The Long-Term Returns to University Infrastructure:

Evidence from Post-WWII Military Surplus Property Transfers

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Abstract

This paper studies the long-run effects of physical capital investments on universities and the geography of science. After World War II, the federal government transferred surplus military real property for educational purposes to over half of U.S. colleges and universities, at a cost of roughly $660 million in 1947 dollars. Exploiting quasi-random variation from wartime military siting and logistical constraints on surplus reallocation, we find that recipient institutions experienced substantially higher survival rates; sizable enrollment gains that faded within two decades; and a persistent rise in scientific publications over six decades. Research quality increased far more than quantity: citations per researcher rose sharply while publications per researcher barely changed, indicating that improved infrastructure raised the impact of the researchers institutions attracted and retained rather than scaling up existing programs. Scientific production effects persist through 2010, long after enrollment convergence and the depreciation of the original structures.

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1 Introduction

The United States spends over $700 billion annually on higher education (National Center for Education Statistics, 2023), and a growing share of federal science policy is channeled through universities. Universities are physical places, yet governments have remarkably little causal evidence on a basic question: does investing in the physical infrastructure of universities improve their research output, and if so, for how long? The question matters because the production of science and innovation is geographically concentrated, and that concentration is persistent (Andrews and Whalley, 2022; Macleod and Urquiola, 2021). If physical capital investment permanently shifts a university's research trajectory, then infrastructure spending is a lever for reshaping the geography of innovation; if the effects are transitory, the billions spent on campus construction may expand access to education without altering the long-run distribution of knowledge creation.

Answering this question has proved difficult because capital investments in universities are endogenous: institutions that produce more research attract more funding, better faculty, and higher-quality students, making it hard to separate cause from effect. The existing causal literature has instead relied on abrupt shocks to the science production function: to research funding (Gross and Sampat, 2023) and to the human and physical capital embodied in scientists and their institutions (Borjas and Doran, 2012; Waldinger, 2016). But the deliberate *provision* of physical infrastructure, a basic and costly input to the university production function, has received comparatively less attention from causal studies.

This paper exploits one of the largest natural experiments in the history of American higher education. Between 1945 and 1947, the U.S. federal government transferred surplus military property (classrooms, laboratories, dormitories, and housing) to 939 colleges and universities, representing 54% of all institutions in the country. The estimated value of this policy was between $608 and $706 million in 1947 dollars, equivalent to roughly 46 to 53% of federal G.I. Bill education expenditures and 52 to 60% of aggregate institutional spending at the time. Despite its scale, this episode has not been studied in the economics literature.

Our identification strategy exploits two institutional features of postwar demobilization. First, the wartime military installations that contained the surplus real property were sited for strategic and tactical reasons largely unrelated to local educational conditions. In 1941, for example, the Army Air Force assigned 110 of 130 evaluation points for

potential base sites to flying weather, terrain, and proximity to training ranges (Shaw, 2004). Second, the transfer of surplus property to universities was logistically constrained: structures could only be moved within a "feasible distance" of the recipient institution, resulting in only 57% of institutional applications being approved (Hollis, 1949). The combination of quasi-random military siting and distance-based eligibility constraints generates variation in the probability and intensity of receiving surplus property that is plausibly independent of prewar institutional characteristics. Consistent with this claim, we find few systematic differences between treated and untreated counties across a wide range of 1940 demographic, economic, and educational characteristics.

To trace the effects over eight decades, we build a novel annual panel of the universe of U.S. institutions from 1928 to 2010, digitizing archival transfer records from two Record Groups at the National Archives and linking them to historical enrollment data and modern bibliometric records from OpenAlex. We collapse the panel to biennial frequency for estimation; the main estimation sample contains 1,742 institutions, of which 1,284 are four-year, with measures of enrollment and of STEM publications and citations in physics, biology, chemistry, and medicine.

A one-time infrastructure shock had large and lasting effects on the research function of universities. Recipient institutions were substantially more likely to survive the postwar decades: fifteen years out, the survival advantage was roughly twelve percentage points for facility-transfer recipients and twenty-two percentage points for housing-transfer recipients. Enrollment rose sharply and stayed elevated for roughly two decades before converging. Most important, research output rose and, unlike enrollment, kept rising: treated universities produced markedly more STEM publications and citations, and these gains *persisted through 2010*, long after enrollment had converged and the original structures had depreciated.

Decomposing the research response into quantity and quality yields the paper's central mechanism finding. Scaling output by the number of researchers, we find that publications per researcher barely move, whereas citations per researcher rise substantially: the transfers raised the impact of the average researcher far more than the number of researchers or their raw output. Two complementary forces drive this quality gain. First, a recruitment channel: treated institutions expanded their faculty, hiring more scientists and drawing more faculty transfers from other institutions after the war. Second, on top of this recruitment, a place effect: using an author-level movers design, we find that a given scholar's own citations rise by roughly one-third after moving to a treated institution, even though treated institutions did not selectively attract schol-

ars who were already more highly cited. The surplus infrastructure thus both helped institutions assemble faculty and made them more productive places in which to do research.

The two programs play distinct roles. Facility transfers, which supplied classrooms and laboratories, directly drive the enrollment and research gains and produce the larger per-dollar effect on scientific output. Housing transfers, which had no direct connection to research infrastructure, nonetheless raise research output as well, and they generate the largest gains in institutional survival. That housing improved scientific production, despite having no direct link to laboratories, points to a complementarity between residential and research capacity: research growth depended not only on instructional and laboratory space but also on the residential capacity to house a growing campus community and the faculty an institution sought to attract and retain.

The enrollment and research effects diverge sharply in how long they last. That enrollment advantages faded is unsurprising: as building costs normalized and other funding expanded, non-recipient institutions closed the physical-capacity gap. That research advantages persisted through 2010, by contrast, implies that a temporary infrastructure shock can set an institution on a permanently higher research trajectory, as early advantages in faculty and reputation compound long after the physical capital itself is gone. The geography of American science, our results suggest, is not immutable; it may require only a one-time push to permanently alter.

These findings contribute to three literatures and speak directly to a current policy debate. First, a growing literature studies the determinants of scientific production, emphasizing the roles of talent, education, access to frontier knowledge, and research funding (Babina et al., 2023; Bianchi and Giorcelli, 2020; Iaria et al., 2018; Moser et al., 2014). We show that a university's physical infrastructure is itself an important, and previously hard-to-isolate, determinant of long-run scientific capacity. Closest to our question is Waldinger (2016), who finds that the wartime destruction of physical capital had only short-lived effects on German science whereas the loss of human capital persisted; our results are the mirror image, in that a one-time shock to physical capital generates six decades of persistent research gains, but does so only through the human capital it helps assemble. Consistent with that reading, the gains operate primarily through research quality rather than quantity, via a faculty recruitment channel and, on top of it, a place-based agglomeration channel in which stronger physical capital makes an institution a more productive place to do research.

Second, we contribute to work on the long-run effects of WWII-era federal invest-

ments, which shows that wartime spending, mobilization, workforce training, and health interventions reshaped individual and economic development for decades (Ferrie et al., 2025; Garin and Rothbaum, 2025; Giorcelli, 2024; Herbst, 2017; Lleras-Muney et al., 2025; Mitrunen, 2025). Closest to our setting is Gross and Sampat (2023, 2025), who show that the war's surge in federal R&D funding jump-started the modern U.S. innovation system and its biomedical research enterprise; we study a different wartime lever, the reallocation of surplus *physical capital* to universities, and trace its effects on the universities themselves. More broadly, we identify higher education as a distinct channel through which surplus wartime assets generated persistent civilian returns, rather than the employment, health, or industrial-production channels emphasized elsewhere.

Third, we speak to the literature on the growth and survival of American universities and their spillovers to surrounding communities (Andrews, 2023; Cutler and Glaeser, 2026; Goldin and Katz, 1999; Howard et al., 2024; Kantor and Whalley, 2019; Lerner et al., 2025; Macleod and Urquiola, 2021). Our findings complement demand-side accounts of the postwar expansion centered on the G.I. Bill (Abramitzky et al., 2025; Althoff and Szerman, 2025; Bound and Turner, 2002; Stanley, 2003) by providing the supply-side counterpart. They also show that the returns extended beyond the campus: counties whose universities received larger transfers experienced lasting increases in local patenting. This connects to work on how place-based investments in knowledge infrastructure raise nearby innovation, whether through Carnegie libraries (Berkes and Nencka, 2025), federal laboratories (Helper et al., 2025), or NSF institutional grants (Cristelli, 2026). Relative to these, we isolate a one-time transfer of physical capital, rather than recurrent research funding, and identify the university itself as the institution through which the shock propagates into local invention.

Finally, and most directly relevant for policy, our findings speak to the design of modern place-based innovation initiatives such as the CHIPS and Science Act and the National Science Foundation's Regional Innovation Engines, which seek to broaden participation in the innovation economy by investing in universities outside traditional research hubs. Our evidence supports the premise that strategic investment can create lasting research capacity where little existed before, but with two qualifications. First, the returns operate less through the direct provision of research space than through the people that infrastructure helps an institution attract and retain. Second, because the surplus buildings were mass-produced to standardized designs and deployed at low cost across hundreds of campuses, place-based policy need not center on cutting-edge facilities: even no-frills, "unglamorous" infrastructure can meaningfully expand research

capacity when deployed at scale.

The paper proceeds as follows. Section 2 describes the historical setting. Section 3 presents the data construction. Section 4 develops the empirical strategy. Section 5 reports the main university-level results. Section 6 extends the analysis to spillovers to local innovation and individuals. Section 7 concludes.

2 Historical Setting

2.1 Universities, Innovation, and the Crisis of Physical Capacity

American universities are central to the nation's innovation system. They train the scientists and engineers who staff private-sector R&D laboratories, produce the basic research necessary for technological innovation, and anchor the surrounding regional economies (Andrews, 2023; Goldin and Katz, 1999; Howard et al., 2024). Yet the physical infrastructure of these institutions, which determines how many students can be trained and how much research can be conducted, has received little attention as a determinant of innovation capacity. The postwar period offers an unusually clear window into the role of physical capital, because a sudden and massive demand shock collided with binding supply constraints.

The Servicemen's Readjustment Act of 1944 (the G.I. Bill) provided up to four years of tuition and living stipends to returning veterans. Out of approximately 16 million WWII veterans, about 2.2 million received some form of postsecondary education through the program. By 1947, of the 2.3 million students enrolled in higher education institutions, 1.15 million, over half, were veterans. From 1946 to 1949, colleges faced a tidal wave of applications from both veterans and non-veterans, driven by the G.I. Bill and the latent demand for college education that had accumulated during the war years (1941–1945).

But colleges lacked the physical infrastructure to accommodate this surge. Classrooms were overcrowded, laboratory space was scarce, and dormitories were full. According to Hollis (1949), the rapid expansion in enrollment, combined with high building costs and shortages of labor and materials in the postwar economy, meant that the urgent need for additional space could not be met through ordinary construction channels. Every institution wanted more enrollment, but most did not have adequate infrastructure to support it. The bottleneck on higher education's expansion, and the corollary postwar growth of America's innovative capacity, was thus not a lack of demand for

higher education but a shortage of supply, sharply limited by physical capital.

2.2 Three Federal Transfer Programs

Congress responded with a series of programs that repurposed surplus military infrastructure for educational use. During the war, the federal government had constructed large numbers of temporary and prefabricated structures at military installations for military personnel and war production workers. Wartime construction technology had advanced the development of two broad types of relocatable real property: prefabricated structures (portable and demountable buildings) and temporary construction (structures that could be torn down and reassembled under standard instructions). According to the Federal Public Housing Authority (1946), trailers traveled across the country to supply temporary shelter, while demountable units were moved from one locality to another either in entirety (for example, via barges down rivers) or taken apart in panels and shipped by freight to be reassembled at their destinations. Three programs directed these assets to colleges and universities.

The **Surplus Property Act of 1944** (Public Law 457, 78th Congress) authorized the disposal of surplus government property, with public institutions given priority over private buyers (Cook, 1948). The War Assets Administration oversaw the transfer of surplus real property (buildings, land, and equipment) from decommissioned installations. The *1947 Directory of Surplus Real Property* documents over 2,700 surplus sites available for disposal nationwide. Suitable surplus real property was then turned over to the National Housing Agency and the Federal Works Agency for their respective programs described below. The majority of surplus came from Army troop training centers, Army and Navy airfields, and ordnance plants.

Public Law 269 (December 28, 1945) created the Veterans' Re-use Housing Program (VEHP), administered by the National Housing Agency. Under this program, surplus military housing, including temporary family dwellings and dormitories, was dismantled at military sites and re-erected on college campuses to house returning veterans and their families.

Public Law 697 (August 8, 1946) established the Veterans' Educational Facilities Program (VEFP), administered by the Federal Works Agency. This program transferred surplus military structures for instructional and research use, including classrooms, laboratories, and administrative buildings. In addition, many Army and Navy facilities that were currently unused and declared surplus were transferred to these programs and

made available to universities (Office of War Mobilization and Reconversion, 1946).

Together, these three programs transferred surplus to 939 institutions of higher education, representing 54% of all colleges and universities in the country. The fiscal magnitude was substantial. According to Russell (1951), the costs of disassembly, transport, and re-erection alone totaled approximately $274 million. Combining this with the estimated fair value of property transferred under both programs (Federal Public Housing Authority, 1946; Hollis, 1949), we estimate the value of the policy to be between $608 and $706 million in 1947 dollars. To put this figure in perspective, it was equivalent to roughly 46 to 53% of total federal expenditure on the education portion of the G.I. Bill and 52 to 60% of aggregate institutional spending in higher education at the time.[1] By any measure, this was one of the largest federal investments in the physical infrastructure of American universities.

Historical evidence suggests that public institutions received more assistance than private ones. The VEFP operated a priorities system in which "state governments and their instrumentalities" (including public colleges and universities) were given precedence over nonprofit private institutions (Cook, 1948). Even under VEHP, which had no formal priorities system, a contemporary survey of 300 colleges found that 30% of large private institutions and many smaller private colleges received no housing transfers, while almost all public universities received at least some federal housing (Hettinger, 1946).

3 Data

We construct an institution-level panel spanning 1928 to 2010 by combining newly digitized archival records on surplus real property transfers, historical enrollment statistics, and modern bibliometric data. This section describes each component.

3.1 Surplus Real Property Transfers

We digitize administrative records on federal surplus allocations to institutions of higher education from two record groups at the National Archives and Records Administration (NARA; Appendix Figure A1).

Surplus property. We digitize the *1947 Directory of Surplus Real Property* (Appendix Figure A2), which reports the name, location, wartime use, acreage, estimated cost, classification, and disposition status for each surplus site. These data characterize the

[1] Detailed calculations provided in Appendix.

broader spatial distribution of military surplus and allow us to account for distance-based measures of institutional exposure to surplus property. Two federal programs then reallocated surplus property to colleges and universities.

Housing transfers. From the *Management Directory of Veterans Re-use Housing Projects* (June 15, 1947), we extract the recipient institution, city, state, housing type (temporary family dwellings or dormitories), number of units allocated, number completed, and federal funds disbursed (Appendix Figure A3).[2]

Facility transfers. From records of the Veterans Educational Facilities Program, we recover the recipient institution, state, total dollar amount approved, and square footage of facilities transferred (Appendix Figure A4).[3] Appendix Figures A5 and A6 show a facility transfer and a housing transfer as deployed on campus, and Appendix Figure A7 shows further examples at the University of California.

3.2 Enrollment Panel

We build an enrollment panel from four historical sources. Prewar enrollment comes from the *Biennial Survey of Education* (1928–1940), which reports institution-level enrollment for the universe of U.S. colleges and universities at two-year intervals. For the early postwar period, we use *Fall Enrollment in Higher Education Institutions* (1947–1967), annual institution-level reports from the Office of Education. From the late 1960s onward, we draw on the *Higher Education General Information Survey* (HEGIS, 1968–1983) and its successor, the *Integrated Postsecondary Education Data System* (IPEDS, 1984–2010). For the University of California campuses, we supplement these national surveys with enrollment carefully gleaned for 1928–1965 from Stadtman (1968), *The Centennial Record of the University of California, 1868–1968.* Appendix Figure A8 shows sample pages from these enrollment sources.

Linking these sources across eight decades requires careful matching of institution names, accounting for mergers, closures, and name changes.[4] We geolocate institutions and surplus sites and assign them to counties using the crosswalks of the Census Place Project (Berkes et al., 2023).[5] The resulting annual panel observes the universe of U.S.

[2]Record Group 207: General Records of the Department of Housing and Urban Development, Library Collection of Agency Issuances, 1934–79, Boxes 2–4.

[3]Record Group 162: General Records of the Federal Works Agency, Information Records, Records Concerning the Bureau of Community Facilities, 1945–47, Box 10.

[4]We describe the matching procedure and report match rates in a data appendix, available upon request.

[5]We construct a place-to-county crosswalk from the Census Place Project files and use it to assign coordinates and counties to both institutions and surplus sites.

institutions from 1928 to 2010. Our main estimation sample contains 1,742 institutions, out of which 1,284 are four year institutions.

3.3 Scientific Production

We measure research output using institution-level bibliometric data from OpenAlex (Priem et al., 2022). For each institution in the panel, we collect metadata on all publications authored by researchers with contemporaneous U.S. affiliations in physics, biology, chemistry, and medicine, fields that account for the majority of scientific output at U.S. universities over the sample period. For each institution–year cell, we observe total publications and total citations in STEM and non-STEM fields. From the bibliometric data, we also construct an author-level panel which allows us to observe the institutions at which each author publishes in each year. This allows us to calculate the number of "new hires" and "transfer authors" publishing in each institution–year cell.

3.4 Patents

We measure local innovation using patent records from the Comprehensive Universe of U.S. Patents (Berkes, 2018), which geolocates the inventors on U.S. patents. Aggregating inventors to their counties, we construct annual counts of patents filed by inventors in each county over the sample period. We also build a citation-weighted count, summing the forward citations each patent subsequently receives, and a per-capita count that normalizes by county population; both are used in the spillover analysis (Section 6.1).[6] The county patent panel covers 857 counties.

3.5 Descriptive Statistics

Table 1 reports summary statistics at the 1940 baseline for the full sample of institutions and, separately, for four-year institutions. The median four-year institution enrolled roughly 430 students; the distribution is highly right-skewed, with a handful of large public universities enrolling over 30,000. Average STEM publication counts are low in the 1930s baseline, reflecting the early stage of American research universities, but grow substantially over the sample period.

[6]We construct the county–year patent counts and the citation-weighted counts from the CUSP citation files, and assemble annual county population from the decennial censuses for the per-capita measure.

Table 2 reports the composition of treatment across Census regions. In the full sample, 46% of institutions received no surplus transfers, 6.7% received housing only, 15.8% received facilities only, and 31.5% received both. Treatment rates were highest in the South (60.4% treated) and West (73.7%) and lowest in the Northeast (36.2%), reflecting the geographic distribution of military installations.

Figures 1 and 2 map the county-level geographic distribution of treatment by intensity and type, respectively, and Appendix Figure A10 shows the same treatment at the level of individual institutions. Treatment is spread across the country, with notable concentrations in the South, Midwest, and West, consistent with the location of wartime military bases in these regions.

4 Empirical Strategy

4.1 Estimating Equations

We estimate the effects of the surplus transfers at two levels of aggregation: the institution, where we observe survival, enrollment, publications, and citations, and the county, where we observe patenting.

We begin with the extensive margin of institutional survival, following Giorcelli (2019). We estimate the Kaplan–Meier survivor function separately for transfer recipients and non-recipients,

$$\hat{S}(t) \;=\; \prod_{t_j \le t} \left(1 - \frac{d_j}{n_j}\right), \tag{1}$$

where d_j institutions close at time t_j, measured in years since the transfers, out of the n_j still open just before it. We test equality of the survivor functions across groups with the log-rank statistic and summarize the treatment effect with a Cox proportional-hazards model,

$$h_i(t) \;=\; h_0(t)\exp(\theta\,\text{Treated}_i)\,, \tag{2}$$

in which $h_0(t)$ is the baseline closure hazard and $\exp(\theta)$ is the hazard ratio for recipients relative to non-recipients.

For the intensive-margin outcomes, our institution-level specification is a dynamic difference-in-differences design that traces the evolution of outcomes at treated versus

untreated institutions over time, conditional on the institution's survival:

$$\text{IHS}(\text{Outcome}_{it}) \;=\; \alpha_i + \nu_t + \sum_{\tau=-10}^{70} \beta_\tau \left(\text{Treated}_i \times \mathbf{1}\{\text{Years After Treatment}_{it} = \tau\}\right) + \varepsilon_{it} \tag{3}$$

where $\text{IHS}(\text{Outcome}_{it})$ denotes the inverse hyperbolic sine of enrollment, publications, or citations at institution i in year t; α_i and ν_t are institution and year fixed effects; and $\text{Years After Treatment}_{it}$ measures calendar time relative to an omitted base period. The coefficients $\{\beta_\tau\}$ trace the dynamic treatment effect. Under the identifying assumption that treated and untreated institutions would have evolved similarly absent the transfers, the pre-treatment coefficients $\{\beta_\tau : \tau < 0\}$ should be zero, and post-treatment coefficients capture causal effects. We estimate three variants of this specification: the *extensive-margin* specification defines Treated_i as a binary indicator for any surplus receipt; the *by-type* specification replaces it with separate indicators for housing-only, facility-only, and combined transfers; and the *intensive-margin* specification replaces it with the dose, $\log(1 + \text{construction dollars per 1940 student})$. Normalizing by size captures the idea that the same transfer is a larger shock to a small college than to a large university; we proxy institutional size by enrollment in 1940, the last prewar year, in the absence of any direct measure of pre-existing campus scale such as square footage. The $\log(1 + \cdot)$ form accommodates the zero dose at untreated institutions and the right skew in dollars per student. We apply the inverse hyperbolic sine following Gross and Sampat (2023): it coincides with the logarithm for enrollment, which is strictly positive, and accommodates the zero-valued school-years for publications and citations without discarding them. Because these outcomes are observed only while an institution is open, we code them as missing after exit rather than as zeros, so the intensive-margin estimates are measured among surviving institutions.

The omitted base period differs across outcomes for data reasons. For enrollment we use 1940, the last prewar year for which institution-level enrollment is recorded; to our knowledge, no systematic institution-level enrollment data exist for the war years. For publications and citations, which the bibliometric data record annually and without interruption, we use 1946, the year before the transfers begin. This choice has little bearing on the estimates: college enrollments collapsed during the war as men entered military service and many campuses were repurposed for military training, so the war years carry little identifying information, and both the pre-trends and the treatment effects are anchored on the prewar and immediate pre-transfer periods in either case.

Our county-level specification for patenting is the analogous event study, estimated by Poisson pseudo-maximum likelihood (PPML):

$$E[\text{Patents}_{ct} \mid \gamma_c, \lambda_t, \text{Treated}_c] \;=\; \exp\left(\gamma_c + \lambda_t + \sum_{\tau} \delta_\tau \left(\text{Treated}_c \times \mathbf{1}\{\text{Years After Treatment}_{ct} = \tau\}\right)\right) \tag{4}$$

where Patents_{ct} is the patent count in county c and year t, γ_c and λ_t are county and year fixed effects, and Treated_c is defined analogously to the institution-level treatment, either as an indicator for any surplus property in the county or, for the intensive-margin estimates, as the county dose $\log(1 + \text{construction dollars per 1940 student})$ aggregated across recipient institutions. PPML delivers the unit-invariant proportional effect advocated by Chen and Roth (2024) and is well suited to county patenting, which is dominated by no single unit. We prefer IHS for the institution-level outcomes but PPML for county patenting for the same underlying reason: scientific output is concentrated in a small number of large universities, so the school-level Poisson estimator, which weights observations by their fitted counts, is dominated by these institutions, whereas county patenting is not. We assess the school-level Poisson estimates and the sensitivity to alternative transformations in the appendix (Table 4, Figure A15), and we report the extensive-margin response, an indicator for any output, separately. Standard errors are clustered at the institution and county level, respectively.

4.2 Validity of the Identification Assumptions

Two features of the surplus transfer process are central to our empirical strategy.

Military siting was driven by strategic, not educational, considerations. The surplus real property originated primarily from military bases and ordnance plants, which tend to be located away from crowded city centers for strategic reasons. The Army Air Force's 1941 site selection criteria allocated 110 of 130 evaluation points to flying weather, terrain suitability, proximity to ranges and ground forces, cost, and strategic and tactical considerations, with only 20 points assigned to availability of local housing (Shaw, 2004). After the attacks on Pearl Harbor in December 1941, base construction accelerated further and site selection boards operated under severe time constraints, prioritizing operational requirements such as climate, soil quality, preparation costs, and degree of air congestion. The spatial distribution of surplus military property was therefore largely orthogonal to prewar educational conditions.

Surplus transfers were logistically constrained. Although prefabrication technology made moving buildings across the country a possibility, distance was a significant bar-

rier. Under VEFP, only 57% of institutional applications were approved for transfers of real property; the remainder received only personal property (equipment and supplies), because "the cost of moving and warehousing personal property is relatively low as compared to the cost of demolition, transportation and re-erection of buildings" (Russell, 1951). Ex post calculations from the program estimate that disassembly and transportation costs accounted for approximately one quarter of total transfer costs, making it necessary for suitable surplus property to be within "a feasible distance of the institutions" before a transfer could proceed (Hollis, 1949). The Federal Public Housing Authority (1946) similarly noted that transfers would only be cost-effective if "the distance moved is not excessive." Several universities, including Miami, Nebraska, and Wisconsin, took over facilities from nearby military airfields, using them on site (Hollis, 1949).

The interaction of these two features generates our identifying variation. Conditional on the quasi-random spatial distribution of military installations, proximity-driven eligibility constraints determined which institutions received surplus property and how much they received. Our identifying assumption is accordingly that, conditional on this quasi-random siting and the logistical constraints on postwar reallocation, the distribution of surplus property is uncorrelated with the determinants of local outcomes. We assess this assumption at each of the two levels of analysis.

At the institution level, identification rests on parallel trends: absent the transfers, treated and untreated institutions would have followed similar trajectories. This assumption is directly testable, and the pre-treatment coefficients of the dynamic event study are flat and statistically indistinguishable from zero for every outcome, as we document in Section 5, supporting a causal reading of the post-treatment path. The same pattern is visible in the raw data, without any regression adjustment: unconditional mean outcomes for treated and untreated institutions track each other closely before the war and diverge only afterward (Appendix Figure A11). Recipients do differ from non-recipients on some observable 1940 characteristics, being larger and more often four-year institutions, and these characteristics are associated with the per-student dose (Figure 3). To confirm that such differences do not drive the results, we go further and re-estimate on a matched sample that pairs recipients with observationally similar non-recipients, and separately reweight so that the dose is orthogonal to pre-treatment characteristics; the effects are robust to both adjustments (Section 5). Because these adjustments hold the observables fixed, they also guard against the concern that unobserved institutional quality, correlated with the observables, drives the estimates.

At the county level, treatment is in addition uncorrelated with observable prewar characteristics. Figure 4 reports standardized differences in 1940 county characteristics between treated and untreated counties, separately for any treatment, VEFP facility transfers, and VEHP housing transfers, including population, educational attainment, log population, racial composition, employment, and age–sex composition. Across all three treatment definitions and all characteristics the differences are small and statistically indistinguishable from zero, so at the county level treatment assignment is orthogonal to observable characteristics as well as to pre-treatment trends.

5 University Development

In this section we examine outcomes at the university level. We begin on the extensive margin, institutional survival, and then turn to teaching and research capacity: enrollment, publications, and citations.

5.1 Extensive Margin: Institutional Survival

We begin with the most fundamental margin of institutional success: whether surplus transfers helped universities survive. Universities are among the most durable organizations in history (Cutler and Glaeser, 2026), yet survival was far from assured for the many small colleges that faced existential financial pressures in the postwar period, and the infusion of federal surplus infrastructure could plausibly have helped some avoid closure.

Institutions that received surplus transfers were substantially more likely to survive the postwar decades than those that did not, and the gap widens with time. Figure 5 plots Kaplan–Meier survival functions separately for facility-transfer and housing-transfer recipients against non-recipients, with the horizontal axis measuring years since the transfers. In the first years the difference is modest: five years out, facility recipients survive at 94 percent and housing recipients at 98 percent, against 85 percent for non-recipients. Rather than closing as institutions matured, the gaps widen over the following decade. By fifteen years, 85 percent of facility recipients and 96 percent of housing recipients remain open, compared with 74 percent of non-recipients, survival advantages of roughly twelve and twenty-two percentage points.[7] The advantage is

[7]The log-rank test rejects equality of the survivor functions between recipients and non-recipients for both the facility and the housing program.

largest for housing, most plausibly because housing was the binding constraint on many institutions' ability to accommodate the postwar enrollment surge.

A Cox proportional-hazards model confirms these magnitudes. Relative to non-recipients, the closure hazard falls by roughly three-quarters for institutions receiving any transfer, by half for facility-only recipients, and by more than four-fifths for housing-only recipients; all three reductions are statistically significant.

5.2 Teaching Capacity: Enrollment

Enrollment is our measure of teaching capacity. We estimate the enrollment response on the full sample of colleges, both two-year and four-year, applying the inverse hyperbolic sine transformation and coding enrollment as missing once an institution exits, so that closures do not enter as zeros. Figure 6 (Panel a) reports the event-study estimates. Treated and untreated institutions follow parallel prewar trajectories, after which enrollment at treated institutions rises sharply and remains elevated for roughly two decades before the two groups converge. Over these two decades, a larger per-student construction dose raises enrollment significantly, though the effect is modest relative to the research response documented below.

The enrollment dynamics also speak to a natural concern: that the gain merely reflects the transitory demand of the G.I. Bill, as returning veterans crowded into the institutions best able to accommodate them. Two facts weigh against a pure veteran-demand reading. First, aggregate enrollment in our panel reproduces the national postwar pattern of a sharp late-1940s surge and an early-1950s dip as the veteran cohort passed through and graduated (Snyder, 1993), confirming that the digitized series captures true enrollment dynamics rather than a measurement artifact. Second, a shock operating only through veterans would have dissipated by the mid-1950s, yet the treated advantage persisted for roughly two decades and was increasingly carried by women rather than veterans (Section 5.4); controlling directly for county wartime mobilization leaves the estimates essentially unchanged (Appendix Table 6). The transfers created durable instructional capacity that outlived the cohort that first filled it.

The response is concentrated in four-year institutions. Estimating separately by institution type, four-year colleges account for essentially all of the enrollment gain, while two-year colleges show no effect that is statistically distinguishable from zero. Four-year institutions faced the largest veteran enrollment pressures and used the transfers to expand instructional capacity; the smaller and less numerous two-year sector did not

respond. The gain also traces entirely to the facility program: splitting the dose by program arm, only facility (VEFP) dollars raise enrollment, while the housing (VEHP) dose has no detectable effect (Figure 7, Panel a; Appendix Figure A14), indicating that enrollment was bound more by instructional and laboratory space, which every additional student requires, than by housing, which accommodates only the residential share of students who do not commute.

5.3 Research Capacity: Publications and Citations

Research output is our measure of research capacity. Our primary outcomes are STEM publications and citations, estimated in inverse hyperbolic sine on the four-year sample. Here the effects are large and, unlike enrollment, do not fade. Figure 6 (Panels b–c) reports the event studies: pre-trends are flat, after which both outcomes rise and continue growing through the end of the sample. Expressed as a per-student dose, a larger construction dose produces a substantial and highly significant increase in both STEM publications and STEM citations over the full horizon, with citations rising by more than publications. The effects estimated over the first two decades alone are considerably smaller, so that most of the research gain accrues in the longer run rather than immediately after the transfers. The research effects are larger and far more persistent than the enrollment effects, indicating that the transfers had a disproportionate and lasting impact on the research function of universities.

Which program drove the research gains is itself informative. Figure 7 (Panels b–c) decomposes the response by program arm. Facility (VEFP) dollars have the larger effect on research output throughout, as one would expect from transfers that supplied laboratories and classrooms. Yet housing (VEHP) dollars also raise research output significantly, despite having no direct connection to research infrastructure. This points against a simple laboratory-space story. The housing served veterans, students, and their families rather than laboratories, but by relieving an acute postwar housing shortage it helped institutions accommodate their growing communities and attract and retain the faculty on whom research capacity ultimately depended. Splitting recipients into mutually exclusive groups (facility-only, housing-only, and both) confirms the pattern: research output rises and persists through 2010 for all three arms, largest for institutions that received both programs and smallest for housing-only recipients (Appendix Figure A14).

The research gains reflect quality more than quantity. The publication and citation

responses could arise because treated institutions employ *more* researchers (a scale effect) or because their researchers are individually more *impactful* (a quality effect). To separate the two, we scale STEM output by the number of STEM authors and re-estimate (Figure 8). Publications per author are flat and statistically indistinguishable from zero, whereas citations per author rise substantially and grow over time. The transfers raised the *impact* of the average researcher without raising output per researcher.

This dissociation is itself a clue to the underlying mechanism. If the transfers had simply given a fixed set of scholars more space and equipment, we would expect each of them to publish more; instead, output per author is unchanged while impact per author jumps discretely after treatment. The natural reading is that the impact gain came not from making incumbent faculty more prolific but from the researchers treated institutions were able to attract and retain. Whether that is because they recruited scholars who were already more accomplished (selection) or because a given scholar became more productive once there (a place effect) is the question we take up in the mechanism analysis (Section 5.4). This connects to Waldinger (2016), who finds that physical-capital destruction had only short-run effects on scientific output while human-capital losses persisted: the surplus transfers operated primarily through the faculty they helped assemble (Macleod and Urquiola, 2021).

5.4 Mechanisms

Teaching. Two features of the enrollment response clarify the teaching-capacity channel (Figure 9). The dose-response is larger at public institutions than at private ones (Panel a), consistent with public colleges being more supply-constrained: they absorbed the bulk of the tuition-sensitive veteran surge and had less scope to finance their own expansion, so a given amount of surplus space relieved a more binding capacity constraint. And the composition of the enrollment gain shifts over time (Panel b): men's enrollment rises immediately after the war, when returning veterans dominated the student body, and then fades, whereas women's enrollment grows steadily and accounts for the persistent expansion. The transfers expanded instructional capacity that was filled first by veterans and increasingly by women. Appendix Figure A12 shows the corresponding raw enrollment series by gender.

Research. The research response operates through faculty recruitment (Figure 10). The publication effect is broad across fields: STEM and non-STEM publications rise in parallel (Panel a), so the gains are not confined to laboratory science. Appendix

Figure A13 plots the corresponding raw publication series for all fields and STEM. The channel is hiring: treated institutions recruited substantially more STEM faculty after the war (Panel b) and drew more faculty transfers from other institutions (Panel c). This speaks to the per-author results in Figure 8: the jump in citations per author with no change in publications per author reflects the impact of the researchers treated institutions were able to attract and retain. Whether that operated through *selection*, hiring scholars who were already more accomplished, or through a *place* effect, making a given scholar more productive once there, is the question we take up next. Surplus infrastructure, including housing, allowed institutions to attract and retain the researchers who generated the persistent research advantage.

Complementary inputs. The transfers raised output only where the institution had the students and faculty to use the new capacity. Figure 11 splits the dose-response by whether the institution's 1940 educational expenditure was above or below the median. Both groups respond similarly in the first postwar years, but they then diverge sharply: at better-resourced institutions enrollment stays elevated and both publications and citations grow steadily through the 1960s, whereas at capital-poor institutions the enrollment response fades toward zero and the research response plateaus. Physical capital on its own did little; its returns depended on the complementary human capital, the students and especially the faculty, that better-resourced institutions could attract and retain. In the language of the production function, physical and human capital are *complements*: the surplus capital raised output only in combination with the people needed to use it, so the returns to infrastructure were largest exactly where the complementary human capital was already in place. This is the mirror image of Waldinger (2016), who finds that it was the loss of human capital, not physical capital, that drove the long-run decline of German science; here physical capital matters, but only through the human capital it helps assemble. It is also the same recruitment channel documented above, viewed from a different angle: the surplus infrastructure paid off where it could be staffed and filled.

Selection versus place. The recruitment channel could work in either of two ways: treated institutions might have attracted scholars who were *already* more accomplished (selection), or a given scholar might have become more productive after arriving (a place, or agglomeration, effect, as in "moving to a stronger center raises output"). To separate the two, we build an author-level panel from the publication data, with one observation per STEM author a and year t recording the author's primary institution and output, and estimate two specifications. The first is an event study among scholars who change

institution,

$$\text{IHS}(y_{at}) \;=\; \alpha_a + \nu_t + \sum_{k \neq -1} \beta_k \left(\text{TreatedDest}_a \times \mathbf{1}\{k_{at} = k\}\right) + \varepsilon_{at}, \tag{5}$$

where α_a and ν_t are author and year fixed effects, k_{at} is time relative to the author's move, TreatedDest_a indicates that the destination institution received surplus property, and the year before the move ($k = -1$) is the omitted reference. Because α_a holds the scholar fixed and the comparison group is scholars who move to non-recipient institutions, the coefficients $\{\beta_k\}$ identify the effect of the destination being a treated institution, net of the generic effect of moving; standard errors are clustered at the author level. The second is a two-way author-by-institution decomposition in the spirit of Abowd et al. (1999),

$$\text{IHS}(y_{at}) \;=\; \alpha_a + \psi_{j(a,t)} + \nu_t + \varepsilon_{at}, \tag{6}$$

in which the institution effect ψ_j, identified from scholars who move between institutions, measures how productive a place institution j is; we then relate $\{\psi_j\}$ to treatment across institutions.

Consider selection first (Table 3, Panel A). Among scholars who change institution, those moving to treated institutions carry *no higher* a pre-move citation record than those moving to non-recipients, and if anything a lower one, with a similar pattern for pre-move publications. Treated institutions did not build their advantage by poaching established stars; if anything, they recruited scholars with thinner prior records, so the subsequent rise in those scholars' output is not an artifact of whom they hired.[8] This level difference does not threaten the design: because the movers specification absorbs author fixed effects, permanent differences in scholar productivity are differenced out, and identification instead rests on the flat pre-move trends in Figure 12, which show the two groups on parallel trajectories before the move. The place effect is where the action is (Figure 12). Estimating Equation (5), a researcher's own citations rise by roughly one-third in the decade after moving to a treated institution and stay elevated, on flat pre-move trends; own publications rise far less, so the gain is concentrated in impact rather than volume, the same signature as the per-author results.

The decomposition in Equation (6) corroborates this (Table 3, Panel B): treated

[8] A broader hiring comparison points the same way: among all new STEM hires in the postwar period, those joining treated institutions are no more cited than those joining untreated institutions, so the quality gain does not reflect selection at the hiring gate.

institutions carry a higher citation "place premium" ψ_j, if imprecisely estimated,[9] even though differences across individual scholars (α_a) account for most of the variance in output.[10] The persistent research advantage thus reflects less *who* treated institutions hired than the fact that the surplus infrastructure made them more productive *places* to do research, consistent with evidence that a scientist's output depends on the local research environment and the quality of nearby peers (Waldinger, 2012). This parallels Lerner et al. (2025), who use academics' moves across universities to attribute a substantial share of the geographic variation in commercialization spillovers to place-specific rather than person-specific factors.

5.5 Long-Run Persistence

A central finding of the paper is the divergent persistence of enrollment and research effects. Figure 13 extends the analysis through 2010 and reveals that enrollment advantages had largely faded by the end of the sample period, consistent with non-treated institutions eventually closing the physical capacity gap through their own construction programs. Research advantages, by contrast, persisted through 2010: treated institutions continued to produce significantly more publications and receive substantially more citations than their untreated counterparts, even six decades after the original transfers.

This persistence is difficult to explain through physical capital alone, as the surplus structures had long since depreciated or been replaced. The mechanism analysis points instead to the place effect documented above: by turning treated institutions into more productive environments for research, the transfers set in motion self-reinforcing dynamics of institutional reputation and faculty quality. Once surplus infrastructure enabled treated institutions to establish productive research departments, these advantages compounded over time through faculty recruitment, hiring networks, doctoral training, and funding success, a dynamic consistent with the "virtuous circles" described by Macleod and Urquiola (2021). The implication is that a one-time infrastructure investment can permanently shift an institution's position in the research hierarchy, even after the physical capital itself is gone.

[9] The institution fixed effects are identified only from scholars who move between institutions; with limited mobility across the connected set they are estimated imprecisely, which biases the estimated place premium toward zero (Andrews et al., 2008). We therefore read the two-way decomposition as corroboration and treat the movers event study, Equation (5), as the primary evidence for the place effect.

[10] A specification that instead compares movers to non-movers (stayers) yields a much larger apparent effect, but it conflates the destination with the generic effect of moving, since scholars relocate at career peaks, and so serves only as an upper bound.

5.6 Robustness Checks

We summarize four sets of robustness checks here and report the supporting exhibits in the appendix.

Refining the control group. Because recipient institutions are larger and more often four-year (Figure 3), the school-level estimates could in principle reflect selection on these observable characteristics rather than the transfers themselves. We address this in two ways. First, we match each recipient to observationally similar non-recipients by 1:1 nearest-neighbor propensity matching on 1940 characteristics (enrollment, faculty, expenditure, gender composition, control, institution type, and distance to the nearest surplus site), which reduces the mean standardized covariate difference from 55% to 7%. Second, because the intensive-margin treatment is continuous, we reweight by the generalized propensity score so that the per-student dose is orthogonal to these characteristics. The estimates survive both adjustments (Appendix Figure A16). Reweighting by the generalized propensity score leaves the publication dose-response essentially unchanged and the citation dose-response, if anything, slightly larger; on the matched sample the effects attenuate but remain significant. The enrollment response survives in the medium run and attenuates over the full horizon, consistent with the convergence documented above. Because both adjustments hold the per-student dose fixed against institutional size and type, we read the matched-sample estimates as conservative bounds; that the research effects persist under them indicates they are not an artifact of larger institutions receiving larger transfers.

Alternative treatment definitions. The results do not depend on how we scale the dose. Measuring intensity by allocated square feet per 1940 student rather than construction dollars, or by discrete quartiles of the dose rather than its continuous value, yields the same pattern: a flat pre-trend followed by a monotone, dose-increasing rise in research output, with the largest effects in the top quartile (Appendix Figure A17).

Alternative specifications. Within the log-like family the estimates are nearly identical under the inverse hyperbolic sine and $\log(1+y)$. School-level Poisson is the exception, but we show it is driven by a handful of the largest universities and fails the parallel-trends test, whereas the county patent results, which are dominated by no single unit, are stable under Poisson (Appendix Table 4 and Figure A15).

Local educational demand. A final concern is that the transfers were not plausibly exogenous but instead flowed to the places that needed them most, so that our estimates capture local educational demand, in particular the postwar influx of veterans, rather

than the transfers themselves. If surplus property and construction funds were directed to counties with more returning servicemen, treatment would be correlated with a local demand shock and could not be read as causal. To address this, we gather county-level WWII enlistment data (Ferrara, 2024) and re-estimate the main specification adding county enlistees interacted with year, which lets counties with different wartime mobilization follow different trajectories. Allowing for this differential demand leaves the dose-response essentially unchanged for all three outcomes, enrollment, STEM publications, and STEM citations alike (Appendix Table 6). Differential local demand tied to wartime mobilization does not account for the results.

6 Beyond the University: Spillovers to Local Innovation and Opportunity

The preceding results establish that surplus transfers permanently raised university research output and temporarily expanded enrollment. A natural next question is whether these institutional gains remained confined to campuses or spilled over to the surrounding community and the individuals who live in it. We present evidence on the first of two channels, spillovers to local innovation as measured by county patenting, and outline our ongoing work on spillovers to individual opportunity in treated communities.

6.1 Spillovers to Local Innovation

Universities affect local economies through multiple channels: they supply human capital to nearby firms, generate knowledge spillovers through faculty consulting and technology transfer, and anchor agglomeration economies that attract innovative enterprises (Andrews, 2023; Kantor and Whalley, 2019). Recent evidence from Helper et al. (2025) shows that postwar federal national laboratories, sited mostly outside existing innovation hubs, generated lasting boosts in patenting by nearby inventors and broad-based local economic gains. More broadly, publicly funded R&D generates sizable innovation spillovers beyond its direct recipients, raising private-sector patenting nearby (Azoulay et al., 2019; Myers and Lanahan, 2022). Whether surplus-induced university research gains transmitted through similar channels is an open empirical question.

We measure local innovation by the count of patents filed by inventors in a county, drawn from the Comprehensive Universe of U.S. Patents (Berkes, 2018), and estimate the county-level event study of Equation (4), with the treatment given by the county

dose $\text{Dose}_c = \log(1 + \text{construction dollars per 1940 student})$ aggregated across recipient institutions. As explained in Section 4, we estimate it by Poisson pseudo-maximum likelihood, since county patenting, unlike school-level output, is dominated by no single county; we report inverse hyperbolic sine and $\log(1 + y)$ estimates as robustness and cluster standard errors at the county level. The identifying assumption parallels the university-level design: conditional on the quasi-random location of military surplus and county fixed effects, treated and untreated counties would have followed parallel patenting trajectories absent the transfers, an assumption supported by the county balance in Section 4.

Figure 14 reports the estimates. County patenting at more heavily treated counties tracks the untreated counties closely before the war, with pre-treatment coefficients flat and statistically indistinguishable from zero, and then diverges: the effect appears immediately as the transfers arrive in the mid-1940s, holds at a modest level through the early 1950s, and then accelerates and grows steadily through 2010. Over the full horizon, a one-log-dollar increase in construction spending per 1940 student raises county patenting by roughly nine percent, an effect precisely estimated and significant at the one percent level (Figure 14). The response is entirely on the intensive margin. A binary indicator for any surplus receipt is small and statistically indistinguishable from zero, so it is the *size* of the local university's transfer, not its mere presence, that drives local innovation. The effect is robust to the functional form, holding under both the inverse hyperbolic sine and $\log(1+y)$ transformations, and to measuring the dose in square feet rather than dollars (Appendix Table 7). It also survives citation-weighting the patents and expressing them per capita (Appendix Figure A18): the citation-weighted response is if anything slightly stronger than for raw counts, and patents per resident still rise, so the spillover reflects high-impact invention rather than mere local population growth. Because the early estimates are modest and imprecise, the pooled medium-run twenty-year effect is small and statistically insignificant, even though the response is visible from the outset; the bulk of the effect accumulates over the long run. Binning the dose into quartiles reveals the same gradient: the top quartile shows the largest and steadily growing effect while the lowest-dose counties decline, though the intermediate quartiles and the split by program arm are imprecise at the county level (Appendix Figure A19).

The dynamics are informative about the mechanism. Local inventive activity responds immediately to the transfers, but the initial effect is small; it then grows steadily over the following six decades to roughly nine percent, with most of the effect accumulating in the long run. An immediate but compounding response is what one would expect

if the transfers strengthened the local research university, which raised nearby inventive activity from the outset and then fed back through the graduates, faculty, and firms that a stronger university attracts (Helper et al., 2025). That the effect grows rather than dissipates, and that it operates on high-impact rather than marginal invention, is consistent with the self-reinforcing dynamics of a local innovation system, in which an initial agglomeration of research activity compounds through subsequent entry, hiring, and knowledge spillovers (Macleod and Urquiola, 2021), with early inventions seeding the follow-on innovation that builds upon them (Sampat and Williams, 2019). The county spillover is smaller than the on-campus effect, but like it, it is durable: a one-time infrastructure shock, mediated by the university it strengthened, leaves a lasting imprint on the innovative capacity of its region.

6.2 Spillovers to Individual Opportunity

A complementary question is whether surplus-induced university growth improved educational attainment and labor market outcomes for individuals in surrounding communities. Understanding these spillovers is central to evaluating the equity and efficiency of place-based higher education investments: if gains extend to nearby children and families who never attend the recipient institution, the social returns are substantially larger than those captured by the university alone.

We plan to pursue two empirical strategies, following Bailey et al. (2024). The county-by-birth-cohort design will compare outcomes for cohorts born in treated versus untreated counties, before and after the surplus transfers:

$$Y_{cb} = \lambda_c + \alpha_b + \delta_{s(c)b} + \beta \cdot (\text{Treated}_c \times \text{Post}_b) + X_c^{1940} \cdot b + \varepsilon_{cb} \tag{7}$$

where Y_{cb} denotes average educational or labor market outcomes for individuals born in county c in birth cohort b; λ_c, α_b, and $\delta_{s(c)b}$ are county, cohort, and state-by-cohort fixed effects; Treated_c indicates whether county c received surplus property; Post_b indicates cohorts that turned 18 on or after 1947; and X_c^{1940} is a vector of prewar county characteristics interacted with birth cohort. We will augment this with an individual-level specification that adds household controls (gender, race, parental education, income, 1940 urban residence):

$$Y_{icb} = \lambda_c + \alpha_b + \beta \cdot (\text{Post}_b \times \text{Treat}_c) + X_c^{1940} \cdot b + H_i'\theta + \varepsilon_{icb} \tag{8}$$

The central identification challenge for the individual-level estimates will be separating the supply-side effects of surplus infrastructure from the demand-side effects of the G.I. Bill. We plan to address this in two ways. First, surplus transfers generated *geographically localized* variation, determined by proximity to military installations, that is distinct from the *nationally uniform* G.I. Bill. Second, the analysis will focus on birth cohorts that turned 18 on or after 1947, individuals who were generally too young to have served in WWII and directly benefited from veteran education benefits (Bound and Turner, 2002).

Our proposal to obtain the restricted-use Census microdata required for this analysis has recently been approved, and the data are currently under construction; results from the individual-level analysis are forthcoming.

7 Conclusion

Between 1945 and 1947, the federal government transferred surplus military property worth roughly $660 million in 1947 dollars to more than half of American colleges and universities. Using newly digitized records on these transfers and a design that exploits the quasi-random wartime siting of the military installations from which the property came, we show that this one-time infrastructure shock reshaped the research trajectories of recipient institutions for six decades. Recipients were substantially more likely to survive the postwar decades and expanded enrollment for a generation; most durably, they produced more scientific research, with an advantage in publications and citations that was still widening in 2010, long after enrollment had converged and the surplus structures had depreciated or been replaced.

What makes this result informative is not that physical capital mattered but *how* it mattered. The transfers raised the citation impact of the average researcher far more than the number of researchers or the volume of their output, and two forces lie behind that quality gain. Treated institutions used the new facilities and housing to recruit more scientists and draw faculty from elsewhere; and, holding the individual scholar fixed, a researcher's own work drew markedly more citations after moving to a treated institution. Surplus infrastructure did not merely let existing faculty do more. It made treated universities more productive *places* to do research, and that place advantage, rather than the buildings, is what persisted. Consistent with this reading, the effect concentrated where institutions had the complementary students and faculty to use the new capacity, and it reached beyond the campus into lasting gains in local patenting.

These findings speak directly to the design of place-based innovation policy. Initiatives such as the CHIPS and Science Act and the National Science Foundation's Regional Innovation Engines seek to build lasting research capacity outside established hubs; our evidence suggests such investment can shift an institution's position in the research hierarchy permanently. The returns, however, accrue not through the physical lifespan of the laboratories, classrooms, and housing themselves, but through their enabling institutions to attract and retain the researchers who do the work. Infrastructure, on this evidence, is most valuable not as an end in itself but as a catalyst for the accumulation of talent.

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Figures and Tables

Table 1: Summary Statistics at 1940 Baseline

	Full sample		Four-year sample	
	Mean	SD	Mean	SD
Panel A: Institutional Characteristics				
Age (years since founding)	71	35	73	34
Public institution	0.315	0.465	0.279	0.449
Total enrollment	960	2,256	1,102	2,477
Male enrollment	579	1,550	669	1,707
Female enrollment	382	850	435	932
Total expenditures (thousands USD)	359	992	431	1,093
Panel B: Research Output (1930s Decade)				
STEM publications	6.8	45.3	9.1	52.6
STEM citations	154.3	1,349.4	208.7	1,568.2
Institutions	1,742		1,284	

Notes: Summary statistics at the 1940 baseline for the full sample of institutions and, separately, for four-year institutions. Enrollment and expenditures are measured in 1940; the number of institutions with non-missing data varies across rows, since 1940 enrollment, expenditure, and founding year are not available for every institution. Publications and citations are aggregated over the 1930–1939 decade using data from OpenAlex (Priem et al., 2022), restricted to physics, biology, chemistry, and medicine.

Table 2: Treatment Composition by Census Region

	Obs	Non-treated	Housing only	Facility only	Both
Full sample	1,742	0.460	0.067	0.158	0.315
Northeast	408	0.637	0.076	0.147	0.140
Midwest	538	0.474	0.082	0.151	0.294
South	583	0.396	0.057	0.141	0.407
West	213	0.263	0.042	0.244	0.451

Notes: Share of institutions in each treatment category by Census region, over the full sample of institutions in the panel. Housing refers to VEHP transfers; facility refers to VEFP transfers; indicates receipt of transfers from both programs. Rows sum to one across the four treatment categories.

Table 3: Selection versus Place: Author-Level Mechanism Estimates

	STEM publications	STEM citations
Panel A: Selection — pre-move record of movers		
Destination received any transfer	−0.047**	−0.197***
	(0.021)	(0.040)
Destination dose (dollars per 1940 student)	−0.009**	−0.036***
	(0.004)	(0.007)
Movers (author-years)	12,112	12,112
Panel B: Place premium and variance decomposition (Eq. 6)		
Place premium, treated institutions	−0.013	0.092
	(0.026)	(0.083)
Place premium, per dollar of dose	−0.004	0.006
	(0.005)	(0.016)
Var(author fixed effects)	0.18	1.76
Var(institution fixed effects)	0.02	0.22
Institution-effect share of variance	6.0%	5.7%
Institutions (connected set)	893	893

Notes: Author-level estimates from the STEM publication panel (one observation per author × year), restricted to the connected set of institutions linked by scholar mobility. *Panel A* tests for selection: among scholars who change institution, a mover's cumulative pre-move record (inverse hyperbolic sine of lifetime publications or citations up to the move) is regressed on whether the destination institution received a transfer, measured either as an indicator for any transfer or as the per-student construction dose, with year controls and standard errors clustered at the author level. Negative coefficients mean treated institutions did *not* attract more-accomplished scholars, so the place effect in Figure 12 does not reflect the poaching of established stars. *Panel B* reports the two-way author–institution decomposition of Equation (6): the "place premium" is the coefficient from regressing the estimated institution fixed effects on treatment across institutions, and the variance rows give the variance of output attributable to institution versus author effects. Institution effects are higher at treated institutions for citations, if imprecisely estimated, and explain only a small share of the variance of output relative to author effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

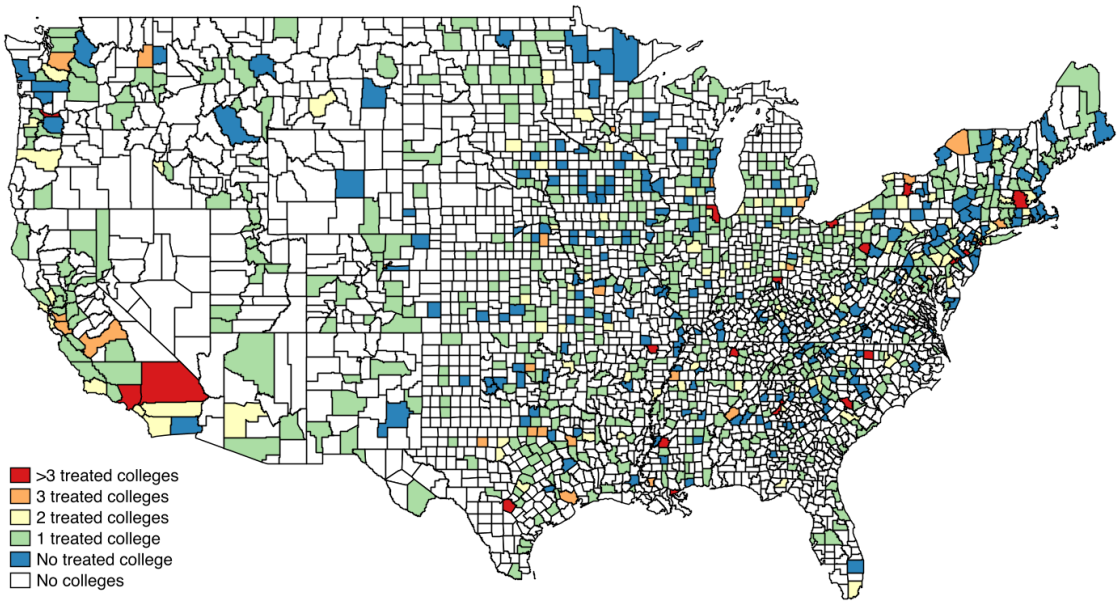


Figure 1: Treated Colleges by Treatment Intensity

Notes: Counties are shaded by the number of colleges that received surplus military property. White counties contain no colleges. Data are from authors' digitization of NARA Record Groups 162 and 207.

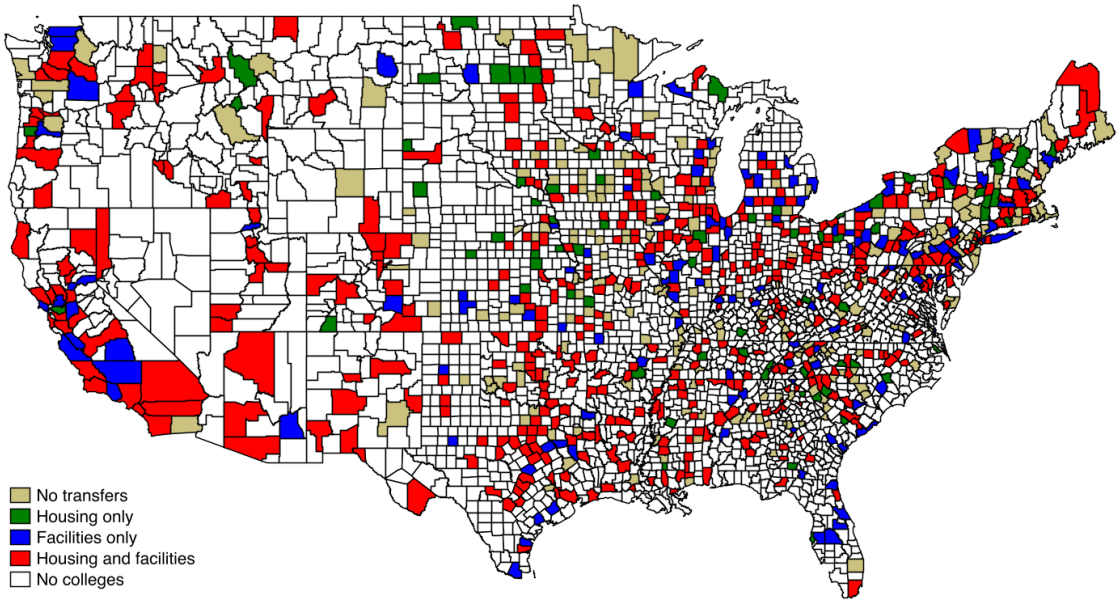
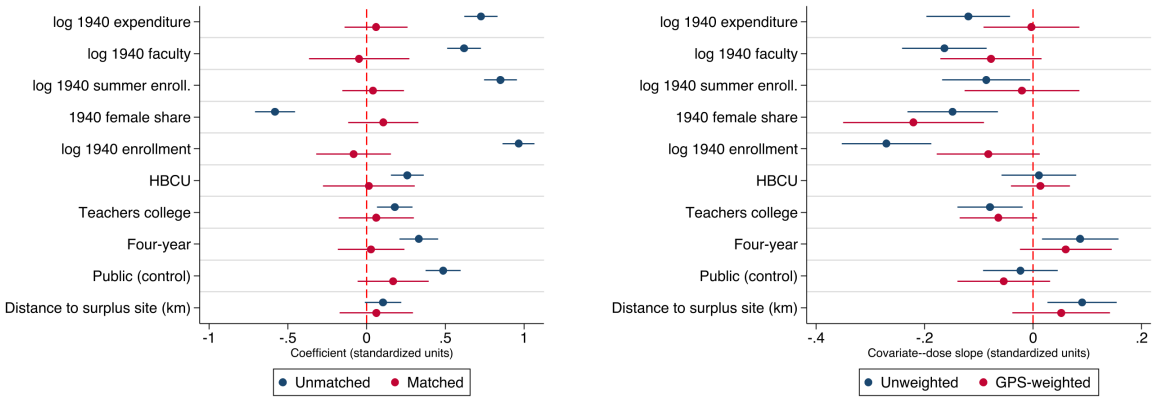


Figure 2: Treated Colleges by Type of Transfer

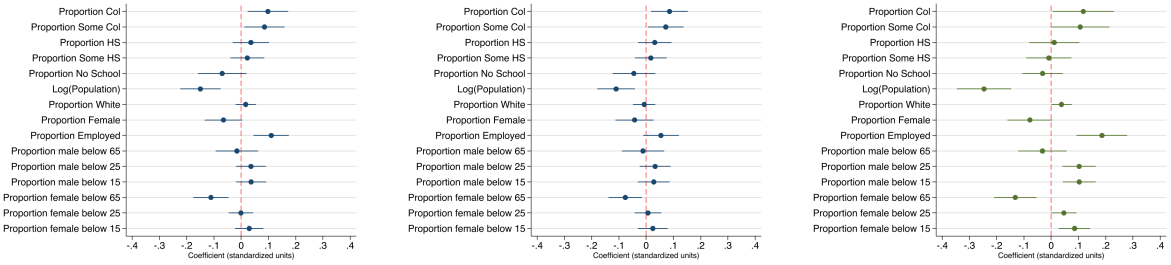
Notes: Counties are shaded by the type of surplus transfer received: housing only (VEHP, green), facility only (VEFP, blue), both programs (red), or no transfers (tan). White counties contain no colleges.



(a) Recipients vs. non-recipients (b) Association with the per-student dose

Figure 3: Prewar Balance: 1940 Institutional Characteristics

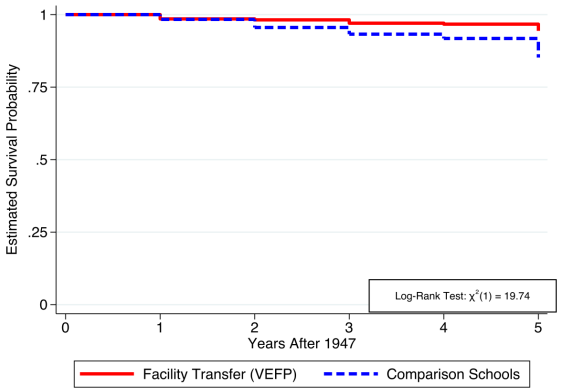
Notes: Standardized differences in 1940 institutional characteristics for the school-level analysis. Panel (a) plots the standardized difference between recipients and non-recipients before matching (Unmatched) and after 1:1 nearest-neighbor propensity matching (Matched). Panel (b) plots the standardized association between each characteristic and the per-student dose of construction dollars, unweighted and after generalized propensity-score reweighting. Recipients are larger and more often four-year institutions; matching and reweighting restore balance.



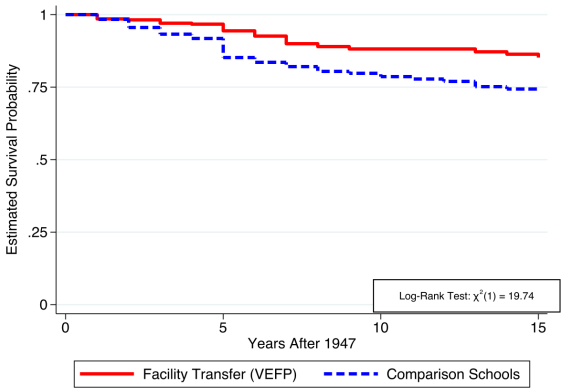
(a) Any Program (b) VEFP Facility Transfers (c) VEHP Housing Transfers

Figure 4: Prewar Balance: 1940 County Characteristics by Treatment Status

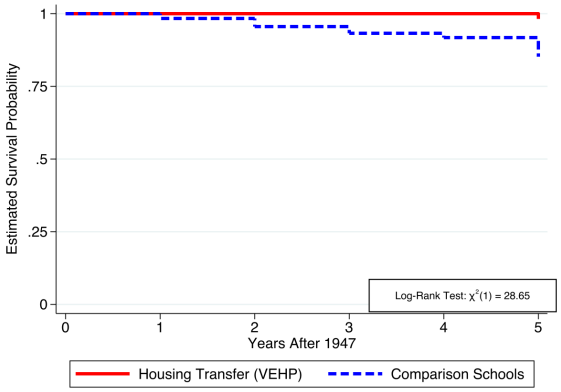
Notes: Standardized differences in 1940 county characteristics between treated and untreated counties for three treatment definitions. Each point is the coefficient from a bivariate regression of the standardized county outcome on the treatment indicator; horizontal lines are 95% confidence intervals. County data from Haines (2005).



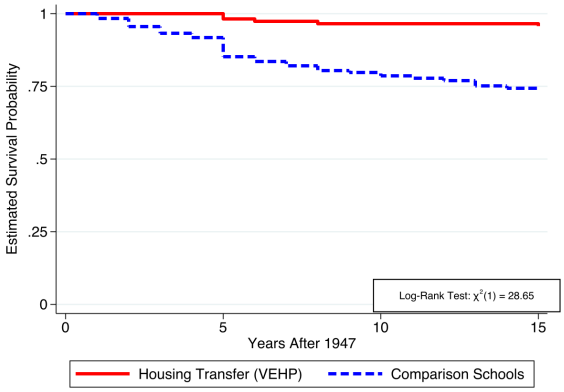
(a) Facility Transfers (VEFP), 5 Years



(b) Facility Transfers (VEFP), 15 Years



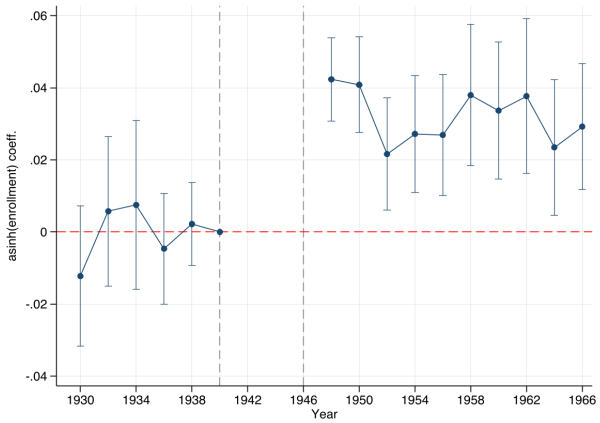
(c) Housing Transfers (VEHP), 5 Years



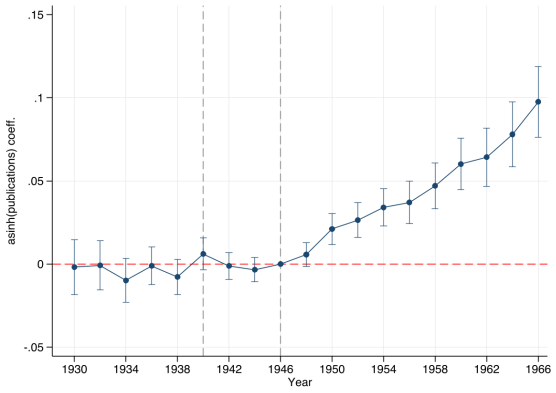
(d) Housing Transfers (VEHP), 15 Years

Figure 5: Kaplan–Meier Survival Estimates: Treated vs. Comparison Institutions

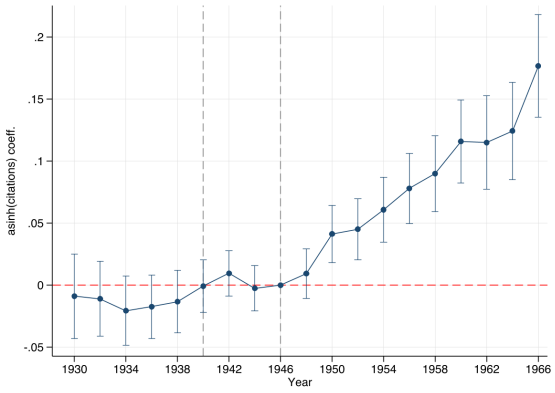
Notes: Kaplan–Meier estimated survival probabilities for institutions that received surplus transfers (solid red) versus comparison institutions (dashed blue). The intervention year (1947) is normalized to zero. Panels (a)–(b) compare VEFP facility-transfer recipients to non-recipients; panels (c)–(d) compare VEHP housing-transfer recipients to non-recipients. Log-rank test statistics: VEFP $\chi^2(1) = 9.59$; VEHP $\chi^2(1) = 55.54$.



(a) Enrollment



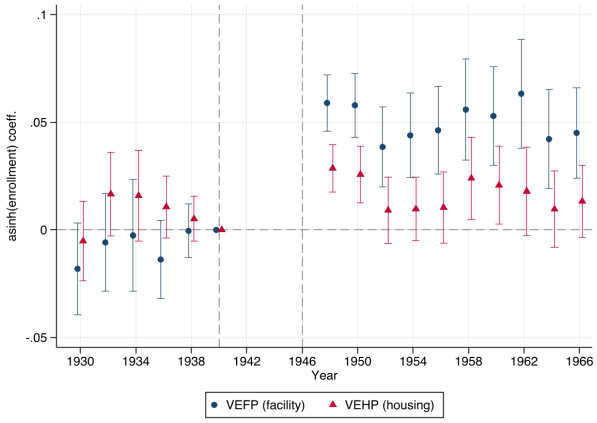
(b) STEM Publications



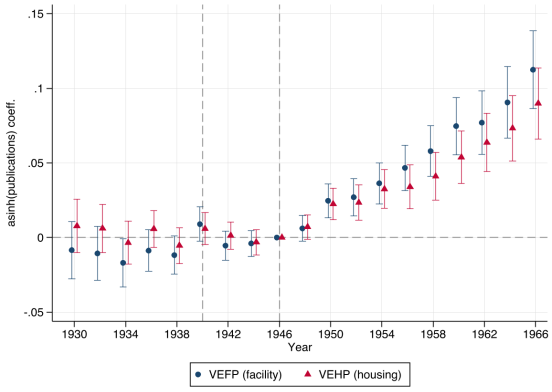
(c) STEM Citations

Figure 6: Effects of the Per-Student Dose on University Outcomes

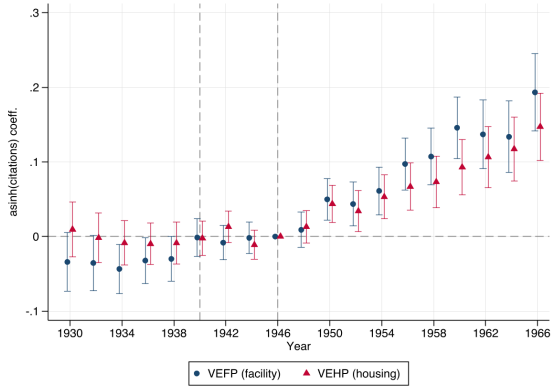
Notes: Event-study estimates of the per-student dose-response from Equation (3). The treatment is $\log(1 + \text{construction dollars per 1940 student})$ interacted with year; outcomes are in inverse hyperbolic sine. The omitted baseline is 1940 for enrollment and 1946 for publications and citations. Dashed vertical lines mark the war years and the onset of surplus transfers. Bars are 95% confidence intervals from standard errors clustered at the institution level. Enrollment uses the full sample of colleges; STEM publications and citations use four-year institutions.



(a) Enrollment



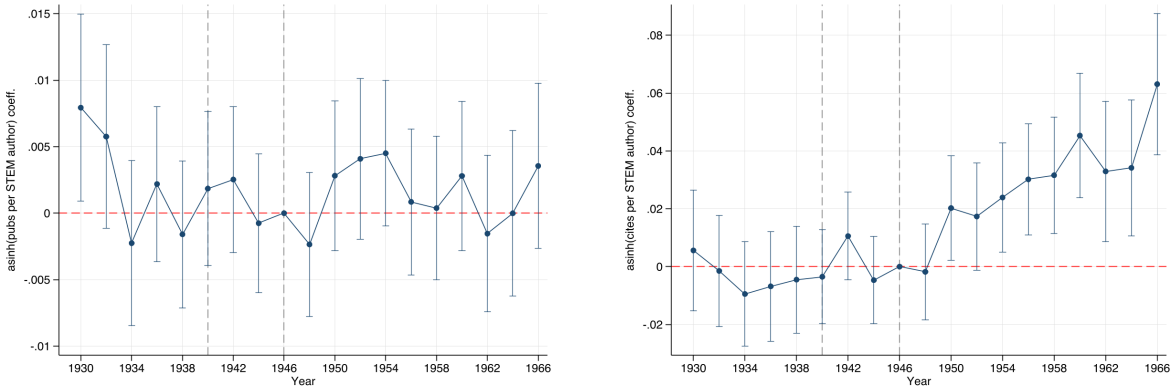
(b) STEM Publications



(c) STEM Citations

Figure 7: Facility versus Housing: Per-Student Dose-Response by Program

Notes: Event-study estimates comparing the per-student dose-response to VEFP facility dollars (navy) and VEHP housing dollars (cranberry), each measured as log(1+program dollars per 1940 student) and estimated in inverse hyperbolic sine. Enrollment uses the full sample; STEM publications and citations use four-year institutions. Facility dollars have the larger dose-response throughout; only facility dollars raise enrollment, whereas housing dollars still raise research output significantly despite having no direct connection to research infrastructure.

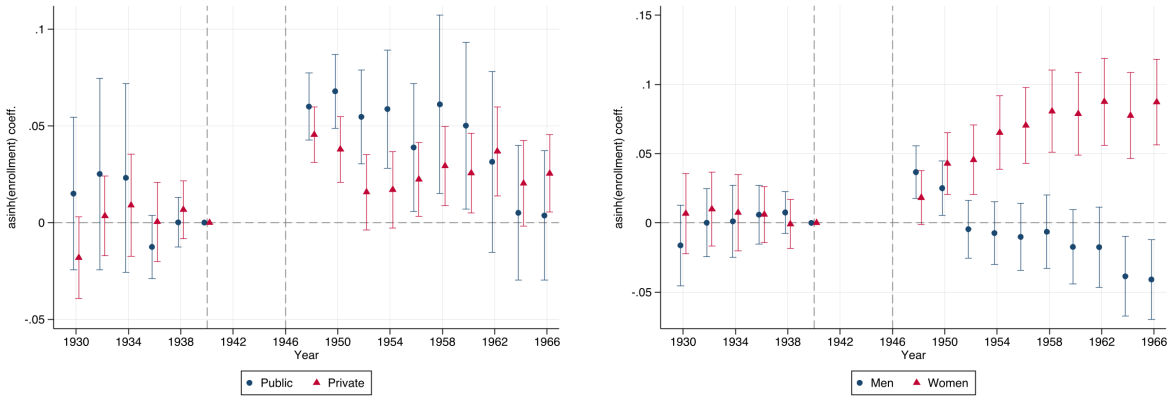


(a) STEM Publications per Capita

(b) STEM Citations per Capita

Figure 8: Research Quality versus Quantity: Per-Capita Effects

Notes: Event-study estimates of the per-student dose on STEM research output divided by the number of STEM authors, on the four-year sample. The treatment is log(1 + construction dollars per 1940 student) interacted with year, and outcomes are in inverse hyperbolic sine. The flat effect on publications per author (panel a) combined with the large, growing effect on citations per author (panel b) implies that the transfers raised research *impact* per researcher rather than raw output per researcher. Other details as in Figure 6.

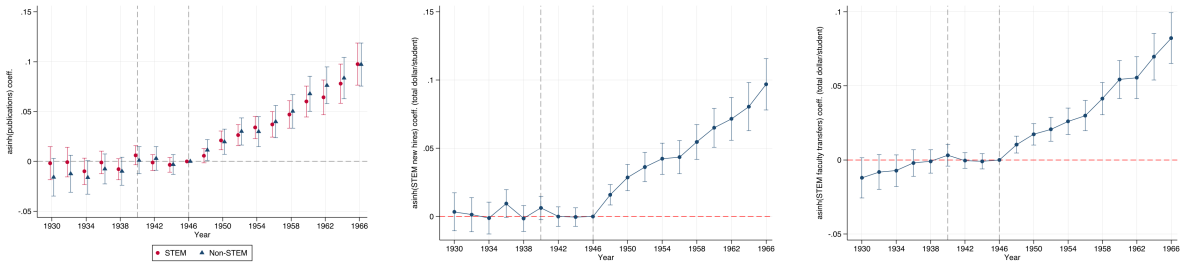


(a) Public vs. private

(b) Men vs. women

Figure 9: Teaching Mechanisms: Enrollment by Institution Control and Gender

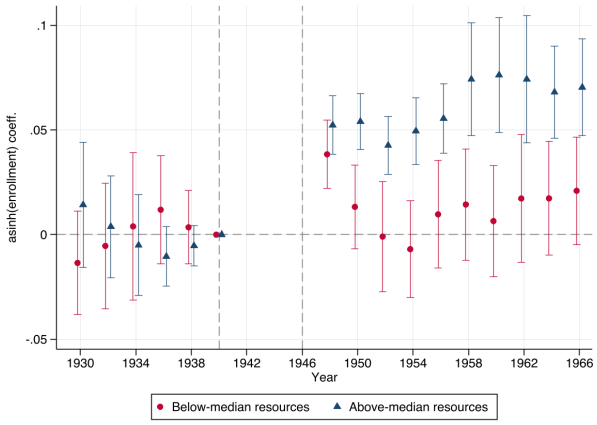
Notes: Event-study estimates of the per-student dose-response for enrollment (inverse hyperbolic sine), full sample, to the mid-1960s. Panel (a) estimates the effect separately for public and private institutions; Panel (b) decomposes enrollment into men and women. The gain is larger at public institutions, and its persistent component is driven by women, while the initial men's response fades as the veteran cohort passed through.



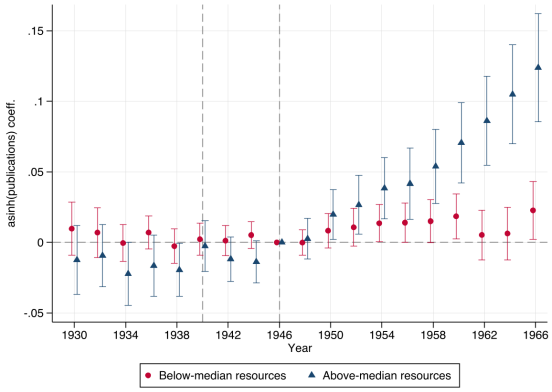
(a) STEM vs. non-STEM (b) STEM new hires (c) STEM faculty transfers

Figure 10: Research Mechanisms: Field Composition and Faculty Recruitment

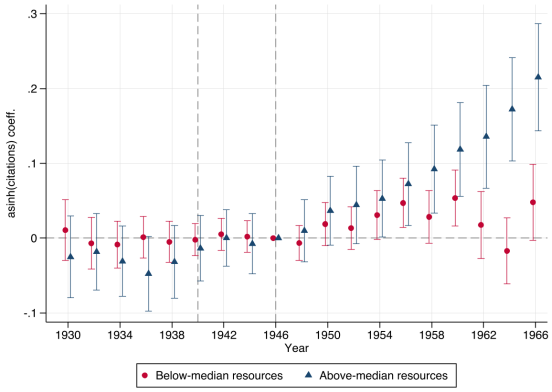
Notes: Event-study estimates of the per-student dose-response (inverse hyperbolic sine), four-year institutions, to the mid-1960s. Panel (a) compares STEM and non-STEM publications; Panels (b) and (c) report STEM new hires and STEM faculty transfers. Research gains span fields and operate through faculty recruitment.



(a) Enrollment



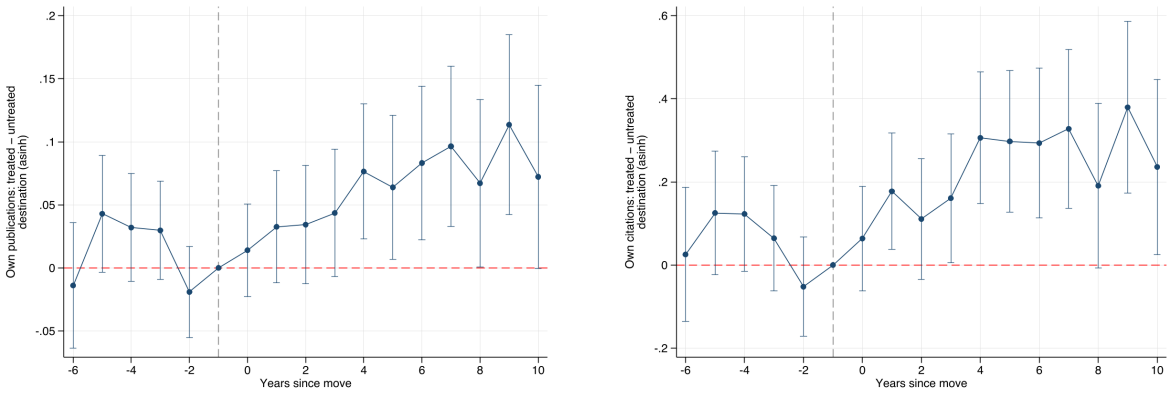
(b) STEM Publications



(c) STEM Citations

Figure 11: Mechanism: The Effect Is Concentrated Where Complementary Inputs Existed

Notes: Per-student dose event studies (inverse hyperbolic sine, four-year institutions), estimated separately for institutions with above- and below-median 1940 educational expenditure, for enrollment (Panel a), STEM publications (Panel b), and STEM citations (Panel c). Both groups respond similarly in the first postwar years, but the response persists and grows only at the better-resourced institutions (navy), while at capital-poor institutions (cranberry) the enrollment effect fades and the research effects plateau. Bars are 95% confidence intervals; standard errors clustered at the institution level.

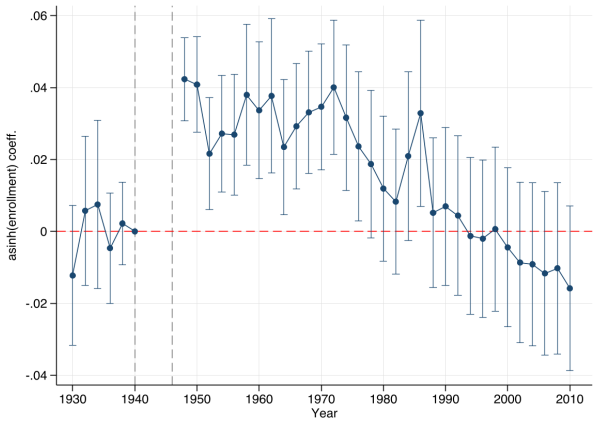


(a) Own Publications

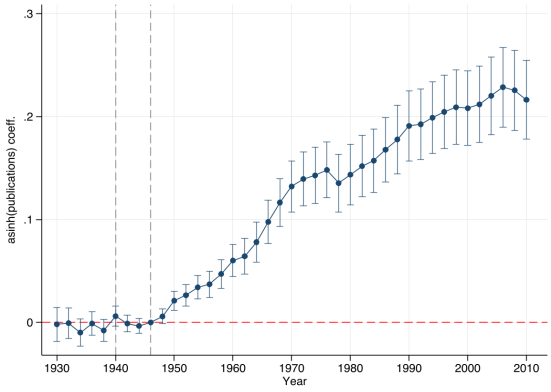
(b) Own Citations

Figure 12: Mechanism: A Place Effect on Scholars Who Move to Treated Institutions

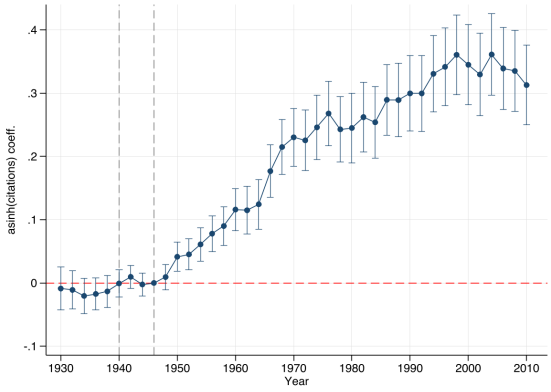
Notes: Event-study estimates from an author-level movers design on the STEM publication panel (author × institution × year). The sample is scholars who change institution; the plotted coefficients are the differential path of a mover's *own* output (publications in Panel a and citations in Panel b, inverse hyperbolic sine) for those moving to a treated institution relative to those moving to a non-recipient institution, measured against the year before the move (event −1, normalized to zero). Author fixed effects are absorbed, so each scholar serves as his own control and the generic effect of moving is differenced out; year fixed effects absorb common trends. Pre-move trends are flat; after a move to a treated institution a scholar's own citations rise by roughly one-third and persist, while publications rise far less, a place effect concentrated in impact. Bars are 95% confidence intervals; standard errors clustered at the author level.



(a) Enrollment



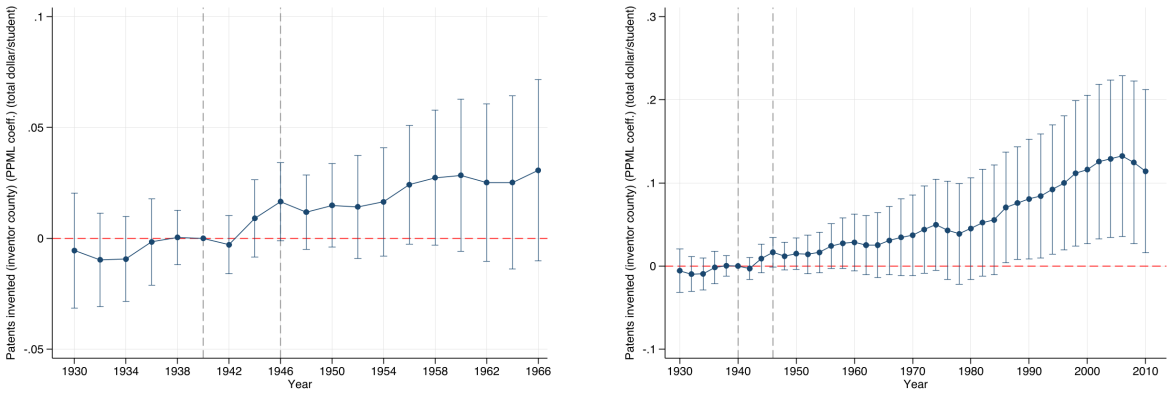
(b) STEM Publications



(c) STEM Citations

Figure 13: Long-Run Effects Through 2010

Notes: Event-study estimates extending the panel through 2010 using HEGIS and IPEDS enrollment data and OpenAlex bibliometric data. Enrollment gains (panel a) fade over time as untreated institutions close the physical capacity gap. Scientific production effects (panels b–c) persist through 2010, six decades after the original transfers. Other details as in Figure 6.



(a) Medium run (to 1966)

(b) Long run (through 2010)

Figure 14: Spillovers to Local Innovation: County Patenting

Notes: Event-study estimates of the per-student construction dose on the number of patents filed by inventors in the county, estimated by Poisson pseudo-maximum likelihood (Equation (4)). The treatment is log(1 + construction dollars per 1940 student) aggregated across recipient institutions in the county, interacted with year, relative to the 1940 base. Pre-treatment coefficients are flat; the effect emerges gradually after the mid-1950s and grows through 2010. The sample is 857 counties. Standard errors clustered at the county level; bars are 95% confidence intervals.

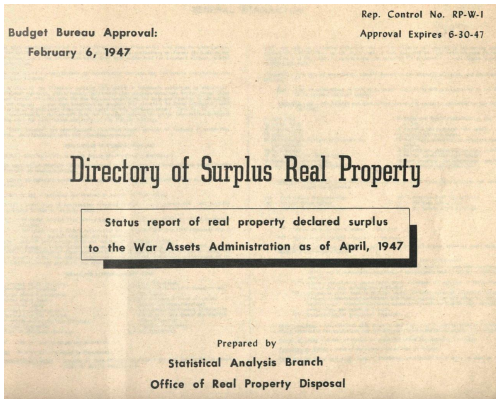
Appendix

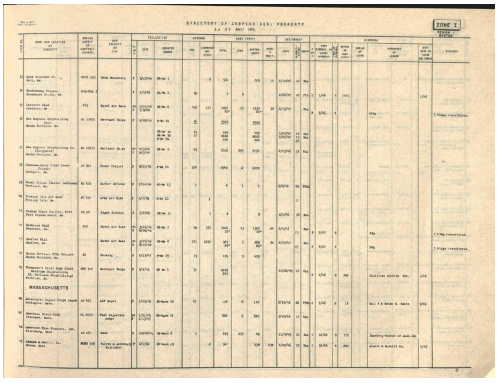
Archival Data Sources



Figure A1: Data Collection at the National Archives (NARA), College Park, MD

Notes: Photograph of the authors' archival research at NARA. Records were obtained from Record Group 162 (General Records of the Federal Works Agency) and Record Group 207 (General Records of the Department of Housing and Urban Development).





Zone I
Region I Boston

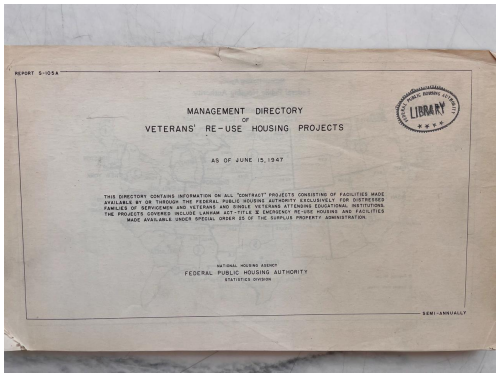
Directory of Surplus Real Property as of April 1947

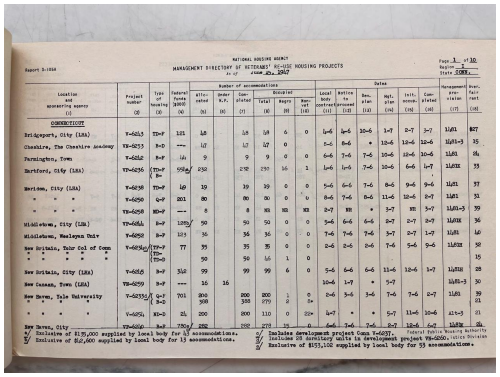
		Location				Declaration			Acreage		Cost ($000)				Assignment			Disposal					
Item No.	Name	City	State	Owning Agency or Contract	War Product or Use	Pr or F	Date	Register Number	Fee	Leasehold	Total	Land	Better-ments	Rack. &	Date	Classi-fication	Agency	F or C	Date Disp	Type of	Returns on	Period	Purchaser or Leasee
18	New Haven AAP (Cant	East Haven	Conn.	JD 193	Troop Housing	F	6/2/45	WH-Conn 2	30		10	10			9/5/45	06	FPHA	C	4/46	S	7		Salvatore Esposito
19	Pataganset Finianing (	Niantic	Conn.	PL 1106	Absorbent Gauze	Pr	12/7/45	HR-Conn 10	5		337				3/25/46	12	WAA	C	4/46	S	272		Pataganset Finishing Co. Inc
20	Portland Housing Proj	Portland	Conn.	2	Housing	F	5/9/45	RH-Conn 1	18		11	11			6/26/46	06	FPHA	C	4/46	S	8		Various
21	Royal Typewriter Co.	Hartford	Conn.	PL 2384	Bomb Fuses	F	4/11/46	FR-Conn 19	3		373	35	338		4/16/46	12	WAA	C	1/46	S	325		Hartford Empire Co.

- Name, City, State, War Product of Use, Acreage, Cost($000), Classification, Purchaser or Leasee

Figure A2: Directory of Surplus Real Property, 1947

Notes: Cover page and sample entries from the *Directory of Surplus Real Property* (War Assets Administration, 1947). Top left: cover page. Top right: original document pages. Bottom: digitized sample showing fields recorded for each surplus site, including name, city, state, war product of use, acreage, cost ($000), classification, and purchaser or leasee.



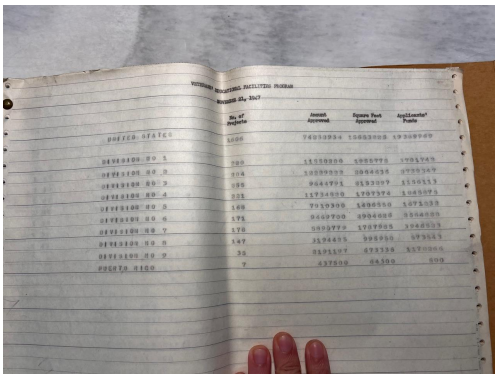


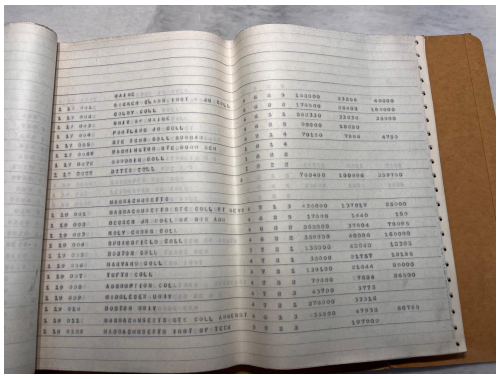
State	Location and sponsoring agency	Project number	Type of housing	Federal funds ($000)	Allocated	Under N.P.	Completed	Occupied - Total	Occupied - Negro	Occupied - Non-Vet	Local Body Contract	Notice to proced	Dev. Plan	Mgt. plan	Init. Occup.	Completed	Management provision	Aver. fair rent
OHIO	New Conoord, Muskingum College	V-33354	Tr-F	20	30		30	29	0	1	3-6	3-6	5-6	5-6	4-6	5-6	1481-1	23
OHIO	New Conoord, Muskingum College	V-33422	B-F	136	18		18	6	0	1	5-6	6-6	10-6	10-6	2-7	2-7	1481	32
OHIO	New Conoord, Muskingum College	V-33422	B-D		112		112	110	0	42*								11
OHIO	Oberlin, Oberlin College	V-33357	Tr-F	31	50		50	48	0	0	2-6	2-6	4-6	4-6	3-6	6-6	1481-1	24
OHIO	Oberlin, Oberlin College	V-33423	TD-D	203	202		202	184	8	13	6-6	6-6	6-6	6-6	NR	1-7	1481	15
OHIO	Oxford, Miami University	VP-33351	Tr-F	141c	215	19	196	195	0	0	12-5	11-5	*	5-6	NR		1481-2	30
OHIO	Oxford, Miami University	V-33421	B-D	318	440		440	483	0	0	5-6	6-6	7-6	10-6	NR	3-7	1481	13

- Name, City, State, Type of housing, Federal funds ($000), Allocated, Completed, Occupied

Figure A3: Management Directory of Veterans' Re-use Housing Projects (VEHP), 1947

Notes: Cover page and sample entries from the *Management Directory of Veterans Re-use Housing Projects* (Federal Public Housing Authority, June 15, 1947). Top left: cover page. Top right: original document pages for Connecticut. Bottom: digitized sample showing fields recorded for each institution, including name, city, state, type of housing, federal funds ($000), units allocated, completed, and occupied.





state	institution	amount_approved	sq_ft_approved	applicants_funds
alabama	ste a & m inst	57429	10200	2300
alabama	ste tchs coll florence	31098	5000	5461
alabama	ste tchs coll jacksonville	27733	4000	1460
alabama	ste tchs coll livingston	46721	10000	2300
alabama	ste tchs coll montgomery	46668	12600	9153
alabama	troy ste tchs coll	53243	11600	7231
alabama	tuskegee inst	83837	20000	4329

- Institution name, State, Amount approved, Square foot approved, Applicant Funds

Figure A4: Veterans' Educational Facilities Program (VEFP) Records

Notes: Original ledger pages and digitized sample from the Veterans' Educational Facilities Program records. Left: summary page showing total amounts approved, square footage, and applicant funds by division. Right: institution-level detail pages. Bottom: digitized sample showing fields recorded for each institution, including institution name, state, amount approved, square footage approved, and applicant funds.

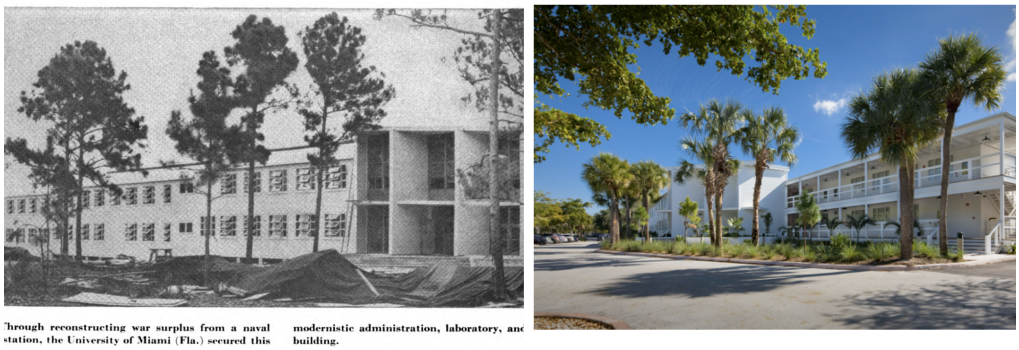


Figure A5: Example of Surplus Transfer: Naval Air Station to the University of Miami

Notes: Left: Original naval air station buildings transferred to the University of Miami. Right: The same site as the University of Miami Administration Building (R. J. Heisenbottle Architects, P.A., 2023). Source: Hollis (1949), Federal Security Agency, Office of Education.



Figure A6: Example of Housing Transfer: War Surplus Housing at the University of Kentucky, Lexington

Notes: Aerial photograph of surplus military housing units deployed at the University of Kentucky campus to accommodate returning veterans and their families under the Veterans' Re-use Housing Program (VEHP).



(1)-(2): UC Berkeley

One of Berkeley's first biotech startups, Bio-Rad *Laboratories*, a developer and manufacturer that specializes in technological products for the life sciences and clinical diagnostics markets, started in Berkeley Quonset hut. Source: A History of Innovation: Berkeley Entrepreneurs, Companies that Changed the Way We Live (2023).

(3): UC San Diego

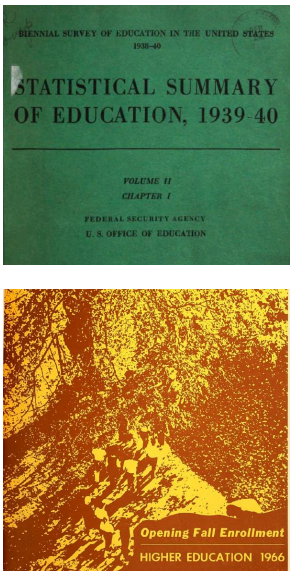
Quonset huts, leftover from the Camp Matthews Marine Base, served as *classrooms* before the first academic facilities were constructed at UC San Diego. Source: Robert Glasheen Photograph Collection, 1979, Special Collections and Archives.

(4)-(5): UC Los Angeles

Gayleyville: UCLA MAG - Cole Holinquist Press Temporary buildings in South Campus ca 1940s (inscribed); Veterans' *housing site* #2, October 1946. Source: UCLA library archives.

Figure A7: Wartime Surplus Structures on University of California Campuses

Notes: Examples of surplus wartime structures reused at University of California campuses. Panels (1)–(2): Berkeley, where an early biotechnology firm, Bio-Rad Laboratories, began in a surplus Quonset hut. Panel (3): San Diego, where Quonset huts from the former Camp Matthews Marine base served as classrooms before the first permanent academic buildings were constructed. Panels (4)–(5): Los Angeles, showing temporary buildings and veterans' housing ("Gayleyville") in the postwar years. Sources: *A History of Innovation: Berkeley Entrepreneurs* (2023); Robert Glasheen Photograph Collection, UC San Diego Library Special Collections and Archives; UCLA Library Archives.



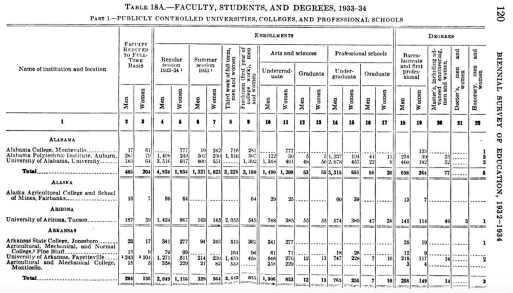


TABLE 18A.—FACULTY, STUDENTS, AND DEGREES, 1933-34

PART 1.—PUBLICLY CONTROLLED UNIVERSITIES, COLLEGES, AND PROFESSIONAL SCHOOLS

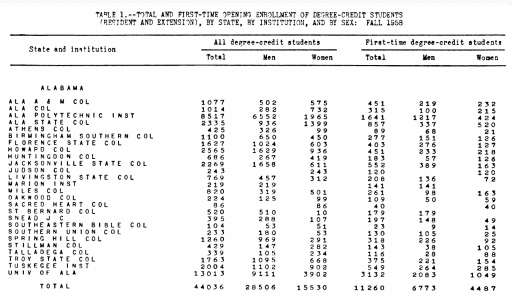


TABLE 1.--TOTAL AND FIRST-TIME OPENING ENROLLMENT OF DEGREE-CREDIT STUDENTS (RESIDENT AND EXTENSION), BY STATE, BY INSTITUTION, AND BY SEX: FALL 1966

State and institution	All degree-credit students			First-time degree-credit students		
	Total	Men	Women	Total	Men	Women
ALABAMA						
ALA A & M COL	1077	502	575	451	219	232
ALA COL	1014	282	732	315	100	215
ALA POLYTECHNIC INST	8517	6552	1965	1641	1217	424
ALA STATE COL	9335	936	1399	857	337	520
ATHENS COL	425	326	99	89	68	21
BIRMINGHAM SOUTHERN COL	1100	650	450	277	151	126
FLORENCE STATE COL	1627	1024	603	403	276	127
HOWARD COL	2565	1629	936	451	233	218
HUNTINGDON COL	686	267	419	183	57	126
JACKSONVILLE STATE COL	2247	1658	611	552	389	163
JUDSON COL	343		343	120		120
LIVINGSTON STATE COL	749	457	312	208	136	72
MARION INST	219	219		141	141	
MILES COL	830	329	501	261	98	163
OAKWOOD COL	334	125	95	109	50	59
SACRED HEART COL	86		86	46		46
ST BERNARD COL	520	510	10	179		
SNEAD J C	395	288	107	197	179	
SOUTHEASTERN BIBLE COL	104	53	51	33	148	42
SOUTHERN UNION COL	233	180	53	130	9	21
SPRING HILL COL	1260	969	291	318	105	55
STILLMAN COL	429	147	282	143	226	92
TALLADEGA COL	339	105	234	116	38	105
TROY STATE COL	1763	1095	668	375	221	88
TUSKEGEE INST	2004	1102	902	549	264	154
UNIV OF ALA	13013	9111	3902	3132	2083	285
						1049
TOTAL	44036	28506	15530	11260	6773	4487

- State
- Institution name
- Total Enrollment

Figure A8: Historical Enrollment Data Sources

Notes: Sample pages from the historical enrollment sources used to construct the institution-level panel. Top left: cover of the *Biennial Survey of Education, Statistical Summary, 1939–40* (Federal Security Agency, Office of Education). Top right: sample page from the Biennial Survey showing institution-level faculty, enrollment, and degrees for publicly controlled universities. Bottom left: cover of *Opening Fall Enrollment, Higher Education 1966*. Bottom right: sample page showing institution-level enrollment by gender. Variables extracted include state, institution name, and total enrollment.

Calculation of the Policy Values of the VEFP and VEHP

The cost to transfer the real property surplus to the colleges was $72,000,000 and $202,000,000 for the VEFP and VEHP respectively (in 1947 dollars) (Russell, 1951). The fair value of all educational facility transfers to colleges under the VEFP is $150,000,000 according to Hollis (1949). From Hollis (1949), the total number of dormitory housing units transferred to colleges was 74,886 and the total number of dormitory units was 52,991. The Federal Public Housing Authority (1946) reports per-unit costs of public war housing built by the Federal Public Housing Authority, the arm of the National Housing Agency that administered the VEHP (Appendix Figure A9). We bracket the value of each transferred unit between two figures from that schedule: a conservative lower bound equal to the cost of the *dwelling buildings and equipment* alone, that is, the physical structure, and an upper bound equal to the *total development cost*, which additionally includes site acquisition, site improvements, architecture and engineering, administration, and contingencies. These bracket the per-unit value at $544 to $947 for dormitories and $1,924 to $2,797 for temporary family dwellings ($701–1,221 and $2,480–3,605 in 1947 dollars). Consequently, the fair value of all housing transfers to colleges under the VEHP is between $183,912,766 to $282,468,361. The total fiscal value of both policies therefore range between $607,912,766 and $706,468,361. Since the amount of federal funds spent on the educational portion of the G.I. Bill is estimated to be $1,324,000,000 in 1947 (Russell, 1951), the fiscal value of the VEFP and VEHP combined are 46-53% of this amount. The total expenditure of higher education institutions in 1946 was $1,172,917,010[11]. Therefore the fiscal value of both policies range between 52 to 60% of this amount.

[11] U.S. Office of Education, Biennial Survey of Education 1944-1946.

What Public War Housing Costs
(Per FPHA unit as of Dec. 31, 1943)

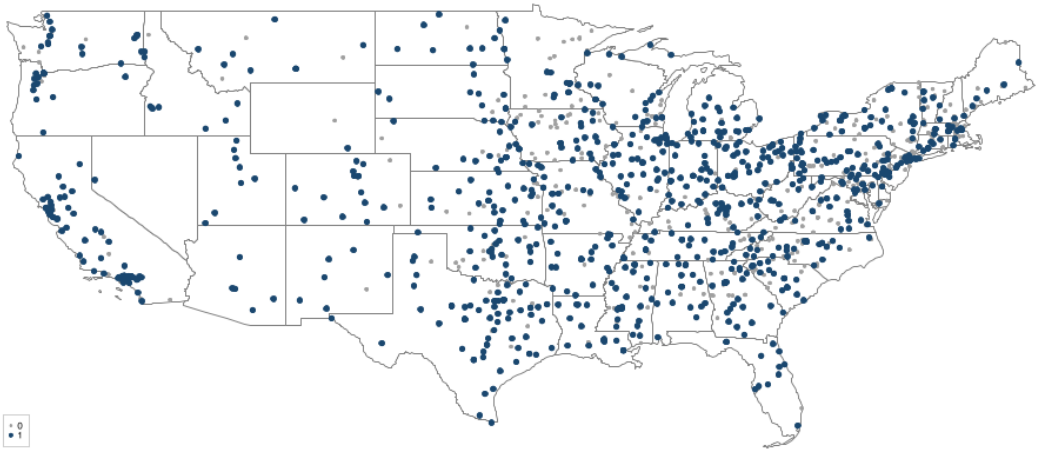
	Family dwellings			Dormitory	Trailer
	Permanent	Demountable	Temporary		
Total development costs	$4,727	$4,242	$2,797	$947	$1,830
Acquisition of sites	414	80	44	26	14
Dwelling buildings and equipment[1]	3,132	3,094	1,924	544	1,452
Furniture and furnishings		6	104	90	11
Site improvements	599	776	454	103	255
Nondwelling buildings, space and equipment	114	147	182	145	58
Architecture and engineering	189	102	76	32	28
Administration and other	156	8	5	2	1
Reserve for contingencies	123	29	2	5	11

Figure A9: Per-Unit Cost of Public War Housing (FPHA)

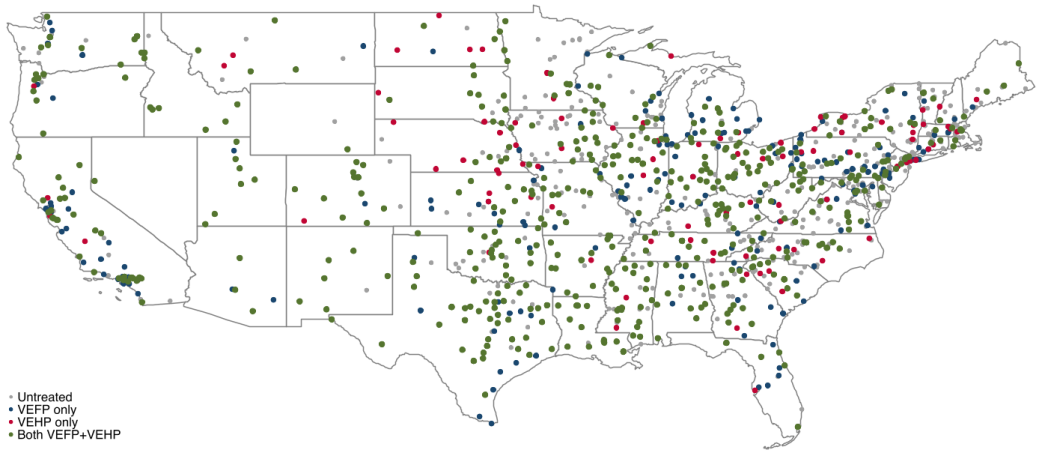
Notes: Per-unit cost of public war housing built by the Federal Public Housing Authority, as of December 31, 1943, reproduced from Federal Public Housing Authority (1946) (page 10). Our lower-bound valuation of the VEHP transfers uses the "Dwelling buildings and equipment" row (the physical structure only); our upper-bound valuation uses the "Total development costs" row, which additionally includes site acquisition, furniture, site improvements, nondwelling buildings, architecture and engineering, administration, and contingencies. Costs are shown separately for permanent, demountable, and temporary family dwellings, dormitories, and trailers.

Supplementary Results

Treatment Geography and Raw Trends. This subsection maps the geography of treatment at the level of individual institutions and reports raw, unadjusted outcome trends for treated and untreated institutions.



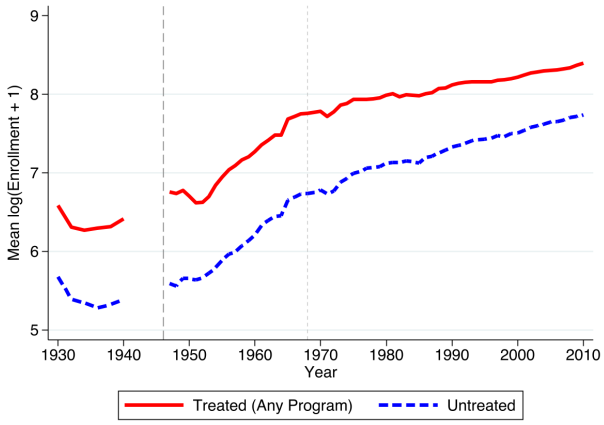
(a) By any treatment



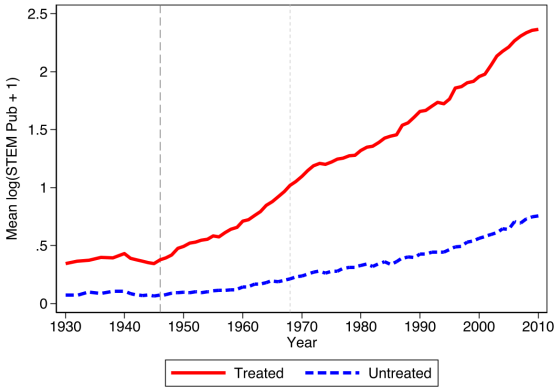
(b) By type of transfer

Figure A10: Treatment at the Institution Level

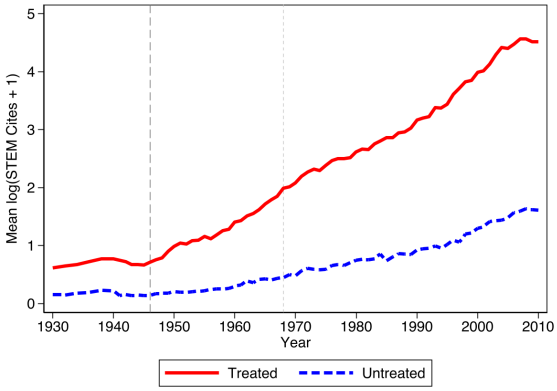
Notes: Each point is a college or university in the estimation sample, located at its campus. Panel (a) distinguishes institutions that received any surplus transfer (navy) from those that did not (gray). Panel (b) distinguishes institutions by the type of transfer received: facility only (VEFP, navy), housing only (VEHP, cranberry), both programs (green), or none (gray). These institution-level maps are the point-level counterpart to the county choropleths in Figures 1 and 2. Data are from the authors' digitization of NARA Record Groups 162 and 207.



(a) Log Enrollment



(b) Log STEM Publications



(c) Log STEM Citations

Figure A11: Raw Trends: Treated vs. Untreated Institutions

Notes: Raw mean log outcomes for treated institutions (those receiving any surplus transfer) and untreated institutions, plotted across all nine benchmark years. These unconditional trends motivate the event-study analysis in Section 5: treated and untreated institutions follow similar trajectories before the war, then diverge sharply in the postwar period for enrollment and persistently for research output.

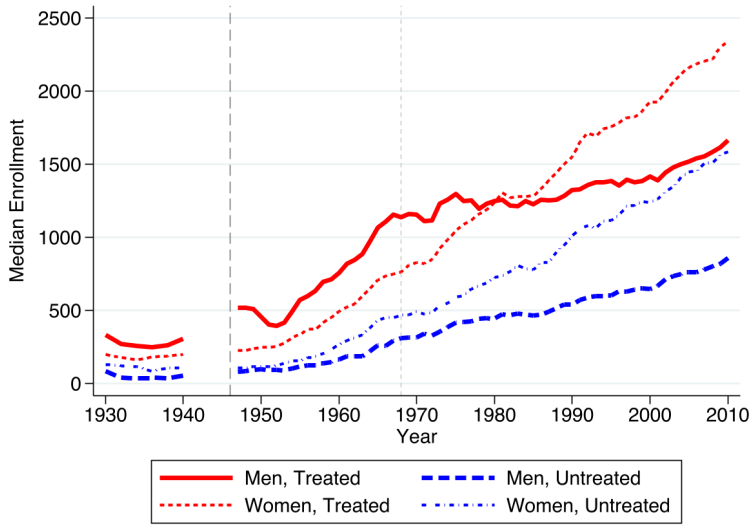


Figure A12: Raw Trends in Enrollment by Gender

Notes: Raw mean enrollment for men and women at treated versus untreated institutions across benchmark years. The postwar enrollment surge was initially driven by male veterans under the G.I. Bill; female enrollment grew more gradually.

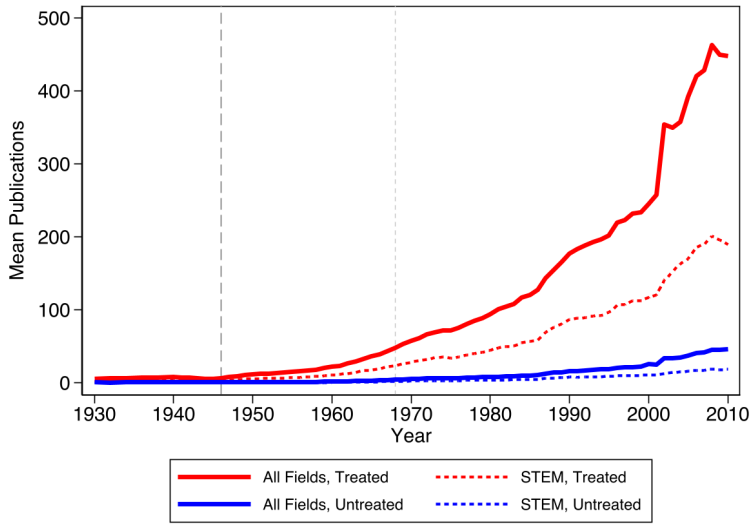
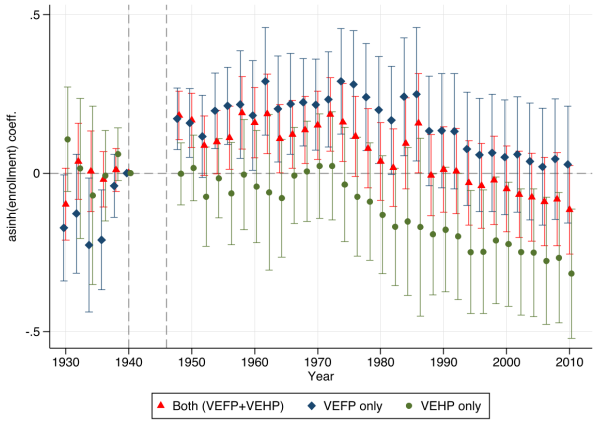


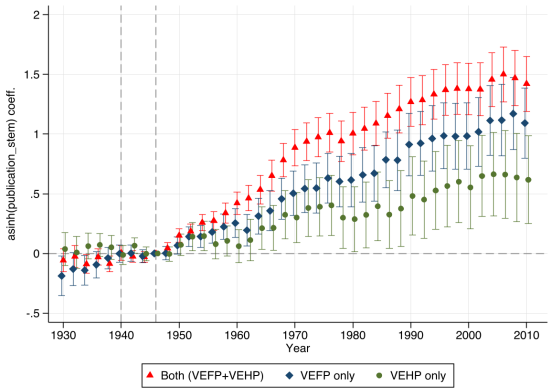
Figure A13: Raw Trends: All Publications vs. STEM Publications

Notes: Comparison of total publications and STEM publications (physics, biology, chemistry, and medicine) at treated versus untreated institutions. STEM fields account for the majority of the publication gap between treated and untreated institutions.

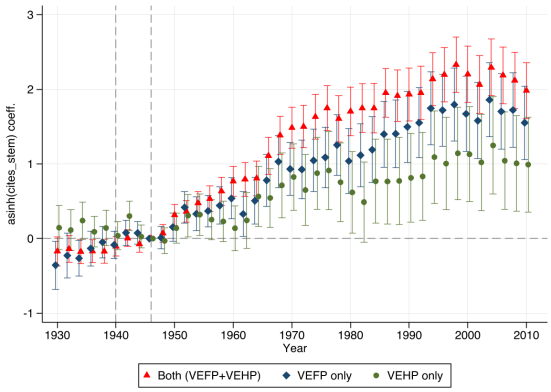
Effects by Program Arm. Figure A14 decomposes the main results by program arm, splitting recipients into three mutually exclusive groups (facility-only, housing-only, and both) as a complement to the continuous dose-response reported in the main text.



(a) Enrollment



(b) STEM Publications



(c) STEM Citations

Figure A14: Effects by Program Arm: Facility Only, Housing Only, and Both

Notes: Event studies that split recipients into three mutually exclusive groups: institutions receiving only a facility (VEFP) transfer, only a housing (VEHP) transfer, and both. Each group is entered as a separate indicator interacted with year in a single regression, so the omitted comparison group is never-treated institutions. Enrollment (base year 1940) uses the full sample; STEM publications and citations (base year 1946) use the four-year sample; all outcomes are in inverse hyperbolic sine with institution and year fixed effects and standard errors clustered at the institution level. Consistent with the dose-response in Figure 7, the enrollment gain is driven by facility recipients while housing-only recipients show no enrollment response; for research output, all three arms rise and persist, with the largest effect for institutions that received both programs and the smallest for housing-only recipients.

Functional Form and Robustness to Alternative Transformations. Publications, citations, and patents are non-negative counts with many zeros, which precludes the logarithm and makes the choice of functional form consequential (Chen and Roth, 2024). We estimate the university outcomes by the inverse hyperbolic sine, IHS$(\cdot)$, and county patents by Poisson pseudo-maximum likelihood (PPML). This appendix documents the reasoning.

School-level PPML is driven by a few large universities. Table 4 re-estimates the pooled effect of any transfer on publications and citations after dropping the ten and twenty-five largest universities by total output. On the full sample the PPML estimates are not merely imprecise but wrong-signed: the citation coefficient is -0.83 and the publication coefficient -0.29, both negative, and both turn positive once the twenty-five largest institutions are excluded. The IHS estimates are, by contrast, nearly invariant to these exclusions. Because PPML weights observations by their fitted counts, a small number of large universities determine, and here reverse, the school-level estimate.

PPML also strains the identifying assumption. Figure A15 plots the enrollment event study under PPML and IHS: the pre-treatment coefficients trend under PPML but are flat under IHS, and the joint pre-trend test statistic is nearly an order of magnitude larger under PPML (Table 5). The IHS and $\log(1+y)$ specifications display no pre-trend for any outcome.

Results are robust within the log-like family. The IHS and $\log(1+y)$ estimates are nearly identical across outcomes and specifications. The extensive-margin response (an indicator for any output) is positive in the medium run and converges over the full horizon, indicating that the persistent research gains operate on the intensive margin. County patenting, which is not dominated by any single county, is stable under PPML and provides the unit-invariant proportional effect reported in the main text.

Table 4: School-level PPML is outlier-driven; IHS is not

	PPML			IHS		
	Full	Drop 10	Drop 25	Full	Drop 10	Drop 25
Publications	−0.291	−0.209	0.233	0.748***	0.731***	0.717***
	(0.327)	(0.332)	(0.261)	(0.060)	(0.060)	(0.059)
Citations	−0.831*	−0.389	0.086	1.264***	1.272***	1.260***
	(0.446)	(0.330)	(0.364)	(0.099)	(0.097)	(0.098)

Notes: Coefficient on Post × Any transfer from the pooled difference-in-differences, four-year institutions. The Drop 10 and Drop 25 columns re-estimate after excluding the ten and twenty-five largest universities by total output. PPML is estimated on raw counts; IHS on the inverse hyperbolic sine. Standard errors clustered at the institution level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

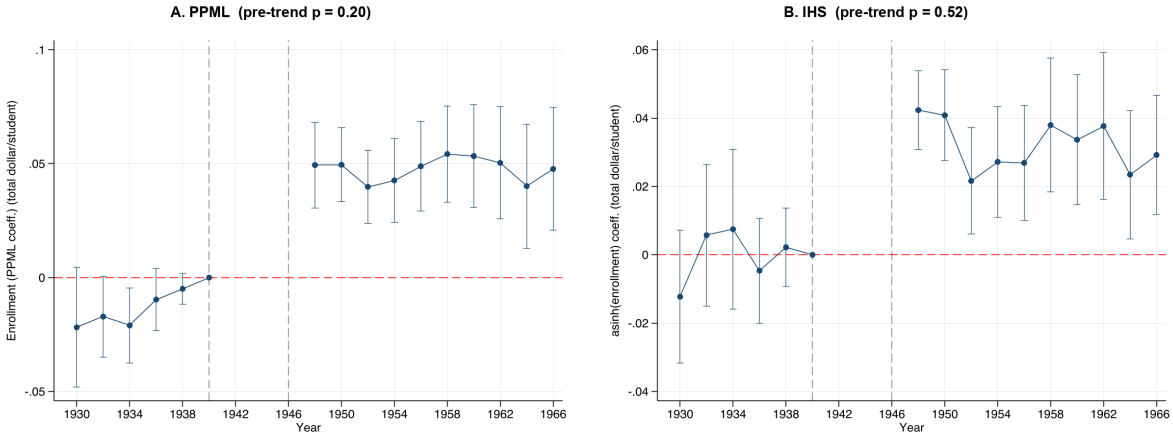


Figure A15: Pre-treatment Trends under PPML and IHS: Enrollment

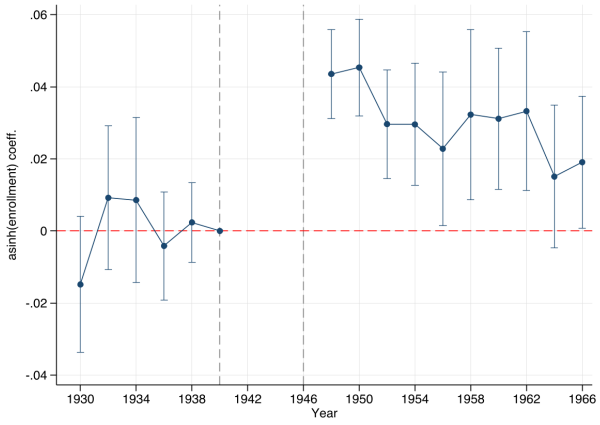
Notes: Per-student intensity event study for enrollment, estimated by PPML (Panel A) and IHS (Panel B). The treatment is log(1 + construction dollars per 1940 student) interacted with year, relative to the 1940 base. Pre-treatment coefficients trend under PPML but are flat under IHS. Reported p-values are joint tests that the pre-period coefficients equal zero.

Table 5: Joint Pre-trend Tests by Transformation (p-values)

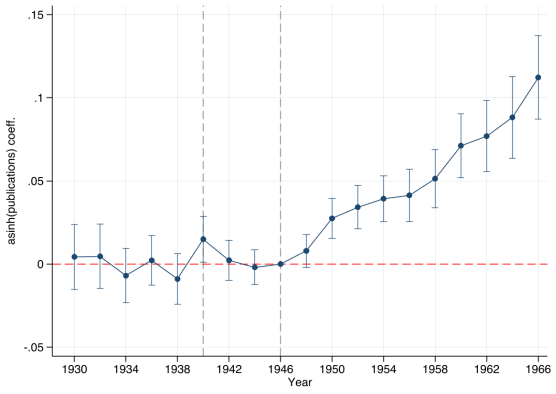
Outcome	PPML	$\log(1+y)$	IHS
Enrollment	0.205	0.564	0.524
Publications	0.575	0.089	0.108
Citations	0.123	0.483	0.441

Notes: p-values from joint tests that the pre-period dose-by-year interaction coefficients equal zero, from the per-student intensity event study. Small values indicate a pre-trend, i.e., rejection of parallel trends. PPML rejects for citations; the IHS and $\log(1+y)$ specifications pass for all three outcomes.

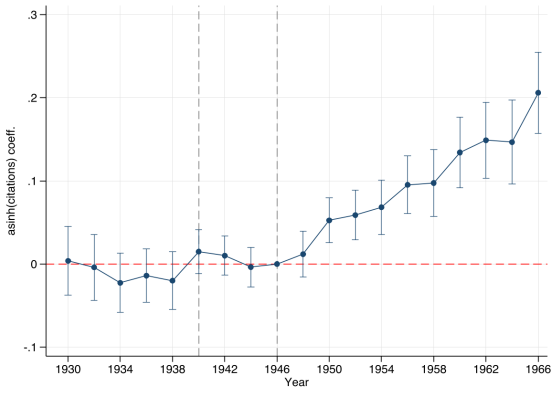
Additional Robustness. This appendix reports the robustness exhibits summarized in Section 5: refining the control group by matching and reweighting, alternative treatment definitions, and controlling for local wartime mobilization.



(a) Enrollment



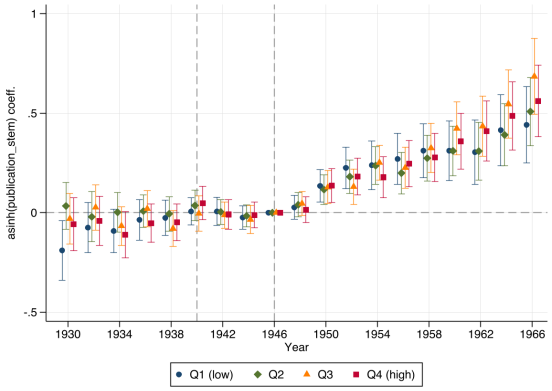
(b) STEM Publications



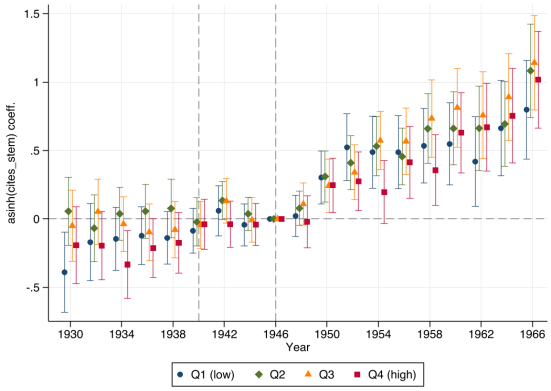
(c) STEM Citations

Figure A16: Refining the Control Group: GPS-Reweighted Dose-Response

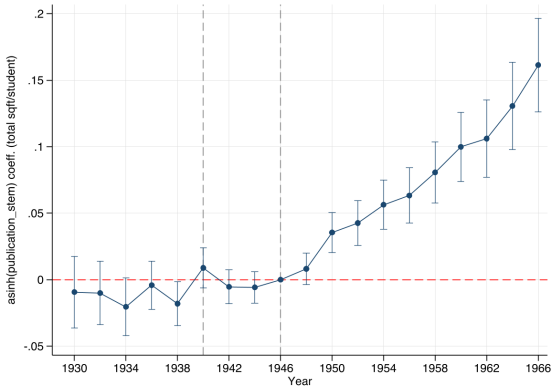
Notes: Per-student dose event studies after reweighting by the generalized propensity score so that the dose is orthogonal to 1940 institutional characteristics. The treatment is log(1 + construction dollars per 1940 student) interacted with year, in inverse hyperbolic sine; enrollment uses the full sample, STEM publications and citations the four-year sample. Estimates are close to the unweighted results in the main text, indicating the effects are not an artifact of larger institutions receiving larger transfers.



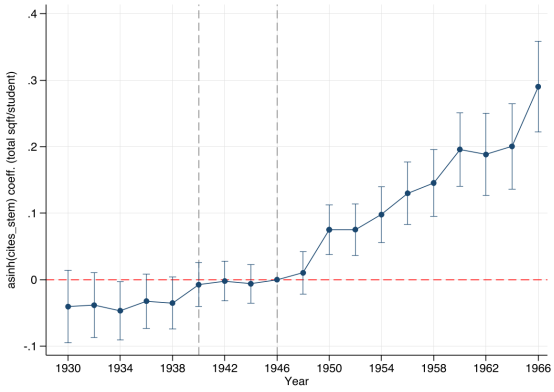
(a) Dose Quartiles: STEM Publications



(b) Dose Quartiles: STEM Citations



(c) Square Feet per Student: STEM Publications



(d) Square Feet per Student: STEM Citations

Figure A17: Alternative Treatment Definitions

Notes: Event studies under alternative definitions of treatment intensity. Panels (a)–(b) replace the continuous dose with its quartiles among recipients (untreated institutions are the omitted base), tracing effects by quartile relative to the 1946 base year. Panels (c)–(d) measure intensity as allocated square feet per 1940 student rather than construction dollars. All estimates are in inverse hyperbolic sine on the four-year sample. The dose-increasing pattern and the timing match the main results.

Table 6: Controlling for Local Wartime Mobilization (Enlistees × Year)

	Enrollment		STEM publications		STEM citations	
	Baseline	+ Enl.	Baseline	+ Enl.	Baseline	+ Enl.
Post × $ per 1940 student	0.020**	0.019**	0.129***	0.128***	0.218***	0.217***
	(0.008)	(0.009)	(0.012)	(0.012)	(0.019)	(0.019)

Notes: Pooled difference-in-differences coefficient on the per-student dose, in inverse hyperbolic sine. Enrollment uses the full sample; publications and citations use four-year institutions and STEM output. The + Enl. columns add county WWII enlistees (log(1 + enlistees)) interacted with year, allowing counties with different wartime mobilization to follow different trajectories. Standard errors clustered at the institution level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

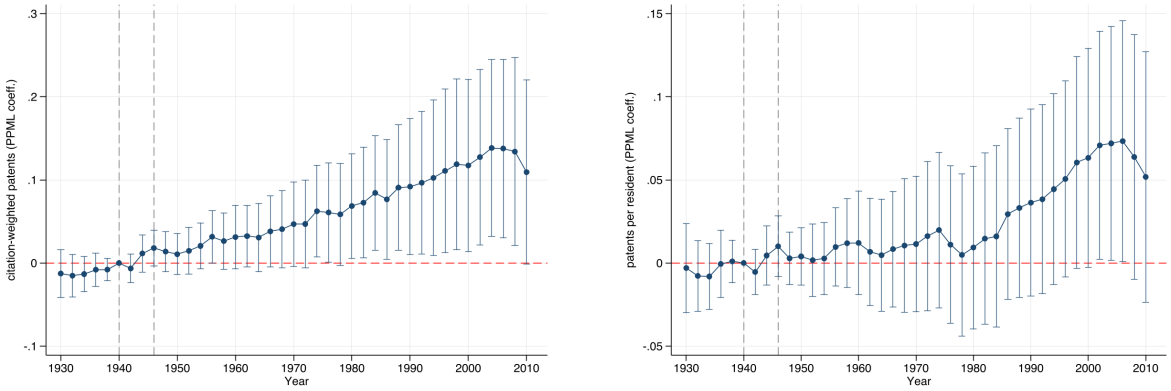
County Patenting. This appendix supports the spillover results in Section 6.1. Table 7 shows that the county patenting response to the per-student construction dose is robust to the transformation and to measuring the dose in square feet rather than dollars. Poisson yields the largest proportional effect, but the inverse hyperbolic sine and $\log(1+y)$ specifications agree in sign, significance, and timing. As in the main text, the full-horizon effect is larger than the twenty-year effect because local inventive activity responds only after the mid-1950s.

Table 7: County Patenting: Robustness to Transformation and Dose Measure

Specification	Full horizon	20-year
Dollars per 1940 student		
PPML	0.088***	0.026
	(0.034)	(0.017)
IHS	0.046***	0.017***
	(0.011)	(0.006)
$\log(1+y)$	0.043***	0.015***
	(0.010)	(0.006)
Square feet per 1940 student		
PPML	0.218***	0.066**
	(0.067)	(0.029)

Notes: Pooled difference-in-differences coefficient on the per-student construction dose for the number of patents filed by inventors in the county, from Equation (4). Rows vary the transformation of the outcome (PPML on raw counts; IHS and $\log(1+y)$ by OLS) and the unit of the dose (dollars or square feet per 1940 student). County and year fixed effects throughout; 857 counties. Standard errors clustered at the county level in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Following Gross and Sampat (2023), we further examine citation-weighted patenting and patenting per capita, both estimated by Poisson to match the baseline (Figure A18). Weighting each patent by the forward citations it receives yields a dose-response of 0.092 ($p<0.05$), slightly larger than the 0.088 for unweighted counts, so the spillover operates on high-impact invention rather than marginal patents. Adding county population as a Poisson exposure, that is, estimating patents per resident, gives a dose-response of 0.043 ($p<0.10$): roughly half of the raw-count effect reflects population growth in treated counties, but a significant per-resident increase remains. Both display flat pre-trends and the same delayed emergence as the baseline.

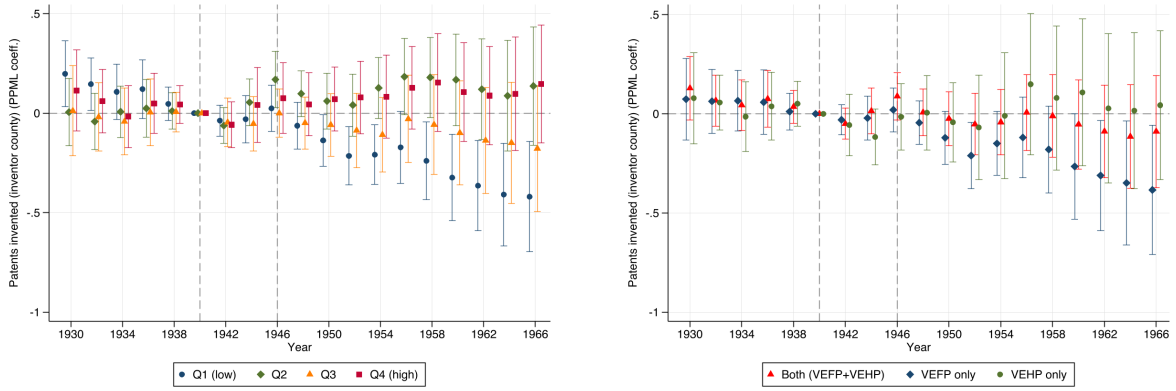


(a) Citation-weighted patents

(b) Patents per resident

Figure A18: County Patenting: Citation-Weighted and Per-Capita

Notes: County patenting event studies of the per-student construction dose, estimated by Poisson pseudo-maximum likelihood, base year 1940. Panel (a) weights each patent by the number of forward citations it receives (citation-weighted patents). Panel (b) adds county population as a Poisson exposure, so the coefficient is the proportional effect on patents per resident; population is interpolated to annual frequency from the decadal county databooks. The citation-weighted response is slightly larger than for raw counts, and the per-resident response remains positive and significant, indicating the spillover is on high-impact invention and not merely population growth. Bars are 95% confidence intervals; standard errors clustered at the county level.



(a) Dose Quartiles

(b) By Program Arm

Figure A19: County Patenting: Discrete Decompositions

Notes: County patenting event studies (PPML). Panel (a) bins the per-student dose into quartiles among recipient counties, with untreated counties as the reference (Gross and Sampat, 2023). The top quartile shows the largest and steadily growing effect and the lowest quartile declines; the intermediate quartiles are imprecise because county patent counts are small. Panel (b) splits treated counties by program arm and is likewise imprecise. The continuous dose in Figure 14 and Table 7 remains the primary specification. Bars are 95% confidence intervals; standard errors clustered at the county level.