing, and wood/paper processing (Text Figure 1). Yet, from 1940 to 1944, annual output of planes rose from approximately 6,000 military planes per year (out of 13,000 total civilian and military aircraft produced) to over 96,000 military planes per year—a sixteenfold increase in output (USAF 1955, 331). During that time, industrial output had nearly tripled. Employment in the chemical- and metal-working sectors had nearly tripled from about three million to nearly eight million, while the other industrial sectors expanded only slightly from the 1939 base employment of five million. The once-tiny aircraft manufacturers increased their employment fourteen-fold. Annual government purchases of these goods amounted to nearly half of the size of the entire US economy in 1939.

While the automobile sector had grown since the dawn of the century, it was still dwarfed by these other, more traditional manufacturing sectors; and, while some now-famous aircraft companies had opened doors before the outbreak of the war, aircraft production comprised a trivial part of American manufacturing capacity in 1939.
While the government was the source of demand for materiel, production was rarely done by public employees; rather, the vast majority of production was done by the private sector under contract. However, few firms had the capacity to meet demanded output levels of any war good. Most contracts required expansion of productive capacities to some extent. In some cases, firms simply enhanced existing plants to increase output of pre-war goods (like canned food, uniforms, or iron). In other cases, producing war goods required complete conversion and retooling of factories to make an entirely new good—automobile plants converted to make airplane engines, appliance companies started making guns, and a Quaker Oats plant even started making TNT. Yet, some orders called for such large-scale output of new types of products that a completely new plant was required.

Given the urgency of the mobilization and the need to make dramatic changes to product lines, competitive bidding was ruled out from the start. Instead, “cost-plus-a-fixed-fee” contracts were directly negotiated by a wide array of government military agencies with manufacturing firms. The War Production Board (along with its predecessors, the National Defense Advisory Committee and the Office of Production Management) was established to help these myriad agencies connect with firms that had the capabilities to take

Text Figure 1: Setoral Shifts in Manufacturing during World War II (Source: US Archives, RG 179)
on major projects without creating bottlenecks or misallocations of resources. Generally, this involved directly approaching the large metal and chemical product manufacturing companies and negotiating what a reasonable payment would be to get the job done quickly. During the process, government agencies and firms negotiated what kind of plant expansion was necessary to fulfill a given order, who was responsible for financing the expansion, and where the expansion would take place (White 1980).

By the end of the War, an incredible amount of new manufacturing capital had been put in place. The value of plant construction put in place to support various materiel orders amounted to over $20 billion in 1940 dollars (approximately $300 billion in 2015 dollars)—that amounts to over twenty percent of 1939 GNP, and approximately fifteen percent of all war outlays. This is a large increase compared to the outstanding manufacturing capital stock at the dawn of the war, which was valued at approximately $40 to $60 billion dollars ($1940). Of the over $20 billion spent on plant expansions, the majority was spent on brand new factories—especially very large new factories. (WPB 1940A, WPB 1940B). Notably, just three hundred large plants account for ten billion dollars of the wartime plant expansion—approximately half of the value of plant put in place. One should note that these plants were valued for their utility during the war—the valuation of these facilities declined precipitously at the end of the war, since many of the machines needed to be replaced, the locations were not ideal, and repurposing would be costly. Yet they were not completely without value—most plants built during the war were either converted to civilian production after the war or put to continued use for defense production as the Cold War began (WAA 1947).

The willingness of firms to raise private capital for plant expansions depended on the amount of risk such a plant entailed. Firms were usually eager to have the government share the cost of smaller plants with clear long-run value—for example, machine tool shops in major industrial hubs or petroleum refineries. However, firms and their financiers were much less eager to risk capital on very large industrial plants with highly uncertain post-war value—for example, ordnance factories or bomber assemblies. If the war lasted for a long time, these plants might have been highly profitable; however, an unexpectedly quick end to the war would radically reduce that profitability. When even generous tax amortization incentives and favorable costing arrangements could not attract private capital to a project deemed necessary to the fulfillment of a crucial

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2One should compare this to the recent American Recovery and Reinvestment act, which authorized $111 billion dollars for infrastructure construction. Even after adjusting prices, the real economy has expanded over sevenfold since WWII—thus, $111 billion is a dramatically smaller intervention relative to the size of the economy than the WWII plant expansion. One might also compare this to about $15 billion (1940 dollars) spent on public works during the New Deal from 1933-1939 (under the PWA and WPA).

3Civilian Production Administration (successor to the WPB) estimates place total wartime investment in plant and equipment through the end of 1945 at $23 billion. They estimate of 1939 manufacturing plant excluding land value as $39.5 billion—amounting to an expansion worth 58.4% over the pre-war plant stock, and the federally financed portion worth 43.5% of the total prewar stock (CBP 1946). The same study notes that capital expenditures in facilities in 1939 totaled $1.2 billion, while annual private investment in war manufacturing facilities was about $1.8 billion per year on average throughout the war; the publicly financed investment—which was approximately three times the amount of private investment—was entirely supplemental.

4If you find the potential incentives to prolong the war alarming now, you can bet it was a subject of major concern at the time. There was widespread concern about war profiteering throughout the war, and Congressional committees (most notably the Truman Committee) were continuously holding hearings to ensure that no one had a pecuniary interest in prolonging the war.
order, the US government financed new plants itself. In these cases, the contracted firm would typically construct and operate the facility, but the facility itself would be fully owned by the US government, usually under the auspices of the Defense Plant Corporation. At the end of the war, the plants were to be auctioned off to private firms, with right of first refusal offered to the operator.\footnote{Business interests and anti-New Dealers were highly concerned about government ownership of productive facilities that might potentially compete with private interests in the post-war. The authorizing legislation therefore required that plants only be operated by private firms after the war, but liquidated in such a way that allowed incumbent firms to get a foothold in the postwar civilian economy first.}

Such arrangements allowed the government to assume the full financial risk of these crucial plants. (White 1980)

When negotiating what new plants would be built, the US government faced important trade-offs. In general, the lowest cost option was to build smaller plants in cities with well-developed industrial infrastructure where major firms already operated—not only could these be built at lower cost, they were most likely to receive private investment. However, despite the low cost, such specifications were spurned by the government when it came to the largest plants. The goal was reach maximum capacity as quickly as possible, given available resources and labor, which required large plants. Production airplanes typically required that plants be built along large airfields, which required rapid assembly of the sorts of large parcels that were difficult to find in major cities. And high output alone was not enough. Interruptions to the production of crucial materiel posed a major threat to the war effort—therefore, security was a paramount concern. Concentration of industry in large hubs posed major risks, both because of the vulnerability of a single city (e.g. bombnings or power outages) and because of service interruptions due to urban congestion. Location of plants along coasts and borders raised the specter of bombing raids; hence, although many key industries were concentrated in coastal cities (aircraft, in particular), the military urged that all new expansions take place two hundred miles or more inland, if possible (USAF Historical Division 1955). Larger plants outside major industrial centers, which satisfied these security and short-run efficiency concerns, were both more expensive to build and less obviously valuable for post-war use.

Thus, private firms often could not be convinced to put up a single dime to finance the largest plants that the US government desired built. After the war, Air Force historians noted that “The industrialists’ reluctance to invest in dispersed plant facilities was at odds with the government’s hope that private capital could finance new inland construction; Hence, the War department could carry out its policy only to the extent that the government was willing to put up the money” (USAF Historical Division 1955).

Nonetheless, in many cases the military decided that such plants were sufficiently necessary to justify full government financing, even at extraordinary cost. Large bomber assemblies, ordnance works, aluminum
and steel plants, chemical processing facilities, and other large plants were built in small cities in small cities and large towns that had little history of large scale chemical processing or metal working. Given that these small cities had few major advantages (beside basic amenities like transport connectivity and water sources, which many small cities had), the actual choice of location was driven by fairly idiosyncratic concerns. Perhaps the foremost was to find a parcel spanning hundreds of acres that could be purchased or seized with minimal difficulty. Often times, entrepreneurial federal representatives and local officials in remote areas that could not attract investment in normal time would offer military officials full disposal of local public services and even free land if they located war plants in their jurisdictions. This willingness to accommodate helped attract plants given the needs to build dispersed plants in the war—however, such incentives had failed to attract private peacet ime investment.

Beyond the need to produce ordnance and airplanes in secure locations, the wartime expansion also required major short-run increases in the supply of particular materials to specific regions. The long-run value of these resources were unclear, so the federal government frequently had to finance plants to rapidly expand production. For example, aluminum and magnesium were important components of aircraft and ordnance, but less commonly used in standard commercial products of the time. The largest manufacturing plant in the history of the State of Nevada, the Basic Magnesium Plant, was built in 1941 on the outskirts of Las Vegas in a location close to both the Magnesium mines at Gabbs and the water and power provided by Lake Mead and the Hoover Dam. (White 1980) The plant cost $126 million to build in contemporary dollars—nearly $2 billion in 2015 dollars. While magnesium was rarely used in manufactured products prior to the war, producers learned about various uses of magnesium through the experience of the war, and the Basic plant was able to covert to civilian production after the war. The plant is still in operation today; and the settlements surrounding the plant became the city Henderson, which is still the second largest municipality in the State of Nevada.

Another example of the government stepping in when private finance would not is the case of western production of steel. The steel industry in the Eastern and Midwestern heartland was relatively mature, and most industries that used steel products were close by and easily accessible by rail. However, World War II brought about both a pressing need for ship construction on the West coast to fight in the Pacific Theater and the closing of the Panamal Canal due its vulnerabilities. Shipping steel in bulk from the forges of Ohio and Pennsylvania to California, Oregon, and Washington States was impractical (particularly given needs to supply the East coast shipbuilding effort). Thus, the federal government supported a massive expansion of steel production facilities in the hearland of the West at sites that were close to major ore deposits and easily accessible from major Pacific shipbuilding centers. The Geneva steel mill built in Utah (near Provo), which opened in 1942, was the largest steel plant ever built west of the Rockies. Whetten (2011) notes that while private financiers had seen little prospect in such a large steel plant in Utah, the federal government stepped in for reasons of short-run necessity: “The officials at the OPM did not aim to foster regional industry or to bring the American West out of the third world and into the first; they simply wanted to address national defense contingencies and the supply and demand issues that loomed ahead of the attack on Pearl
Harbor” (Whetten 2011). 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). Similar stories of political entrepreneurs attracting federal investment during this unique wave of public investment were common among the other federally-financed construction projects. Yet, without a major government intervention in manufacturing plant investment, such efforts would have likely borne little fruit.

These large, new plants that no firm was willing to stake a dime on are the focus of this research. While these plants were not located at random–plants so expensive have never been located at random–the reasons for their construction and siting had very little to do with forecasts of long run profitability in peacetime markets. If the government had not stepped in to finance their construction, it is unlikely that similarly large plants would be have been built in those same locations.

Large plants have never been randomly built. However, the construction of these plants the closest thing to a random assignment of major industrial infrastructure works that has ever occurred in the Western world at such a large scale.

3 Data and Research Design

3.1 Treatment Notion

To study large plants, I draw from a rich, newly-digitized War Production Board (WPB) database of all substantial capital expenditures made for the sake of war production. Although procurement contracts were negotiated by more than a dozen distinct government and military offices, all contracts for the production of war goods were reported the War Production Board and all requests for public funds, tax write-offs, and other public resources for plant expansion required WPB approval, so as to ensure efficient use of scarce materials and elimination of redundancies. Hence, the WPB was able to compile administrative databases detailing the placement of each supply contract and the universe of investments with a war rationale.\footnote{All capital expansions with a war rationale were eligible for tax incentives or public funds, which required WPB approval. All plants involved in war production sought these incentives, and WPB approval was almost never denied, since the requests occurred after military procurement agencies had completed negotiations. One should note that use of scarce construction inputs (steel, iron, concrete, etc.) was highly restricted in the civilian sector during the war, so these plant investments account for the universe of industrial construction during 1941-1945.} This paper draws from a 1945 WPB data book, War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator (WPB 1945a).\footnote{Very few new projects were authorized in the final year of the war–these data account for approximately 90% of all authorizations.} The data book has plant-level detail on each plant’s ...
operator, the 1939 industry of the operating firm, the city in which the plant is located, the plant’s war products and output volumes as specified in the operator’s contract, the date of completion, and the cost of facilities expansion. The cost data is subdivided into privately financed and publicly financed amounts, and those amounts are further subdivided into expenditures on structures and equipment.

While the data document plant expansions at tens of thousands of establishments, the greatest part of the plant expenditure was spent on a handful of major expansions. Approximately half of the expenditure on plant expansions was spent in only three hundred large plants. Figure 1, which plots the distribution of expenditure amounts across plants on a log scale, illustrates how thick the upper tail is. I use these data to identify large, publicly-financed, new plants. I define these as plants costing at least $1 million ($1940) built at new sites, for which one-hundred percent of investment in durable, immobile structures was publicly financed. 910

There are 582 plants fitting this definition in the data. However, even within this group the bulk of expenditure occurred in a small subset of very large plants.

In the county-level analysis, the “treatment” notion is having received extremely large investments in new public plants. In practice, I define a county as “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Only 209 counties have any large new public plants. The core treatment notion narrows to the focus to the 99 counties in which there were dramatically large plant openings (relative to the baseline size of the labor market).11

9A 1945 WPB book of county-level tabulations subdivided plant investment into three categories: “expansions” of existing facilities producing similar goods, “conversions” of existing facilities producing substantially different goods, and “new plants” that were substantively new establishments (WPB 1945b). Those tabulations found that slightly over $12 billion of the plant expenditures nationwide fell into the latter category. My “new plant” designation is meant to replicate that categorization as best as possible in the plant level data. Many plants are directly labelled as “new plants” in the plant level data; these account for $8 billion of the total expenditure. There are numerous large plants in the data without this label that have been confirmed to be new plants based on external sources; a common and intuitive feature of such plants is that the expenditure on structures is very large relative to the expenditure on equipment (since the sites are being constructed from scratch). I thus label all plants with over 40% of total plant expenditure attributed to structures as “new” that approximately matches the $12 billion target. In comparison to the published county level tabulations, the county-level totals based on my micro-data classification has a .85 correlation (as opposed to .73 when only using plants listed as “new”).

10 Many plants had some, but not all, construction of new structures financed by private capital. Most large plants have multiple buildings or wings, and in some cases a firm was willing to finance buildings with high expected post-war value but not supplemental buildings or wings that were dedicated to very specialized parts of the production of war material. I distinguish between these “partially privately financed” plants from plants that received absolutely no private investment in any structure at the facility.

11 The only difference between using total spending and per-worker spending to define the threshold in practice is that very large cities (such as Chicago and Detroit), which were orders of magnitude larger than other cities prior to the war, are not counted as “treated” simply by virtue of their size.
Text Figure 2: Locations of Large Public Plants

The cutoff is chosen as approximately where the “thick tail” begins, as shown in Appendix Figure A2 (Displayed also as Text Figure 3). In the 99 counties above this cutoff, a single enormous plant typically accounts for the overwhelming majority of expenditures on large new publicly-financed manufacturing facilities. Figure 2 maps these 99 counties and the industry of the largest public plant. These large plants produced aircraft and ordnance (both typically inland); dramatically expanded capacity to produce key metallic inputs to aircraft (aluminum and magnesium) and ships (steel)–particularly inland off the Pacific seaboard; and supplied chemicals that were used for explosives, fuel, and synthetic rubber. Table 1 reports that the 130 large publicly-financed new plants in the 99 treatment counties account for nearly one-fifth of all plant investment during the war.

3.2 Empirical Specification and Identification

As discussed above, all available evidence suggests that the 100% publicly-financed war plants were sited for highly idiosyncratic reasons. The historical evidence suggests that no firm would have constructed such a large plant in that location if not for circumstances created by the war. In what sense, then, might this provide as-good-as random variation across counties? The experimental framework I propose is to compare two observably similar counties, neither of which would have likely received a large plant opening in the counterfactual world where no war occurred, but only one of which received a plant due to circumstantial concerns raised by short-run strategic war needs. If the only reason one county got a plant and the other

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12I initially employ a binary treatment rather than a continuous one for clarity in light of the large number of zeros and the highly skewed distribution of expenditures. I explore heterogeneity effects corresponding to differential treatment intensity later in the paper.
similar county did not is due to war-specific advantage—that is, features that were relevant for plant siting during the war, but would not have affected the path of industrial and labor market development otherwise—then the latter can be used to infer the counterfactual potential outcomes of the former if it had not been “treated.”

I identify the causal impacts of plant openings on several county-level output and labor-market using event-studies, which compare treated counties to comparable controls before and after the war. Because many observably underpopulated or agrarian counties could not have supported plants in any scenario, and other large cities would have doubtless received major manufacturing expansions in any scenario, I allow for selection into treatment on certain 1930-1935 observables. The assumption is not that plant assignment was random between the two observably similar counties. Rather, it is that conditional on a core set of pre-war observable characteristics, the remaining sources of variation in plant assignment are statistically independent of other latent determinants of post-war economic development.

I estimate the treatment effect in pre- and post-war years under this assumption using two approaches. My primary approach is to estimate linear regression equations for each outcome year $t$ of the form:

$$\ln Y_{it} = \alpha_t + \beta_t TREAT_i + \gamma_t X_{1930}^i + \delta_{st} + \epsilon_{it}$$ (1)

where $\delta_{st}$ is a state fixed effect for the specific outcome year, $X_{1930}^i$ is the 1930s vector of observable covariates, and $\beta_t$ is the year specific treatment effect. Under the linear effects selection-on-observables assumption (conditional independence)

$$\epsilon_{it} \perp TREAT_i | X_{1930}^i$$ (2)

each $\beta_t$ is identified. When the OLS is implemented, I exclude counties with missing pre-war data on the core outcomes (including those with no reported manufacturing) from the analysis, but include all other counties—yielding 1483 counties total. Rather than define an explicit control group, this approach relies on the linear structure of the model to partial out the effects of confounding covariates.

Second, I relax the linear and constant effects assumptions to estimate average treatment effect on the treated (ATOT) using propensity score weighting. This essentially amounts to creating a synthetic control group based on the selection variables, and nonparametrically estimating the treatment effects. To do this, the control variables include the following 1930 variables, measured both in logs and per-capita levels, and the squares of each transformation: Population; Employment; Population Density; Manufacturing Value Added, Employment, Payroll, Average Earnings, Employment of Production Workers, and Average Production Wage; Retail Employment, Payroll, average Wages, and Sales; Value of All, Owner-Farmed, Manager-Farmed, and Tenant-Farmed land; Wholesale Wages, Employment, and Stocks; Black Population; Foreign Born Population; Urban Population; and Workers Laid Off. I also include certain important 1930s government spending variables from the New Deal, specifically: 1933-1935 and 1933-1939 Log Total Public Works Spending, 1933-1937 AAA Road Construction Grants, and 1937 Log Emergency Workers. Because no 1930 data is available, I also include one potentially important 1940 variable: Log Median Housing Value, as one might be concerned if housing prices were not accounted for in the selection-on-observables assumption.
I first estimate a probit regression of $TREAT_i$ on the same covariate set $X_{i1930}$, that is:

$$\Pr(Treat_i = 1|X_{i1930}) = \Phi(\beta X_{i1930})$$

(3)

I estimate propensity scores $\hat{p}_i$ as the the predicted treatment probabilities from the fitted model $\hat{p}_i \equiv \Phi(\hat{\beta} X_{i1930})$. All observations with propensity scores outside the overlap region (almost the entirety of which are control observations with propensity scores below the range observed in the treatment group) are trimmed from the sample, as they have no comparable counterpart in the opposing sample. I define the propensity score weight $W_i$ as equal to one for all treatment observations, and equal to $W_i = \frac{\hat{p}_i}{1-\hat{p}_i}$ for the remaining controls. Following the classic result of Rosenbaum and Rubin (1983), Imbens (2004) notes that under the selection on observables assumption\(^{14}\), the propensity score weighting estimator

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

(4)

is a consistent estimator of the average treatment effect on the treated. One reason for using this technique is that one can transparently assess which counties are contributing most the (synthetic) control group that is being used and also assess whether they are indeed a valid control group using tests of covariate balance. It is also possible to show the control counties on a map. The map in Panel (b) of Appendix Figure A1 (displayed as Text Figure 3) displays both the treatment and control counties that are not outside the overlap region. There are 83 treatment counties in the trimmed sample (each with weight 1) and 1,051 control counties in the trimmed sample, where the sum of weights in the control group is 80.7. The map divides counties into four differently-shaded bins such that each bin has equal amounts of weight (rather than split control counties into weight quartiles with equal numbers of counties). Thus, the sum of weights of the 32 counties in the highest weight bin is the same as the sum of the weights of the 70 counties in the next weight bin. This is meant to highlight the extent to which a relatively small number of counties disproportionately drive the control group average, as intended in the reweighting method.

\(^{14}\)In the canonical treatment effects literature, where county $i$’s potential outcome if treated is $Y_{1i}$ and its potential outcome if untreated is $Y_{0i}$, the conditional independence assumption can be formalized as $Y_{0i}, Y_{1i} \perp TREAT_i | X_{i1930}$. 
While the assumption of selection on 1930s observables only is strong given the availability of a wealth of 1940 data, it allows for a crucial falsification test of the identifying assumption. If treatment is truly randomly assigned conditional on 1930 observables, then there should be no systematic relationship between the treatment and outcomes in 1939 before war broke out. Thus, every event study for outcomes with available pre-war data tests for a 1940 pre-trends. in the analysis includes

Under the conditional independence assumption and correct specification of the propensity score, one should observe covariate balance between the treatment and re-weighted control group for both conditioning variables and other pre-treatment variables that are likely not to be independent of potential outcomes. Testing for covariate balance can partially validate the identifying assumptions. Table 2 presents tests for covariate balance for both select 1930 variables included in estimation of propensity score and for 1940 variables that are not used in determination of the weights. While the treatment was clearly not unconditionally randomly assigned, there is no evidence of imbalance between the reweighted control group and the treatment group. However, even with no observable pre-trends, the casual interpretation could be confounded if other economic changes that occurred 1940s (for example, technological innovations that benefit certain industries) would have disproportionately affected treatment counties, in which case a spurious result might be found.

The county-level outcome data are tabulations of data from the Censuses of Manufactures, the Censuses of Business, and the Decennial Population Censuses compiled in the County Data books and in work by Haines (2010). First, I examine the effects of war plants on post-war manufacturing value added (which measures the production level), employment of production-line workers and wages of those workers, since manufacturing is the sector directly impacted by plant investment. I then turn to broader labor-market
outcomes both in other sectors and the aggregate. I control for a second order polynomial in the log levels and per-capita levels of most outcome variables in 1930, as well as sets of variables collected by 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.

4 Long Run Impact on Manufacturing

The primary reason one should expect that the construction of durable war plants would have lasting impact on local labor markets is that the durable industrial infrastructure is likely to attract some degree of manufacturing activity in the post-war period. Thus, I first establish that manufacturing activity expands in treatment counties relative to similar untreated counties, and that this effect appears to be causal.

The event studies in Figure 3 plots the year-specific ATOT effects estimated from the specification in (4) above for four county-level manufacturing outcomes in each year that those variables are available (the regression estimates are similar, as visible in Table Figure 4 and 5). The outcomes are log value added (which is measures the production level as value of outputs produced less the cost of purchased input parts), log average annual earnings of manufacturing workers involved in production (as opposed to clerical and managerial positions), the log employment level of manufacturing production workers, and the log manufacturing establishment count. Each point estimate is obtained using 1930 and Great Depression controls, state fixed effects, and standard errors clustered at the state level. Panel (a) in Figure 3 displays the basic effect on manufacturing production—these results are reproduced in Text Figure 4. If plants were assigned for reasons uncorrelated with potential outcomes, there should be no evidence of a treatment effect in the pre-period. Consistent with the conditional independence assumption, there is no apparent pre-trend in manufacturing value added in either 1940 or 1920. Yet a pronounced effect appears after 1947—throughout the 1950s and 1960s, total value added is approximately 30 percent higher in counties that received war plants relative to those that did not. While this difference fades somewhat over time, manufacturing activity remains at a higher level in treatment counties throughout the balance of the century.

Text Figure 4: Effects on County Manufacturing Production (Log Value Added)
The lack of a full treatment effect in 1947 is to be expected. Unlike private plants, which raced to reconvert to civilian production after V-J day, public plants could not immediately begin civilian operation. Because these plants were owned by the government under military sponsorship, the military had to first deem these plants surplus and no longer necessary for immediate defense purposes before they could be sold or leased. Following the surplus declaration, plants were auctioned off by government bureaucracies designed to assure the public that they were getting a good deal and to assure big business that they were not trying to flood any particular market with cheap capital. Once a deal had been agreed to, the plants needed to be re-equipped for production of civilian goods. As a result, few large public plants began civilian production before 1947—and many plants only began civilian operations even later. Thus, few treatment plants would have been operating at full capacity until the very end of the decade. (WAA 1947; White 1980).

One would expect a rise in manufacturing production to increase labor demand for production workers, which should be reflected in either wages or employment levels. Panels (b) and (c) show that while treatment counties pay similar wages to production works and hire similar quantities before the war, there is a pronounced boost to local production employment and wages in the post-war period. Wages are approximately 5 to 10 percent higher in counties that had a publicly-financed plant built during the war—this effect appears to be permanent. This result is inconsistent with simple models with homogenous production labor, homogenous production tasks, and perfect mobility across locations and jobs—however, many types of frictions (geographic mobility costs, search frictions, efficiency wages, rent-sharing, etc.) and sources of heterogeneity (across latent skills or task qualities) could lead to such a result. Employment of production workers is over 20 percent higher in the 1950s and 1960s, but converges back slightly later in the century. The effects on establishment counts are less clear; but given the bias of war production towards large firms, it is not clear whether one should have predicted any particular effect.  

The absence of pre-trends across all outcomes with notable treatment effects is crucial to the interpretation.  

Text Figure 5: Effects on Average Production Worker Earnings

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

15
of these results as causal. One may wonder whether the absence of pre-trends only arises due to a specific choice of specification—however, the absence of pre-trends is a fairly generic result. In Appendix Figure A3, I plot the raw difference in mean outcome levels between the treatment counties and the 211 “control” counties discussed in the previous section (I also plot the level trends for each group). Notably, the gap between treatment and control groups is essentially flat and very close to zero throughout the pre-war period, despite the fact that these plots do not condition on anything. Even more strikingly, when one plots the raw difference in mean outcome levels between the treatment counties and all other counties with some pre-war manufacturing, the gap is mostly flat throughout the pre-period, despite the fact that these plots do not condition on anything or limit to comparable counties. This is consistent with no differential trends in treatment counties, although both treatment and “comparable counties” are substantially larger than the average county (of course, the unweighted average of all counties is highly influenced by a large number of mostly rural counties).

While the absence of pre-trends does not prove causation—contemporaneous shocks and unobserved latent trends could still be at play—the finding of no pre-trends is also not trivial. Many aspects of the economic mobilization (contract placement, privately-financed expansion, etc.) were associated with latent trends that were occurring regardless of the events of the war, and those trends are evident in the pre-war period. One focal example are the war plants that were constructed with some degree of private financing. A priori, one would think these plants were sited with long-run, post-war forecasts in mind. I conduct the following exercise: I change the treatment definition to include only the 100 counties with the most per-1939-employee spending on new war plants that were privately financed at least in part. I then replicated Figure 3 using this altered treatment definition. The results are plotted in Appendix Figure A4. While these “private-finance treatment” counties did experience a persistent post-war increase in manufacturing, they also experienced significantly higher growth in the pre-war years. These findings suggest that the assignment of privately-financed plants reflected underlying trends, and did not necessarily cause the full post-war increment.

Select point estimates from OLS regressions are displayed in Table 3. Although one must exclude 1940 conditioning variables to examine a pre-trend, including those conditioning variables can increase power given that the identification is valid. Thus, Table 3 includes controls all specifications also control for the 1940 level of all available outcome variables. If the initial selection-on-1930’s-observables assumption is valid, inclusion of these controls should not significantly affect the point estimates—this is consistent with the results obtained.

One might wonder whether the treatment effects are more pronounced in treatment counties that received larger plant investments (relative to baseline size of the local economy). To investigate this, I divide the 192 counties in the analysis sample with at least one large publicly-financed plant into four equal-sized bins corresponding to treatment intensity quartiles (that is, quartiles of spending on large public-financed war

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16 Because these specifications control for nothing, the point estimates are fairly noisy. With controls the point estimates remain similar, but the standard errors shrink notably.
plants per 1939 employee), and estimate the effect by quartile in the following regression specification

\[ \ln Y_{it} = \alpha_t + \sum_{q=4}^{4} \beta_q^TreatQuartile_q + \gamma_{09}X_{1930} + \delta_{it} + \epsilon_{it} \]

The point estimates\textsuperscript{17} for the three primary manufacturing outcomes are plotted in Appendix Figure A5. While the pattern of effects are similar across quartiles for all outcomes, the effect size is generally increasing monotonically in intensity level and the largest effects are concentrated in the top quartile. Thus, it does appear that larger investments are associated with larger long-run effects on local manufacturing production and employment.

Though the mere presence of sizable post-war manufacturing effect may not be surprising given the physical presence of a large plant, the persistence of these effects is of note. The increments to manufacturing employment and activity in treatment counties outlasts any realistic depreciation horizon for the initial war-time infrastructure. Such persistence implies that either the operating firms continued to reinvest and upgrade these former war plants throughout the post-war period, or other firms made additional investments in the locale of the former war plant that outpaced investment in untreated counties. While county-level data on pre-war manufacturing capital expenditures is not currently available, Figure 3(e) shows that post-war manufacturing capital expenditures was indeed substantially higher during the post-war period in counties that had received a publicly-financed war plant.

5 Long Run Impacts on Local Labor Markets

Does higher post-war manufacturing employment and wages spill over to broader employment outcomes? Figure 4 plots event studies for five broader labor market outcomes for which pre- and post-war data are available. Panels (a) and (b) plot effects on (log) aggregate population and employment levels. Both display similar patterns—minimal differences between treated and untreated counties in 1940, but somewhat higher levels peaking in 1960 (though these estimates are imprecise). The employment point estimates never exceed 10 log points, and the population effect estimate barely surpasses 5% at its peak.

Panels (c) and (d) provide detail on retail and service employment—both of which reflect employment in relatively non-tradable sectors. The results are highly imprecise, but are consistent with sectoral employment growth that is proportional to the broader employment and populations effects (between 5-10% higher). It appears, then, that manufacturing employment growth outpaces growth in other sectors. Panel (e) confirms that manufacturing’s share of employment does in fact grow in treated counties—rising by about 3 percentage points relative to untreated counties in 1960.

Figure 5 examines broader effects on wages, income and housing prices. Somewhat surprisingly, I find that

\textsuperscript{17}Standard error bands are not displayed due to space constraints; however, the point estimates for the smaller, 46-county quartile bin dummies are noisier than the baseline results.
while wages rose substantially in manufacturing jobs, there is essentially zero wage effect for retail and service employees, as shown in panels (a) and (b). This suggests that either production workers earn premiums for harder work in unfavorable conditions, or the markets for production workers and service/retail employees are stratified—which could stem both from the employment of differentiated skill/occupational groups or from firms that pay collective bargaining or efficiency wages and thus are not hiring on the margin.

![Graphs showing wage effects](image)

**Text Figure 6: Effects on Median Family Income**

Nonetheless, panel (c), reproduced as Text Figure 6 above, shows that median family incomes in treatment counties appear to be nontrivially higher during the post-war decades. Yet housing prices were not notably different, as shown in panel (d). This suggests that either incomes are growing in other sectors not examined here, or the coupled increases in earnings and employment in the manufacturing sector are large enough to make a notable dent on the aggregate income distribution.

6 Discussion of Results and Possible Mechanisms

The results above provide strong evidence that construction of a fully-publicly-financed large war plant had local impacts that far outlived the wartime conditions that necessitated its construction. Treatment counties had notably larger manufacturing sectors for most of the balance of the 20th century. Moreover, as employment levels expanded, so did the average blue-collar wage. Yet while these wage gains were larger enough to impact the local median income level, there was no clear boost to wages in other sectors (although there is weak evidence of small employment expansions). In fact, it is unclear whether the increase in the average wage is due to increases in wages for incumbent local workers or new external hiring of higher-wage workers. While the effects found above are of economically significant magnitude, the implications of these findings depends crucially on the specific channels through which war-time investment boosted local income and employment in the post-war era. I will briefly discuss several of the economic mechanisms that could be at play and their implications; however, at this stage, I cannot do much to rule out any specific mechanisms given available data. After discussing several rival theories, I will posit several tests that might be implemented using data I am in the process of accessing.
Perhaps it is best to start with the potential explanation that is least heartening—but is nonetheless entirely plausible. It may simply be the case that these effects reflect shifts in economic activity across space with no notable aggregate or distributional impacts. The following story presents itself: an industrialist, looking to build a new plant in the post-war era, initially considers construction from scratch in a new location. However, upon learning that a large facility in a less-than-ideal location is being offered by the federal government at a low discount price, this industrialist decides to re-tool and utilize this high-capacity facility since the low price justifies the imperfect location. Suppose this high-tech private establishment employs a large number of relatively skilled manufacturing workers (more skilled than the typical production employee in the locale if the war plant), who are either retained from war work or recruited to move to the locale of the plant. Suppose also that labor and goods are perfectly mobile, land is plentiful, and there are no productivity externalities. In this case, one would observe production employment and the average production wage grow simply by virtue of the single large plant, which employs skilled workers who would earn more wherever they worked, with no effect on any other aspect of the local economy. Moreover, although none of these workers would have located locally if this plant had not been built during the war, they would have been employed wherever else the industrialist chose to build a new plant from scratch—any local gains to employment and earnings in the former locality are the other’s loss.

The pessimistic interpretation above highlights several important theoretical distinctions that affect the interpretation of these results. First is the existence or non-existence of non-pecuniary production externalities; that is, benefits that the plant provides that it does not internalize in its own revenues. The standard example are those highlighted in Alfred Marshall’s seminal textbook (1890): productivity enhancements via local know-how and social interactions, easier searches for specialized labor due to the initial attraction of a base pool of skilled laborers, access to dense markets at low cost, or construction of high-fixed cost core infrastructure that benefits other business. These non-pecuniary externalities should boost productivity of other local firms at no cost, leading to increased entry and heightened productivity among incumbents; in closed economies, it is Pareto-improving to subsidize plant construction if these externalities exist.

But heightened employment and wages need not occur due to non-pecuniary externalities—they might be driven by labor demand within a single large establishment. There may be pecuniary price externalities that operate through the market mechanism—in particular, through changes in the local wage for a given occupation group—but these only have distributional, not Pareto-efficiency, consequences. Competitive labor markets could require all firms to increase wages, which could diminish hiring and output at other firms while still boosting manufacturing labor income on net. Yet it might also be the case that labor markets are not perfectly competitive, such that wages are not set at marginal product. For example, if the new firms have strong unions that achieve a stake in the surplus from production, or if large plants require on-site training on product-line specific knowledge that given incumbent employees a productivity advantage, then higher wages at the new plant may not translate into higher wages at other plants since the new high-wage plant will only be willing to hire a set number of workers. Another form of pecuniary externality would be an increased demand for local services resulting from the increase in local manufacturing incomes, which
could boost employment and/or wages depending on local labor supply responses (I find evidence of the former, but not the latter). Again, such localized pecuniary externalities might be small if most spending is on nationally or globally tradable goods and services, and even then the existence of these market-mediated effects do not evidence any market failure.

Since I only observe aggregate earnings and employment data in the current outcome data, I cannot determine whether the employment and wages effects I find are evidence of true non-pecuniary agglomeration spillovers, market-level impacts, or simply a reflection of activity at one large plant. If there are agglomeration effects, then the plants were an unambiguous win for local counties—even beyond the profits they provided to their owners. If there are increases in market wages for given types of labor (or new high-paying job opportunities for the kinds of workers that would have worked at other places in the locale), then although the size of the local pie would not grow more than the surplus produced internal to the plant, there may have been shifts that disproportionately benefited local labor. However, if there were no externalities or local market-level effects, and all wage increases simply reflect workers with specific skills or tolerance for industrial work moving into the county to take jobs at the plant, then there are no direct gains to local residents—beyond the basic advantages of having a larger population and tax base, perhaps.

Yet, the example above illustrates a further, crucial distinction. Even if externalities and market-mechanism effects were present that rationalized policy intervention from the perspective of local authorities, these plant openings may not have been net gains from a national perspective if they simply shifted activity away from another locality. From the perspective of the federal-government, this would appear as near-perfect crowd-out. Any productivity spillovers and wage effects gained in the winning county are spillovers and wage increases lost in the county that was “crowded-out.” This point about the general equilibrium consequences of place-based policies was examined in depth in Moretti and Kline (2014), who note that the gains in the “winning” county have to be disproportionately larger than the losses in the county that was “crowded-out” to justify public influence in where manufacturing investment takes place (for economic, not military-strategic reasons). Thus, beyond determining the form of local spillovers, it is important to infer the extent to which investments would have been made elsewhere in the counterfactual scenario before one makes conclusions about the aggregate economic benefit of local investments.

With plant-level data on productivity, output, payroll, employment, and investment, it is possible to empirically distinguish between pure productivity spillovers, price effects, and internal economies. Each of these contingencies yield different predictions about how other local firms should behave, compared to the former war plant. If productivity, scale, and wages grow at incumbent competitor firms or entry of competitors increase nearby, such a finding would constitute evidence of agglomeration externalities. If wages rise at other firms, but productivity and scale fall, then this suggests that the dominant force is heightened competition in competitive markets. If outside firms are entirely unaffected, than this suggests either that former war plants do not hire at the margin or that localized economies do not matter much. In order to implement such tests, I would need to construct longitudinal plant-level data on pre-war incumbent firms, the post-war operations of the war plants, and post-war entrants. Such a database does not exist, but I am
working with the Census bureau to assess the feasibility of the construction of such a database based on newly recovered plant-level data from the 1930’s and 1950’s.

It is more difficult to distinguish between heightened opportunities for local workers and attraction of skilled workers from external regions in available data—but in theory, this can be tested. The simplest tests require panel data on pre-war residents of treated and untreated counties. If individuals who lived in the treatment county at the outset of the war (i.e. in the 1940 Census) were found to earn higher wages or to be more likely to work in manufacturing after the war regardless of where they lived afterwards, this would support causal beneficial impacts on local labor markets—if the test were correctly specified, such effects would not be present if the total post-war labor market effects were driven by external migrants. However, while the complete 1940 Census is publicly available, it is extremely difficult to obtain post-war individual-level datasets that can be linked to the 1940 Census at the moment.

By contrast, testing for general-equilibrium “shifting” or “crowd-out” is a fundamental challenge, both in theory and in practice. This is because in general equilibrium, an investment in one location could affect a whole host of other locations in a number of ways. From this perspective, everyone is “treated” in some sense by every plant opening. Without a clear control group, it is impossible to identify causal crowd-out effects. As a result, any inference on this subject must be guided by structural assumptions about the nature of competition across space and the types of heterogeneity and complementary that exist among different production factors and consumption goods.

7 Conclusion

This paper has examined the extent to which durable investments in productive capacity made as part of the World War II mobilization effort had local labor market effects that long outlived the war itself. I have argued that the highly idiosyncratic location and investment decisions concerning a specific subset of plants built during the war—large plants that were constructed in new locations with absolutely no private capital that were necessitated by short-run strategic concerns for the war—are the closest to a random “helicopter drop” of major industrial infrastructure improvements that has ever occurred in the Western world. This claim is backed up both by the qualitative history of the war and, more importantly, by robust absence of pre-trends across treatment and non-treatment counties. Using this “natural-experimental” setting, I test for the causal, long-run impacts of receiving a large industrial plant for idiosyncratic reasons. I find that post-war manufacturing output, employment, and payroll in the recipient county are markedly higher in recipient counties than in similar, untreated counties. While a short-run increment to manufacturing activity is not surprising, I also find that these effects are remarkably persistent over the course of five decades. These effects in the manufacturing sector carry over to aggregate labor-market outcomes, such as total employment and median wages. Yet, wages in other sectors in the recipient counties remain unaffected.

While these economically significant findings very plausibly can be interpreted as causal, it is nonetheless unclear whether there are impacts on economic actors beyond those who actually operate the former war
plant, whether the war plant actually created new high-wage work opportunities for local residents, or whether such local gains came at the expense of other regions that would have received more post-war investment were it not for the construction of the war plants. I am engaged in ongoing work that will use new micro-level data to better answer these outstanding questions.

Nonetheless, these findings are important. To date, little evidence has been proffered that suggests a clear local link between wartime spending and post-war economic performance. I have shown that a clear statistical link—a link that is very likely causal—does indeed exist when one focuses on a specific type of spending: investment in durable productive infrastructure and capital. Having adopted this focus, I find that short-run strategic considerations during the war had important impacts on the economic geography of the United States in the postwar. Even though the sole imperative that drove public investment was to win the war, these durable plants nonetheless left a distinctive mark on the longer-run performance of regional economies. Thus, even though construction of these plants was planned with little regard to industrial policy, place-based labor policy, or macroeconomic policy, one might glean lessons about the costs and benefits of these policy levers. My hope is that future research on mechanisms can garner lessons for policy makers aiming to spur growth in underdeveloped regions, provide good-paying work opportunities for low-skilled workers in specific regions, and to boost aggregate productivity.
Works Cited


Figures and Tables

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

![Distribution of Capital Expenditures across Plants](image-url)
Figure 2: Location of Large Publicly-Financed Plants
Figure 3: **Manufacturing Outcomes: Reweighting Estiates of TOT**

(a) Log Manufacturing Value Added
(b) Log Average Wage of Production Workers
(c) Log Production Workers
(d) Log Establishments
(e) (Log) Manufacturing Capital Expenditures

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
Figure 4: Broader Employment Outcomes: Reweighting Estimates of TOT

(a) Log Total Population

(b) Log Total Employment

(c) Log Retail Employment

(d) Log Service Employment

(e) Share of Employment in Manufacturing

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
Figure 5: **Broader Wage/Earnings Outcomes: Reweighting Estimates of TOT**

(a) Log Retail (Annual) Wage

(b) Log Services (Annual) Wage

(c) Log Median Family Income

(d) Log Median Housing Value

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
Table 1: New Public Plants in Total War Expansions

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<th>Category</th>
<th>Cost ($Mil)</th>
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<td>(With Pre-war Mfg Data)</td>
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Table 2: Covariate Balance: Treatment (N=81) Versus Control (N=1,039) Mean Differences

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<th>$t$-stat</th>
<th>Reweighted $\Delta \bar{Y}$</th>
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Balancing Variables Excluded from Score

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</tr>
<tr>
<td>1940 Log Avg Production Wage</td>
<td>0.13</td>
<td><strong>3.19</strong></td>
<td>-0.02</td>
<td><strong>-0.91</strong></td>
</tr>
<tr>
<td>1939 Per Capita Retail Sales</td>
<td>113.36</td>
<td><strong>3.85</strong></td>
<td>-1.83</td>
<td><strong>-1.12</strong></td>
</tr>
<tr>
<td>1939 Retail Wage</td>
<td>0.07</td>
<td><strong>2.91</strong></td>
<td>-0.01</td>
<td><strong>-1.18</strong></td>
</tr>
<tr>
<td>1939 Service Wage</td>
<td>0.13</td>
<td><strong>3.92</strong></td>
<td>0.00</td>
<td><strong>-0.12</strong></td>
</tr>
<tr>
<td>1940 % Black</td>
<td>-0.01</td>
<td>-0.80</td>
<td>0.00</td>
<td><strong>0.54</strong></td>
</tr>
<tr>
<td>1940 Log Median Contract Rent</td>
<td>0.14</td>
<td><strong>3.23</strong></td>
<td>0.00</td>
<td><strong>-0.10</strong></td>
</tr>
<tr>
<td>1940 Log Male Clerical Emp</td>
<td>0.80</td>
<td><strong>6.12</strong></td>
<td>0.01</td>
<td><strong>0.12</strong></td>
</tr>
<tr>
<td>1940 Log Female Clerical Emp</td>
<td>0.81</td>
<td><strong>5.95</strong></td>
<td>0.00</td>
<td><strong>0.02</strong></td>
</tr>
<tr>
<td>1940 Log Craftsmen</td>
<td>0.74</td>
<td><strong>5.95</strong></td>
<td>0.00</td>
<td><strong>-0.03</strong></td>
</tr>
<tr>
<td>1940 Log Craftswomen</td>
<td>0.79</td>
<td><strong>5.09</strong></td>
<td>0.01</td>
<td><strong>0.08</strong></td>
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<tr>
<td>1940 Log Male Operatives</td>
<td>0.74</td>
<td><strong>5.73</strong></td>
<td>0.05</td>
<td><strong>0.70</strong></td>
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<tr>
<td>1940 Log Female Operatives</td>
<td>0.75</td>
<td><strong>4.12</strong></td>
<td>0.03</td>
<td><strong>0.34</strong></td>
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### Table 3: Point Estimates, Conditioning on 1930 and 1940 Observables

<table>
<thead>
<tr>
<th></th>
<th>Log Value Added</th>
<th></th>
<th></th>
<th>Log Production Workers</th>
<th></th>
<th></th>
<th>Log Production Wage</th>
<th></th>
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</thead>
<tbody>
<tr>
<td>$\beta^T_{t}$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\rho_{t}$</td>
<td>0.416***</td>
<td>0.275***</td>
<td>0.189***</td>
<td>0.255***</td>
<td>0.217***</td>
<td>0.094</td>
<td>0.100***</td>
<td>0.070***</td>
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<tr>
<td>$se$</td>
<td>(0.079)</td>
<td>(0.063)</td>
<td>(0.067)</td>
<td>(0.047)</td>
<td>(0.052)</td>
<td>(0.058)</td>
<td>(0.023)</td>
<td>(0.018)</td>
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<tr>
<td>N</td>
<td>1,483</td>
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<td>1,481</td>
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<td>1,483</td>
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<tr>
<td>$R^2$</td>
<td>0.925</td>
<td>0.863</td>
<td>0.780</td>
<td>0.929</td>
<td>0.877</td>
<td>0.810</td>
<td>0.824</td>
<td>0.698</td>
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</tbody>
</table>

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and all three outcome years listed. Each point estimate is the coefficient on the Treatment dummy in a separate regression corresponding to the outcome in a given year. A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers. These specifications also control for the 1939 or 1940 levels of all available outcome variables used in the paper.

### Table 4: Point Estimates, Conditioning on 1930 and 1940 Observables

<table>
<thead>
<tr>
<th></th>
<th>Log Population</th>
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<th>Log Employment</th>
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<th>Log Median Family Income</th>
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<tbody>
<tr>
<td>$\beta^T_{t}$</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\rho_{t}$</td>
<td>0.051***</td>
<td>0.069**</td>
<td>0.041</td>
<td>0.041</td>
<td>0.102***</td>
<td>0.059</td>
<td>0.030*</td>
<td>0.035***</td>
<td>0.005</td>
</tr>
<tr>
<td>$se$</td>
<td>(0.018)</td>
<td>(0.033)</td>
<td>(0.045)</td>
<td>(0.029)</td>
<td>(0.036)</td>
<td>(0.051)</td>
<td>(0.016)</td>
<td>(0.012)</td>
<td>(0.015)</td>
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<tr>
<td>N</td>
<td>1,483</td>
<td>1,483</td>
<td>1,483</td>
<td>1,483</td>
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<td>1,483</td>
<td>1,483</td>
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<tr>
<td>$R^2$</td>
<td>0.989</td>
<td>0.947</td>
<td>0.905</td>
<td>0.988</td>
<td>0.951</td>
<td>0.898</td>
<td>0.924</td>
<td>0.838</td>
<td>0.742</td>
</tr>
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</table>

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and all three outcome years listed. Each point estimate is the coefficient on the Treatment dummy in a separate regression corresponding to the outcome in a given year. A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers. These specifications also control for the 1939 or 1940 levels of all available outcome variables used in the paper.
Appendix A: Supplemental Figures

Figure A.1: **County Treatment Description**

- (a) Treatment Counties
- (b) Matched Control Counties
- (c) Pre-War Log Manufacturing Value Added
- (d) Wartime vs. Pre-War Manufacturing
Figure A.2: County-Level Distribution of Spending on Large Publicly-Financed Plants

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

Figure A.3: Raw Difference in Means of Treatment Group and All Other Counties

(a) Log Manufacturing Value Added, Difference
(b) Log Average Wage of Production Workers, Difference
(c) Log Employment of Production Workers, Difference

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Each point estimate is the raw difference and means of the outcome variable between treatment groups and all other counties in a given year.
Figure A.4: Manufacturing Event Studies, Privately-Financed Plant “Treatment”

(a) Log Manufacturing Value Added
(b) Log Average Wage of Production Workers
(c) Log Production Workers
(d) Log Establishments

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. Each point estimate is the coefficient on the Treatment dummy in a separate regression corresponding to the outcome in a given year. A county is “treated” if it was one of the 100 counties with the most per-1939-employee spending on new war plants that were privately financed at least in part. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers.
Figure A.5: **Treatment Intensity Heterogeneity**: *Darker red → Higher Intensity*

(a) Log Manufacturing Value Added  
(b) Log Average Wage of Production Workers  
(c) Log Employment of Production Workers

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. There are 192 counties in the analysis sample with at least one large publicly-financed plant; these are divided into four equal-sized bins corresponding to treatment intensity quartiles (that is, quartiles of spending on large public-financed war plants per 1939 employee). Each point estimate is the coefficient on an intensity-quartile dummy, with all four quartile effects estimated simultaneously. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers.