NBER WORKING PAPER SERIES

THE COLD WAR AND THE U.S. LABOR MARKET

Ilyana Kuziemko
Donato A. Onorato
Suresh Naidu

Working Paper 35008
http://www.nber.org/papers/w35008

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2026

We thank our outstanding research assistants, Baz Crow, Catriona Farquharson, Joaquin Armas Muguerza, Zaria Roller, and Lucia Valdivieso. We are grateful for helpful comments from seminar and conference participants at Benelux PECO, Berkeley, Columbia, The Kiel Institute, Harvard, LSE, NBER DAE, Princeton, Rutgers, Sciences Po, Stanford SITE, Mannheim, Pittsburgh, and UCL. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w35008

The Cold War and the U.S. Labor Market
Ilyana Kuziemko, Donato A. Onorato, and Suresh Naidu
NBER Working Paper No. 35008
March 2026
JEL No. J01, N42

ABSTRACT

We argue that the Cold War contributed to the inclusive growth of the post-war decades. On the labor-demand side, we isolate exogenous shifts in military procurement across states and firms. We show that military procurement increases manufacturing employment and reduces inequality. Overall, the 1950s-to-1990s decline in defense production explains roughly one-quarter of the decline in manufacturing employment and nearly one-tenth of the rise of top-ten income share. On the labor-supply side, the Cold-War-era draft removed millions of young men from the labor force, significantly reducing young male civilian unemployment. Military procurement also increased voter support for hawkish foreign policy.

Ilyana Kuziemko
Princeton University
Department of Economics
and NBER
kuziemko@princeton.edu

Suresh Naidu
Columbia University
and NBER
sn2430@columbia.edu

Donato A. Onorato
Columbia University
dao2123@columbia.edu

> "If we can have full employment and full production for the negative ends of war, then why can't we have a job for every American in the pursuit of peace?" — Walter Reuther, President of the United Auto Workers, at the March on Washington for Jobs and Freedom, 1963

1 Introduction

There is broad consensus that the decades immediately following World War II were a golden age for the American worker. In the 1950s, for example, annual growth in GDP per capita averaged four percent and the earnings Gini coefficient for men reached a historic nadir of 32. Cohorts born in the immediate postwar decades also experienced much more upward mobility than either their predecessors or successors (Jácome *et al.*, 2024), and the racial gap in income declined (Bayer and Charles, 2018). In the 25 years that followed World War II, inflation remained low and the federal government ran largely balanced budgets. In explaining this robust and equitable growth, economists have proposed a strong labor movement that gave less-educated workers bargaining power (Farber *et al.*, 2021a); a large supply of educated workers to suppress the skill premium (Goldin and Margo, 1992); high marginal tax rates that discouraged executives from pushing for higher salaries (Piketty *et al.*, 2014); and a binding minimum wage that pushed up the left tail of the wage distribution (Levy and Temin, 2007).

In this paper, we seek to revive another explanation that was widely discussed contemporaneously but has received little attention in the more quantitative, modern inequality scholarship: the Cold War. On the labor-demand side, we leverage geographic shifts in military procurement patterns to show that defense spending increased manufacturing employment and reduced income inequality. On the labor-supply side, we shows that shifts in active-duty needs pulled young men out of the civilian labor force, reducing unemployment rates for a group that often faces high rates of joblessness and low wages. The economic benefits of procurement feed back into support for hawkish foreign policy, as we find that changes in state per capita procurement spending induced by the Korean and Vietnam wars increase support for those wars. In general, Democratic and union voters are the most supportive of aggressive foreign policy and military spending, highlighting the role of the Cold War in preserving the New Deal postwar political coalition.

The effect of the Cold War on the economy and the labor market in particular was widely discussed at the time (Reich, 1972; Melman, 1972). Joan Robinson wrote in 1962 that: "In the United States, the declared military budget accounts for nearly 10 per cent of national income, and is equal to 60 per cent of gross investment....[I]n fact Keynesian

prosperity has been a by-product of the Cold War." (Robinson, 1962). Around the same time, Robert MacNamara, President Kennedy's Secretary of Defense, derisively called the Pentagon's procurement program "a gigantic WPA [Works Progress Administration]."[1]

We analyze little-used data on defense procurement contracts aggregated to the state-year level from fiscal year (FY) 1951 (the twelve months ending in June 1951) onward. From FY 1958 onward, we make use of novel data that separates annual state procurement dollars into those allocated to research and development (R&D) versus all other purposes. We combine these data with more widely used prime contracts microdata made available by the National Archives that begin in 1966. Our outcome variables come from historical publications from the Internal Revenue Service (IRS), Bureau of Labor Statistics (BLS), Bureau of Economic Analysis (BEA), and annual corporate reports. In many cases we newly digitize these sources and are, to the best of our knowledge, the first to use them.

The first part of the paper focuses on labor-demand shocks and makes use of three moments in the Cold War that lead to plausibly exogenous shifts in the geographic distribution of Cold War procurement spending.[2] Due to data availability, our key outcomes will change somewhat across these episodes, though in all cases we can examine manufacturing employment and income inequality (as proxied by the share of income captured by the top ten percent of tax units).

The first episode is the shift from the Korean War to the "the New Look," the Eisenhower administration's military posture that heavily favored missiles over conventional procurement. While procurement during the Korean War benefited the traditional industrial states of the Northeast and upper Midwest, the shift to missiles benefited the Mountain West—this shift was large and rapid and, we argue in the paper, triggered by geographic and topographical considerations. We show in state-year event-study analysis that procurement spending leads to increases in manufacturing and formal employment and wages, while decreasing income inequality—consistent with it pushing out the demand for low-skill workers.

From the late 1950s until the early 1970s, three big shifts occur in rapid succession. Confirmation in 1961 that the USSR significantly trails the US in missile production alongside Defense Secretary Robert McNamara's budget cuts abruptly end the missile boom. But the

[1]The pedigree is even older: Keynes' 1933 letter to Roosevelt notes that pre-Depression it seemed that "war as the only legitimate excuse for creating employment by governmental expenditure." The basic idea pre-dates even Keynes, with Thorsten Veblen writing "it was the [1898] Spanish-American War, coupled with the expenditures for stores, munitions, and services incident to placing the country on a war footing, that lifted the [Panic of 1896] depression and brought prosperity to the business community" (Veblen, 1904, pp. 251, quoted in Barker, 2022).

[2]Ramey and Shapiro (1998) also use a "narrative" approach to classify big changes in military spending along the same war-induced big military build-up events we do, but focus solely on the time-series variation in aggregate spending, while we also consider the cross-sectional variation across states induced by the changes in procurement priorities.

reduction in defense spending quickly reverses with the mid-1960s escalation in Vietnam. By 1970, Nixon's withdrawal of troops from Vietnam sharply reduces the overall defense budget. Using variation from this period, all of the results in the previous paragraph replicate. For this period, we also digitize state-level data on quits per 100 workers, and show that procurement spending leads to labor market tightness as proxied by the quit rate, suggesting one possible mechanism. Moreover, during this period, we can separate procurement into R&D versus other spending. In all cases, the incidence of spending by type are as expected: non-R&D spending increases manufacturing employment, reduces inequality, and raises the quit rate, while R&D spending has much more muted if not null effects on these outcomes.

The final episode we study is the end of *détente*, which historians roughly date to the 1979 Soviet invasion of Afghanistan. We replicate the results on manufacturing employment using variation from this period, but in this particular episode we find no evidence that procurement spending reduces inequality. As before, the gains to manufacturing are specific to non-R&D procurement.

When we combine all years and estimate a two-way-fixed-effects regression using variation within states in procurement spending, we find an elasticity of manufacturing employment per capita with respect to procurement spending of roughly 0.09, which is similar though somewhat larger than the elasticities implied using the various hot-war and weapons-systems episodes. We also show that this elasticity is close to that obtained from an annual time-series regression controlling for linear time effects, suggesting that the results we find in the state-year analysis do not arise merely from zero-sum interactions between states. Our pooled elasticity estimates imply that the decline in defense production from the 1950s to the 1990s explains 29 percent of the decline in manufacturing employment per capita (and eight percent of the rise in top-ten share inequality). In the pooled specification we also interact procurement with attributes that past literature has connected to the "Great Compression." Indeed, we find that procurement reduces inequality more in settings with high union density, high "policy liberalism" scores (see Caughey and Warshaw, 2016, which includes, e.g., the state's minimum wage, top marginal rate), and greater skill shares. The effects of procurement on inequality are also significantly larger outside the South. Thus, the equalizing effects of procurement spending seem sensitive to state-level economic institutions.

We then turn to the role of the Cold War in contracting the labor supply of young men, a segment of the labor force that consistently faces high unemployment and low wages. Strikingly, in the 1950s, 24.6 percent of men aged 18-24 were on active duty service at any given moment, compared to 4.2 percent in the 1990s. Standard theory suggests that unemployment rates among young men who remain in the civilian labor market will decline when active-duty needs rise. We find a strong, negative relationship between civilian unemployment rates

and age-specific military active-duty shares. Consistent with the draft reducing civilian unemployment for young men, the relative unemployment of this group increases significantly after the draft ends in 1973. Extrapolating our results from the draft-era suggests that the end of the draft explains roughly three-fourths of the large, relative rise in ages 18-24 male unemployment in the ten years that follow.

The final section of the paper examines political support for the Cold War. State per capita procurement spending strongly predicts survey respondents' support for the two hot wars of the era (Korea and Vietnam). This result is interesting in terms of the political economy of military spending but also serves to demonstrate that the labor-demand benefits of military procurement documented above are sufficiently salient to voters. Moving beyond the geographical footprint of Cold War procurement spending, we find that in general Democratic and union voters are the most supportive of hawkish foreign policy and military spending. Our quantitative work supports the more qualitative work by historians (Brenes, 2020) that the Cold War helped sustain the New Deal political coalition.

The effects of military spending on economic activity is well-trod territory. A large macroeconomics literature uses U.S. military spending to estimate fiscal multipliers (Barro, 1981; Hooker and Knetter, 1997; Fisher and Peters, 2010; Nakamura and Steinsson, 2014; Ramey, 2019; Dupor and Guerrero, 2017). We add to this literature by focusing on compositional and distributional impacts (e.g., manufacturing employment and inequality). We also focus on medium-run outcomes by isolating geographic shifts in military spending caused by exogenous changes in procurement priorities, which allow assessment of the identification assumptions, looking at pre-trends and a variety of placebo outcomes. Our emphasis on separating R&D from other procurement spending builds on a recent macroeconomics literature emphasizing that the form of government spending matters for the magnitude of the multiplier (Cox *et al.*, 2020).

There is also a large literature on the decline in manufacturing in high-income countries. In the U.S. context, authors have studied the roles of automation (Acemoglu and Restrepo, 2020), globalization (Autor *et al.*, 2013), and the transition to a service economy as countries grow richer (Comin *et al.*, 2021). Our work suggests that the secular decline of military procurement as a share of GDP deserves further attention. Put differently, the massive Cold-War procurement budgets of the 1950s and 1960s likely propped up manufacturing employment above its counterfactual level had the country returned to pre-World-War-II levels of military preparedness. Cold War defense planners pushed for high baseline levels of industrial capacity such that conversion to war-time production could be executed quickly. Indeed, corporations often complained that the government pressured them to overproduce (in his 1949 State of the Union address, Truman threatens that the government would build its own

steel plants if the steel corporations didn't expand production). The Defense Production Act of 1950 gave the government new tools to expand production, including the power to force private corporations to accept government contracts and even to seize private property.[3]

The current policy debate over industrial policy in the United States and Europe (Juhász *et al.*, 2024) make it an opportune moment to revisit the impact of Cold War procurement. It is often claimed that the United States has managed to avoid industrial policy, but we argue that over several decades Cold War procurement developed local manufacturing capacity and provided sustained demand for low-skill workers, two common goals of industrial policy.[4] Moreover, policymakers at the time discussed procurement as a "jobs program" and the DoD explicitly favored contracts performed in "labor surplus areas." While there is a large literature on the economic effects of U.S. defense production during World War II (see Fishback and Jaworski, 2016 for an overview), it is hard to draw relevant lessons regarding industrial policy from a period of total war (the government ran massive deficits, enforced wage and price caps, and outright banned the production of consumer durable goods such as passenger cars). By contrast, the peak decades of Cold War spending were associated with balanced budgets, low inflation, and steady GDP growth. Thus, it serves as a much more sustainable model of "industrial policy" than does the 1940-1945 period.

Other papers have examined the effects of military spending on R&D and innovation. Gruber and Johnson (2019) documents the large role that defense spending played in scientific research and innovation during World War II and the decades that followed (and advocates an increase in this type of government investment to match Cold-War levels). Similarly, Kantor and Whalley (2023) focuses on the early years of the NASA program to show that areas that received NASA procurement dollars enjoyed lasting benefits in terms of manufacturing productivity.[5] However, little work has focused on *non-R&D* procurement, which is more likely to support the bottom half of the wage distribution. Moreover, while defense spending indeed becomes more research-focused over the course of the 1950s, R&D never exceeds twenty percent of the total procurement budget during the Cold War. Thus, non-R&D spending is vastly larger during this period while at the same time less studied.[6]

[3]See Barker (2022) for more on the 'overcapacity' debate. In the State of the Union address, Truman called on Congress to "to authorize an immediate study of the adequacy of production facilities for materials in critically short supply, such as steel; and, if found necessary, to authorize Government loans for the expansion of production facilities to relieve such shortages, and to authorize the construction of such facilities directly, if action by private industry fails to meet our needs."

[4]See (Kline and Moretti, 2014) on an earlier example of industrial policy, the Tennessee Valley Authority.

[5]Other papers studying the effect of military R&D spending include Pallante *et al.* (2021), Danzer *et al.* (2023), Howell *et al.* (2021) and the book by Ruttan (2006).

[6]A smaller literature has studied the effects of the Cold War on politics and economics in developing countries see e.g. Malis *et al.* (2021); Miguel and Roland (2011); Dell and Querubin (2018); Berger *et al.* (2013); Dube *et al.* (2011). While U.S. casualties in Cold War theaters of war are small by historical standards

Turning to our labor-supply results, beginning with Angrist (1990), labor economists have long considered the individual effect of military service on a variety of future outcomes (and economists have long contributed to the debate about conscription versus a voluntary system, see Oi, 1967). But fewer papers have examined the spillovers of the draft onto the civilian labor market more generally.[7] Warner (1995) provides a survey of the literature on military manpower, in which a robust result is that that poor labor market conditions increases *voluntary* enlistment.

A large comparative and historical literature examines how warfare shapes political economy outcomes. On inequality, Titmuss (1950), Scheve and Stasavage (2012), and Scheidel (2017) all argue war is an important equalizing force, a pattern Piketty (2014) documents in the capital-destruction and top income reductions that followed the two World Wars. On state capacity, Tilly *et al.*'s generative framework, that war made state fiscal capacity that in turn financed more war-making ability, is formalized by Besley and Persson (2009) and Gennaioli and Voth (2015), with corroborating cross-country empirical evidence in Dincecco and Prado (2012) and Queralt (2019). Voigtländer and Voth (2013) and Hoffman (2015) show interstate conflict was a primary driver of European economic development. On the 20th century United States, a qualitative account of the importance of military spending for aggregate economic prosperity is found in Higgs (2006), and for regional patterns of economic development in Markusen (1991). Our contribution extends this literature by combining individual survey data on political coalition formation with high-frequency identifying variation from a sustained modern military engagement.

The paper proceeds as follows. In the next section we briefly describe our data sources. Section 3 isolates results from the three episodes we highlight above and also provides historical context for why defense spending during these periods is plausibly exogenous to local labor market conditions. Section 4 shows the basic two-way-fixed-effects results that pool our entire 1951-2000 sample period. Section 5 documents the decline in civilian youth, male unemployment rates when military active-duty needs rise. Section 6 presents the political analysis and Section 7 concludes and offers directions for future work.

2 Data sources

Below we describe the key explanatory and outcome variables used in the paper. We provide greater detail on data sources in Appendix B.

(30-50 thousand in Korean and Vietnam war each), there are cataclysmic losses of life in developing countries, e.g., 3 million dead in Korean war, and over one million civilian deaths in Vietnam.

[7] One exception is the spillovers of the World War II draft on female labor force participation, see, e.g. (Acemoglu *et al.*, 2004).

2.1 Military procurement data

Our main explanatory variable comes from a DoD publication which tabulates "prime contracts" separately for the Army, Navy and Air Force by state from 1951 onward. Beginning in 1966, the National Archives provides contract-level data (used by Nakamura and Steinsson (2014)) and we confirm that our historical tabulations coincide with aggregations from the contract-level data in the years they overlap.[8]

From FY 1958 onward, we can disaggregate these state-year totals into the share spent on R&D versus all other procurement (we found these tabulations in various Congressional contemporaneous hearings, exact sources provided in the Appendix). To the best of our knowledge, no one has used these data before.

Military procurement data have a number of well-known limitations. First, the year of the contract may not reflect the year that the production is performed. Second, these data are noisy reflections of actual production, as contracts can be "clawed back" at the end of a conflict. Third, while each contract reflects in which state the work will be done (i.e., when General Motors receives a contract, the data reflect the location of the factory GM tells the government it will be carried out and does not simply assign the procurement dollars to its headquarters in Detroit, Michigan) it does not capture potential sub-contracting. However, Nakamura and Steinsson (2014) digitized Census of Manufacturing data from 1963 to 1983 on shipments to the government by state, and shows that this alternative measure closely matches state-level totals from the contracts data. Thus, subcontracting appears to stay within the state.

2.2 Historical outcome data

Inequality measures: As we hypothesize that the Cold War increased labor demand for less-skilled workers, we would ideally have state-year measures of earnings inequality such as the 90/10 or 90/50 ratios. Those measures cannot be consistently constructed annually until the 1977 CPS. We instead obtain tax-based state-year inequality measures from the World Inequality Database. In particular, we focus on the top-ten share of income and in the earlier decades the share of tax units in the state that earn enough to pay federal taxes. The share paying taxes has to our knowledge not been previously used as a state-level inequality measure. In Appendix C we show in the 1940-1970 Censuses, it strongly correlates with the share of households in a state with income above the federal filing threshold (roughly the

[8]To the best of our knowledge, these data from 1951-1965 have been little used (they were by Dupor and Guerrero (2017) in their extension of Nakamura and Steinsson (2014) and used as a control variable in the analysis of NASA spending in Kantor and Whalley, 2023).

thirtieth percentile of the household income distribution) as well as the the log 90/10 and 10/50 ratios. It thus serves as a reliable, annual, tax-based measure of state-level inequality sensitive to the *bottom* part of the distribution. In more recent decades, the share paying taxes is only weakly correlated with inequality measures, as the threshold falls in relative terms and more individuals file for the purposes of qualifying for the EITC.

Labor market measures: Beyond manufacturing employment (which we take from historical BLS tabulations), we also focus on the share of workers covered by the unemployment insurance (UI) system. In the 1950s and 1960s, workers in certain low-wage and largely Black occupations such as domestic service and agriculture remain outside the unemployment insurance system, so covered employment is a proxy for formality or a "good job."[9] We also obtain new data on separations and layoffs. While not used much by contemporary economists, the Bureau of Labor Statistics collected detailed information on quits (and layoffs and accessions) from 1958 through 1981 at the state level. Systematic information on quits and turnover does not become available again until JOLTS in the 2000s. We digitize these previously unavailable data at the state-month level.

2.3 Firm-level data

Top 100 Defense Contractors: Congressional reports published lists of the "Top 100" contractors, together with the sum of procurement dollars allotted. These reports aggregated contracts to subsidiaries, and then ranked the overall corporation in terms of dollars received from defense contracts. Reports are available most years between 1950 and 1961, albeit with inconsistent timing. We collected these reports and merged them to databases of public firm information (Computstat and CRSP, both available from the Wharton Research Data Services).[10]

Executive Compensation: We use the historical CEO pay data from Frydman and Saks (2010). Frydman and Molloy collected executive pay for the top executives of the largest companies in select years. We then link this data to the lists of Top 100 military contractors noted earlier. We present the list of the companies that we are able to merge both before and after the Korea-to-Missile shift in the Appendix.

[9] We thank Larry Katz for sharing historical data on coveraged employment and wages by state.

[10] We also have the prime contracts data from NARA beginning in 1966, which also has firm names. In order to keep the analysis comparable, we aggregate the NARA data into lists of "Top 100" firms ourselves, and check that these are similar in 1969, a year where both sources are available.

3 Isolating procurement shocks during the Cold War

In this section we focus on three periods where we argue shifts in procurement patterns are plausibly exogenous. While this approach allows us to isolate sharp changes in state-level procurement spending, focusing on states makes us in principle vulnerable to the "missing intercept" critique. We defer this concern to the next section, where we examine aggregate time-series evidence. In this section we focus on showing that wherever military procurement spending went, good jobs for low-skill workers seemed to follow.

3.1 Korean War to the "New Look"

3.1.1 Background

The procurement needs of the Korean War were broadly similar to those of World War II (see Figure 1): aircraft constituted 27.3% and 31.5%, respectively, of total procurement dollars, and in both conflicts traditional artillery (tanks, weapons, ammunition) made up the plurality of procurement resources.[11] However, as the Eisenhower administration quickly wound down the Korean War they inherited from Truman, they promoted the "New Look," a comprehensive reprioritization of military procurement toward missile production.[12] That the Soviets had progressed more quickly than expected on their own nuclear program further bolstered the case for more missile research and production, even before the shock of Sputnik in 1957.[13] Figure 1 shows that by 1962, missiles account for over one-third of military hard-good procurement, rising from essentially zero during Korea.

Missile production mostly fell under the procurement authority of the Air Force. Low population density, large areas available for runways, the need to project force over long distances in the Pacific theatre, and the Great Circle distances to the Soviet Union over the Arctic all pushed Air Force and missile production towards the Western and Mountain states. Moreover, large stretches of empty space was needed for *testing*, another advantage these sparsely populated states presented. And for some more classified procurement items, "regulations stipulated that defense factories be built in isolated regions away from popula-

[11] This subsection draws heavily from *History of the Office of the Secretary of Defense, Volume I: The Formative Years* (Chapter 11, "The Military Budget for Fiscal Year 1949: Rearmament Begins.").

[12] In fact, it was a continuation of nascent reforms during the late 1940s interrupted by Korea. "The New Look" had its antecedent in the immediate pre-Korean War policies of the Truman administration, which had begun to emphasize the role of airpower and nuclear weapons in an effort to diminish reliance on the manpower-intensive ground forces and hold down the cost of national defense." The prevailing belief after World War II is that the Soviets had innate advantages in conventional warfare because of the size of the Red Army.

[13] The CIA had originally estimated that the Soviets would develop an atomic bomb by 1953. In fact, the Soviets tested atomic, thermonuclear, and hydrogen bombs in 1949, 1953 and 1955, respectively.

tion centers" (Brenes, p. 213). Florida was another big winner, as Cape Canaveral had been identified since the early 1940s as an ideal launch site for missiles and rockets (being closer to the equator meant launches could take advantage of greater velocity from the earth's rotation and being a cape allowed launches surrounded on three sides by the Atlantic Ocean to avoid civilian harm in case of accidents).[14] Meanwhile, the declining demand for tanks and trucks pulled funds away from the industrial areas of the Midwest and Northeastern.

3.1.2 Shifting procurement patterns

Based on this historical record, we generate a "missile shift" variable $\Delta_s^{missile}$ for each state s as follows:

$$\Delta_s^{missile} = \log(Procurement_{s,1954-1961}) - \log(Procurement_{s,1951-1953}). \quad (1)$$

FY 1954 almost entirely excludes the Korean war and the FY 1961 budget is the last in the Eisenhower administration, so these years capture the New Look pivot to missiles described above. We compare this value to spending in FY 1951-1953 (Korean War, and also simply the first three years of our annual state procurement dataset). To facilitate interpretation for the event studies, we standardize the variable to have mean zero and standard deviation one.

To more formally test the claim that the procurement shift was driven by geographic and topological considerations, we examine a variety of pre-shift placebo outcomes in Appendix Table A.4. We show that the missile-shift variable is uncorrelated with pre-period economic activity in the state as well as political outcomes such as the party of the governor, Senators, House members or total state representatives on the House Armed Services Committee. It is also uncorrelated with all presidential election vote shares in 1940s. All of these pre-treatment placebos lend support to the historical accounts showing that the primary determinant of the shift in military expenditures was driven more by military technological priorities interacting with state geographies, rather than underlying economic or political variables that could confound our analysis.

Figure 2 panel (a) shows how the magnitude of the missile shock varies geographically over the 48 contiguous states. As expected based on the historical record, the Mountain states plus Florida are the major winners. States that once served as the "Arsenal of Democracy" during World War II, namely the upper Midwest, lose out, as the military turns away from

[14]While most of these missiles were offensive in nature, the late 1950s also saw early steps toward anti-ballistic missiles (missiles that intercept enemy missiles) production. In 1957, the US and Canada established the North American Air Defence Command (later known as North American Aerospace Defense Command, or NORAD), headquartered in Colorado Springs, Colorado.

earlier styles of combat. Panels (b) and (c) of the figure show how the missile-shock variable relates to state-year procurement spending across time. Panel (b) shows the raw patterns by breaking up states into terciles based on $\Delta_s^{missile}$. As an annual measure of state procurement, we plot logged procurement divided by once-lagged state GDP (to normalize by the size of the state economy). This "first-stage" graph is largely mechanical, but it depicts the swift nature of the shift.

Panel (c) shows the result from estimating the following event-study equation:

$$\log\left(\frac{Procurement_{st}}{GDP_{s,t-1}}\right) = \sum_{k\neq 1953} \beta_k \Delta_s^{missile} \mathbb{I}^{t=k} + \eta_s + \delta_{f(s)t} + e_{st}, \tag{2}$$

where the event-study coefficients β_t capture the relationship between log procurement to GDP ratio and the missile shock relative to the omitted year 1953 and conditional on state, η_s, and year, δ_t fixed effects. We can interact state characteristics $f(s)$ with year fixed effects to probe robustness.

Panel (c) echoes the results from (b) but in this case we show that the relationship is robust to $region \times year$ fixed effects (second series). It is also easier to read off magnitudes from the event-study graph: as the shock variable is standardized, multiplying by three gives a rough sense of a "winner" versus a "loser" state. In just a few short years, the shift from loser to winner states is akin to roughly 150 log points increase in procurement dollars over state GDP.

Recall that we do not have state-year procurement data before 1951. For many of the event-study outcomes in the next section, we can examine longer pre-trends as we have longer sample periods for most variables, but unfortunately we cannot study long pre-trends in the first-stage due to data limitations.

3.1.3 Effects on manufacturing and formal employment

We follow the same logic as in the first-stage event study but now labor market outcomes serve as our dependent variables. Figure 3 estimates manufacturing employment, both per capita (panel a) and total (panel b). In both figures, no pre-period coefficient can be distinguished from the omitted year of 1953. But after 1953, manufacturing employment becomes highly positively correlated with the missile-shift variable, in a manner very similar to the "first stage" in Figure 2 (b). Note that manufacturing workers are throughout our period significantly less educated than other workers (see Appendix Figure A.1), so while we cannot directly observe annual measures of state-level employment by education, we interpret these results as directly increasing employment for low-skill workers.

We can use this event study alongside that of the first stage to calculate an implied

elasticity of per capita manufacturing employment with respect to procurement of $0.045 \div 0.757 \approx 0.059$. We can also multiply the implied reduced-form effect by three to examine the effects on a typical "winner" versus "loser" state (assuming they are 1.5 standard deviations above and below the mean, respectively). A winner state enjoys approximately $4.5 \cdot 3 = 13.5$ log points of additional manufacturing employment per capita in the post- relative to the pre-period. As large as this effect is, the effect is even larger when we do not divide by population size, as depicted in panel (b). In this case, the same comparison produces a 20.4 log point effect. The comparison between the two subfigures suggests that population shifted modestly toward missile states.

It is possible that the rise in manufacturing employment merely reflects workers shifting from other sectors. For this reason, we also examine "covered employment" from historical UI data (panel c). During the 1950s and 1960s, the disproportionately Black occupations of agricultural and domestic laborers (along with a small number of government employees) were excluded from the UI system, so "covered employment" in this period is a proxy for a "good job" in the formal sector. Panel (c) shows that in fact there is a larger rise in per capita covered employment than manufacturing employment, suggesting that instead of merely shifting workers into manufacturing from other covered sectors, procurement spending created positive spillovers and created new, quality jobs outside of manufacturing as well.

As a plausibility check on these results, we note that the DoD explicitly acknowledges that the missile shift created winners and losers (while at the same time emphasizing objective military needs precipitated it). A 1962 report titled "The changing patterns of defense procurement" notes that: "This phenomenon [the shift to missiles] has created significant change in the military industrial base in many communities, states and regions...It should be recognized, however, that Defense must seek its needs where capability exists, in order to be responsive to the technological requirements of modern warfare....It is not surprising that many communities in these five states [MI, OH, IL, IN, and WI] have encountered recurrent unemployment problems."

3.1.4 Effects on inequality and wages

We begin with two tax-return-based measures of inequality: the share of income accruing to the richest ten percent and the number of federal income tax returns as a share of the adult population. The first measure will capture whether the overall gains from procurement accrue to the bottom 90 percent, as we would expect. The second focuses more on the bottom of the distribution, given that roughly thirty percent of households during this period were too poor to have any federal income tax liability. Thus, an increase in this measure indicates a decline in state income inequality. See Appendix C for a validation of this claim

using decennial Census data—in particular, this measure correlates significantly with wage inequality measures such as the 90/10 ratio and 50/10 ratio.

Figure 4 panel (*a*) shows that the missile shift is associated with a significant decline in top-ten-share inequality in the post-period. Relative to a typical loser, a typical winner state exhibits a $3 \cdot 0.5 = 1.5$ percentage-point decline in the top-ten share (nearly one-half of the standard deviation of 3.32 percentage points). Panel (*b*) shows that inequality also significantly declines in missile states when proxied by the share of the population that owe federal income tax. Using the same winner-loser calculation, the effect size is equal to one-third of a standard deviation.

The bottom two panels of Figure 4 examines wage effects. Given that the manufacturing and covered sectors expand rapidly due to the missile shift, we might expect wages in these sectors to decline due to composition effects. In fact, if anything, we find the opposite effect. While somewhat noisy, manufacturing wages shows a significant increase due to the missile shift (panel *c*). The missile shift appears to boost covered wages as well, though in this case the missile shift appears to have reversed a negative pre-trend (panel *d*). These wage effects provide further support to the idea that procurement pushed out the labor demand curve, especially for manufacturing workers.

In the Appendix, we test robustness of the above results to controlling for factors that have been linked to the Great Compression. Appendix Figures A.3 shows robustness to controlling for state-year union density (Farber *et al.*, 2021b). Given the massive mobilization demanded by World War II, Appendix Figure A.4 controls for state-level World War II procurement interacted with year fixed effects to confirm that our results arise specifically from Cold-War era procurement shifts and not lingering effects from World War II (Garin and Rothbaum, 2025). Piketty *et al.* (2014) argue that the historically high top marginal rates of post-war income tax schedules kept inequality low, so in Appendix Figure A.5 we control for state-year top rates. Finally, given the evidence that the expanding supply of skilled workers helped tamp down inequality (see Katz and Murphy, 1992; Autor *et al.*, 1998; Goldin and Katz, 2008), Appendix Figure A.6 control for the state-level skill share in 1950 interacted with year fixed effects (intercensal measures are not available until the 1970s CPS, so we take interacting the 1950 measure as a second-best solution). In all cases, the basic pattern of coefficients hold, and we conclude that the procurement effects on employment and inequality are independent of these other factors.

Interestingly, there is no effect of 1950s-era procurement on state-level union density (see Appendix Figure A.7). Unions organize most heavy industry during World War II, when the federal government explicitly facilitated unionization in return for a no-strike pledge (Farber *et al.*, 2021b). But no such arrangement existed beyond 1945 and the expansion

of procurement to missile states did not lead to rising union density in those states. It is also worth noting that many of the military contractors key to missile procurement (e.g., Lockheed, Boeing) had already been unionized during World War II, perhaps reducing the potential scope for procurement effects on density.

3.1.5 Firm-level effects on executive pay

Did Cold-War procurement revenue affect within-firm inequality? Just as the shift toward missiles changes the geography of procurement, it also shifts procurement contracts from automotive to aeronautic firms (see Figure 5 panel (a) for two illustrative examples for automotive and aeronautic, General Motors and General Dynamics, respectively). We do not have data on the full earnings distributions within firms from this period, but data on executive pay is available. The government did not impose salary caps on executive pay of military contractors during the Cold War (in contrast to World War II). However, Congressional hearings did routinely publish the salaries of executives at defense contractors, and in this section we ask whether this more informal monitoring kept executive salaries lower at major contractors than we would otherwise expect. If, within firm, the benefits of procurement flowed differentially to the C-suite, it would call into question our state-level results that procurement reduced top-ten-share inequality.

For our outcome measure, we follow the literature on CEO pay and look at the log of CEO pay over market capitalization.[15] As military procurement shocks increase market capitalization (see Appendix Figure 5), and CEO compensation often includes a pay-for-performance component, the outcome we are interested in is whether the pay-for-performance component of compensation is attenuated in firms that receive defense contracts.

We focus on an event-study approach, paralleling our state-year analysis. Both the Frydman and Molloy sample, as well as the Top 100 sample are selected on a number of dimensions, so we aim for internal validity and examine robustness to a few different definitions of treatment. Our baseline specification counts the number of times a company appears in the Top 20 contractors, and we assign all the other companies for which we have data a value of 0. Because we do not see companies below the Top 100, we do not want to impute zero procurement dollars to those companies. Top 20 contractors also account for a large share of the procurement spending. In the Appendix we show robustness to varying the treatment definition from Top 5 to Top 50. We define a firm level treatment measure as:

[15] In particular, we focus on the levels analogue of the "b^I" measure suggested by Edmans *et al.* (2009). We probe robustness to alternative specifications (the "b^2" and "b^3 measures in Edmans *et al.* (2009)) in the Appendix.

$$\Delta_f = \sum_{t=1954}^{1961} \frac{\mathbf{1}\{f \in Top20_t\}}{7} - \sum_{t\in[1951,1953]} \frac{\mathbf{1}\{f \in Top20_t\}}{3}$$

We then estimate the following specification using an event-study, where w_{ft} is total executive compensation at firm f and year t, V_{ft} is total company market capitalization, μ_f are firm fixed effects, and $\eta_{ind(f),t}$ are year fixed effects interacted with (one-digit-SIC) industry codes:

$$\log(w_{ft}^{exec}/V_{ft}) = \sum_{k=1948}^{1962} \beta_k \mathbb{I}[t = k]\Delta_f + \mu_f + \eta_{ind(f),t} + \epsilon_{ft}. \tag{3}$$

We plot the β coefficients in Figure 5 panel (b). Executive pay relative to market capitalization is balanced between "winning" firms and "losing" firms prior to the missile shift, after which the "winning" firms experience a significant decline in the pay-to-market-cap ratio.

In Appendix Figure D.1, we show that these treated firms also increase employment (measured in Compustat) as well as payroll (measured from annual reports). The payroll numbers are somewhat difficult to interpret, as they are from annual reports where what is reported is sometimes global and sometimes national payroll. The payroll effects are roughly identical to the employment effects, suggesting that we cannot detect any fall in wages that might be driven by negative selection of workers into these expanding firms. The employment and payroll effects lend firm-level evidence to our overall state-level identification strategy. The firms that received procurement contracts raised employment, did not depress average wages, and did not raise CEO pay proportionate to the increase in the market capitalization.

This moment where defense procurement shifts from the automotive to the aeronautic sectors is unique in the Cold War. After the late 1950s, the identities of the prime contractors (e.g., Lockheed, Boeing, General Dynamics) do not shift again and thus we lack a "first stage" to replicate the CEO-pay analysis in later years. Famously, the rapid rise of prime defense contractors over this decade (who, unlike consumer-facing companies like GM and Ford in earlier years, depend almost entirely on the government for sales) prompted Eisenhower to decry the "military industrial complex" in his 1961 farewell address.

beyond the control of management influenced influenced 1951 a results to large degree. Foremost among them were the requirements of national defense...GM defense deliveries in 1951, although sufficient to meet scheduled Government orders....were not enough to offset the decline in production of passenger cars, trucks and other civilian products

3.2 Procurement shifts in the 1960s

3.2.1 Background and empirical strategy

During the 1960s, a number of geopolitical events had large and distinct effects on R&D and non-R&D military procurement. A September 1961 National Intelligence Estimate confirmed that the Soviets were far behind the US in missile production, prompting Kennedy to replace "New Look" with "Flexible Response," a philosophy that favored conventional procurement under the assumption that combatting the Soviets would require contesting smaller proxy wars and could not be accomplished via "massive retaliation." Furthermore, President Kennedy's Defense Secretary Robert McNamara embarked on a reorganization of Pentagon procurement to reduce duplication and waste. McNamara was a staunch opponent of using the Pentagon as a jobs program.[16] McNamara's economizing along with the end of the missile boom led to an abrupt plateauing and then eventual decline in the procurement budget in real terms, especially R&D, as we show in panel (a) of Figure 6.

This decline was short-lived, as U.S. involvement in Vietnam grew rapidly in the years after Kennedy's assassination. As the top panel of Figure 6 shows, both categories of spending rise during Vietnam, but (as is generally the case during "hot" wars) non-R&D spending rises more. By 1970, Nixon's "Vietnamization" strategy (where the Pentagon explicitly reduced U.S. troop levels and transferred combat responsibilities to the South Vietnamese) allows substantial cuts to the procurement budget. Beyond the shift in the Vietnam War strategy, in the early 1970s Nixon and his advisor Henry Kissinger made substantial progress on arms deals with the Soviets, ushering in the era of *détente*.

We take two approaches in estimating procurement effects during this period. First, we note that the biggest shock during this period is the Vietnam War, so we can use the Gulf of Tonkin incident as the "event" and perform event-study analysis that parallels that from the previous subsection.[17] Results (see Appendix Figures A.8 – A.10) are quite similar to what we find in the 1950s. Note that we use our newly-digitized data on quits (not available during the Korea period) to show that procurement is associated with higher quit rates, consistent with greater labor demand leading to tight labor markets.

A second approach is to make use of our more disaggregated state-year procurement data from this period and include state-year R&D and non-R&D spending simultaneously

[16]From Brenes (2020), pp. 108-109: "McNamara rejected the notion that the military budget should ever generate full employment, despite a twenty-year precedent for being a New Deal-like jobs program. 'We elected a Democratic President in 1960, and now we find out a Republican Secretary of Defense is running the country,' said one defense worker."

[17]The incident is controversial in that LBJ may have been looking for an excuse to escalate. Unlike the North's invasion of South Korea in June 1950, there is no obvious "start" to the Vietnam War, but this August 1964 event is commonly cited as an inflection point.

in a two-way fixed-effects regression. This "horse-race" style regression allows us to test whether the manufacturing and inequality effects are driven by non-R&D procurement, as we would expect based on the much larger magnitude of the non-R&D spending and our implicit labor-demand model. This approach also recognizes that the Vietnam build-up is not the only shock to procurement spending during this period and thus allows us to use the variation that arises from the end of New Look as well as the decline in procurement associated with Vietnamization and *détente*.

3.2.2 Main results

For the sake of comparison, panel (*a*) of Table 1 shows the relationship of our outcome variables to *total* state-year procurement, not yet taking advantage of the additional information on R&D versus other spending. For the most part, results are similar to what we found in our event-study specification of the Korea-to-Missile transition. The coefficients suggest that a one percent increase in procurement per capita is associated with a 0.049 log-point increase in manufacturing employment per capita, quite close to the implied elasticity from the event-study analysis in the previous subsection (0.059). We again find a negative effect of procurement on top-ten share inequality (though somewhat smaller than those implied in the previous subsection) and a large, positive effect on the share of the population filing a tax return. Thus, we again find that state-level procurement reduces income at the top and pulls up the left tail of the distribution.

As before, procurement pulls workers into covered employment but in spite of likely deleterious composition effects, log covered wages rise. As noted, the quit rate rises. As in the Korea-period analysis, we find no effects on union density (not shown).

In panel (*b*) we repeat our analysis but disaggregate spending into R&D and all other. While the two measures are correlated, to the extent we are able to disentangle them, it appears that it is the *non*-R&D component of procurement has a far more significant effect than the R&D component, as we would expect given the much larger share of GDP constituted by non-R&D spending. The inequality measures are also much more sensitive to non-R&D procurement, again as we would expect from a labor-demand story. The top-ten share declines significantly with non-R&D procurement and if anything rises (though not significantly) with the R&D component. Covered employment is an exception, where both components of procurement have similar, positive and significant effects, though, again, only the non-R&D component increases covered *wages*. The quit rate is also sensitive to both R&D and non-R&D spending.

In summary, aggregate procurement during the 1960s has very similar effects to those estimated from the Korea-to-Missile event-study analysis. And when we break up aggregate

procurement into R&D versus others, the two components have the effects we would expect, with the non-R&D component responsible for increasing manufacturing employment and reducing inequality. Thus, while past work has generally focused on R&D procurement alone, we show here that non-R&D spending had large effects on employment, wages, and inequality.

3.3 The end of *détente*

President Nixon and his advisor Henry Kissinger along with Soviet premier Leonid Brezhnev pursued a policy of *détente* in the early and mid-1970s.[18] Kissinger emphasized arms deals with the Soviets over confrontation and rivalry. This period witnessed a general thawing of tensions as well as important arms control agreements.[19]

But by the late 1970s, leadership in both countries had become more aggressive. President Carter's National Security Advisor Zbigniev Brezinski renewed ambitions of "rolling back" (as opposed to merely "containing," the official policy since Truman) communism in Eastern Europe and hawkish generals and industrialists held sway over an aging Brezhnev. The death of the *détente* era is typically dated to the 1979 Soviet invasion of Afghanistan, though tensions had been rising earlier. Military budgets reverse their decline under Carter with this trend accelerating under Reagan (see Figure 6, panel b).[20]

3.3.1 Shifts in procurement, 1973-1986

For the sake of completeness, we again perform event-study analysis, using 1979 (Soviet invasion of Afghanistan) as our "event" (see Appendix Figures A.11 – A.13). But as in the 1960s, such an analysis makes only limited use of the available variation. As we show in the bottom panel of Figure 6, the increase in non-R&D pre-dated that of R&D spending by two years (and of course benefits different sets of states). Again, we turn to a two-way-fixed-effects regression as the most efficient way to make use of all variation.

Table 2 panel (a) shows the TWFE estimates from the Carter-Reagan build-up, again starting with aggregate procurement as the variable of interest. Consistent with the previous

[18]Much of the information in this subsection is taken from Hoffman (2009), Chapter 1.

[19]SALT (Strategic Arms Limitations Talks) produces two key arms-control milestones in 1972: SALT I (which put limits on new nuclear missile silos as well as submarine-launched nuclear missile capabilities) and the Anti-Ballistic-Missile (ABM) Treaty, which limits the use of missile defense to the national capital and one other site. Ballistic missile defense was viewed as especially dangerous because it could in theory give a country first-strike capability (thus evading the logic of mutually-assured destruction).

[20]It may surprise modern readers that *détente* ended with Carter and not Reagan, given the large military budgets that Reagan oversaw. But in fact the military build-up associated with the 1980s begins under the Carter administration. In his memoirs, the Soviet ambassador to Washington from this period writes: "[I]t had been quite impossible for me at that moment to imagine anything much worse than Carter."As quoted in Hoffman (2009), pp. 33-34.

two sample periods, manufacturing employment and covered employment increases in treated states. Panel (b) shows that, as in the previous subsection, these effects are essentially fully driven by the non-R&D spending.

But in this case there is no effect of procurement on wages or inequality. These null results hold whether we aggregate procurement spending in panel (a) or disaggregate in panel (b). While we can no longer use the share paying taxes as a reliable measure of inequality, from 1977 onward we can generate state-level inequality measures using CPS microdata. We again find null results when the log 90/10 ratio serves as the outcome. We return to this null result in the next section as well as in the Conclusion.

4 Pooled analysis

4.1 Main results

Pooling all of the variation in the postwar period allows us to get a sense of the overall explanatory power of the defense spending variation. We estimate a basic two-way-fixed-effects regression with our main state-year-level outcomes as the dependent variable and state-year procurement dollars as the explanatory variable:

$$Y_{st} = \beta \log\left(\frac{Procurement_{st}}{GDP_{st-1}}\right) + \eta_s + \delta_{r(s)t} + \gamma X_{st} + e_{st}, \tag{4}$$

where notation follows from previous equations and X_{st} are time-varying state-level controls we include in certain robustness specifications. Appendix Table A.5 again shows a variety of lagged economic and political outcomes, similar to our placebo exercise for Korea. Again, we reassuringly find that there are no correlations between lagged shares of agriculture or Democratic political support with the procurement variables. As our goal in this exercise is to extend our sample period as far back as possible, we focus on total procurement (which extends back to FY 1951) instead of breaking up expenditure into R&D versus other.

Table 3 focuses on the outcomes that are of interest and measured consistently over the entire sample period: manufacturing employment and overall inequality (in this case, as measured by the top-ten share of income). Col. (1) shows a strong, positive relationship between log procurement as a share of state GDP and log manufacturing employment per capita. The manufacturing employment estimate is similar though somewhat larger than those that use isolated variation from the missile-shift and Vietnam eras. Col. (2) shows these results are robust to using levels instead of logs, and we return to these results in a moment when we compare our results to recent papers on manufacturing decline. Cols. (3) and (4) show the analogous results for top-ten inequality as the outcome. The coefficient in

col. (3) suggests that the procurement effect on inequality over the pooled period is slightly smaller than that for the Korea-to-missiles period but larger than those found in the Vietnam era.

We calculate the share of the total changes in our dependent variables that the decline in procurement can "explain" in a regression sense. Using our preferred log-log specification, the decrease in procurement explains nearly thirty percent of the decline in manufacturing employment per capita. The decline in procurement explains 8-9 percent of the rise in the top-ten share. We suspect that other measures of inequality would have shown larger effects, given that the top-ten measure was never as robust as our "share return" inequality measure, which better picks up improvements in the bottom part of the earnings distribution. Unfortunately, we cannot use the "share return" measure of inequality over the entire sample period given it no longer serves as a proxy for wage inequality in the 1980s and onward.

We added the specifications in levels to more directly compare our results to those in the recent literature on the effect of trade shocks and automation on manufacturing employment. The estimate in col. (2) suggests that the 6.6 pp decline in procurement as a share of aggregate GDP from the beginning to end of our sample period can explain $100 \cdot .00117 \cdot 6.6 = 0.779$ percentage points of the decline in manufacturing employment per capita, or 18% of the entire 4.26 percentage point decline. As a comparison, Autor *et al.* (2013) find that the China shock explains 25% of the decline in manufacturing jobs per capita between 1990 and 2007, though the total decline in manufacturing over that period is smaller than over our longer 1951-2000 sample period. Similarly, the results in Acemoglu and Restrepo (2020) suggest that the increase in industrial robots per capita can explain up to fourteen percent of the decline in manufacturing employment per capita over the same 1990-2007 period.[21]

In summary, our defense spending estimates explain large shares of the decline in manufacturing employment and are estimated during the heyday of manufacturing employment. The massive amounts of government expenditure stimulated by Cold War procurement policies propped up manufacturing for decades, and its decline was as large a blow to American

[21] Note that we compare the first five to the last five years of our sample period to perform this calculation (i.e., 1951-1955 versus 1996-2000). Our estimate for manufacturing employment per capita in 1951-1955 and 1996-2000 are, respectively, 0.105 and .063, based on aggregating up our state-year level data and weighting by population. Note that FRED does not report aggregate *total* population until 1959, so to check these estimates we instead divide manufacturing employment by the *civilian over 16 population*. This FRED-based aggregate measure declines from 0.143 to 0.084 (a 5.96 pp decline), almost identical proportionately to the decline in our weighted state-year data (note that unlike the aggregate FRED data, we drop Alaska, DC, and Hawaii throughout). By comparison, in 1990 and 2007 (the start and end points of the Autor et al. analysis), the FRED data suggest that manufacturing jobs over civilian 16+ population is .0935 and .0599, respectively, a 3.37 pp decline. The analogous measure when we divide by total population is .0707 and 0.0460, a 2.48 pp decline. Note that the main focus of Acemoglu and Restrepo (2020) is total employment to population ratio, so we use the largest weighted result in their Appendix Table A14 to examine the effect of robots on *manufacturing* employment per capita.

manufacturing jobs as globalization or technological change.

4.2 Robustness and extensions

We examine robustness of the pooled estimates in the Appendix. To examine robustness to functional form, Panel A of Appendix Table A shows an analogue of Table 3, except replacing the independent variable with the *share* of procurement over (lagged) GDP, without a log transformation. This log-on-levels specification allows us to interpret the coefficient as the percent effect of a one-percentage-point increase in procurement over GDP. In both of these specifications we find results consistent with Table 3: significant effects of procurement on employment and inequality.

Panel B of Appendix Table A presents results using a Bartik instrument to further probe the robustness of our main estimates. Given the historical narrative, we feel comfortable using the observational variation over time within state, which is also more easily interpretable than an IV regression. However, as further robustness, we construct an instrument by interacting the national procurement in a given *military branch* (i.e., Army, Navy, Air Force) with the initial share (measured in 1951) of a branch's procurement in a given state, all divided by lagged GDP. To minimize violations of the exclusion restriction, we use the leave-one-out aggregate procurement, omitting state s from the aggregate spending that is interacted with the 1951 initial share in s. We reproduce the specifications in Panel A of that table using 2SLS, and find IV coefficients generally larger than the OLS. This difference could be due to different average versus local-to-compliers treatment effects or the inherent measurement error in the procurement variables discussed earlier (e.g., delays and clawbacks, meaning that reported awarded expenditure is only a rough proxy for completed expenditure). Nonetheless, the overall similarity in the pooled analysis and the event-specific estimates on the variables that exist throughout the fifty years of data is reassuring.[22]

In Figure 7 we examine heterogeneity in the effects of procurement, focusing on attributes past work has linked to the "Great Compression": union density, variation in other labor market policies, and large increases in the skill share. We also explore regional variation. In panel (a) we show that the effect on manufacturing employment is significantly higher in states with higher union density and higher values of the 'policy liberalism' index (while differences by region and skill share cannot be distinguished from zero).[23] However, in panel (b), the heterogeneity in the treatment effect is significant across all four measures. Procure-

[22] As Appendix A.8 of Borusyak *et al.* (2025) discuss the functional forms chosen in Bartik instruments, and point out the log of a shift-share instrument is not a shift-share instrument, so we are restricted to using this IV in our levels specification.

[23] The "policy liberalism index" varies over time, but includes whether a state has a minimum wage, its right-to-work status, and its top marginal income tax rate. See Caughey and Warshaw (2016) for detail.

ment reduces inequality more in settings of higher union density, more policy liberalism, higher skill shares, and outside the South. These patterns are consistent with the absence of an inequality effect during the Carter/Reagan defense boom, given rapidly falling levels of union density during this period and a general move toward the right on policy. Procurement was also more tilted toward Southern states during this period relative to earlier years.

4.3 Evidence from aggregate time series

As a final exercise to probe the relationship of military spending on manufacturing employment and inequality, we ask if the relationships we have documented at the state-year level hold in the *aggregate* time series. As noted, so far we have shown that *relative* variation, which we argue is plausibly exogenous, across states over time correlates with increased manufacturing employment and reduced inequality. But as this result depends on relative comparisons across states (or, given our inclusion of region-year fixed effects, across states within the same region), it cannot directly speak to aggregate effects.

Figure 8 takes up this task. To extend the relationship back to World War II, we use the share of GDP spent on defense (as opposed to only procurement). While the state-year data availability means that much of our analysis focuses on 1951 and later, it is instructive to examine the aggregate time series going back to FY 1940 both to capture the effect of World War II but also the massive *demobilization* that followed. Indeed in 1947, the defense share of GDP reached a low it would not again attain until the collapse of the Soviet Union.

Figure 8 panel (*a*) shows that manufacturing employment is highly sensitive to defense spending, consistent with our state-year evidence. As is well known, manufacturing booms during World War II, but, less discussed, plummets in the years between World War II and Korea. After bouncing back during Korea, it slowly declines alongside the declining defense share of GDP, until both series experience an uptick during Vietnam. The relationship is predictably weakest in the 1970s and 1980s, when manufacturing is buffeted by two oil crises as well as the Volcker interest-rate shock. Overall, the relationship remains highly significant even after taking out a linear time trend and dropping the World War II period. The point-estimates (between 0.06 and 0.07) fall in the range of those we estimate at the state-year level either in the pooled two-way fixed effects or when we isolate episodes of plausibly exogenous variation.

Figure 8 panel (*b*) shows a similarly robust, negative relationship between top-ten inequality and the defense share of GDP. To our knowledge, the rise in inequality in the interwar years of 1945-1950 has received little attention in the literature. Inequality was on the rise until the outbreak of the Korean War and then remains low during the post-Korea sustained

military spending. Again, the period where the relationship is weakest is the early 1980s when inequality and defense spending briefly rise in tandem. Overall, the relationship is negative and significant after adjusting for a linear time trend. This analysis yields a coefficient on defense spending of a somewhat larger magnitude (-2 as opposed to a range around -0.2 to -0.66 from the state-year analysis). Note that, unlike manufacturing employment, inequality does not "aggregate up" from smaller units, so it's harder to compare the time-series and the state-year analysis. But we are reassured that the sign of the relationship appears robust.

5 The military draft and the labor supply of young men

So far we have argued that military procurement increased labor demand for low-skill workers. In this section, we document the effects of removing low-skilled workers—namely, young men—from the civilian labor force.

5.1 Background on the Cold-War-era draft

The active-duty population during the Cold-War-era draft period (1947 until the draft ended in January 1973) dwarfs that of the modern period as a share of the population. In 2019, the U.S. active duty population numbered 1.3 million, or 0.39 percent of the population. During Korea, that same share was 2.25 percent and even in a peace-time year such as 1955 it was 1.75 percent (see the first series in Figure 9).

Not all active-duty military were drafted (synonymous with "inducted"), though the DoD estimated that most volunteers were in fact induced by the draft. In most years of the Cold-War draft, men were at risk of induction from ages 18.5-26. Volunteering (allowed from age 17 onward) provided more certainty over the timing and branch (Navy, Army, Air Force) of service. We thus model unemployment as a function of the total active-duty population, not just those drafted.

5.2 Data sources

From the BLS, we obtain unemployment rates by age group, sex and year from 1947 through 1983 (we use the last ten years of data in exercises where we construct counterfactual unemployment rates had the draft continued at the active-duty-rates of the draft era). We obtain active-duty population by age group from Selective Service Reports (see Data Appendix for more detail).

Our analysis sample includes annual active-duty population shares and unemployment rates for the following age groups: 16-17, 18-19, 20-24, 25-34, 35-44, 45-54, and 55-64. We

display the 1947-1972 average active-duty share for select age groups in the notes to Figure 9: during the period, 14.0 percent of male 18-19 year olds and 19.8 percent of 20-24 year olds were on active duty at any give moment. The share for other groups is smaller but not zero: 1.9 percent of 16-17 year olds and 5.8 percent of 25-34 year olds. Outside of these age groups, the share is minimal.

5.3 Estimation and results

While we will more fully exploit variation in active-duty risk in our regression analysis, we begin with a more transparent graphical representation of the result. In Figure 9 panel (a), the first series (blue solid line with no markers) depicts the active-duty share of the population—the peaks due to Vietnam and especially Korea are evident, but the share in the interwar period never falls below 1.25 percent. The second series (red solid diamonds) depicts civilian unemployment rates for the two age groups most affected by the draft: 18-19 and 20-24 year olds (averaged in the figure). This series displays a tight, negative correlation with the active-duty share, consistent with the draft contracting civilian labor supply for this group. The final series (green, hollow squares) averages the male unemployment rates for all other age groups. While this series also varies negatively with the active duty-share, the relationship is muted. Indeed, we would expect some negative relationship because (a) procurement increases alongside the active-duty share in times of "hot wars" (and procurement pushes out labor demand, as demonstrated in the previous section); (b) as discussed above, active-duty rates for some of the age groups included in the third series, while smaller than those for 18-24 year olds, were also significant; and (c) workers across age groups may be substitutes in firm production functions.

To more fully exploit variation by age in active duty risk, we estimate variants of the following regression:

$$Unemployment_{at} = \beta Active\,duty\,share_a \times Active\,duty\,pop_t + \mu_a + \delta_t + e_{at}, \qquad (5)$$

where a indexes age-groups and t year; $Active\,duty\,share_a$ is the average active duty share over the sample period for age-group a and $Active\,duty\,pop_t$ is a measure of annual active-duty needs of the military (we use various proxies to test robustness); μ_a and δ_t are age-group and year fixed effects.

Col. (1) of Table 4 displays the β coefficient when unemployment is in levels and the proxy for annual military needs is the active-duty share of the full population (standardized to have mean zero and standard deviation one). We defer discussion of magnitudes for the moment and focus more on inference: the negative relationship between the proxy for active-

duty risk is highly statistically significant. As Figure 9 showed, the unemployment levels of the most at-risk age groups are substantially higher than other age groups, so in col. (2) we show that the result holds in logs. The next two columns replicate the first two, but use female unemployment rates as a comparison. The coefficients are negative but not statistically significant: again, we would expect a negative relationship given spillovers to substitute groups of workers plus the labor-demand effects of procurement spending.

The last two columns do not exploit all the variation in our data but provide more interpretable coefficients. Col. (5) interacts a dummy for being in the two most at-risk age groups (18-20 and 20-24) with the standardized annual aggregate active-duty measure. For every one standard deviation increase in the annual aggregate active-duty measure, unemployment for these age groups declines by nearly a percentage point relative to other men. One concern might be that the active-duty population is endogenous to labor-market conditions, so in the final column we isolate more exogenous shocks to active-duty needs: the peak years of the Korean and Vietnam conflicts (1951-1953 and 1966-1969, respectively). During these years of "hot wars," male civilian age 18-24 unemployment rates decline by 2.5 percentage points relative to other men, closing nearly half the average difference between young men and other men.

So far, we have used variation in active-duty-risk intensity over time within the draft era, but another test of the labor-supply story is that unemployment for young men should rise when the draft ends. Importantly, the end of the draft coincided with the end of the Vietnam conflict and a massive and permanent decline in the active-duty population, so the implicit experiment is that beginning in the early 1970s, millions of young men remain in the civilian labor supply who would earlier have been active-duty military personnel.

As Figure 9 panel (*b*) shows, indeed, unemployment for men ages 18-24 increases relative to that for other men in the first decade after the draft ends (in fact, this increase begins in 1970 when the draft technically still exists but the active-duty population plummets to its lowest point since the outbreak of Korea). To model the counterfactual unemployment had the draft continued at the average active-duty share from 1947-1972, we take the coefficient in col. (5) that relates male 18-24 relative unemployment to a one-standard deviation in the active-duty share. The post-period active duty share is 1.59 standard deviations below the draft-era share, suggesting that unemployment would have been 1.52 percentage points lower for men 18-24 relative to all men. This calculation suggests that the end of the draft accounts for three-quarters of the large rise in relative unemployment for men 18-24 in the decade that followed.

6 Cold War political coalitions

Cold War defense spending was both very large and had a substantial effect on the labor market. It was also a very politically durable source of government spending, staying high year after year, through both Republican and Democratic administrations. It is natural to hypothesize that voters who benefited from the labor demand created by procurement would support aggressive national defense spending, and hawkish foreign policy. In this section, we examine (*a*) how support for the two key Cold War conflicts (Korea and Vietnam) relate to local military procurement contracts and (*b*) the political leanings of respondents who supported greater military spending and hawkish foreign policy.[24]

6.1 Did procurement manufacture public support for conflict?

We identify and harmonize dozens of surveys that ask respondents their general support for either the Korean War (1950-1953) or the Vietnam War (1965-1970) and relate their responses to procurement spending in their state (see Data Appendix for exact phrasing of all questions).

We begin in col. (1) of Table 5 by simply regressing support for the war on state-level procurement from FY 1951-1953 (as before, we divide by 1950 state GDP, log the ratio, and then standardize the expression) with no additional controls except survey-level fixed effects (which subsume year effects). For now, we only examine respondents from surveys fielded in 1951-1953, returning to 1950 momentarily. The coefficient is economically and statistically significant and only somewhat reduced by adding region fixed effects in the subsequent column. Taking the col. (2) coefficient, a winner versus loser state (as captured by a three-standard deviation difference in the explanatory variable) differentially supports the war by over five percentage points (on a base of 49 percent). The next two columns show robustness to controlling for *future* Vietnam spending in the state and additional demographic controls, respectively.

While North Korea invades the South in June 1950, U.S. defense production for the Korean War does not start in earnest until January 1951 (see Appendix Figure A.14) and in fact Truman does not declare a state of emergency until mid-December 1950.[25] However, in the immediate months after the surprise invasion, Gallup frequently asked respondents'

[24]In studying this question, we contribute to a recent literature in political science that has emphasized the political consequences of policies. In this literature, policies themselves alter the composition of political coalitions and the salience of issues (Pierson, 1993; Galvin and Hacker, 2020; Mettler and Soss, 2004). In economics, work by Acemoglu *et al.* (2021) and Sandholtz (2023) look at political effects of education policies.

[25]The announcement was prompted by the entrance of Communist China on the side of the North Koreans in November 1950.

opinion on what should be done. Panel (*a*) of Figure 10 shows that *future* Korean War procurement had if anything a negative effect on support for intervention in 1950. Panel (*b*) repeats this analysis for 1951-1953, showing a strong positive relationship, consistent with the evidence in Table 5. Thus, the states that would eventually support the Korean War once procurement dollars started flowing in 1951 were not inherently hawkish about the conflict in its early months.

The remainder of Table 5 focuses on support for the Vietnam War. We again find that support for the war is highly sensitive to procurement spending in the state, and that this relationships is robust to controlling for region fixed effects, Korean-era procurement, and basic demographics.

We conclude that for the two major "hot wars" of the Cold War era, local procurement spending significantly increased public support for military intervention. This result also serves as an implicit plausibility check on our local economic results in Sections 3 and 4. If the labor-demand benefits of procurement are as large as we claim, then we should see support for military intervention under simple models of "pocket-book" voting.

6.2 Cold War political coalitions

We conclude our political analysis by moving away from local procurement and instead identifying voting blocs especially supportive of Cold War military intervention and spending. Table 6 shows that both union members (col. 1) and Democrats (col. 2) differentially supported more defense spending (conditioning on time and state fixed effects). For both groups, the differential support is roughly five percentage points (on a base of roughly fifty percent). During this period, the Democratic party had a large base in the former Confederate states, but when we drop them (col. 3) we find if anything a slightly larger coefficient on the Democratic dummy.

The next set of three columns show a very similar pattern for support for the Korean War, with Democrats and union members especially supportive. Interestingly, the beginnings of the coalition unravelling can be seen in Vietnam (final three columns), where Democrat partisan support is no longer significant, but that of union members is unchanged (indeed the AFL-CIO president George Meaney was a vocal supporter of the war). Indeed by 1968 the Vietnam War had split the Democratic Party, most famously illustrated during the party convention that year in Chicago. But for roughly twenty years after the death of FDR, we find that the "New Deal coalition" of voters was united in its support of government spending in the form of military procurement and hawkish anti-communism.

7 Conclusion

We have shown that in the decades immediately following World War II, the Cold War was a major factor sustaining tight labor markets. On the labor demand side, military procurement increased demand for manufacturing workers and put downward pressure on the share of income flowing to the richest households. We can complement this inequality effect with firm-level data showing that CEOs of firms that rely heavily on defense contracts are paid less than their counterparts at similarly-valued firms. Whether we carefully isolate moments where military priorities shifted exogenously or pool data across our entire 1951-2000 sample period, we find that procurement spending increases employment (especially manufacturing employment) and reduces income inequality. We can explain roughly between 18 and 30 percent of the decline in manufacturing and just under ten percent of the rise in top-ten inequality by the 1950s to 1990s decline in procurement spending.

On the labor-supply side, the Cold-War era draft removed millions of young men from the civilian labor market, even in peacetime. We document that this negative shift in labor supply indeed has the predicted effect and significantly reduces unemployment for draft-age men. Indeed, after 1972 the draft ends and the active-duty population falls substantially, we see a rise in male 18-24 unemployment rates relative to other age groups. We estimate that three-quarters of that rise would be reduced had the draft continued with active-duty shares equal to the 1947-1972 average.

As further evidence of the material benefits of procurement to U.S. workers, we find that households living in states receiving disproportionate procurement contracts differentially support the two hot wars of the Cold War period. We further show that throughout the Cold War period, Democrats and union member differentially support military intervention and spending, suggesting procurement helped sustain the New Deal political coalition.

We thus conclude that Cold War procurement—while under-studied in the modern, quantitative economics literature on inequality—played a major role in sustaining tight labor markets for low-skill workers in the decades immediately after World War II and creating political coalitions in support of hawkish foreign policy and military expenditure. While our focus was historical, there has been a recent push to increase defense budgets as a share of national income among the rich democracies. The most recent National Defense Authorization Act, passed in December 2025, provides for FY 2026 spending of $900 billion, a fifteen percent annual increase. NATO is expected to increase its defense-spending target to five percent of GDP, substantially higher than even the U.S. level currently.[26] To much fanfare,

[26]See `https://www.politico.eu/article/mark-rutte-embrace-5-percent-defense-goal-nato-summit/`.

Germany recently excluded defense spending from its "debt break" calculations, opening the door to defense spending growing faster than the rest of the economy. In the US case, the return of great-power rivalry, this time with China (the last Biden defense budget in 2024 cited the "challenge from the People's Republic of China" as the primary threat), alongside continued military engagement in the Middle East, has spurred greater spending. In the European case, Russia invading Ukraine and Trump's threatening the NATO alliance has served as a catalyst.[27]

We close, however, by urging caution in extrapolating the results to the modern day and near future. First, we showed that throughout the Cold War, it was *non-R&D* procurement that drove our manufacturing employment and inequality results. During the Cold War, R&D never comprised more than twenty percent of the total procurement budget. Estimates suggest that same share is closer to fifty percent today.[28]

Second, while we show that the labor demand effect of Cold-War procurement is robust to controlling for other factors linked to the "Great Compression" (e.g., union density, skill shares, high marginal tax rates), we also show that these factors *interact* with procurement. Over the pooled period, the effect of procurement on manufacturing employment and especially on inequality is larger in settings with higher union density, more skilled workers, and with more "liberal" policies. To the extent the economic and policy landscape today has shifted away from that of the post-war, "New Deal Consensus" period (Gerstle, 2022), we should expect more muted effects of modern-day procurement. Similarly, the massive post-war rise of skilled workers facilitated by the GI Bill and public university expansion has slowed since the 1980s.

While it is difficult to predict the labor market changes wrought by renewed geopolitical tension, this paper has documented that the effect of Cold-War-era production commitments played a major roll in supporting wages and employment of manufacturing workers and reducing inequality. Compared to other explanations of widely shared post-war prosperity—high union density and minimum wages, confiscatory taxes, expanded access to education—government-funded production of deadly, military matérial fits less comfortably into an egalitarian economic playbook.

[27] Interestingly, and echoing the Congressional oversight of the 1950s that we discussed in the paper, Trump has also recently announced caps on defense corporation executive compensation, as well as restrictions on share buybacks, though enforcement and follow-through remain unclear. See `https://www.whitehouse.gov/presidential-actions/2026/01/prioritizing-the-warfighter-in-defense-contracting/`.

[28] See, e.g., `https://www.pgpf.org/article/budget-explainer-national-defense/`.

References

Acemoglu, D., Autor, D. H. and Lyle, D. (2004). Women, war, and wages: The effect of female labor supply on the wage structure at midcentury. *Journal of political Economy*, **112** (3), 497–551.

—, Pekkarinen, T., Salvanes, K. G. and Sarvimäki, M. (2021). *The Making of Social Democracy: The Economic and Electoral Consequences of Norway's 1936 Folk School Reform*. Tech. rep., National Bureau of Economic Research.

— and Restrepo, P. (2020). Robots and jobs: Evidence from us labor markets. *Journal of political economy*, **128** (6), 2188–2244.

Angrist, J. (1990). Lifetime earnings and the vietnam era draft lottery: Evidence from social security administrative records. *The American Economic Review*, **80** (3), 313–336.

Autor, D., Dorn, D. and Hanson, G. (2013). The geography of trade and technology shocks in the united states. *American Economic Review*, **103** (3), 220–225.

—, Dube, A. and McGrew, A. (2023). *The unexpected compression: Competition at work in the low wage labor market*. Tech. rep., National Bureau of Economic Research.

Autor, D. H., Katz, L. F. and Krueger, A. B. (1998). Computing inequality: Have computers changed the labor market? *Quarterly Journal of Economics*, pp. 1169–1213.

Baicker, K., Goldin, C. and Katz, L. F. (1998). A Distinctive System: Origins and Impact of U.S. Unemployment Compensation. In *The Defining Moment: The Great Depression and the American Economy in the Twentieth Century*, University of Chicago Press, pp. 227–264.

Barker, T. (2022). *Cold War Capitalism: The Political Economy of American Military Spending, 1947-1990*. Ph.D. thesis, Harvard University.

Barro, R. J. (1981). Output effects of government purchases. *Journal of political Economy*, **89** (6), 1086–1121.

Bayer, P. and Charles, K. K. (2018). Divergent paths: A new perspective on earnings differences between black and white men since 1940. *The Quarterly Journal of Economics*, **133** (3), 1459–1501.

Berger, D., Easterly, W., Nunn, N. and Satyanath, S. (2013). Commercial imperialism? political influence and trade during the cold war. *American Economic Review*, **103** (2), 863–896.

Besley, T. and Persson, T. (2009). The origins of state capacity: Property rights, taxation, and politics. *American Economic Review*, **99** (4), 1218–1244.

Borusyak, K., Hull, P. and Jaravel, X. (2025). A practical guide to shift-share instruments. *Journal of Economic Perspectives*, **39** (1), 181–204.

Brenes, M. (2020). *For Might and Right: Cold War Defense Spending and the Remaking of American Democracy (Culture and Politics in the Cold War and Beyond)*. University of Massachusetts Press.

Caughey, D. and Warshaw, C. (2016). The dynamics of state policy liberalism, 1936–2014. *American Journal of Political Science*, **60** (4), 899–913.

Comin, D., Lashkari, D. and Mestieri, M. (2021). Structural change with long-run income and price effects. *Econometrica*, **89** (1), 311–374.

Cox, L., Müller, G., Pasten, E., Schoenle, R. and Weber, M. (2020). *Big G*. Tech. rep., National Bureau of Economic Research.

Danzer, A., Danzer, N. and Feuerbaum, C. (2023). Military spending and innovation: Learning from 19th century world fair exhibition data.

Dell, M. and Querubin, P. (2018). Nation building through foreign intervention: Evidence from discontinuities in military strategies. *The Quarterly Journal of Economics*, **133** (2), 701–764.

Dincecco, M. and Prado, M. (2012). Warfare, fiscal capacity, and performance. *Journal of Economic Growth*, **17** (3), 171–203.

Dube, A., Kaplan, E. and Naidu, S. (2011). Coups, corporations, and classified information. *The Quarterly journal of economics*, **126** (3), 1375–1409.

Dupor, B. and Guerrero, R. (2017). Local and aggregate fiscal policy multipliers. *Journal of Monetary Economics*, **92**, 16–30.

Edmans, A., Gabaix, X. and Landier, A. (2009). A multiplicative model of optimal ceo incentives in market equilibrium. *The Review of Financial Studies*, **22** (12), 4881–4917.

Farber, H. S., Herbst, D., Kuziemko, I. and Naidu, S. (2021a). Unions and inequality over the twentieth century: New evidence from survey data. *The Quarterly Journal of Economics*, **136** (3), 1325–1385.

—, —, — and — (2021b). Unions and inequality over the twentieth century: New evidence from survey data. *The Quarterly Journal of Economics*, **136** (3), 1325–1385.

Fishback, P. V. and Jaworski, T. (2016). World war ii and us economic performance. *Economic History of Warfare and State Formation*, pp. 221–241.

Fisher, J. D. and Peters, R. (2010). Using stock returns to identify government spending shocks. *The Economic Journal*, **120** (544), 414–436.

Frydman, C. and Saks, R. E. (2010). Executive compensation: A new view from a long-term perspective, 1936–2005. *The Review of Financial Studies*, **23** (5), 2099–2138.

Galvin, D. J. and Hacker, J. S. (2020). The political effects of policy drift: Policy stalemate and american political development. *Studies in American Political Development*, **34** (2), 216–238.

Garin, A. and Rothbaum, J. (2025). The long-run impacts of public industrial investment on local development and economic mobility: Evidence from world war ii. *The Quarterly Journal of Economics*, **140** (1), 459–520.

Gennaioli, N. and Voth, H.-J. (2015). State capacity and military conflict. *Review of Economic Studies*, **82** (4), 1409–1448.

Gerstle, G. (2022). *The rise and fall of the neoliberal order: America and the world in the free market era*. Oxford University Press.

Goldin, C. and Margo, R. (1992). The great compression: The wage structure in the united states at mid-century. *Quarterly Journal of Economics*, **107** (1), 1–34.

Goldin, C. D. and Katz, L. F. (2008). *The race between education and technology*. Harvard University Press.

Gruber, J. and Johnson, S. (2019). *Jump-starting America: How breakthrough science can revive economic growth and the American dream*. Hachette UK.

Higgs, R. (2006). *Depression, war, and cold war: Studies in political economy*. Oxford University Press.

Hoffman, D. (2009). *The dead hand: the untold story of the cold war arms race and its dangerous legacy*. Anchor.

Hoffman, P. T. (2015). *Why Did Europe Conquer the World? Princeton University Press*. Princeton University Press.

Hooker, M. A. and Knetter, M. M. (1997). The effects of military spending on economic activity: Evidence from state procurement spending. *Journal of Money, Credit, and Banking*, pp. 400–421.

Howell, S. T., Rathje, J., Van Reenen, J. and Wong, J. (2021). *Opening up Military Innovation: Causal Effects of Reforms to US Defense Research*. Tech. rep., National Bureau of Economic Research.

Jácome, E., Kuziemko, I. and Naidu, S. (2024). *Mobility for all: Representative intergenerational mobility estimates over the 20th century*. Tech. rep., National Bureau of Economic Research.

Juhász, R., Lane, N. and Rodrik, D. (2024). The new economics of industrial policy. *Annual Review of Economics*, **16** (2024), 213–242.

Kantor, S. and Whalley, A. T. (2023). *Moonshot: Public R&D and growth*. Tech. rep., National Bureau of Economic Research.

Katz, L. F. and Murphy, K. M. (1992). Changes in relative wages, 1963–1987: Supply and demand factors. *Quarterly Journal of Economics*, **107** (1), 35–78.

Kline, P. and Moretti, E. (2014). Local economic development, agglomeration economies, and the big push: 100 years of evidence from the tennessee valley authority. *The Quarterly journal of economics*, **129** (1), 275–331.

Levy, F. and Temin, P. (2007). Inequality and institutions in 20th century america.

Malis, M., Querubin, P. and Satyanath, S. (2021). Persistent failure? international interventions since world war ii. In *The Handbook of Historical Economics*, Elsevier, pp. 641–673.

Markusen, A. R. (1991). *The rise of the gunbelt: The military remapping of industrial America*. Oxford University Press.

Melman, S. (1972). Ten propositions on the war economy. *The American Economic Review*, **62** (1/2), 312–318.

Mettler, S. and Soss, J. (2004). The consequences of public policy for democratic citizenship: Bridging policy studies and mass politics. *Perspectives on politics*, **2** (1), 55–73.

Miguel, E. and Roland, G. (2011). The long-run impact of bombing vietnam. *Journal of development Economics*, **96** (1), 1–15.

Nakamura, E. and Steinsson, J. (2014). Fiscal stimulus in a monetary union: Evidence from us regions. *American Economic Review*, **104** (3), 753–792.

Oi, W. Y. (1967). The economic cost of the draft. *The American Economic Review*, **57** (2), 39–62.

Pallante, G., Russo, E. and Roventini, A. (2021). *Does mission-oriented funding stimulate private R&D? Evidence from military R&D for US states*. Tech. rep., LEM Working Paper Series.

Pierson, P. (1993). When effect becomes cause: Policy feedback and political change. *World politics*, **45** (4), 595–628.

Piketty, T. (2014). *Capital in the Twenty-first Century*. Harvard University Press.

—, Saez, E. and Stantcheva, S. (2014). Optimal taxation of top labor incomes: A tale of three elasticities. *American economic journal: economic policy*, **6** (1), 230–271.

Queralt, D. (2019). War, international finance, and fiscal capacity in the long run. *International Organization*, **73** (4), 713–753.

Ramey, V. A. (2019). Ten years after the financial crisis: What have we learned from the renaissance in fiscal research? *Journal of Economic Perspectives*, **33** (2), 89–114.

— and Shapiro, M. D. (1998). Costly capital reallocation and the effects of government spending. In *Carnegie-Rochester conference series on public policy*, Elsevier, vol. 48, pp. 145–194.

Reich, M. (1972). Does the us economy require military spending? *The American Economic Review*, **62** (1/2), 296–303.

Robinson, J. (1962). Latter day capitalism. *New Left Review*, **16**, 37–46.

Ruttan, V. W. (2006). *Is war necessary for economic growth?: military procurement and technology development*. Oxford University Press.

Sandholtz, W. A. (2023). The politics of public service reform: Experimental evidence from liberia. *Available at SSRN 4178202*.

Scheidel, W. (2017). *The great leveler: Violence and the history of inequality from the stone age to the twenty-first century*. Princeton University Press.

Scheve, K. and Stasavage, D. (2012). Democracy, war, and wealth: lessons from two centuries of inheritance taxation. *American Political Science Review*, **106** (1), 81–102.

Tilly, C. *et al.* (). *Coercion, capital, and European states, AD 990-1992*, vol. 19. Oxford Blackwell.

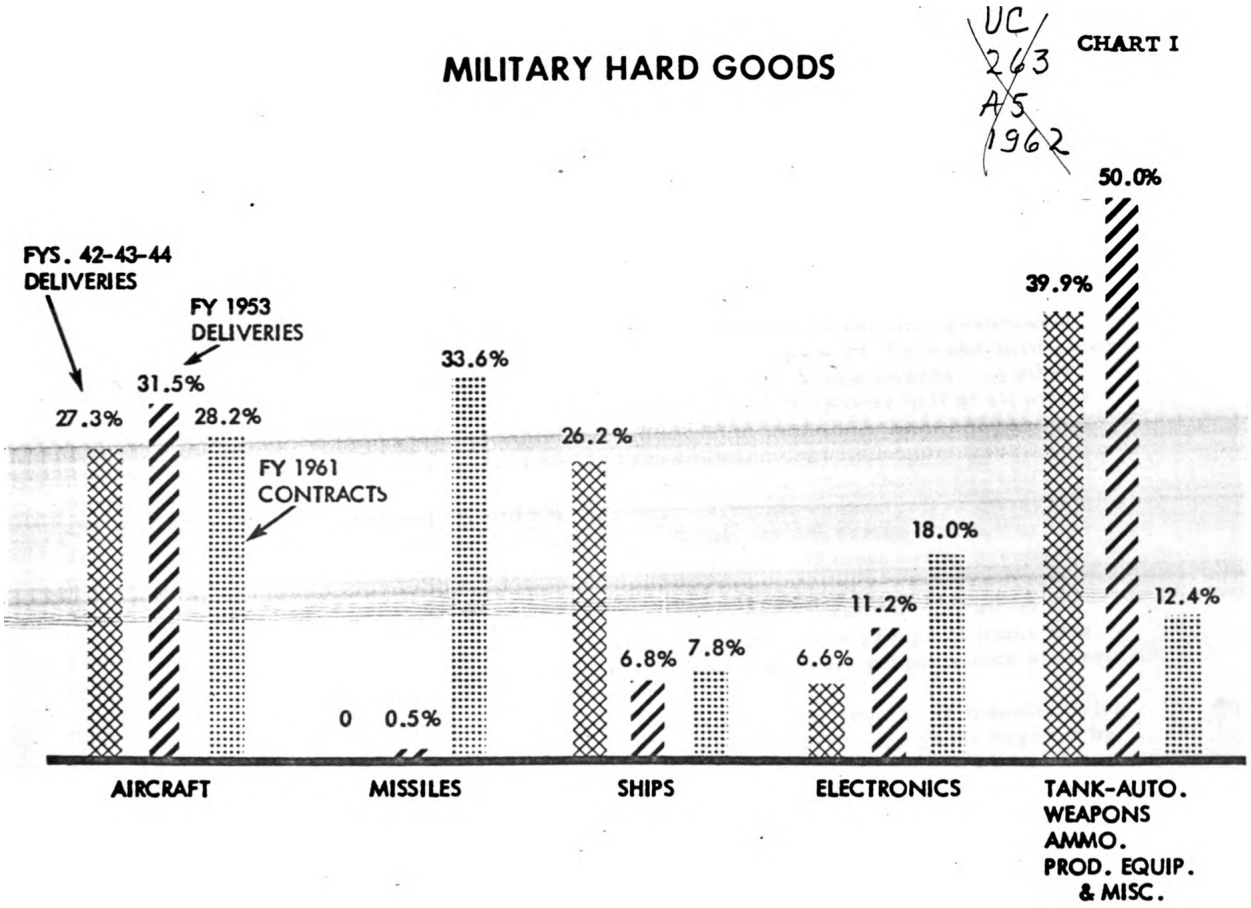
Titmuss, R. M. (1950). *Problems of social policy*, vol. 1. HM Stationery Office.

Veblen, T. (1904). *The theory of business enterprise*. New English Library.

Voigtländer, N. and Voth, H.-J. (2013). Gifts of mars: Warfare and europe's early rise to riches. *Journal of Economic Perspectives*, **27** (4), 165–186.

Warner, J. T. (1995). The economics of military manpower. *Handbook of defense economics*, **1**, 347–398.

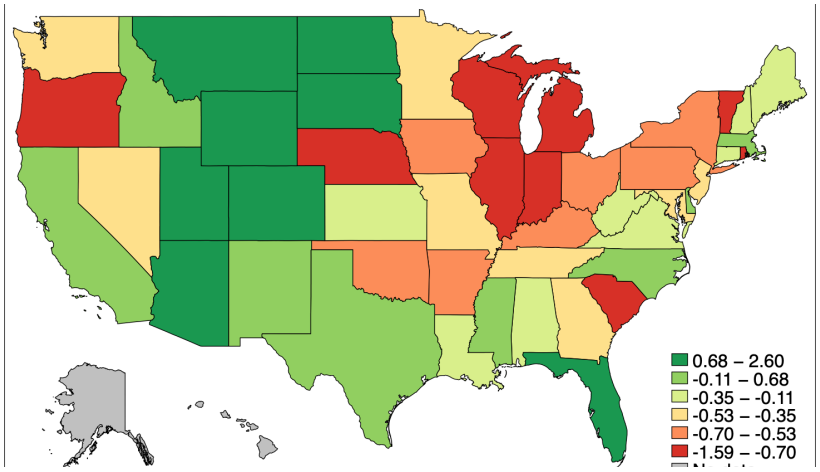
Figure 1: Shifting military procurement patterns



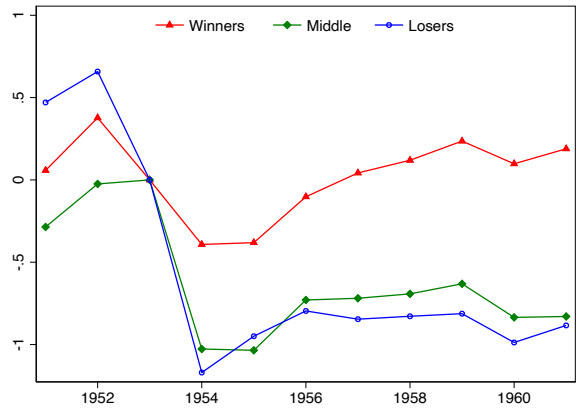
Notes: From the DoD Report "Changing Procurement Patterns," 1962.

Figure 2: Geographic and temporal variation of the Korea-to-Missiles shift

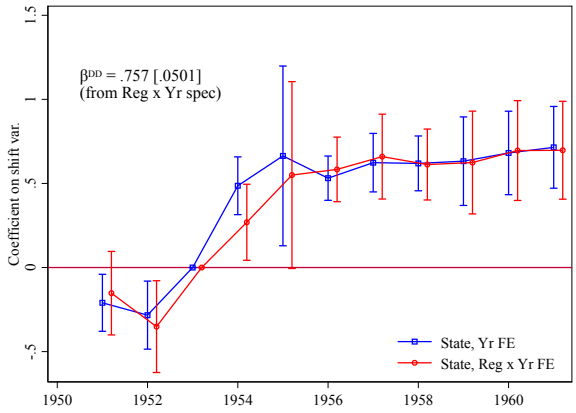
(a) Geographic distribution of "missile shift" variable



(b) Procurement over time by terciles of missile shift



(c) Procurement over time as function of missile shift



Notes: In panel (a), colors correspond to the magnitude of the "missile shift" variable $\Delta_s^{missile}$:

$$\Delta_s^{missile} = \log(Procurement_{s,1954-1961}) - \log(Procurement_{s,1951-1953}). \tag{6}$$

We standardize this expression so $\mu = 0, \sigma = 1$.

In panel (b) we plot, annually across time, the average $\log(\frac{Procurement_{st}}{GDP_{s,t-1}})$ (again, standardized) for terciles of the "missile shift" variable.

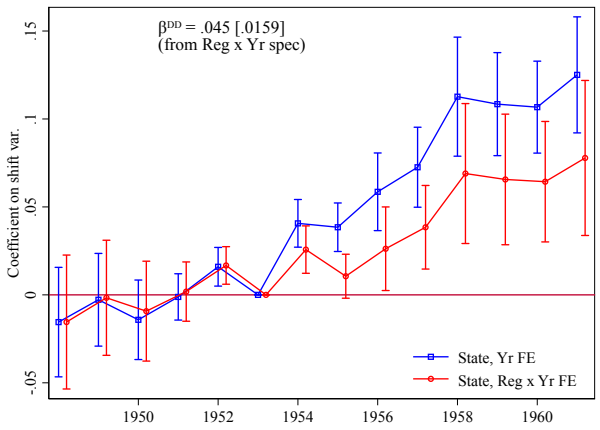
In panel (c), we plot the β coefficients (and 95% confidence intervals) from the following regression equation:

$$\log\left(\frac{Procurement_{st}}{GDP_{s,t-1}}\right) = \sum_{k \neq 1953} \beta_k \Delta_s^{missile} \mathbb{I}^{t=k} + \eta_s + \delta_{f(s)t} + e_{st}, \tag{7}$$

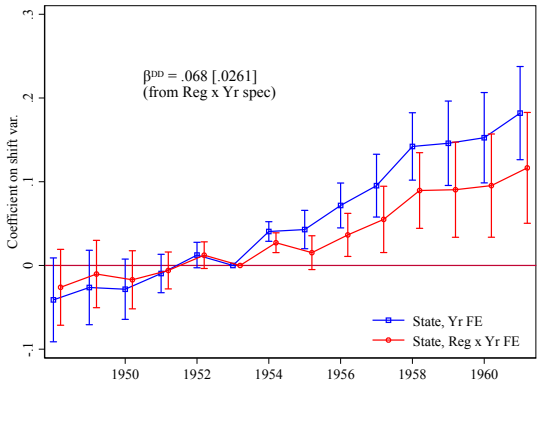
when $\delta_{f(s)t}$ denote either year fixed effects or $region \times year$ fixed effects.
The β^{DD} refers to the coefficient from an analogous regression (with $region \times year$ fixed effects) but where $Missile\ Shock \times \{year > 1953\}$ replaces the year interactions. Standard errors are clustered by state in both the event-study graph and the differences-in-difference regression. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$.

Figure 3: Manufacturing and covered employment as a function of missile shift

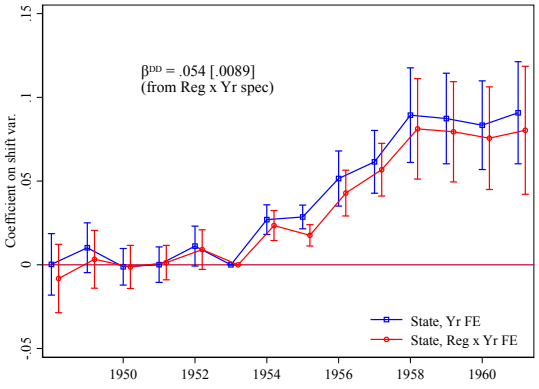
(a) Log per capita manufacturing employment



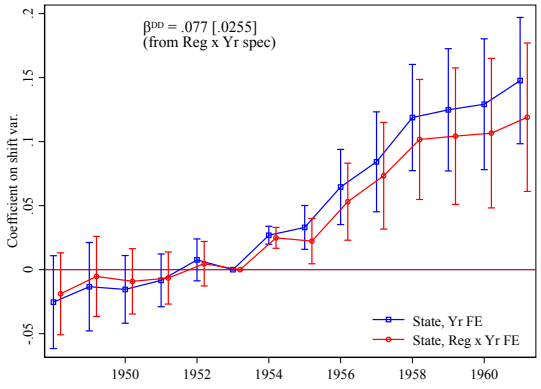
(b) Log total manufacturing employment



(c) Log per capita covered employment



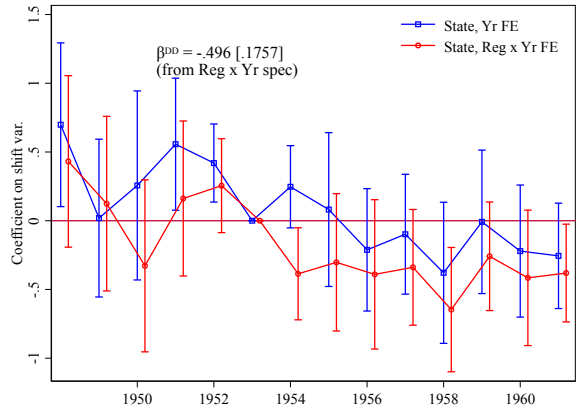
(d) Log total covered employment



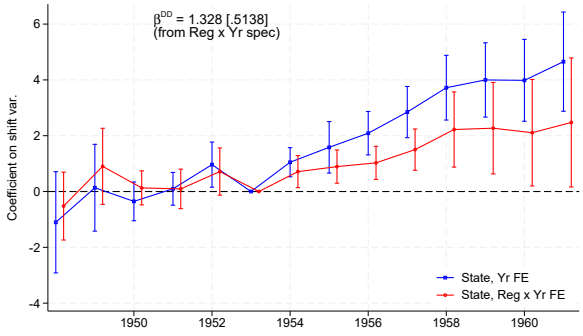
Notes: In each panels, each plotted coefficient (and 95% confidence interval) is from an event-study specification that regresses the outcome variable on the *missile shift* variable interacted with year effects (with 1953 as the omitted year), as well as state and year (or region×year) fixed effects. β^{DD} refers to the coefficient from an analogous regression but where *missile shift*$\times\{year > 1953\}$ replaces the year interactions. Standard errors are clustered by state in all analyses.

Figure 4: Inequality proxies and wages as function of missile shift

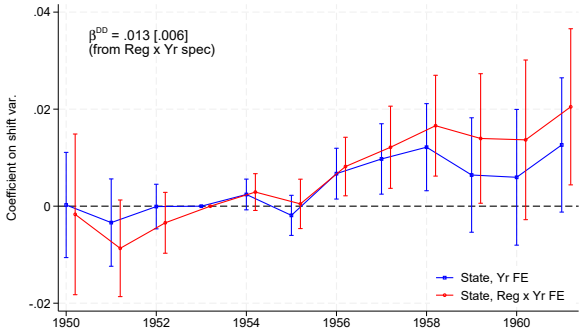
(a) Top-ten income share



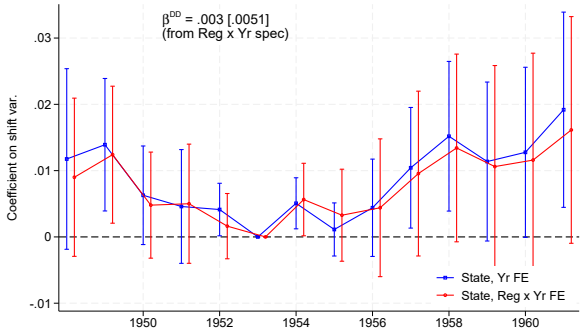
(b) Share paying federal income tax



(c) Manufacturing wages (logged)



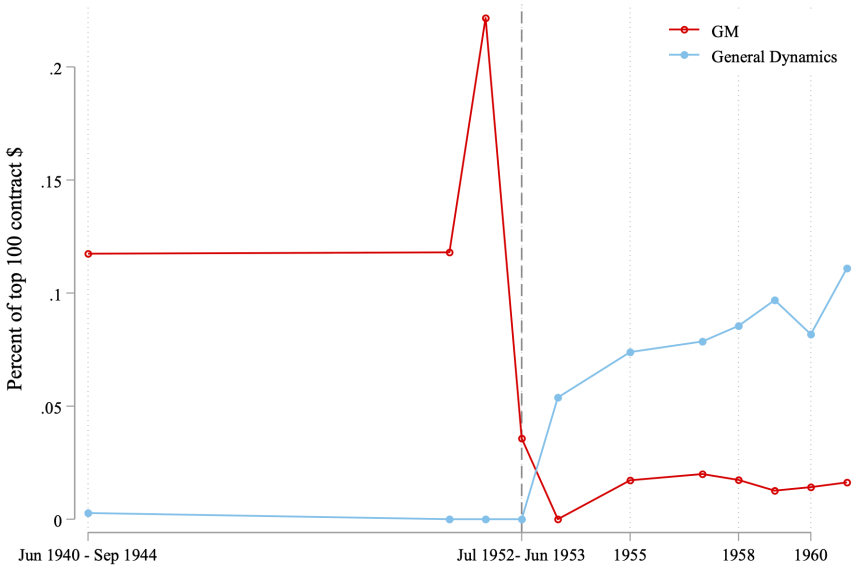
(d) Covered wages (logged)



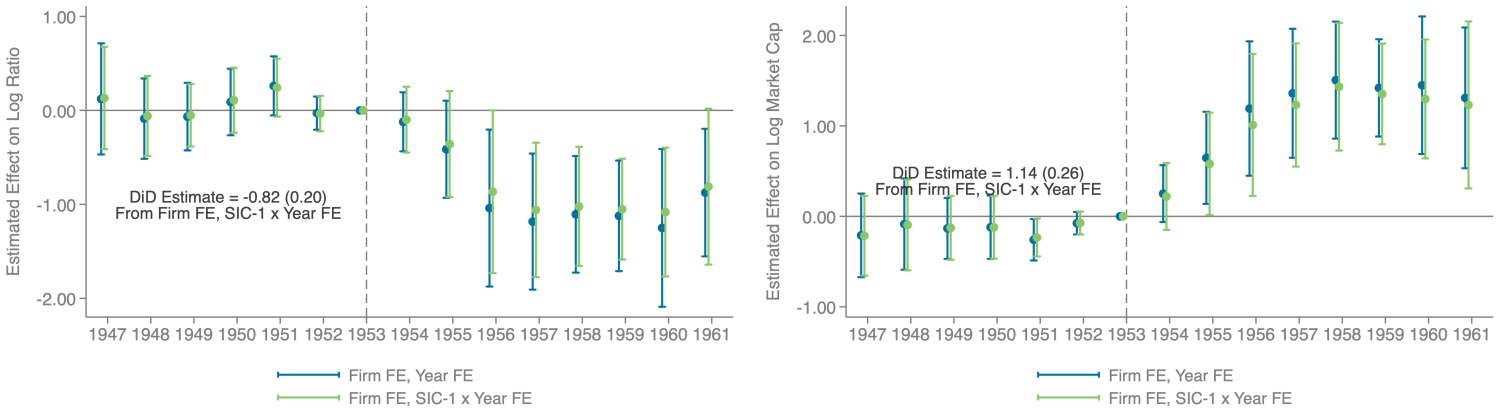
Notes: In both panels, each plotted coefficient (and 95% confidence interval) is from an event-study specification the regresses the outcome variable on the *Missile Shock* interacted with year effects (with 1953 as the omitted year), as well as state and $region \times year$ fixed effects. β^{DD} refers to the coefficient from an analogous regression but where *Missile Shock*$\times\{year > 1953\}$ replaces the year interactions. Standard errors are clustered by state in both the event-study graph and the differences-in-difference regression.

Figure 5: Shift in procurement from automotive to aeronautic firms, 1940-1960

(a) Total procurement share, GM and General Dynamics



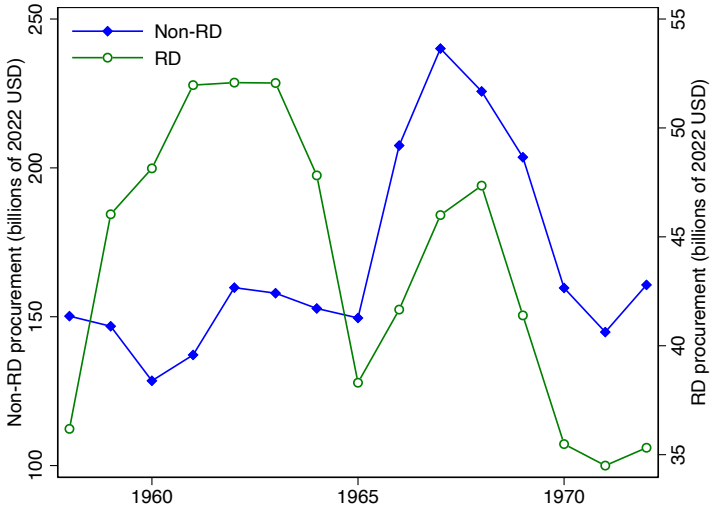
(b) Effect of being among Top 20 contractors on CEO pay relative to market capitalization



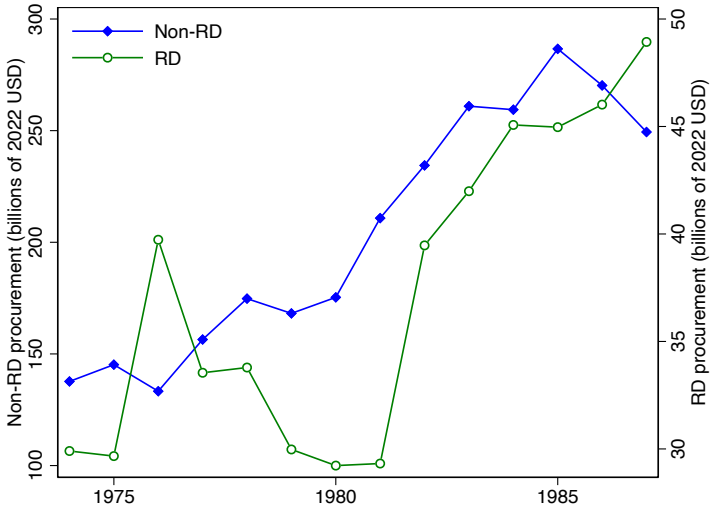
Notes: The top panel shows the change in procurement between General Motors and General Dynamics after the Korean war. The botton panel shows the coefficients (and 95% confidence intervals) from equation 3, where the outcome is the log of the ratio between CEO pay and market capitalization. Sample is limited for firms for which we have at least 10 observations in the sample window.

Figure 6: Aggregate procurement disaggregated into R&D and non-R&D

(a) Vietnam (1958-1972)



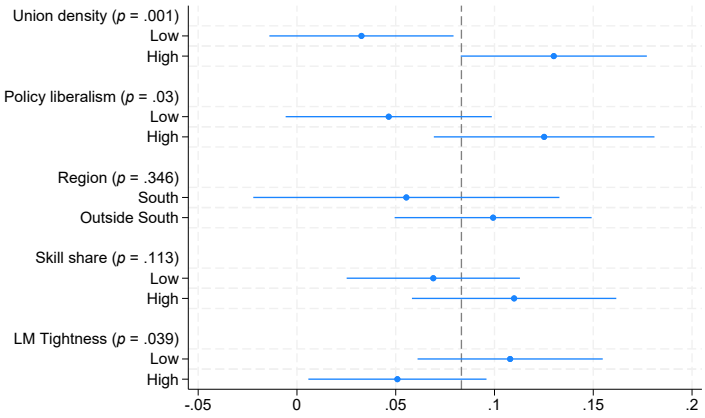
(b) End of *détente* (1974-1986)



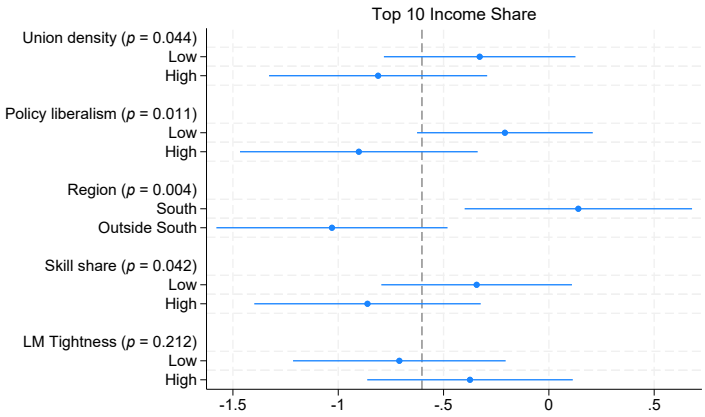
Notes: This figure displays variation in R&D and non-R&D spending during two of our highlighted episodes (this breakdown is not available until 1958, so we cannot perform identical analysis for the Korea-to-missiles episode).

Figure 7: Heterogeneity in the effect of procurement on manufacturing employment and inequality

(a) Manufacturing share of emp (logged)



(b) Top-ten percent of income



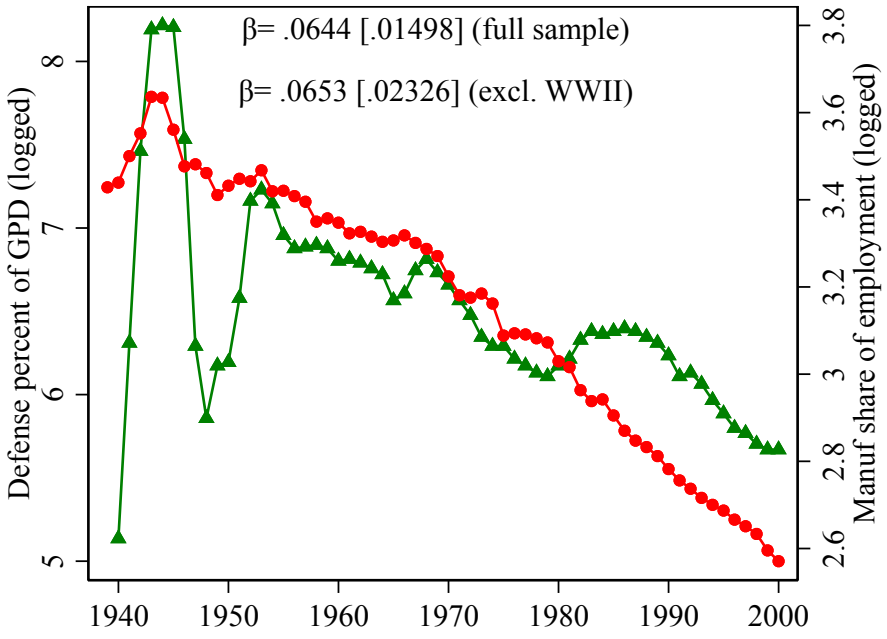
Notes: This figure plots the β_1 and $(\beta_1 + \beta_3)$ alongside corresponding 95% confidence intervals from the following regression:

$$Y_{st} = \beta_1 \log(\frac{Procurement_{st}}{GDP_{s,t-1}}) + \beta_2 X_{st} + \beta_3 X_{st} \times \log(\frac{Procurement_{st}}{GDP_{s,t-1}}) + \eta_s + \delta_{reg,t} + e_{st}$$

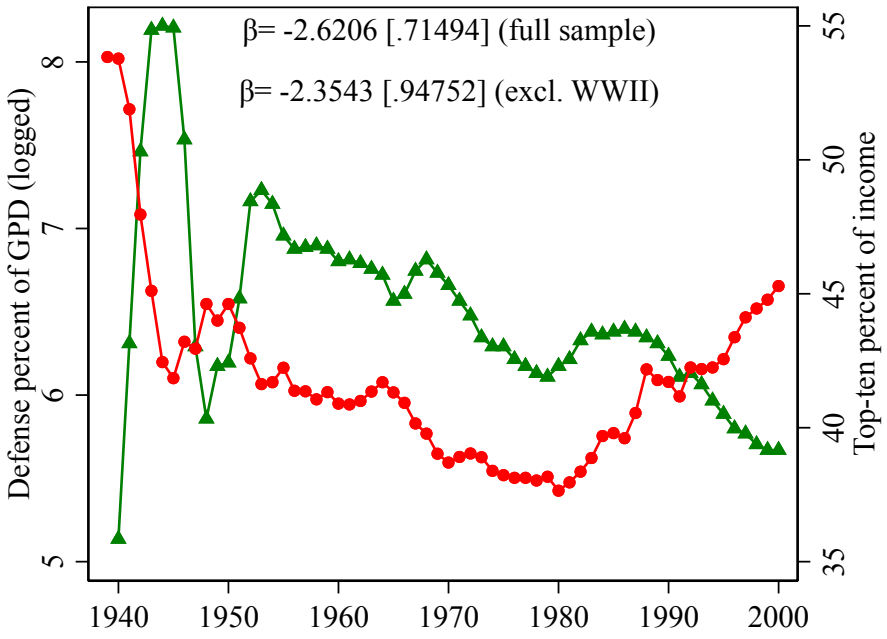
where all notation follows from before. In all cases X_{st} is a binary variable. For union density and skill share, $X_{st} = 1$ if state s is above the median value in year t (as these variables have strong trends over time, we do not simply want time trends to drive whether X_{st} is positive). For policy-liberalism, $X_{st} = 1$ if state s in year t is above the *global* median, since this variable does not exhibit strong trends over time. In the headings, p refers to the test of the hypothesis $\beta_3 = 0$.

Figure 8: Manufacturing and inequality as a function of defense spending, aggregate time-series relationship

(a) Manufacturing share of employment (logged)



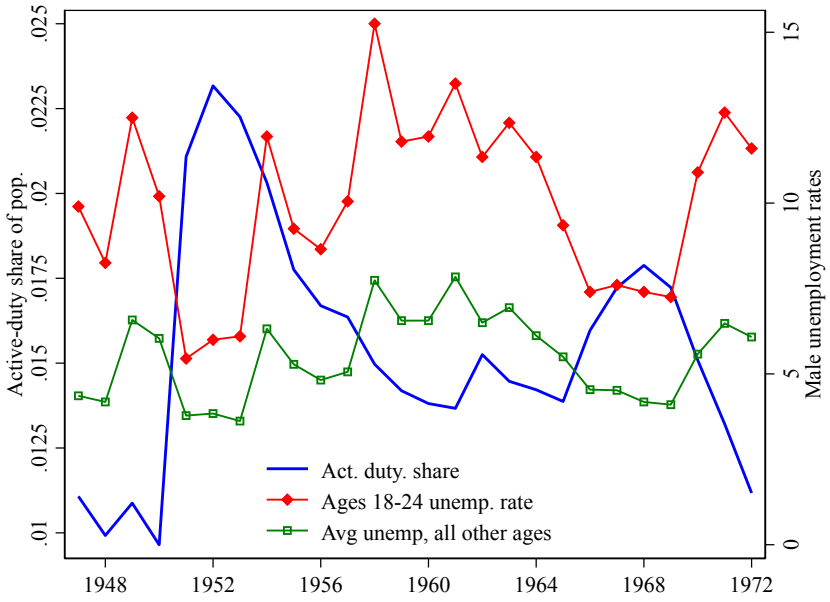
(b) Top-ten percent of income



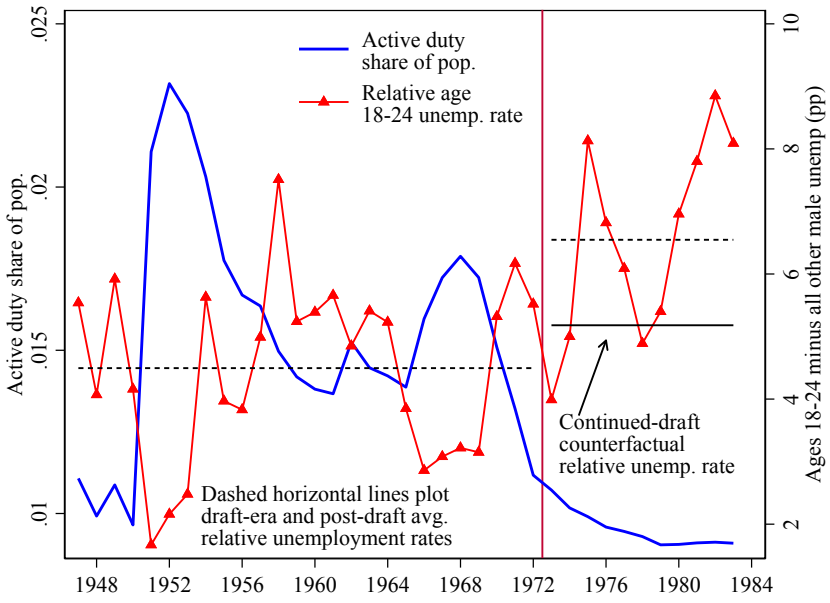
Notes: Defense spending as share GDP from Office of Management and Budget historical tables (and is reported in terms of *fiscal* year). Manufacturing data from FRED and top-ten inequality measures from WID (reported as *calendar* year). The reported β is from a regression of manufacturing (panel a) and inequality (panel b) on defense spending, controlling for a linear time trend.

Figure 9: Cold-War active-duty levels and male civilian unemployment rates

(a) Male unemployment rates by age during Cold-War draft (1947-1972)



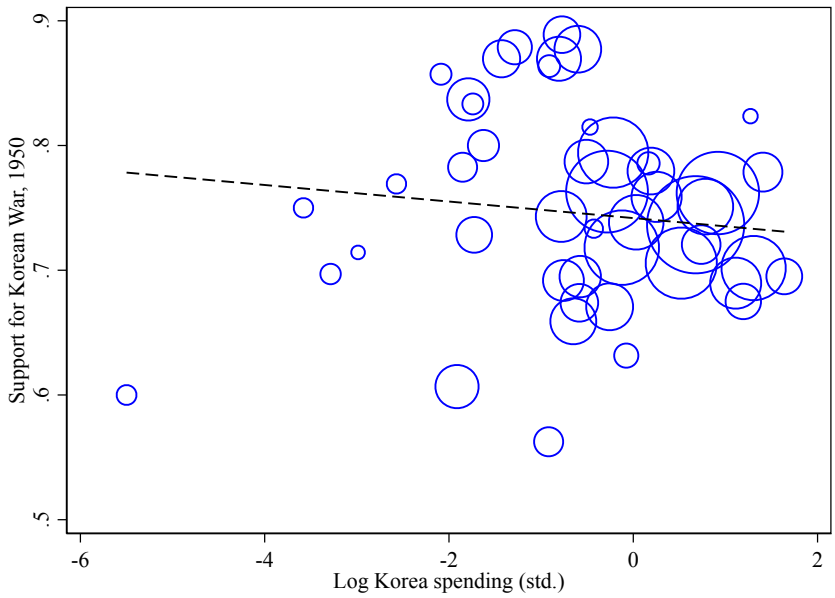
(b) Relative youth male unemployment rates and the end of the draft



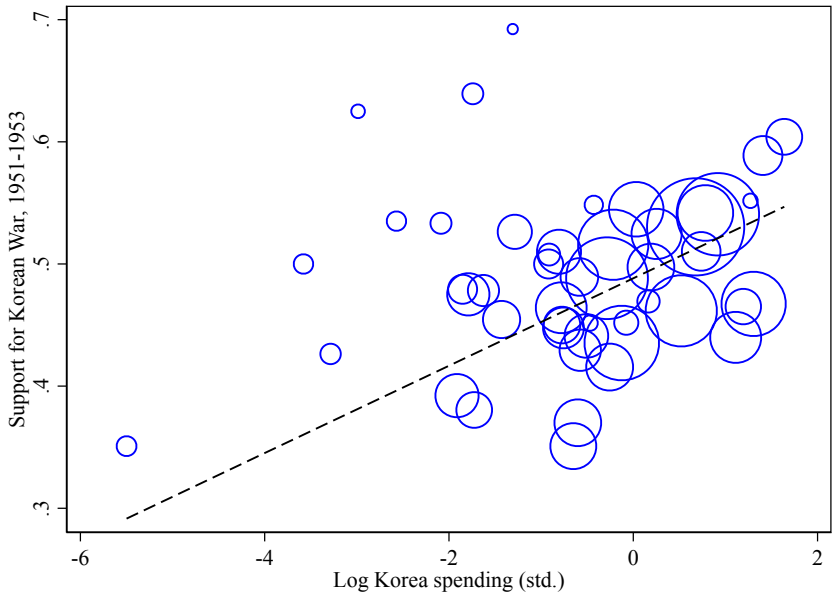
Notes: All unemployment data come from BLS and all active-duty data from the Selective Service Administration. In panel (*b*) we simulate relative male 18-24 unemployment had the draft continued at the 1947-1972 active-duty level. That is, we estimate the relationship between relative male 18-24 unemployment and active duty share in the 1947-1972 period (in particular, we use the coefficient from col. 5, Table 4). We then multiply that coefficient by the difference in active-duty levels between 1947-1972 and 1973-1983 (the ten years following the end of the draft) and add it to the observed relative male 18-24 unemployment in 1973-1983.

Figure 10: Support for the Korean War

(a) Surveys taken in 1950



(b) Surveys taken in 1951-1953



Notes: Each marker in these scatter plots represents a state, with size based on state population in 1950. Most survey data are from Gallup (see Data Appendix for more detail). Log "Korea spending" is the log of total procurement spending in the state in FY 1951-1953 divided by state GDP in 1950 (and we then standardize this expression). Fitted lines are based on the micro data (and thus implicitly weighted as more populous states have more observations). The fitted line is nearly identical if we instead fit using the state averages weighted by 1950 population.

Table 1: State labor-market outcomes as a function of procurement, 1958-1972

Panel (a): Key explanatory variable is total procurement (not disaggregated)

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Share tax	(7) Quit
Total spending	0.0490	0.0201	0.00387	0.00924	-0.234	1.245	0.111
	[0.0164]	[0.00876]	[0.00487]	[0.00383]	[0.143]	[0.421]	[0.0626]
Obs.	720	720	720	720	720	720	683

Panel (b): Key explanatory variables are non-R&D and R&D procurement

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Share tax	(7) Quit
Non-RD	0.0411	0.0142	0.00171	0.00730	-0.238	0.938	0.0800
spending	[0.0145]	[0.00803]	[0.00455]	[0.00381]	[0.134]	[0.400]	[0.0484]
RD spending	0.00677	0.00649	0.000496	0.000200	0.0296	0.212	0.0534
	[0.00605]	[0.00331]	[0.00192]	[0.00171]	[0.0479]	[0.137]	[0.0366]
Obs.	707	707	707	707	707	707	672

Notes: The unit of analysis is a state-year and the sample consists of the 48 continental states. All regressions include state and region × year fixed effects and are weighted by state population with standard errors clustered by state. Manufacturing employment and covered employment are both per capita (and then logged). Manufacturing and covered wages are logged. Top 10 and share tax are both out of 100. Quits is the number of quits in the manufacturing sector per 100 workers. Total spending is the ratio of procurement spending and once-lagged state GDP and then logged. RD and non-RD are defined analogously. Data on quits is not available for all states. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

Table 2: State labor-market outcomes as a function of procurement, 1974-1987

Panel (a): Key explanatory variable is total procurement (not disaggregated)

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Log9010
Total spending	0.0381	0.0348	-0.00484	0.00298	-0.142	-0.000506
	[0.0162]	[0.0133]	[0.00670]	[0.00813]	[0.347]	[0.0221]
Obs.	670	670	651	670	670	517

Panel (b): Key explanatory variables are non-R&D and R&D procurement

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Log9010
Non-RD	0.0386	0.0367	-0.00517	0.000923	0.0804	0.000568
spending	[0.0172]	[0.0137]	[0.00688]	[0.00774]	[0.358]	[0.0197]
RD spending	0.0108	0.00796	-0.000226	0.00308	-0.0586	-0.0129
	[0.00833]	[0.00502]	[0.00332]	[0.00311]	[0.150]	[0.00860]
Obs.	662	662	646	662	662	514

Notes: The unit of analysis is a state-year and the sample consists of the 48 continental states. All regressions include state and region × year fixed effects and are weighted by state population with standard errors clustered by state. Manufacturing employment and covered employment are both per capita (and then logged). Manufacturing and covered wages are logged. Top 10 share of income is out of 100. Total spending is the ratio of procurement spending and once-lagged state GDP and then logged. RD and non-RD are defined analogously. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 3: Manuf. employment and income inequality as a function of log procurement (1951-2000)

	Manufacturing		Income Share	
	(1) Log Emp.	(2) Employment	(3) Top 10%	(4) Top 10%
Log(total spending / gdp_{t-1})	0.0906		-0.608	
	[0.0216]		[0.222]	
Total spending / gdp_{t-1}		0.00117		-0.136
		[0.000340]		[0.0391]
Obs.	2393	2400	2393	2400

Notes: The unit of analysis is a state-year and the sample consists of the 48 continental states from 1951-2000 (inclusive). All regressions include state and region × year fixed effects are are weighted by state population. Total spending is the ratio of procurement spending and once-lagged state GDP and then logged. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

Table 4: Unemployment as a function of age-based risk and military active-duty needs

	Dept var.: Age-group-specific unemp. rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Age-based risk (a) x	-5.407	-0.348	-2.504	-0.175		
Std. act duty share (t)	[1.577]	[0.152]	[1.675]	[0.169]		
Dummy 18-24 (a) x					-0.952	
Std. act duty share (t)					[0.254]	
Dummy 18-24 (a) x						-2.512
War years (t)						[0.369]
Level or log?	Level	Log	Level	Log	Level	Level
Mean, dept. var.	6.789	1.669	7.173	1.795	6.789	6.789
Sample	Male	Male	Female	Female	Male	Male
Observations	182	182	182	182	182	182

Notes: Unemployment is measured as percentage points (0-100). Unit of observation is an age category $a \times$ year t (1947-1972, inclusive). Age categories are: 16-17; 18-19; 20-24; 25-34; 35-44; 45-54; 55-64. All regressions include age-category and year FE. Robust standard errors in brackets. In col. (6), "war years" are peak active-duty years for Korea (1951-1953) and Vietnam (1966-1969).

Table 5: Support for military intervention by state procurement spending

	Dept. var.: Support for escalation/continuation (binary)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Korea spending	2.363	1.715	1.233	1.652			-0.395	
	[0.703]	[0.417]	[0.460]	[0.417]			[0.712]	
Viet. spending			0.884		1.731	2.063	2.257	1.763
			[0.610]		[0.799]	[0.860]	[1.010]	[0.715]
Mean, DV	49.14	49.14	49.14	49.27	60.90	60.90	60.90	60.90
Period	Korea	Korea	Korea	Korea	Viet.	Viet.	Viet.	Viet.
Covariates?	No	No	No	Yes	No	No	No	Yes
Region FE?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	16077	16077	16077	15702	35531	35531	35531	35531

Notes: All regressions have survey (subsuming year) fixed effects. Korean War questions are taken from 1951-1953. Vietnam war questions taken from 1965-1971. "Covariates" include sex, race, and age fixed effects (in ten-year bins). Standard errors clustered by state. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 6: Support for military intervention by party and union status

	Mil spending			Korean War			Vietnam War		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Union Household	4.903			2.852			3.216		
	[2.400]			[1.412]			[1.175]		
Democrat		5.519	5.832		8.956	9.292		0.680	0.254
		[0.918]	[0.978]		[0.981]	[1.068]		[1.294]	[1.569]
DV mean	45.61	50.52	50.21	54.30	55.79	56	61.68	60.38	60.45
Ex. South	No	No	Yes	No	No	No	No	No	Yes
Observ.	4892	11615	10399	11158	21707	19025	19258	37992	28710

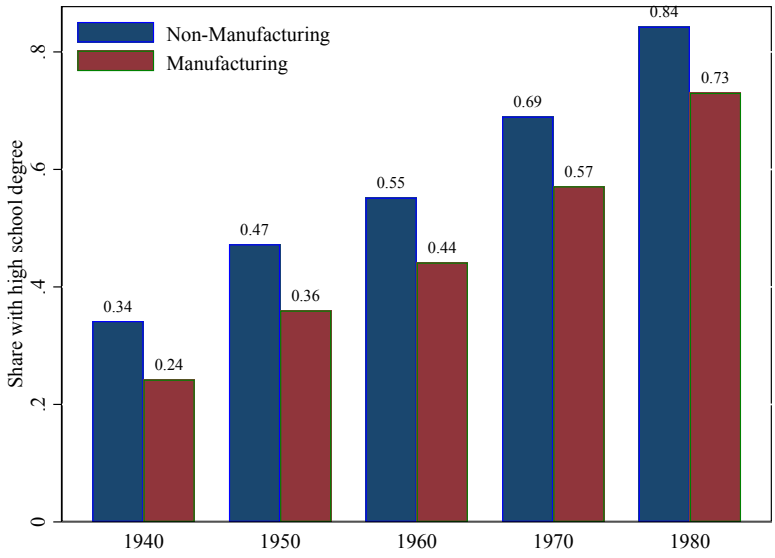
Notes: All regressions have survey (subsuming year) fixed effects and state fixed effects. Korean War question, 1950-1953. Military spending questions, 1947-1955. "Ex. South" drops the 11 states of the Confederacy. Note that the number of observations for the union regressions are smaller because union membership is not asked as consistently as party ID. Standard errors clustered by state. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix

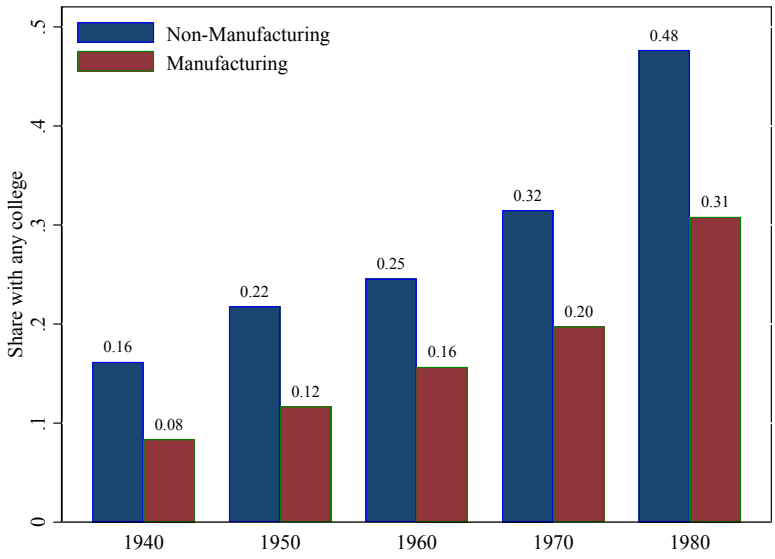
Appendix A. Supplementary figures and tables noted in the text

Appendix Figure A.1: Educational attainment of manufacturing versus other workers

Panel A: Share with a high school degree



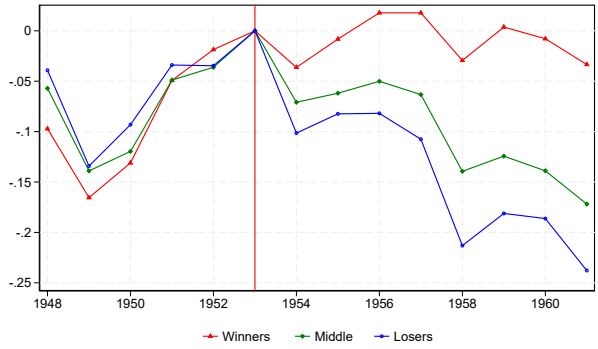
Panel B: Share with any college attendance



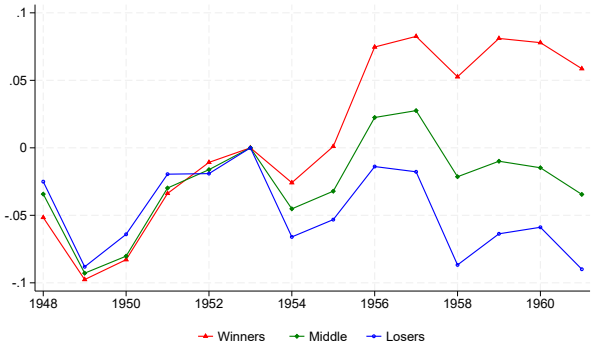
Notes: Data taken from IPUMS census files. Sample is restricted to prime-age, employed workers with positive earnings.

Appendix Figure A.2: Main missile-shift results shown as raw differences

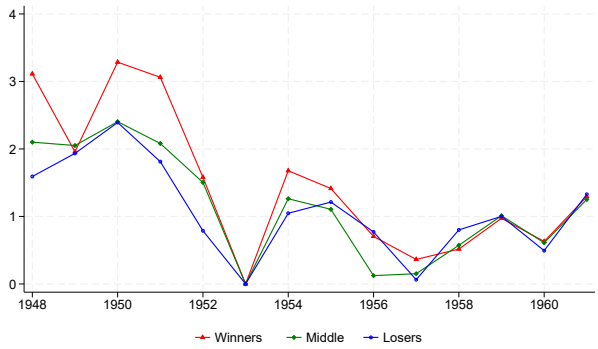
(a) Manufacturing employment p.c.



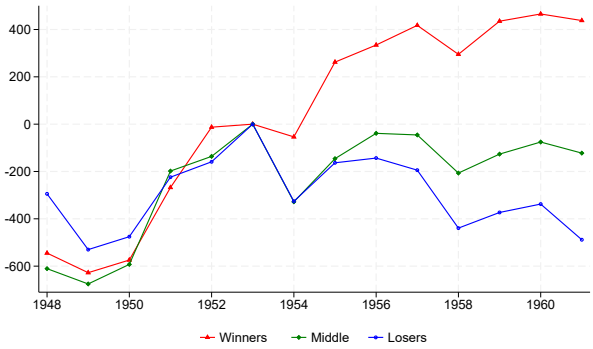
(b) Covered employment p.c.



(c) Top-ten income share



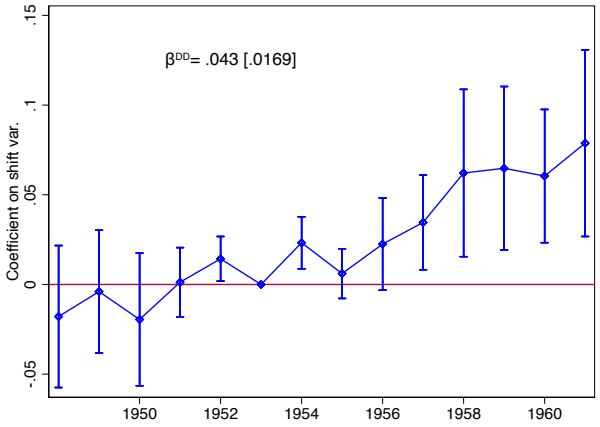
(d) Share paying taxes



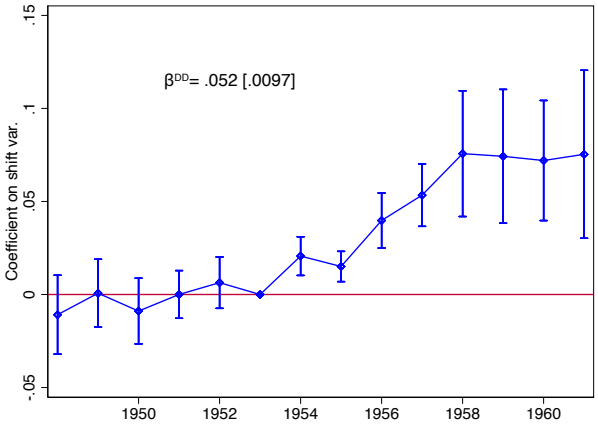
Notes: These figures show the means across time for selected outcome variables by tercile of the missile-shift variable. Within tercile the outcome variable is weighted by state population. To facilitate comparisons, we normalize by setting 1953 to zero.

Appendix Figure A.3: Main missile-shift event study graphs controlling for state-year union density

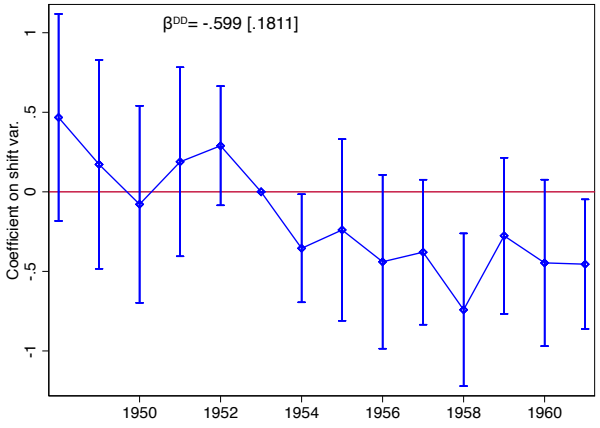
(a) Manufacturing employment p.c.



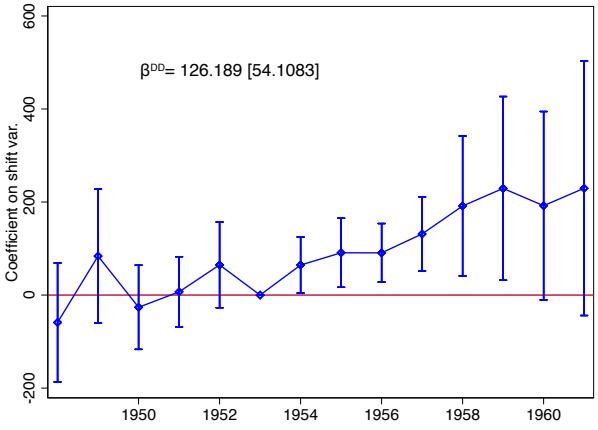
(b) Covered employment p.c.



(c) Top-ten income share



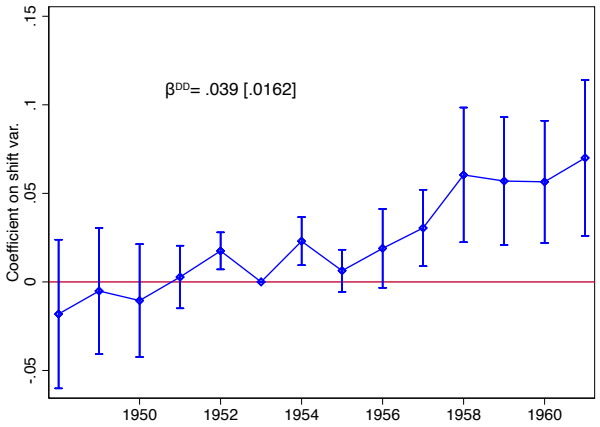
(d) Share paying taxes



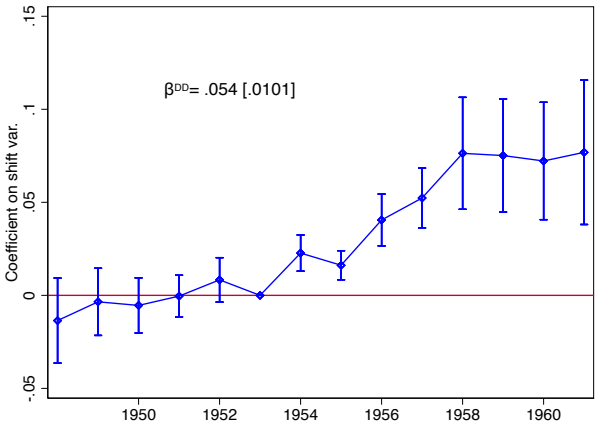
Notes: These graphs follow our standard event-study specification as in equation (2) in the text (with region × year fixed effects) but now includes *Union density*$_{st}$ as an additional control.

Appendix Figure A.4: Main missile-shift event study graphs with $WW2\,spending \times Year$ FE

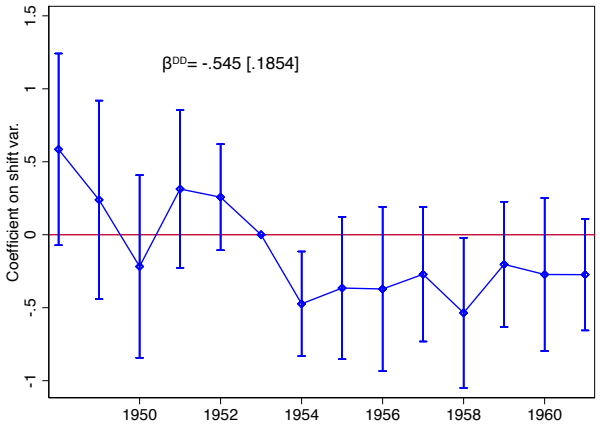
(a) Manufacturing employment p.c.



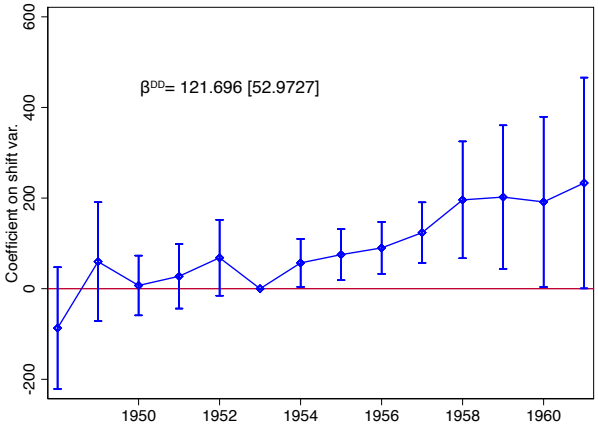
(b) Covered employment p.c.



(c) Top-ten income share



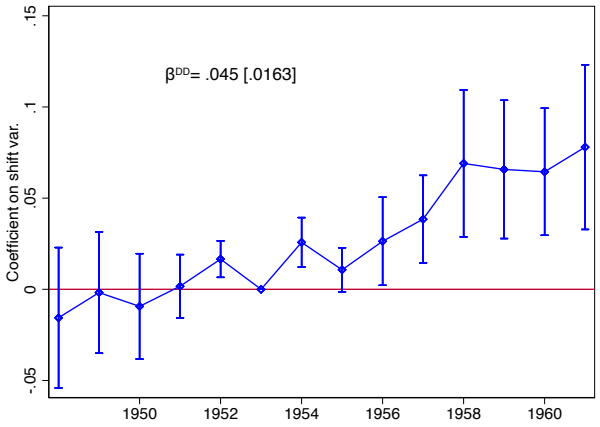
(d) Share paying taxes



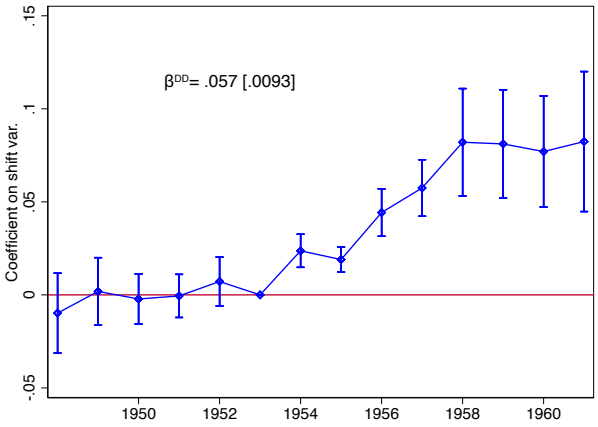
Notes: These graphs follow our standard event-study specification as in equation (2) in the text (with region × year fixed effects) but now includes *World War II*$_s$ × year fixed effects as an additional control. *World War II* is the logged ratio of total procurement spending in state s between 1940 and 1945 divided by 1940 state GDP.

Appendix Figure A.5: Main missile-shift event study graphs controlling for top state marginal tax rate

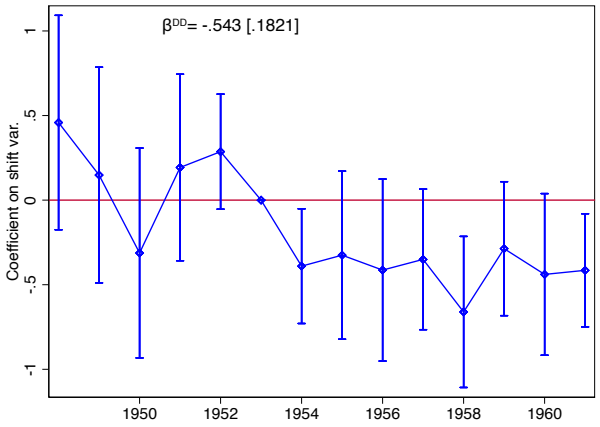
(a) Manufacturing employment p.c.



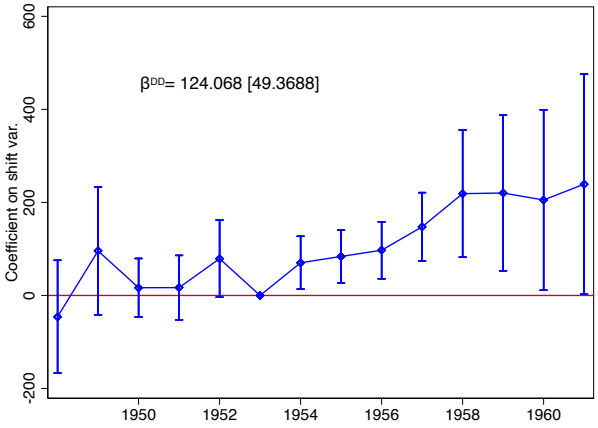
(b) Covered employment p.c.



(c) Top-ten income share



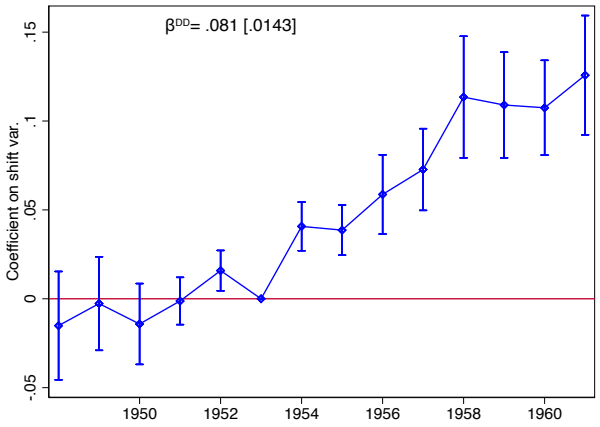
(d) Share paying taxes



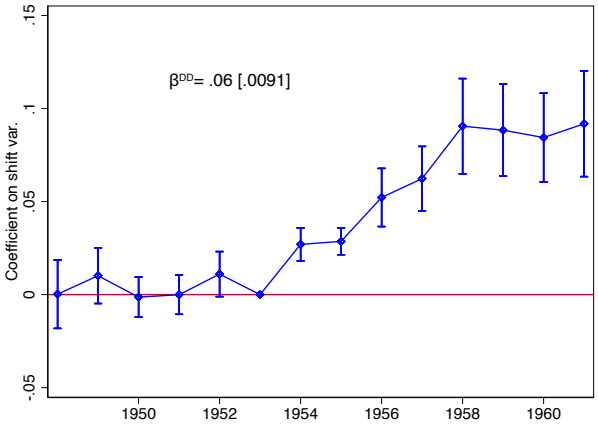
Notes: These graphs follow our standard event-study specification as in equation (2) in the text (with region × year fixed effects) but now includes the top marginal tax rate in state s in year t as an additional control.

Appendix Figure A.6: Main missile-shift event study graphs with 1950 skill share $\times Year$ FE

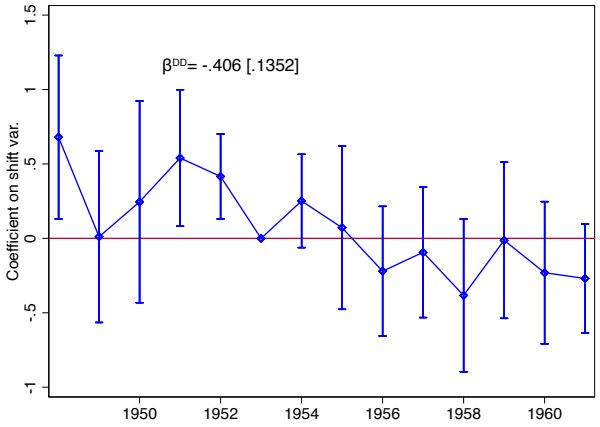
(a) Manufacturing employment p.c.



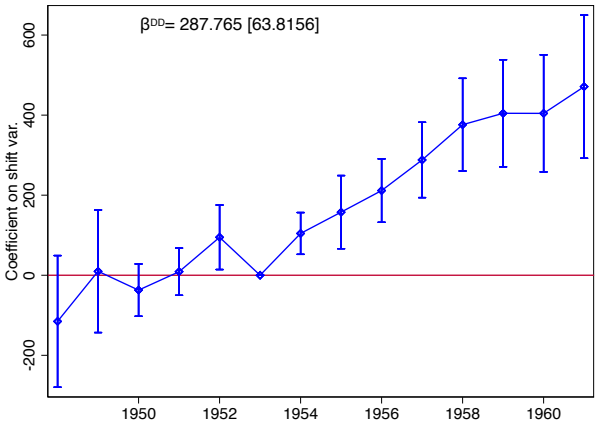
(b) Covered employment p.c.



(c) Top-ten income share

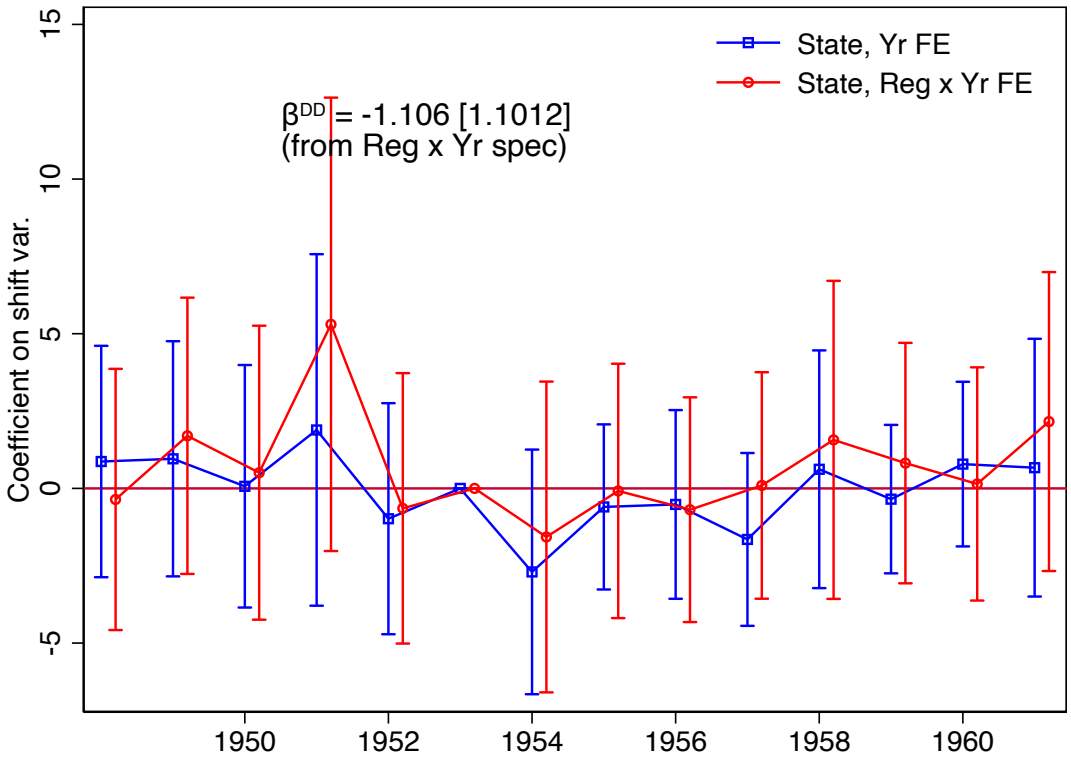


(d) Share paying taxes



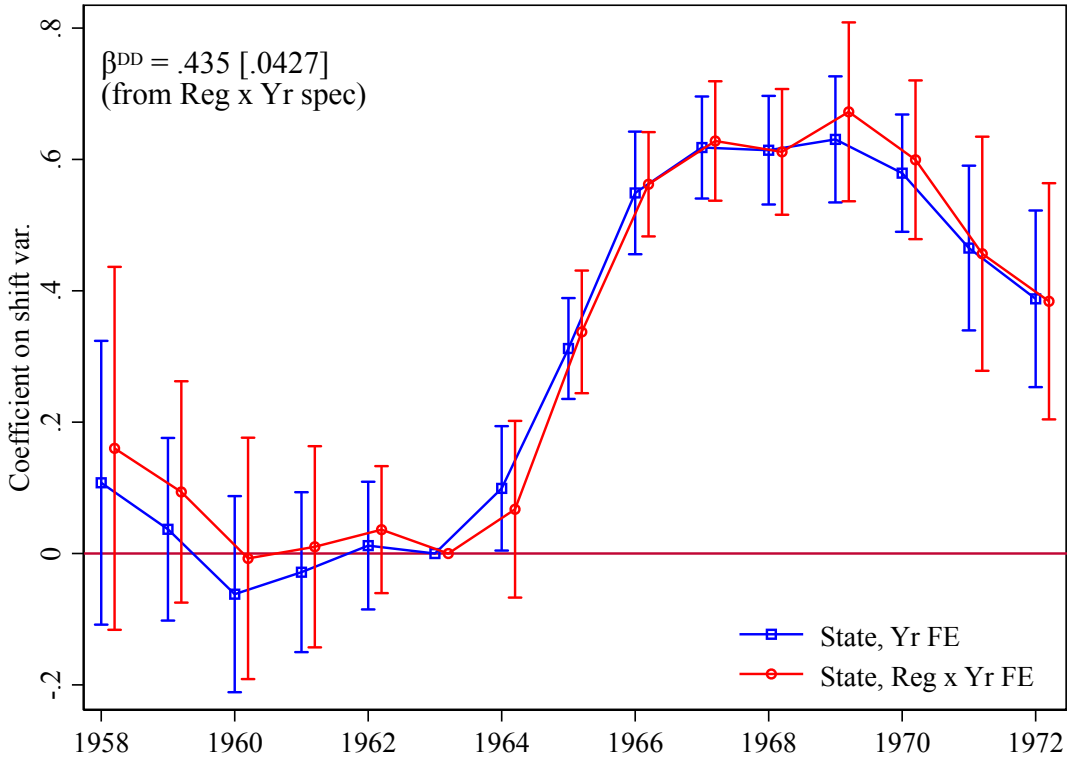
Notes: These graphs follow our standard event-study specification as in equation (2) in the text (with region × year fixed effects) but now includes *Skill share 1950*$_s$ × year fixed effects as an additional control. *Skill share 1950*, roughly the 1950 college-vs-high-school ratio weighted by efficiency units, is calculated as in Goldin and Katz (2008) but at the state level.

Appendix Figure A.7: Union density as a function of missile-shift



Notes: This figure displays coefficients (and 95% confidence interval) from an event-study specification that regresses state-level union density on the *missile shift* variable interacted with year effects (with 1953 as the omitted year), as well as state and year (or region×year) fixed effects. β^{DD} refers to the coefficient from an analogous regression but where *missile shift*×$\{year > 1953\}$ replaces the year interactions. Standard errors are clustered by state in all analyses.

Appendix Figure A.8: "First stage" (procurement as a function of "Vietnam shift"

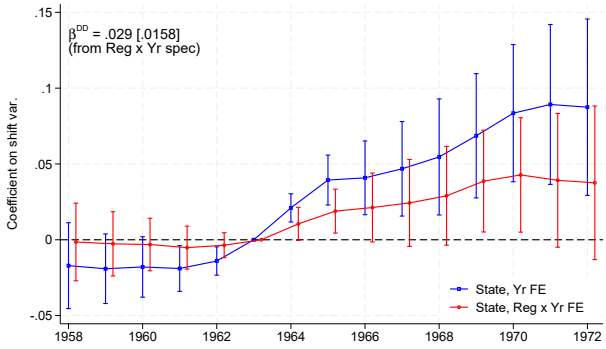


Notes: This figure uses our standard event-study specification with (log) procurement over once-lagged state GDP (standardized) as the outcome, as defined below:

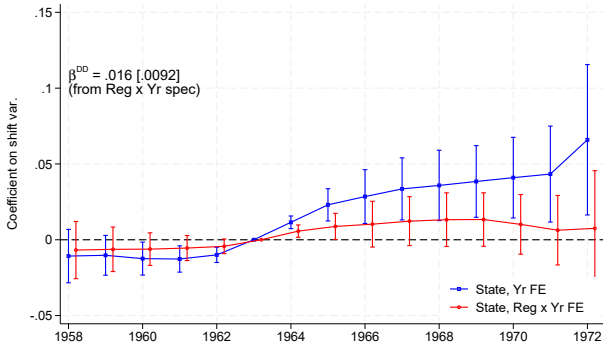
$$\log\left(\frac{Procurement_{st}}{GDP_{s,t-1}}\right) = \sum_{k\neq 1963} \beta_k \Delta_s^{Vietnam} \mathbb{I}^{t=k} + \eta_s + \delta_{f(s)t} + e_{st},$$

Appendix Figure A.9: Employment outcomes as a function of"Vietnam shift"

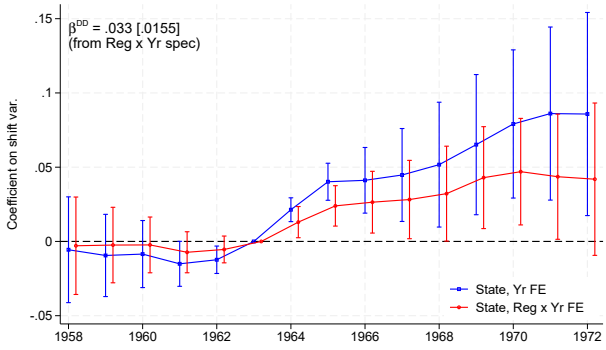
(a) Manufacturing employment p.c.



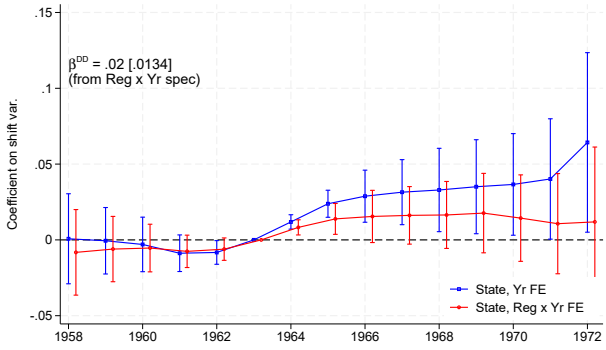
(b) Covered employment p.c.



(c) Total manuf employment



(d) Total covered employment

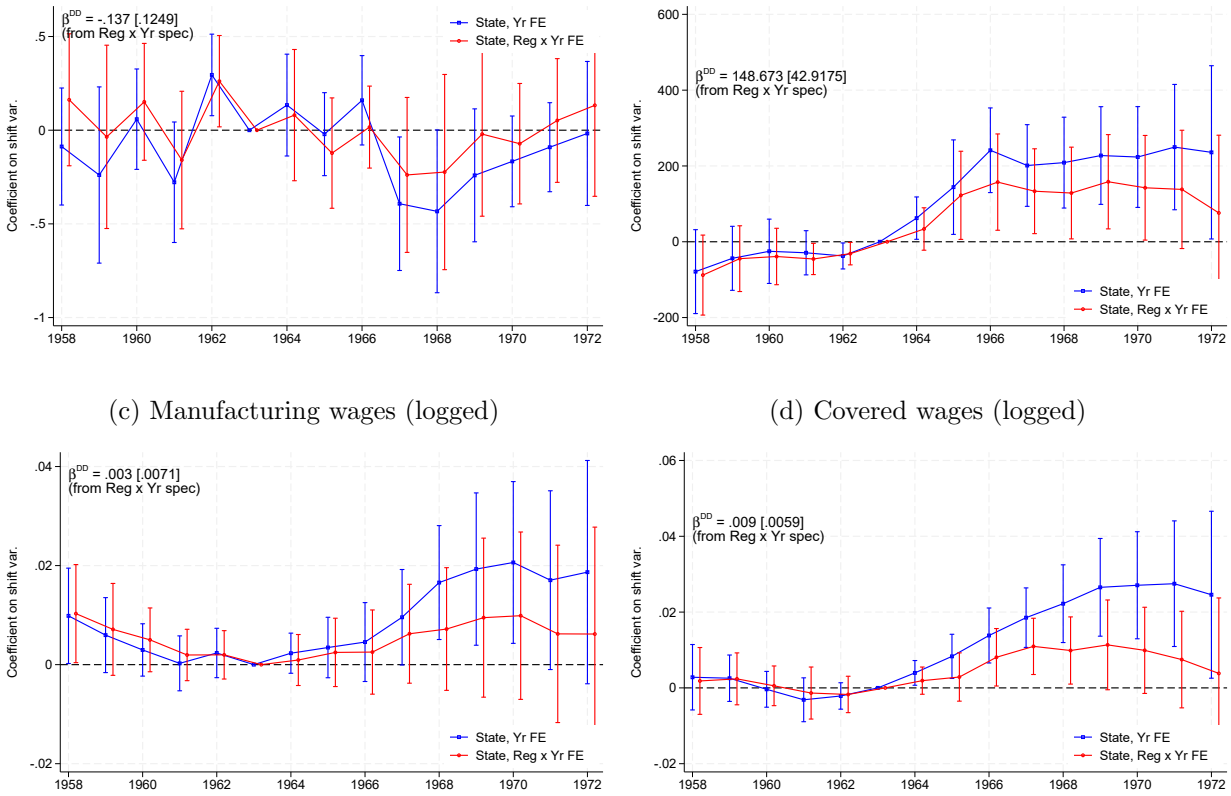


Notes: These figures are identical to those in Figure 3 but for the 1958-1972 period.

Appendix Figure A.10: Inequality and wage outcomes as a function of "Vietnam shift"

(a) Top-ten income share

(b) Share filing tax return

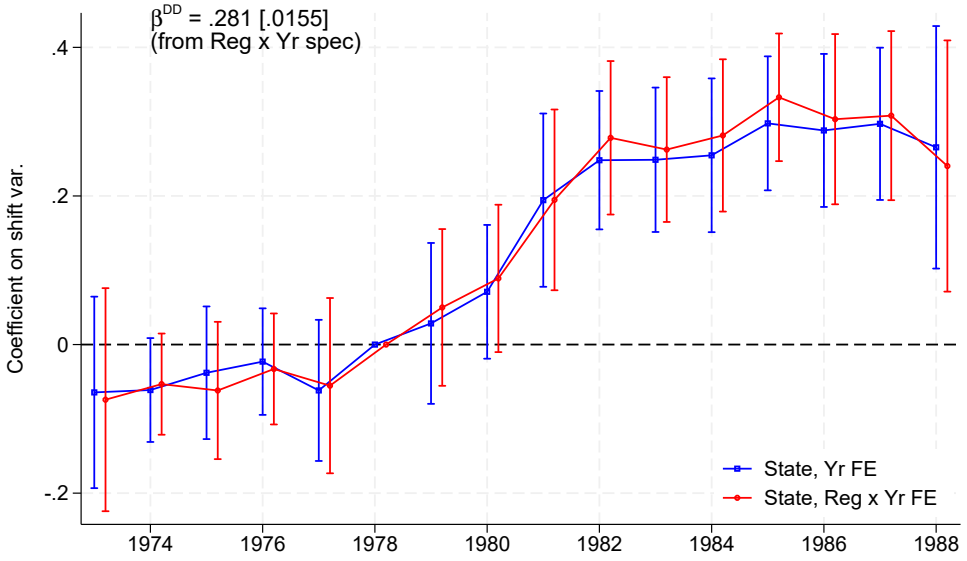


(c) Manufacturing wages (logged)

(d) Covered wages (logged)

Notes: These figures are identical to those in Figure 4 but for the 1958-1972 period.

Appendix Figure A.11: "First stage" (procurement as a function of "end of *détente*" shift)

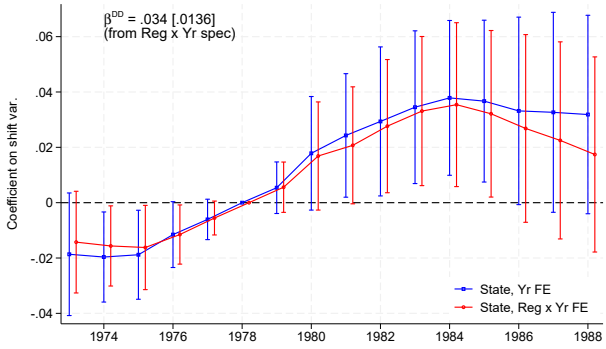


Notes: This figure uses our standard event-study specification with (log) procurement over once-lagged state GDP (standardized) as the outcome, as defined below:

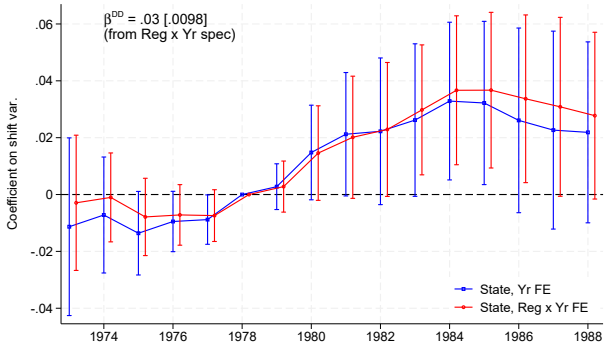
$$\log\left(\frac{Procurement_{st}}{GDP_{s,t-1}}\right) = \sum_{k \neq 1978} \beta_k \Delta_s^{détente} \mathbb{I}^{t=k} + \eta_s + \delta_{f(s)t} + e_{st}$$

Appendix Figure A.12: Employment outcomes as a function of "end of *détente*" shift

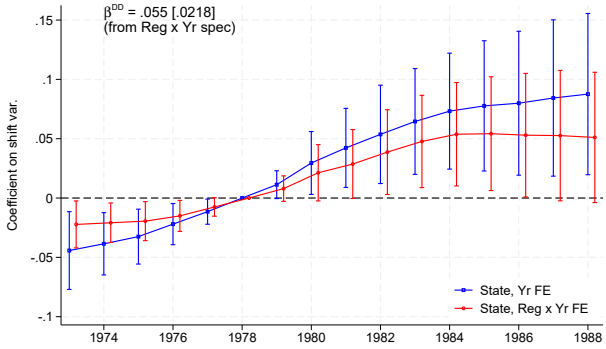
(a) Manufacturing employment p.c.



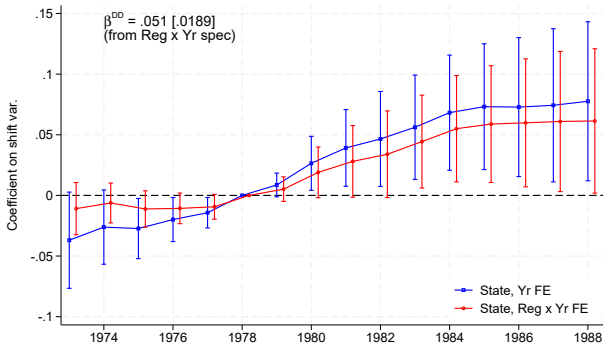
(b) Covered employment p.c.



(c) Total manuf employment



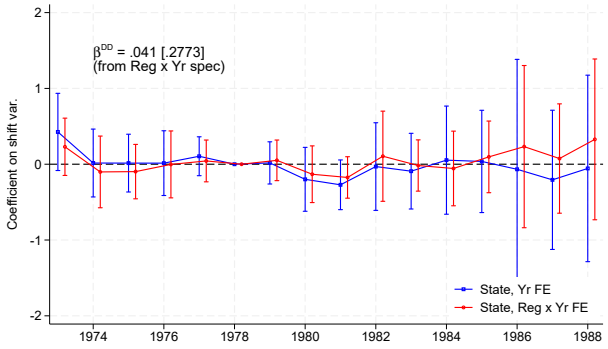
(d) Total covered employment



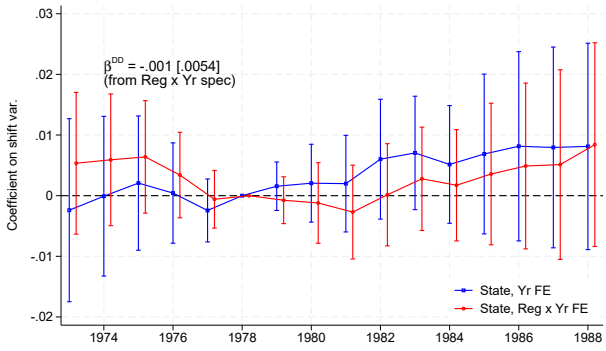
Notes: These figures are identical to those in Figure 3 but for the 1974-1986 period.

Appendix Figure A.13: Inequality and wage outcomes as a function of "end of *détente*" shift

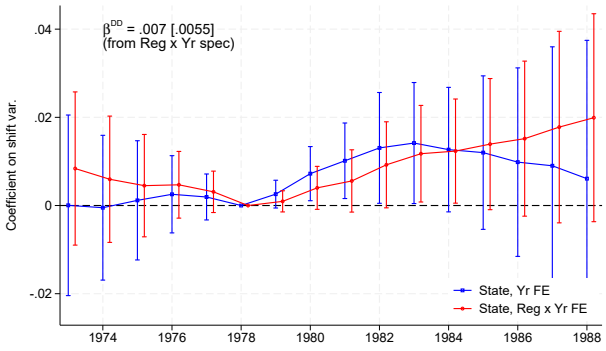
(a) Top-ten income share



(b) Manufacturing wages (logged)

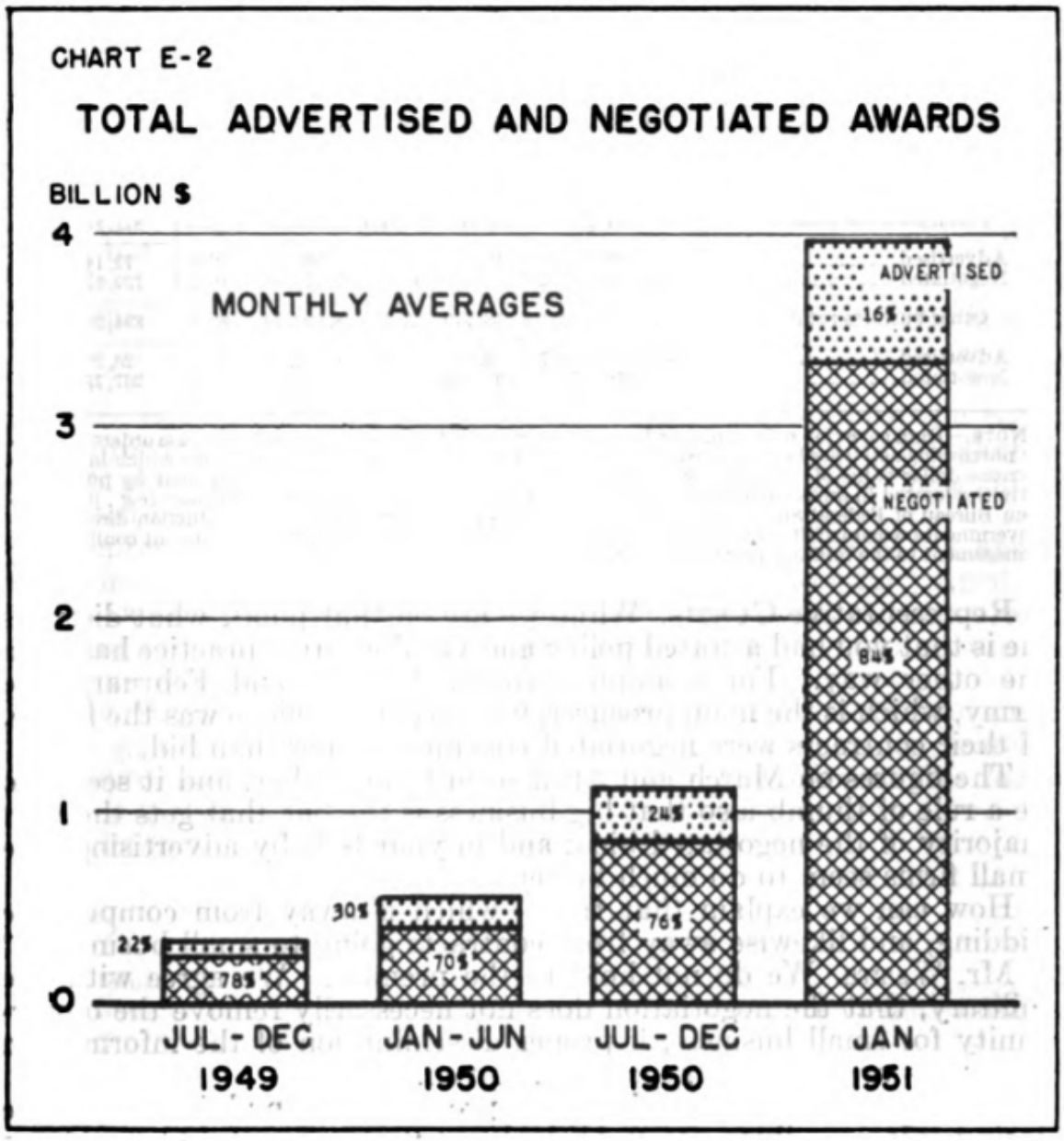


(c) Covered wages (logged)



Notes: These figures are identical to those in Figure 4 but for the 1974-1986 period.

Appendix Figure A.14: Procurement spending by month, 1949-January 1951



Source: Small business participation in military procurement. Joint hearings before the Select Committee on Small Business, United States Senate, and Select Committee on Small Business, House of Representatives, under the sponsorship of Subcommittee no. 2 (Government Procurement) Eighty-second Congress, first session. Page 61. Accessed via Hathi.

Appendix Table A.1: State labor-market outcomes as a function of procurement with additional controls, 1958-1972

Panel (a): Key explanatory variable is total procurement (not disaggregated)

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Share tax	(7) Quit
Total spending	0.0440	0.0182	0.00256	0.00576	-0.192	1.309	0.0720
	[0.0167]	[0.00903]	[0.00528]	[0.00391]	[0.134]	[0.425]	[0.0678]
Obs.	641	641	641	641	641	641	613

Panel (b): Key explanatory variables are non-R&D and R&D procurement

	(1) Manuf emp	(2) Cov emp	(3) Manuf wage	(4) Cov wage	(5) Top10	(6) Share tax	(7) Quit
Non-RD	0.0354	0.0120	0.0000570	0.00347	-0.206	0.963	0.0355
spending	[0.0135]	[0.00803]	[0.00464]	[0.00387]	[0.126]	[0.395]	[0.0505]
RD spending	0.00804	0.00732	0.000102	0.000287	0.0252	0.252	0.0674
	[0.00690]	[0.00377]	[0.00217]	[0.00156]	[0.0454]	[0.148]	[0.0378]
Obs.	636	636	636	636	636	636	609

Notes: These tables are identical to those in Table 1 except we add additional controls to the regression: state-year union density, state-year top marginal tax rate, state-year minimum wage, and the state's 1960 skill share interacted with year fixed effects. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.2: State labor-market outcomes as a function of procurement, additional controls, 1974-1986

Panel (a): Key explanatory variable is total procurement (not disaggregated)

	(1)	(2)	(3)	(4)	(5)	(6)
	Manuf emp	Cov emp	Manuf wage	Cov wage	Top10	Log9010
Total spending	0.0383	0.0375	-0.00524	0.00408	-0.109	0.000966
	[0.0148]	[0.0131]	[0.00620]	[0.00785]	[0.342]	[0.0204]
Obs.	642	642	631	642	642	513

Panel (b): Key explanatory variables are non-R&D and R&D procurement

	(1)	(2)	(3)	(4)	(5)	(6)
	Manuf emp	Cov emp	Manuf wage	Cov wage	Top10	Log9010
Non-RD	0.0397	0.0399	-0.00537	0.00231	0.0939	0.000847
spending	[0.0160]	[0.0133]	[0.00629]	[0.00749]	[0.358]	[0.0184]
RD spending	0.00628	0.00575	0.00147	0.00249	-0.0942	-0.00918
	[0.00736]	[0.00437]	[0.00302]	[0.00311]	[0.155]	[0.00864]
Obs.	639	639	628	639	639	510

Notes: These tables are identical to those in Table 2 except we add additional controls to the regression: state-year union density, state-year top marginal tax rate, state-year minimum wage, and state-year skill share. We can calculate the state-year skill share annually from 1977 onward (using the CPS) and in earlier years we interpolate from 1970 (Census) and 1977. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table A.3: Manuf. Employment and Income Ineq. as a Function of Procurement Levels (1952-2000) – Shift Share IV

	OLS			2SLS		
	(1) Log Manuf. Emp.	(2) Manuf. Emp.	(3) Top 10%	(4) Log Manuf. Emp.	(5) Manuf. Emp.	(6) Top 10%
Total spending / gdp_{t-1}	0.0136	0.00117	-0.136	0.0431	0.00358	-0.366
	[0.00444]	[0.000340]	[0.0391]	[0.0159]	[0.000982]	[0.0988]
Obs.	2400	2400	2400	2400	2400	2400
First stage F-stat				19	19	19

Notes: The unit of analysis is a state-year and the sample consists of the 48 continental states. All regressions include state and region × year fixed effects and are weighted by state population with standard errors clustered by state. Panel A presents estimates from OLS regressions and panel B presents estimates from 2SLS regressions using our shift-share IV. The IV is defined as $Z_{st} = \frac{\sum_b \left(\sum_{s',s' \neq s} Proc_{bs't}\right) \times \frac{Proc_{b,s,1951}}{Proc_{b,1951}}}{GDP_{s,t-1}}$, where $Proc_{bst}$ denotes procurement expenditure by branch b of the armed forces (Navy, Army, or Air Force), s denotes state, and t denotes year. Total spending is the ratio of procurement spending and once-lagged state GDP and then logged. The shares in the shift-share are 1951 military branch-specific procurement shares for each state s and the shifts are national branch-specific procurement (leaving out state s). (Logged) Manufacturing employment is per capita. Top 10 share of income is out of 100. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

Appendix Table A.4: Pre-Korea-shock state-level agricultural and political outcomes as a function of missile shock

Panel (*a*): Agricultural outcomes

	(1) Δ Log Farm Earnings	(2) Δ Log PC Farm Empl.	(3) Δ Log Num. Farms
Missile Shock	-0.0459	-0.000453	0.00424
	[0.0418]	[0.0115]	[0.0252]
DV Mean	0.0866	-0.138	-0.223
Years	1943-1953	1950-1953	1943-1953
Region FE?	Y	Y	Y
Observ.	48	48	48

Panel (*b*): Political outcomes

	(1) Avg D Gov	(2) D Gov 1953	(3) Avg D Sens	(4) D Sens 1953	(5) N. HASC Reps
Missile Shock	-0.00361	-0.0255	0.0278	0.0276	-0.155
	[0.0752]	[0.0614]	[0.206]	[0.220]	[0.314]
DV Mean	0.507	0.375	0.967	0.980	0.0101
Years	1948-1953	1953	1948-1953	1953	1953
Region FE?	Y	Y	Y	Y	Y
Observ.	48	48	48	48	48

Panel (*c*): Presidential election Democrat vote shares

	(1) 1944	(2) 1948	(3) 1952	(4) Avg
Missile Shock	0.00198	0.0202	-0.0204	0.00107
	[0.0201]	[0.0317]	[0.0150]	[0.0115]
DV Mean	0.566	0.483	0.439	0.496
Years	1944	1948	1952	1944/1948/1952
Region FE?	Y	Y	Y	Y
Observ.	48	48	48	48

Notes: We regress the pre-shock outcome of interest on the missile shock variable as a cross-sectional placebo test. All regressions include region fixed effects and are weighted by state population. *N. HASC Reps* refers to the number of representatives from a given state on the House Armed Services Committee in 1953. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.5: Post-shock political and agricultural outcomes as a function of procurement, full sample

Panel (*a*): Key explanatory variable is total procurement (not disaggregated)

	(1) Log Farm Earnings	(2) Log Num. Farms	(3) D Gov	(4) D Vote %
Total spending	0.00500	0.00595	0.0135	-0.00672
	[0.0414]	[0.0310]	[0.0480]	[0.00606]
Observations	2377	2393	2339	2393
Sh. expl., 1990s v. 1950s	-.005	.009	.086	.747

Panel (*b*): Key explanatory variables are non-R&D and R&D procurement

	(1) Log Farm Earnings	(2) Log Num. Farms	(3) D Gov	(4) D Vote %
RD spending	-0.00604	-0.0199	-0.0193	0.00109
	[0.0235]	[0.0140]	[0.0257]	[0.00320]
Non-RD spending	0.0310	0.0329	0.0219	-0.00688
	[0.0446]	[0.0372]	[0.0577]	[0.00772]
Observations	2016	2032	1995	2032

Notes: We regress the lagged pre-shock outcome of interest on the procurement variable. All regressions include state, year, and region × year fixed effects and are weighted by state population. The Democrat vote percentage is given by the share of voters in a state voting Democrat in the most recent election for any given year prior to 2001. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix B. Data Appendix

B.1. Procurement

Total Procurement. We construct state-level military procurement spending from 1956–2006 from various sources. We OCR tables from the Department of Defense's annual publications titled "Military Prime Contract Awards by State" to get state-level total procurment from 1951–1981. These reports provide information on total military prime contract awards over $10,000 for each state. We construct state-level procurement spending from 1982–2006 using "Records of Prime Contracts Awarded by the Military Services and Agencies" from the National Archives. This series contains micro-data on procurement contracts with a value larger than $10,000 from 1965–1982 and a value larger than $25,000 starting in 1983. Total contract spending for the entire U.S. and a selection of states are plotted in Figure B.6 and Figure B.7.

Appendix Figure B.1: Military Prime Contracts by State

Net Value and Percent Distribution of DOD Military Prime Contract Awards Over $10,000, By State, According to Department: FY 1951

(Amounts in Thousands)

STATE	TOTAL		ARMY	NAVY	AIR FORCE
	AMOUNT	PER-CENT			
TOTAL	$29,619,569	100.0	$13,313,033	$7,768,229	$8,538,307
ALABAMA	147,933	0.5	128,031	19,187	715
ARIZONA	51,961	0.2	45,214	5,915	832
ARKANSAS	52,476	0.2	13,879	38,451	146
CALIFORNIA	3,897,915	13.2	828,875	1,349,292	1,719,748
COLORADO	64,838	0.2	53,330	9,058	2,450
CONNECTICUT	1,724,439	5.8	293,691	1,201,747	229,001
DELAWARE	12,256	*	7,427	3,314	1,515
DISTRICT OF COLUMBIA	312,928	1.1	85,853	207,551	19,524
FLORIDA	75,101	0.3	50,219	20,543	4,339
GEORGIA	182,192	0.6	148,831	25,561	7,800
IDAHO	13,919	0.1	13,832	87	0
ILLINOIS	1,585,630	5.4	1,282,372	159,317	143,941
INDIANA	1,866,767	6.3	664,803	85,605	1,116,359
IOWA	249,715	0.8	159,609	58,397	31,709
KANSAS	301,772	1.0	107,319	16,966	177,487
KENTUCKY	69,500	0.2	44,675	20,103	4,722
LOUISIANA	181,307	0.6	127,787	52,711	809
MAINE	90,642	0.3	69,630	21,012	0
MARYLAND	595,806	2.0	129,445	210,030	256,331
MASSACHUSETTS	894,637	3.0	482,276	229,142	183,219
MICHIGAN	2,470,032	8.3	1,284,119	387,769	798,144
MINNESOTA	223,724	0.8	144,731	29,569	49,424
MISSISSIPPI	42,278	0.1	38,989	2,995	294
MISSOURI	492,404	1.7	200,526	183,023	108,855
MONTANA	9,777	*	9,576	180	21
NEBRASKA	94,145	0.3	77,959	3,835	12,351
NEVADA	2,019	*	1,699	320	0
NEW HAMPSHIRE	73,608	0.3	41,039	32,426	143
NEW JERSEY	1,586,819	5.4	593,023	327,370	666,426
NEW MEXICO	33,827	0.1	32,885	207	735
NEW YORK	5,378,758	18.2	2,169,208	1,611,537	1,598,013
NORTH CAROLINA	209,813	0.7	150,464	35,132	24,217
NORTH DAKOTA	2,966	*	2,641	0	325
OHIO	2,219,754	7.5	1,265,517	423,780	530,457
OKLAHOMA	84,291	0.3	61,567	11,478	11,246
OREGON	98,825	0.3	86,366	11,705	754
PENNSYLVANIA	1,418,668	4.8	785,352	450,675	182,641
RHODE ISLAND	204,384	0.7	166,700	34,931	2,753
SOUTH CAROLINA	118,070	0.4	101,682	13,230	3,158
SOUTH DAKOTA	10,891	*	10,397	44	450
TENNESSEE	206,645	0.7	171,242	32,947	2,456
TEXAS	658,587	2.2	304,062	205,081	149,444
UTAH	32,869	0.1	30,331	2,459	79
VERMONT	22,872	0.1	9,855	12,515	502
VIRGINIA	230,484	0.8	118,123	101,427	10,934
WASHINGTON	638,944	2.2	200,327	54,742	383,875
WEST VIRGINIA	38,464	0.1	24,882	10,984	2,598
WISCONSIN	633,510	2.1	491,233	44,932	97,345
WYOMING	10,407	*	1,440	8,947	20

Note: An "*" in the percent column represents a figure of less than 0.05 percent. Percentages may not add up due to rounding.

R&D Procurement. We construct state-level R&D procurement spending from 1958–1961 using hand entered tables from the Congressional Report titled "Background Material on Economic Aspects of Military Procurement and Supply." For 1962–1965 we use hand entered

data from the Department of Defense's report titled "Military Prime Contract Awards by Region and State." Lastly, for 1966–2006 we use the National Archives micro-data discussed above. Non-R&D spending is calculated as the difference between total procurement spending and R&D procurement spending. Total R&D contract spending for the entire U.S. and a selection of states are plotted in Figure B.8 and Figure B.9.

Appendix Figure B.2: Military Prime Contracts Awards by State

TABLE 15.—*Net value of military prime contract awards for experimental, developmental, test, and research work, by States,[1] fiscal years 1958–62*

[Amounts in thousands]

State	Fiscal year 1958		Fiscal year 1959		Fiscal year 1960		Fiscal year 1961		Fiscal year 1962	
	Amount	Percent	Amount	Percent	Amount	Percent	Amount	Percent	Amount	Percent
Grand total	$4,056,410	100.0	$5,207,464	100.00	$5,521,435	100.0	$6,027,495	100.00	$6,113.115	100.00
New England:;										
Maine	117	(2)	152	(2)	670	(2)	128	(2)	496	.01
New Hampshire	2.365	.1	7,109	.1	8,842	.2	10,664	.18	8,204	.13
Vermont	2,905	.1	1,800	(2)	3,030	.1	1,386	.02	1,899	.03
Massachusetts	232,318	5.7	304,945	5.9	397,517	7.2	348,452	5.78	361,973	5.92
Rhode Island	1,824	(2)	2,045	.1	1,512	(2)	5,601	.09	6,312	.10
Connecticut	56,120	1.4	105,105	2.0	91,979	1.7	123,295	2.05	65,005	1.06
Total	295,649	7.3	421,156	8.1	503,550	9.2	489,526	8.12	443,889	7.25
Middle Atlantic:										
New York	569,710	14.0	667,218	12.8	533,169	9.7	734,934	12.19	664,844	10.87
New Jersey	200,382	5.0	161,274	3.1	434,654	7.9	228,280	3.79	293,237	4.80
Pennsylvania	276,692	6.8	256,444	4.9	189,385	3.4	224,239	3.72	235,998	3.86
Total	1,046,784	25.8	1,084,936	20.8	1,157,208	21.0	1,187,453	19.70	1,194,079	19.53
East North Central:										
Ohio	138,615	3.4	173,595	3.3	179,349	3.3	137,502	2.28	132,603	2.17
Indiana	45,799	1.1	54,058	1.0	34,065	.6	29,488	.49	39,405	.64
Illinois	56,627	1.4	67,700	1.2	67,287	1.2	61,984	1.03	56,296	.92
Michigan	71,655	1.8	117,542	2.3	84,503	1.5	92,313	1.53	58,550	.96
Wisconsin	4,424	.1	5,005	.1	64,079	1.2	74,239	1.23	63,487	1.04
Total	317,120	7.8	417,900	7.9	429,283	7.8	395,526	6.56	350,641	5.73
West North Central:										
Minnesota	53,436	1.3	64,826	1.2	59,968	1.1	51,378	.85	52,082	.85
Iowa	13,924	.3	20,117	.4	14,617	.3	5,051	.09	5,563	.09
Missouri	26,871	.7	40,115	.8	24,154	.4	18,226	.30	17,237	.28
North Dakota	0	0	85	(2)	0	0	0	0	0	0
South Dakota	72	(2)	149	(2)	548	(2)	292	(2)	401	.01
Nebraska	28	(2)	11	(2)	22	(2)	5,011	.09	2,910	.05
Kansas	4,971	.1	3,963	.1	6,054	.1	3,092	.05	6,198	.10
Total	99,302	2.4	129,266	2.5	105,363	1.9	83,050	1.38	84,392	1.38

See footnotes at end of table, p. 43.

ECONOMIC ASPECTS OF MILITARY SUPPLY 41

B.2. Economic Variables

Personal Income, GDP, and Population. State-year level data on personal income and population from 1947–2006 and GDP from 1963–2006 are obtained from the Bureau of Economic Analysis.

B.3. Inequality Measures

Top Income Shares. State-year top 1% and top 10% income shares from 1947–2006 are obtained from the World Inequality Database.

Share Filing. We obtain the share of adults filing a tax return from 1947–2006 from the World Inequality Database. We argue this is a good measure for the bottom part of the income distribution in Appendix C.

Log 90–10, 90–50, and 10–50 Ratios. 90–10, 90–50, and 10–50 percentile log wage ratios are taken form Farber *et al.* (2021a).

B.4. Labor Market Variables

Quit Rate. We construct state-year quit rates (number of separations from quits per 1,000 workers) from a variety of sources. We obtain quite rates for the manufacturing sector from 1952–1956 from the Department of Labor's "The Labor Market and Employment Security" (LMES) publications. These data are derived from bimonthly reports received by the DOL's Bureau of Employment Security from affiliated State employment agencies. We obtain state-level manufacturing quit rates from 1957–1979 the Department of Labor's "Employment and Earnings" publications, which are available through FRASER. These data were hand entered from the corresponding tables. Quit rates from 2001–2006 are obtained from the BLS's Job Openings and Labor Turnover Survey (JOLTS) data. Population weighted quit rates for the entire U.S. and a selection of states are plotted below. The national series and series for several states are plotted in Figure B.10 and Figure B.11.

Appendix Figure B.3: The Labor Market and Employment Security

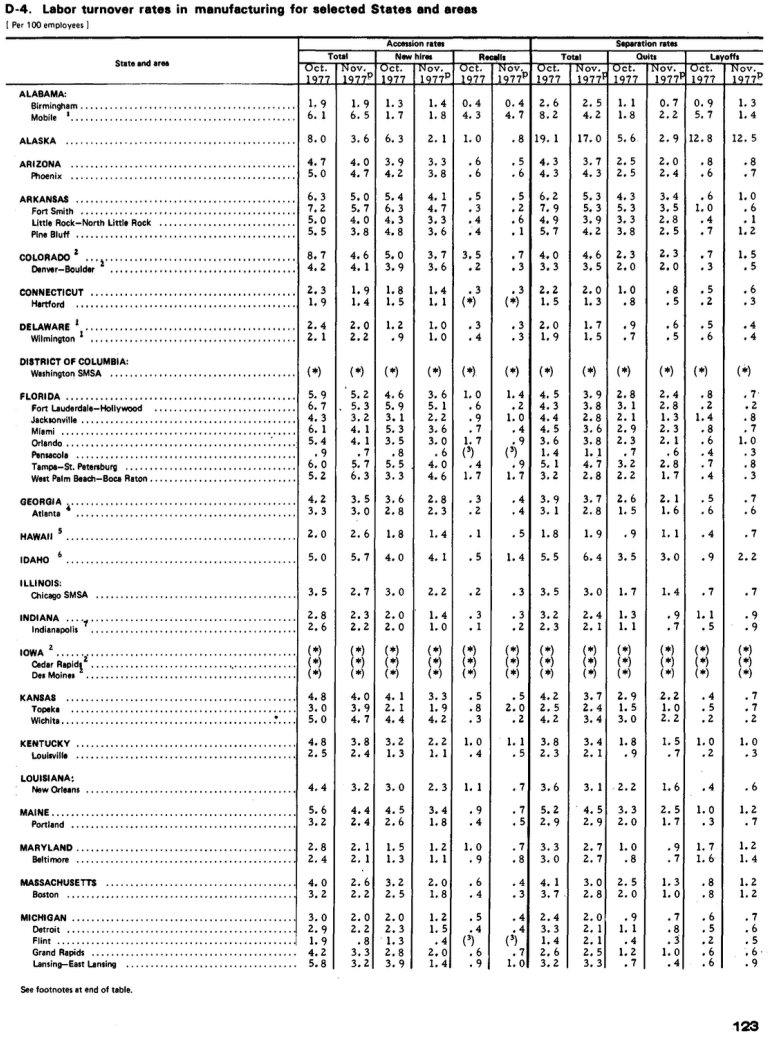
Labor Market Indicators

State and Area	Classification according to relative adequacy of local labor supply 1/			Nonagricultural wage and salary employment (in thousands)							Women as a percent of nonagricultural wage & salary employment	Turnover
				Total			Manufacturing					Manufacturing quits per 100 workers
				Nov. 1954	Percentage change from--		Nov. 1954	Percentage change from--		Employer forecasts of labor requirements 2/		
	Jan. 1955	Nov. 1954	Jan. 1954		Sept. 1954	Nov. 1953		Sept. 1954	Nov. 1953	Nov. 1954-Mar. 1955	Nov. 1954	Oct. 1954
Alabama												
*Birmingham	III	III	III	187.8	-0.4	-2.9	60.0	-2.0	-6.3	No change	24.7	0.5
*Mobile	III	III	III	79.1	+1.4	-0.8	16.9	-0.6	+9.0	Moderate decrease	26.9	3.0
Arizona												
*Phoenix	III	III	III	100.3	+4.6	+3.8	16.0	+4.6	+7.4	Slight increase	30.1	1.7
Arkansas												
*Little Rock-North Little Rock	III	III	III	68.0	+1.6	-3.4	11.8	+0.4	-13.9	No change	36.3	1.6
California												
Fresno	III	III	III	71.8	-1.3	+3.0	14.9	+0.3	+12.3	Substantial decrease	31.2	INA
Los Angeles	III	III	III	1,840.9	+0.4	-0.3	646.6	+0.6	+1.5	No change	32.5	INA
Sacramento	III	III	III	115.1	-1.5	+1.7	10.2	-15.7	+1.5	Moderate decrease	31.8	INA
San Bernardino-Riverside	III	III	III	127.0	-0.6	-2.7	24.8	-3.3	-11.9	No change	30.2	INA
San Diego	III	III	II	176.2	-1.0	-5.0	48.6	-1.3	-7.0	No change	32.8	INA
San Francisco-Oakland	III	III	III	865.1	-1.1	-1.9	197.6	-5.6	-3.8	Slight decrease	32.9	INA
San Jose	III	III	III	96.6	-10.3	+5.9	26.4	-32.3	+2.7	Slight increase	32.5	INA
Stockton	III	III	III	54.0	-5.7	-0.1	11.2	-21.1	+3.5	Substantial decrease	29.1	INA
Colorado												
**Denver	II	II	II	232.0	-0.4	0.0	41.0	-1.9	-9.7	Slight decrease	39.7	INA
Connecticut												
*Bridgeport	III	III	II	115.2	-0.8	-6.5	64.9	-2.0	-11.3	No change	32.4	1.2
*Hartford	II	II	I	197.6	+1.2	-2.1	74.6	-0.5	-6.8	No change	36.2	1.3
*New Britain	III	III	II	40.7	-1.2	-8.7	26.0	-2.6	-13.9	Slight increase	32.2	0.9
*New Haven	II	II	II	120.1	+0.4	-1.3	47.1	+0.6	-5.2	Slight increase	36.1	1.7
*Stamford-Norwalk	III	III	II	73.8	-0.5	-2.0	33.8	0.0	-4.0	No change	34.4	1.3
*Waterbury	III	III	II	67.8	+1.2	-6.0	43.2	+0.9	-9.1	Moderate decrease	33.8	1.3
Delaware												
Wilmington	III	III	III	98.6	-0.2	-2.2	42.7	+0.4	-5.0	Slight increase	9.9	0.7
District of Columbia												
*Washington	III	III	III	617.8	+0.5	(3/)	26.1	+1.1	+1.1	No change	37.7	1.9
Florida												
*Jacksonville	II	II	II	115.4	+1.3	+2.3	19.0	+5.0	+5.2	Moderate decrease	31.3	5.2
*Miami	III	III	II	216.8	+6.6	+7.8	27.7	+15.9	+18.4	Substantial increase	32.2	2.9
*Tampa-St. Petersburg	III	III	III	128.3	+5.2	+2.6	24.1	+5.5	-6.6	Substantial increase	35.8	0.6
Georgia												
*Atlanta	II	II	II	306.2	+2.0	+0.9	81.6	+2.0	-0.9	Slight increase	33.5	1.7
Augusta	See Aiken-Augusta, South Carolina											
Columbus	III	III	III	43.4	+1.4	-1.4	18.5	+2.1	-0.7	Slight increase	39.3	2.8
Macon	III	III	II	57.6	+0.4	+0.7	13.3	-0.1	-1.8	No change	31.8	2.3
*Savannah	III	III	III	48.9	+1.0	-3.6	13.9	-0.9	-3.5	Slight increase	26.7	1.3
Hawaii												
Honolulu	IV-A	IV-A	III	106.6	-1.8	+0.5	12.1	-20.5	-0.1	Slight increase	30.6	1.4
Illinois												
Aurora	III	III	II	30.4	-0.7	-6.0	15.4	-3.5	-12.9	No change	36.3	2.2
Chicago	III	III	II	2,303.3	+0.1	-3.5	900.2	-0.3	-7.8	No change	30.6	1.9
Davenport-Rock Island-Moline	III	IV-A	III	89.3	+0.9	-2.2	40.5	-0.2	-4.5	Moderate increase	31.4	0.9
Joliet	III	IV-A	III	47.2	-0.4	-10.9	21.4	-0.7	-17.6	Slight increase	31.0	1.1

See explanatory notes at end of table.

57

Appendix Figure B.4: Employment and Earnings



D-4. Labor turnover rates in manufacturing for selected States and areas

[Per 100 employees]

State and area	Accession rates						Separation rates					
	Total		New hires		Recalls		Total		Quits		Layoffs	
	Oct. 1977	Nov. 1977[p]	Oct. 1977	Nov. 1977[p]	Oct. 1977	Nov. 1977[p]	Oct. 1977	Nov. 1977[p]	Oct. 1977	Nov. 1977[p]	Oct. 1977	Nov. 1977[p]
ALABAMA:												
Birmingham	1.9	1.9	1.3	1.4	0.4	0.4	2.6	2.5	1.1	0.7	0.9	1.3
Mobile [1]	6.1	6.5	1.7	1.8	4.3	4.7	8.2	4.2	1.8	2.2	5.7	1.4
ALASKA	8.0	3.6	6.3	2.1	1.0	.8	19.1	17.0	5.6	2.9	12.8	12.5
ARIZONA	4.7	4.0	3.9	3.3	.6	.5	4.3	3.7	2.5	2.0	.8	.8
Phoenix	5.0	4.7	4.2	3.8	.6	.6	4.3	4.3	2.5	2.4	.6	.7
ARKANSAS	6.3	5.0	5.4	4.1	.5	.5	6.2	5.3	4.3	3.4	.6	1.0
Fort Smith	7.2	5.7	6.3	4.7	.3	.2	7.9	5.3	5.3	3.5	1.0	.6
Little Rock–North Little Rock	5.0	4.0	4.3	3.3	.4	.6	4.9	3.9	3.3	2.8	.4	.1
Pine Bluff	5.5	3.8	4.8	3.6	.4	.1	5.7	4.2	3.8	2.5	.7	1.2
COLORADO [2]	8.7	4.6	5.0	3.7	3.5	.7	4.0	4.6	2.3	2.3	.7	1.5
Denver–Boulder [2]	4.2	4.1	3.9	3.6	.2	.3	3.3	3.5	2.0	2.0	.3	.5
CONNECTICUT	2.3	1.9	1.8	1.4	.3	.3	2.2	2.0	1.0	.8	.5	.6
Hartford	1.9	1.4	1.5	1.1	(*)	(*)	1.5	1.3	.8	.5	.2	.3
DELAWARE [1]	2.4	2.0	1.2	1.0	.3	.3	2.0	1.7	.9	.6	.5	.4
Wilmington [1]	2.1	2.2	.9	1.0	.4	.3	1.9	1.5	.7	.5	.6	.4
DISTRICT OF COLUMBIA:												
Washington SMSA	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)
FLORIDA	5.9	5.2	4.6	3.6	1.0	1.4	4.5	3.9	2.8	2.4	.8	.7
Fort Lauderdale–Hollywood	6.7	5.3	5.9	5.1	.6	.2	4.3	3.8	3.1	2.8	.2	.2
Jacksonville	4.3	3.2	3.1	2.2	.9	1.0	4.4	2.8	2.1	1.3	1.4	.8
Miami	6.1	4.1	5.3	3.6	.7	.4	4.5	3.6	2.9	2.3	.8	.7
Orlando	5.4	4.1	3.5	3.0	1.7	.9	3.6	3.8	2.3	2.1	.6	1.0
Pensacola	.9	.7	.8	.6	(*)	(*)	1.4	1.1	.7	.6	.4	.3
Tampa–St. Petersburg	6.0	5.7	5.5	4.0	.4	.9	5.1	4.7	3.2	2.8	.7	.8
West Palm Beach–Boca Raton	5.2	6.3	3.3	4.6	1.7	1.7	3.2	2.8	2.2	1.7	.4	.3
GEORGIA [3]	4.2	3.5	3.6	2.8	.3	.4	3.9	3.7	2.6	2.1	.5	.7
Atlanta [3]	3.3	3.0	2.8	2.3	.2	.4	3.1	2.8	1.5	1.6	.6	.6
HAWAII [5]	2.0	2.6	1.8	1.4	.1	.5	1.8	1.9	.9	1.1	.4	.7
IDAHO [6]	5.0	5.7	4.0	4.1	.5	1.4	5.5	6.4	3.5	3.0	.9	2.2
ILLINOIS:												
Chicago SMSA	3.5	2.7	3.0	2.2	.2	.3	3.5	3.0	1.7	1.4	.7	.7
INDIANA [7]	2.8	2.3	2.0	1.4	.3	.3	3.2	2.4	1.3	.9	1.1	.9
Indianapolis [7]	2.6	2.2	2.0	1.0	.1	.2	2.3	2.1	1.1	.7	.5	.9
IOWA [2]	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)
Cedar Rapids [2]	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)
Des Moines [2]	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)	(*)
KANSAS	4.8	4.0	4.1	3.3	.5	.5	4.2	3.7	2.9	2.2	.4	.7
Topeka	3.0	3.9	2.1	1.9	.8	2.0	2.5	2.4	1.5	1.0	.5	.7
Wichita	5.0	4.7	4.4	4.2	.3	.2	4.2	3.4	3.0	2.2	.2	.2
KENTUCKY	4.8	3.8	3.2	2.2	1.0	1.1	3.8	3.4	1.8	1.5	1.0	1.0
Louisville	2.5	2.4	1.3	1.1	.4	.5	2.3	2.1	.9	.7	.2	.3
LOUISIANA:												
New Orleans	4.4	3.2	3.0	2.3	1.1	.7	3.6	3.1	2.2	1.6	.4	.6
MAINE	5.6	4.4	4.5	3.4	.9	.7	5.2	4.5	3.3	2.5	1.0	1.2
Portland	3.2	2.4	2.6	1.8	.4	.5	2.9	2.9	2.0	1.7	.3	.7
MARYLAND	2.8	2.1	1.5	1.2	1.0	.7	3.3	2.7	1.0	.9	1.7	1.2
Baltimore	2.4	2.1	1.3	1.1	.9	.8	3.0	2.7	.8	.7	1.6	1.4
MASSACHUSETTS	4.0	2.6	3.2	2.0	.6	.4	4.1	3.0	2.5	1.3	.8	1.2
Boston	3.2	2.2	2.5	1.8	.4	.3	3.7	2.8	2.0	1.0	.8	1.2
MICHIGAN	3.0	2.0	2.0	1.2	.5	.4	2.4	2.0	.9	.7	.6	.7
Detroit	2.9	2.2	2.3	1.5	.4	.4	3.3	2.1	1.1	.8	.5	.6
Flint	1.9	.8	1.3	.4	(*)	(*)	1.4	2.1	.4	.3	.2	.5
Grand Rapids	4.2	3.3	2.8	2.0	.6	.7	2.6	2.5	1.2	1.0	.6	.6
Lansing–East Lansing	5.8	3.2	3.9	1.4	.9	1.0	3.2	3.3	.7	.4	.6	.9

See footnotes at end of table.

123

Manufacturing Wages. We construct state-year hourly manufacturing wages from 1947–1991 from the Bureau of Labor Statistics (BLS) publication titled "Employment, hours, and earnings in states and other areas of the United States, 1940-1991." These are supplemented by data from the Statistical Abstract of the United States in 1947 and 1950 which report annual wages which we convert into hourly wages by dividing by 40 hours/week $\times$ 50 working weeks.

Manufacturing Employment. State-year manufacturing employment from 1947–1991 come from the BLS publication "Employment, hours, and earnings in states and other areas of the United States, 1940-1991." Data for 1992–2002 are obtained from the BLS directly. The national series and series for a few states are plotted in Figure B.12 and Figure B.13.

Hourly Covered Wage, Employment We construct state-year average annual hourly wage for workers covered by unemployment insurance ("covered") (calculated as the weekly wage divided by 40) from 1947–1994 from Baicker *et al.* (1998) (data were shared with us by Larry

Katz). Covered employment for 1947–1994 is obtained from the same source.

Unemployment Rate. We construct state-year insured unemployment rates (IUR) from 1947–1994 from Baicker *et al.* (1998) (see above) and for 1995–2006 from FRED. We also obtain state-year unemployment rates (UR) from 1976–2006 from FRED. The national series and series for several states are plotted in Figure B.14 and Figure B.15.

Unemployment Rates by Age and Sex. We compile national unemployment rates by age and sex from 1948–1983 from the Bureau of Labor Statistics's publication titled "Labor Force Statistics Derived from the Current Population Survey: A Databook, Volume I and Volume II."

Appendix Figure B.5: Labor Force Statistics Derived from the CPS

MONTHLY DATA
NOT SEASONALLY ADJUSTED

A-28. Unemployment rates by sex, age, race, and Hispanic origin, 1948-81—Continued

(Percent)

Year	January	February	March	April	May	June	July	August	September	October	November	December	Annual average
White workers, 16 years and over													
1954	5.3	5.7	5.8	5.7	5.2	5.2	5.1	4.9	4.9	4.1	4.3	4.2	5.0
1955	5.3	4.9	4.7	4.4	3.7	4.0	3.6	3.2	3.1	3.0	3.3	3.5	3.9
1956	4.1	4.3	4.3	3.7	3.7	4.1	3.8	3.0	2.9	2.7	3.4	3.6	3.8
1957	4.4	4.2	3.9	3.6	3.5	4.1	3.7	3.3	3.3	3.3	4.2	4.5	3.8
1958	6.1	6.8	6.9	6.8	6.4	6.9	6.6	6.0	5.4	5.0	5.0	5.3	6.1
1959	6.2	6.2	5.6	4.7	4.3	4.8	4.5	4.1	4.2	4.1	4.8	4.5	4.8
1960	5.4	5.1	5.4	4.6	4.3	5.3	4.8	4.7	4.4	4.6	5.0	5.7	5.0
1961	7.0	7.3	7.0	6.3	5.9	6.6	6.2	5.4	5.1	4.9	5.0	5.1	6.0
1962	5.9	5.7	5.4	4.7	4.5	5.2	4.6	4.6	4.4	4.1	4.7	4.6	4.9
1963	5.9	6.1	5.6	5.1	4.9	5.5	5.0	4.5	4.3	4.1	4.7	4.7	5.0
1964	5.7	5.6	5.3	4.7	4.3	5.4	4.2	4.1	4.0	3.9	4.0	4.2	4.6
1965	4.9	5.1	4.6	4.4	4.0	4.9	3.9	3.7	3.4	3.3	3.4	3.4	4.1
1966	4.0	3.8	3.6	3.3	3.3	4.1	3.3	3.1	2.9	2.9	2.9	3.0	3.4
1967	3.8	3.7	3.4	3.1	2.8	4.1	3.6	3.3	3.3	3.3	3.3	3.1	3.4
1968	3.6	3.8	3.4	2.9	2.8	4.0	3.5	3.1	3.0	2.8	2.9	2.7	3.2
1969	3.3	3.4	3.1	2.8	2.6	3.6	3.4	3.1	3.3	3.2	3.0	3.0	3.1
1970	3.9	4.3	4.2	3.9	3.8	4.9	4.8	4.6	4.8	4.7	5.2	5.2	4.5
1971	6.1	6.1	5.9	5.2	4.9	5.8	5.5	5.4	5.2	4.9	5.3	5.1	5.4
1972	5.9	5.8	5.6	5.1	4.6	5.5	5.2	5.0	4.8	4.6	4.4	4.3	5.1
1973	5.0	5.1	4.7	4.3	3.9	4.7	4.3	4.2	4.1	3.8	4.0	4.1	4.3
1974	5.1	5.3	4.8	4.4	4.2	5.2	5.0	4.8	5.2	4.9	5.6	6.1	5.0
1975	8.3	8.5	8.4	7.9	7.7	8.3	7.9	7.4	7.3	7.1	7.1	7.1	7.8
1976	8.1	7.9	7.4	6.7	6.1	7.1	7.0	6.8	6.7	6.5	6.7	6.7	7.0
1977	7.6	7.8	7.2	6.2	5.7	6.6	6.0	5.9	5.8	5.5	5.6	5.2	6.2
1978	6.2	6.1	5.8	5.0	4.7	5.3	5.3	5.0	5.1	4.7	4.8	5.0	5.2
1979	5.7	5.6	5.4	4.7	4.5	5.1	5.0	5.2	5.0	4.9	4.9	5.0	5.1
1980	6.1	6.0	5.9	5.9	6.2	6.9	6.8	6.7	6.3	6.2	6.3	6.1	6.3
1981	7.5	7.3	6.8	6.2	6.3	6.6	6.3	6.1	6.3	6.5	7.0	7.3	6.7
White males, 16 years and over													
1954	5.0	5.6	5.7	5.6	5.0	4.7	4.8	4.6	4.6	3.9	4.0	4.3	4.8
1955	5.2	5.0	4.8	4.6	3.5	3.8	3.3	2.8	2.5	2.6	2.9	3.4	3.7
1956	4.1	4.3	4.2	3.5	3.4	3.6	3.2	2.6	2.5	2.4	3.0	3.5	3.4
1957	4.3	4.2	3.8	3.5	3.1	3.6	3.3	2.9	3.0	3.1	3.9	4.8	3.6
1958	6.3	7.1	7.5	6.9	6.3	6.5	6.4	5.7	5.0	4.7	4.8	5.5	6.1
1959	6.5	6.5	5.7	4.4	3.9	4.2	3.9	3.7	3.7	3.7	4.6	4.5	4.6
1960	5.4	5.1	5.5	4.6	4.1	4.8	4.4	4.3	4.0	4.2	4.8	5.9	4.8
1961	7.2	7.5	7.1	6.2	5.7	5.8	5.5	5.0	4.4	4.3	4.6	5.2	5.7
1962	5.8	5.7	5.5	4.6	4.2	4.7	4.1	4.1	3.7	3.5	4.3	4.7	4.6
1963	5.9	6.3	5.7	4.9	4.4	4.7	4.3	3.9	3.5	3.5	4.1	4.7	4.7
1964	5.5	5.4	5.1	4.4	3.8	4.6	3.6	3.5	3.3	3.3	3.4	4.0	4.1
1965	4.8	4.8	4.3	4.0	3.6	4.0	3.3	3.1	2.7	2.7	2.7	3.2	3.6
1966	3.7	3.6	3.4	2.8	2.7	3.2	2.7	2.4	2.3	2.2	2.4	2.9	2.8
1967	3.2	3.2	3.0	2.7	2.3	3.2	2.8	2.5	2.2	2.3	2.6	2.8	2.7
1968	3.2	3.3	3.0	2.4	2.1	3.1	2.7	2.3	2.1	2.1	2.2	2.4	2.6
1969	2.9	2.8	2.7	2.3	2.0	2.7	2.7	2.2	2.4	2.4	2.4	2.7	2.5
1970	3.6	4.0	3.9	3.6	3.4	4.3	4.3	3.9	3.8	4.0	4.5	5.1	4.0
1971	5.9	5.9	5.6	4.8	4.5	5.0	4.9	4.5	4.2	4.1	4.6	5.0	4.9
1972	5.8	5.8	5.4	4.7	4.1	4.7	4.2	4.0	3.8	3.8	3.8	4.0	4.5
1973	4.7	4.8	4.4	3.9	3.4	3.9	3.5	3.3	3.1	3.0	3.4	3.8	3.8
1974	4.7	4.9	4.3	4.0	3.6	4.3	4.1	3.9	4.0	4.1	4.8	5.7	4.4
1975	7.8	8.2	8.2	7.6	7.2	7.6	7.2	6.5	6.5	6.3	6.6	6.8	7.2
1976	7.8	7.6	7.1	6.4	5.7	6.4	6.2	5.7	5.6	5.6	6.1	6.4	6.4
1977	7.3	7.6	6.8	5.8	5.2	5.7	5.1	4.7	4.5	4.5	4.7	4.8	5.5
1978	5.7	5.9	5.6	4.5	4.0	4.3	4.3	4.0	3.9	4.0	4.0	4.6	4.6
1979	5.3	5.3	5.0	4.3	3.8	4.3	4.3	4.2	4.1	4.1	4.4	4.6	4.5
1980	6.0	5.9	5.9	6.0	6.2	6.7	6.7	6.3	5.9	5.9	6.1	6.1	6.1
1981	7.6	7.8	7.2	6.2	6.1	6.3	5.7	5.6	5.6	6.0	6.8	7.7	6.5
White females, 16 years and over													
1954	6.0	6.0	5.9	6.0	5.9	6.3	6.0	5.5	5.7	4.5	4.9	3.9	5.5
1955	5.3	4.6	4.4	4.1	4.3	5.0	4.3	4.0	4.4	3.9	4.1	3.7	4.3
1956	4.3	4.1	4.4	4.1	4.5	5.2	4.3	4.1	3.9	3.4	4.2	3.7	4.2
1957	4.7	4.3	4.0	3.7	4.4	5.1	4.5	4.1	4.1	3.8	4.7	4.0	4.3
1958	5.8	6.2	5.8	6.5	6.8	7.7	7.2	6.6	6.2	5.5	5.5	4.8	6.2
1959	5.8	5.8	5.5	5.3	5.1	6.1	5.6	5.0	5.1	5.0	5.3	4.4	5.3
1960	5.4	5.0	5.2	4.7	4.9	6.3	5.7	5.4	5.1	5.4	5.5	5.4	5.3
1961	6.4	6.8	6.8	6.4	6.3	8.2	7.5	6.4	6.6	6.1	5.8	4.8	6.5
1962	5.0	5.5	5.4	5.0	5.1	6.2	5.7	5.7	5.8	5.2	5.4	4.5	5.5
1963	5.8	5.7	5.3	5.3	5.8	7.2	6.3	5.7	5.9	5.5	5.7	4.8	5.8
1964	6.1	6.1	5.7	5.5	5.3	6.9	5.4	5.3	5.3	5.2	5.2	4.6	5.5
1965	5.3	5.8	5.0	5.1	4.8	6.5	4.9	4.9	4.8	4.3	4.8	3.7	5.0
1966	4.4	4.2	4.0	4.2	4.6	5.9	4.6	4.2	4.1	4.1	3.9	3.3	4.3
1967	4.7	4.6	4.2	3.9	3.8	5.6	5.0	4.8	5.2	5.0	4.4	3.6	4.6
1968	4.3	4.6	4.0	3.8	3.6	5.5	4.9	4.6	4.5	4.0	4.0	3.3	4.3
1969	4.0	4.3	3.8	3.8	3.6	5.2	4.5	4.6	5.0	4.5	4.0	3.4	4.2
1970	4.4	4.8	4.9	4.5	4.6	6.0	5.7	5.8	6.4	5.9	6.2	5.4	5.4
1971	6.4	6.4	6.3	5.9	5.5	7.2	6.7	6.9	6.9	6.1	6.4	5.2	6.3
1972	6.0	5.9	5.8	5.7	5.4	6.9	6.7	6.6	6.5	5.9	5.2	4.7	5.9
1973	5.6	5.7	5.2	5.0	4.6	6.0	5.6	5.6	5.7	5.0	5.1	4.7	5.3
1974	5.8	5.9	5.6	5.0	5.1	6.7	6.4	6.3	7.1	6.2	6.9	6.6	6.1
1975	9.1	8.9	8.8	8.3	8.5	9.3	9.0	8.9	8.6	8.3	7.9	7.6	8.6
1976	8.6	8.3	7.8	7.2	6.7	8.2	8.2	8.6	8.4	7.8	7.6	7.0	7.9
1977	8.1	8.0	7.8	6.9	6.5	8.0	7.4	7.6	7.7	6.9	6.8	5.8	7.3
1978	6.9	6.4	6.0	5.7	5.8	6.7	6.8	6.6	6.7	5.7	5.9	5.5	6.2
1979	6.2	6.1	5.9	5.3	5.4	6.4	6.0	6.6	6.3	6.0	5.7	5.4	5.9
1980	6.3	6.3	5.8	5.8	6.3	7.1	7.1	7.3	6.8	6.7	6.5	6.0	6.5
1981	7.2	6.8	6.4	6.2	6.6	7.1	7.1	6.9	7.2	7.1	7.2	6.8	6.9

Union Density. State-year union density from 1947–2006 come from Farber *et al.* (2021a) and are derived from a combination of CPS and Gallup data.

Labor Market Tightness. We construct a measure of labor market tightness from 1952–1979 as the average of the standardized quit rates and negative unemployment rates (Autor *et al.*, 2023).

B.5. Military Variables

Active Duty Military. We compile national active duty counts for men in different age categories from 1948–2000 from the Department of Defense's Selected Manpower Statistics reports. To compute the active duty share for each of the age categories we use the Census Bureau's intercensal population estimates by age, sex, and race 1940–2000.

B.6. Public Support for War

Korean War Support. We measure public support for the Korean War using a series of questions asked in nationally representative public opinion polls. These polls were obtained from the Roper Center for Public Opinion Research's iPoll database. The list of questions used to measure support are listed in Table B.1.

Vietnam War Support. We measure public support for the Vietnam War using a series of questions asked in nationally representative public opinion polls. These polls were obtained from the Roper Center for Public Opinion Research's iPoll database. The list of questions used to measure support are listed in Table B.2.

Appendix Table B.1: Korea War Support Questions

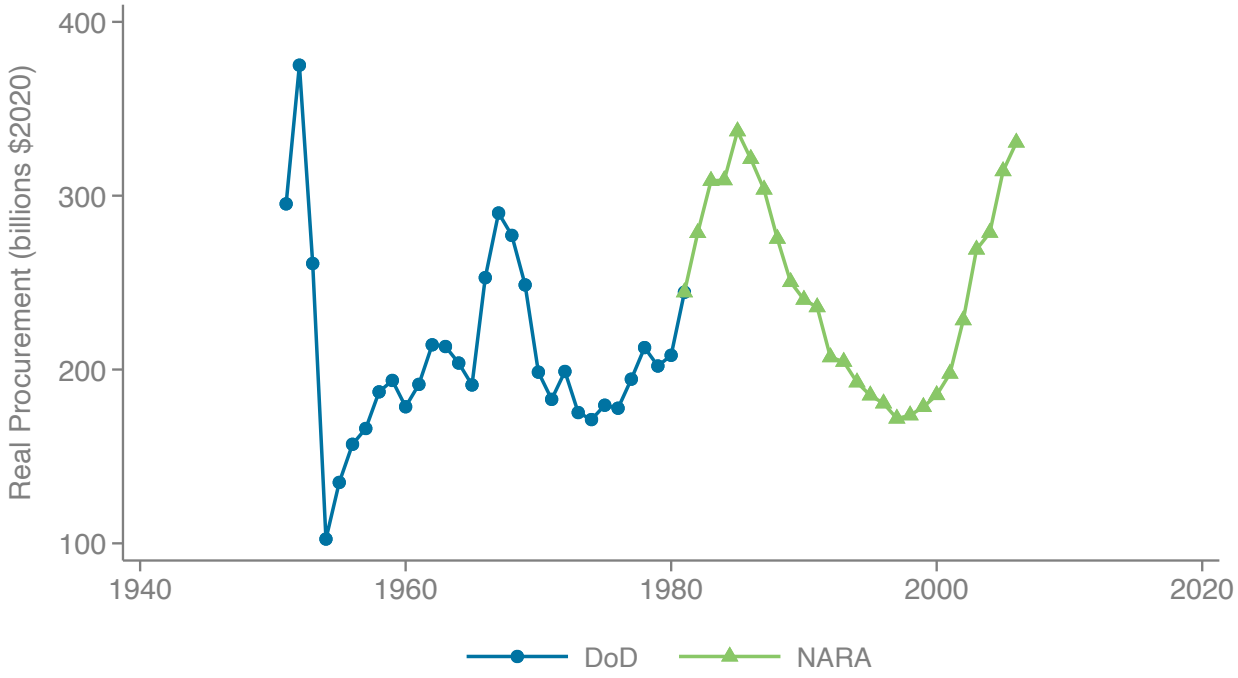
Year	N	Question
1950	1,147	Some people say that the United States should pull our troops out of Korea and stop fighting there. Other people say that we should go on fighting in Korea. With which point of view do you agree?
1950	1,330	Which of these two things do you think is more important: A, that this country keep out of a major war, or B, that Russian expansion in Asia and Europe be stopped?
1950	1,225	Some people say the United States should stop fighting and take her troops out of Korea to avoid a third world war. Other people say we should keep our troops there even if it does mean a world war. What do you, yourself, think–should we take our troops out of Korea, or not?
1950	1,129	In view of the development since we entered the fighting in Korea, do you think the United States made a mistake in deciding to defend Korea, or not?
1950	1,110	If the Chinese communists refuse to stop fighting in Korea, do you think the United Nations should or should not declare war on China?
1951	1,207	In view of developments since we entered the fighting in Korea, do you think the United States made a mistake in deciding to defend South Korea, or not?
1951	6,325	Do you think the United States made a mistake in going into war in Korea, or not?
1951	1,256	Do you think the United States made a mistake in going into the war in Korea, or not?
1952	1,764	Do you think U.S. (United States) troops should be withdrawn (pulled out) from Korea, or should they continue to fight there?
1952	4,192	Do you think the United States made a mistake in going into war in Korea, or not?
1953	1,333	Do you think the United States made a mistake in going into the war in Korea, or not?

Appendix Table B.2: Vietnam War Support Questions

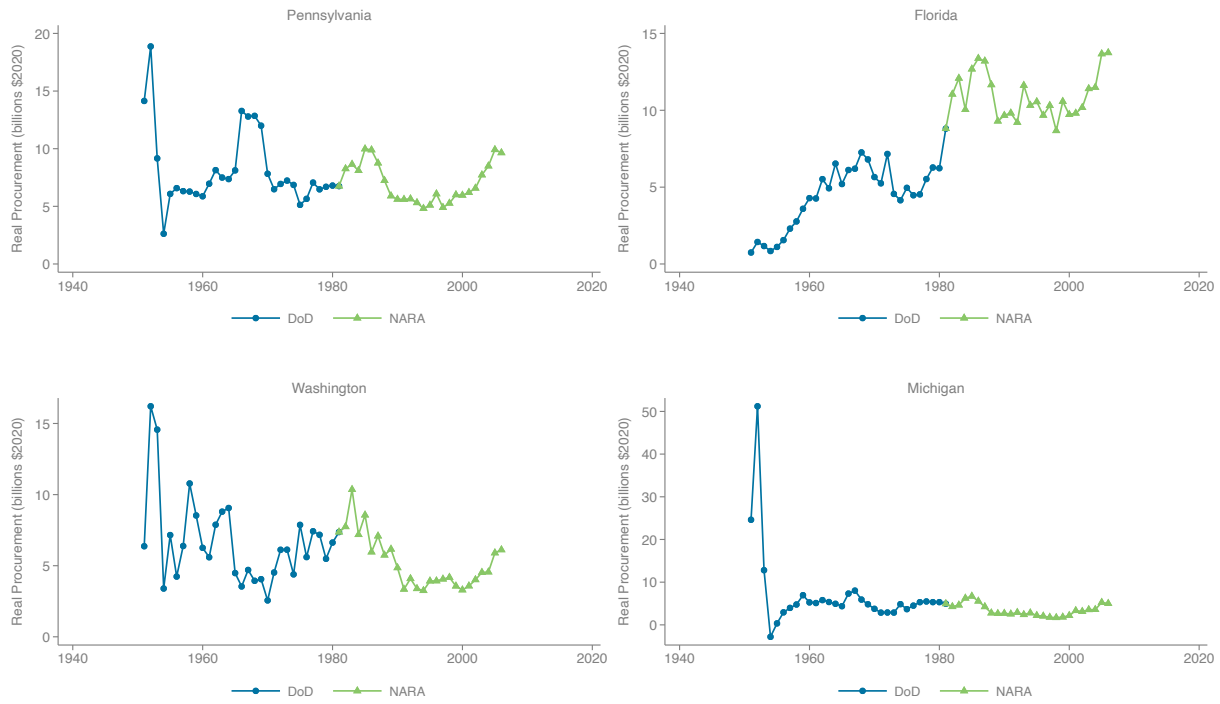
Year	N	Question
1964	1,899	What do you think the U.S. should do next in regard to Vietnam?
1964	2,620	What do you think should be done next in Vietnam?
1964	1,318	There would seem to be three basic courses the U.S. could follow in Vietnam. On balance, which one of these courses would you favor?
1965	2,551	Should the U.S. continue its present efforts in Vietnam or should we pull our forces out?
1965	2,976	In view of the developments since we entered the fithing in Vietnam, do you think the U.S. made a mistake sending troops to fight in Vietnam?
1965	2,560	In your opinion, what would you like to see the U.S. do next about Vietnam?
1965	3,536	In your opinion what would you like to see the U.S. do next about Vietnam?
1966	2,885	In view of the developments since we entered the fighting in Vietnam, do you think the United States made a mistake sending troops to fight in Vietnam?
1966	2,526	Are you more inclined to agree with the 'hawks' or the 'doves' (on the issue of fighting in Vietnam)?
1966	3,081	Would you favor or oppose bombing big cities in North Vietnam?
1966	5,966	In view of the developments since we entered the fighting in Vietnam, do you think the U.S. made a mistake sending troops to fight in Vietnam?
1966	935	Do you think we did the right thing in getting into the fighting in Vietnam or should we have stayed out?
1966	3,122	Just from what you have heard or read, which of these statements comes closest to the way you, yourself, feel about the war in Vietnam?
1966	3,062	Here is a question about U.S. policy in Vietnam. Which one of these policies do you most prefer?
1967	3,134	Just from what you have heard or read, which of these statements comes closest to the way you, yourself, feel about the war in Vietnam?
1967	1,357	Turning to Vietnam...people are called hawks if they want to step up our military effort in Vietnam. People are called doves if they want to reduce our military effort in Vietnam. How would you describe yourself – as a hawk or as a dove?
1968	1,276	Do you think we did the right thing in getting into the fighting in Vietnam or should we have stayed out?
1969	2,479	Would you please read carefully all the statements on this card. Which one of the statements comes closest to your feelings about the war in Vietnam?
1969	1,551	Turning to Vietnam...people are called hawks if they want to step up our military effort in Vietnam. People are called doves if they want to reduce our military effort in Vietnam. How would you describe yourself – as a hawk or as a dove?
1970	1,199	Do you think we did the right thing in getting into the fighting in Vietnam or should we have stayed out?

B.7. Figures

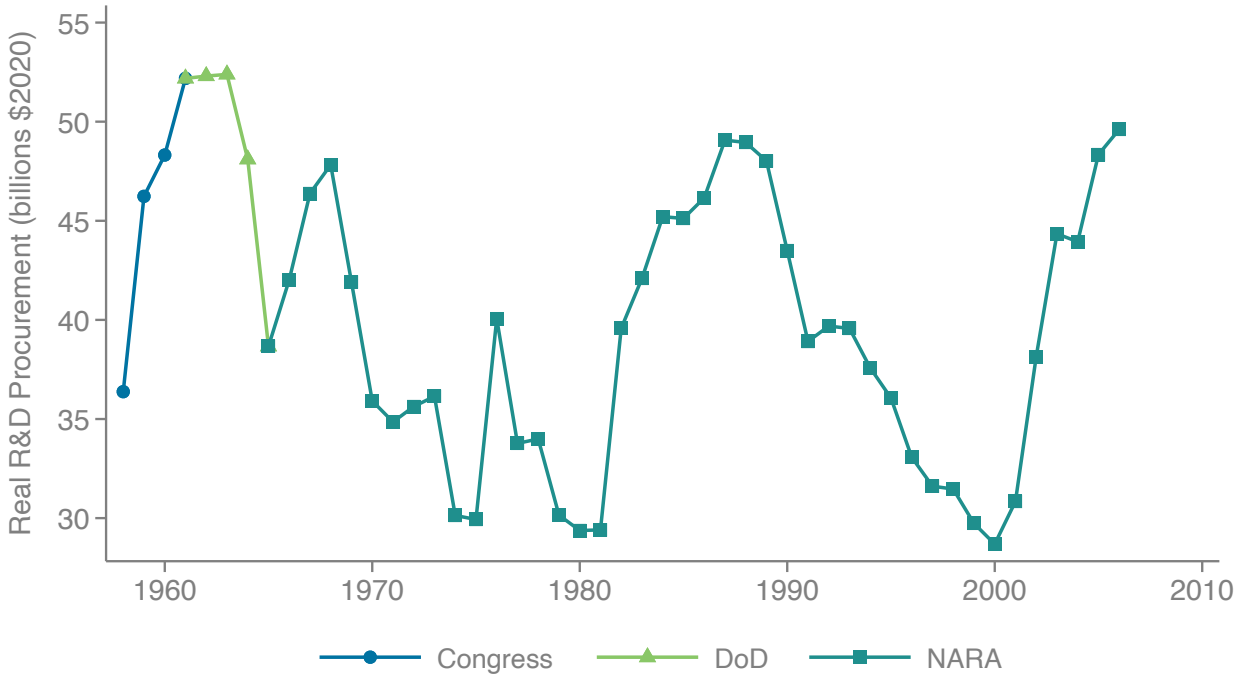
Appendix Figure B.6: Total Procurement



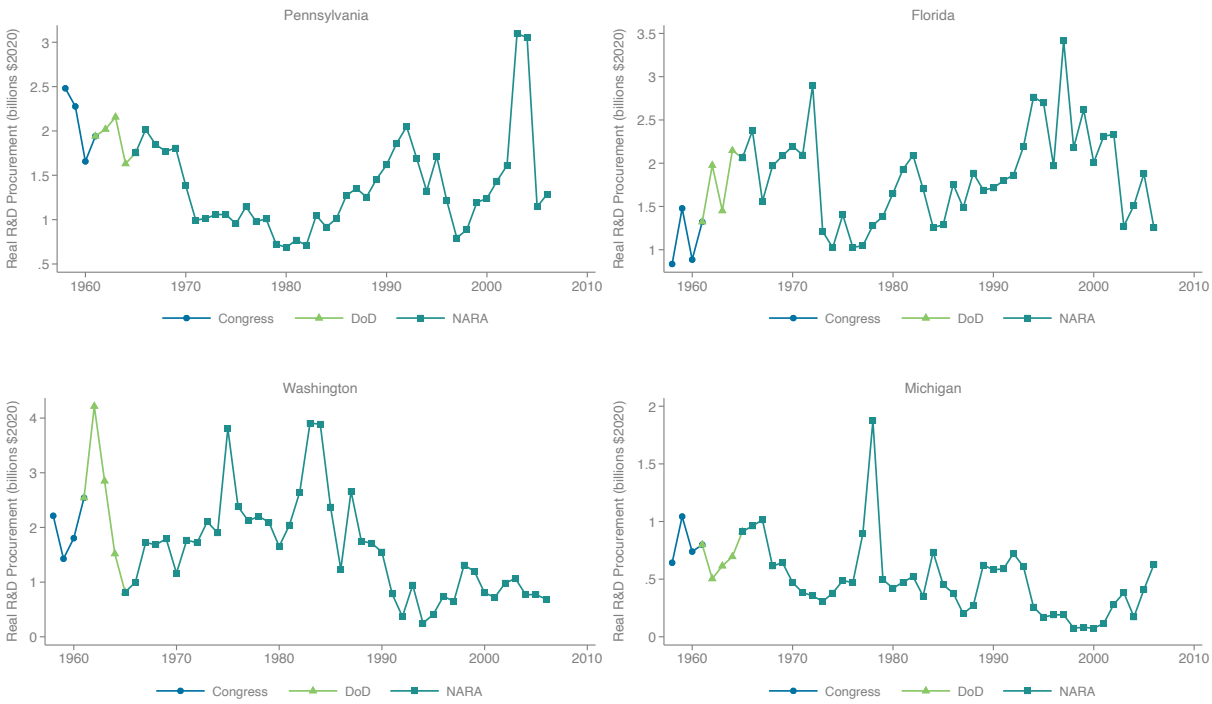
Appendix Figure B.7: Total Procurement by State



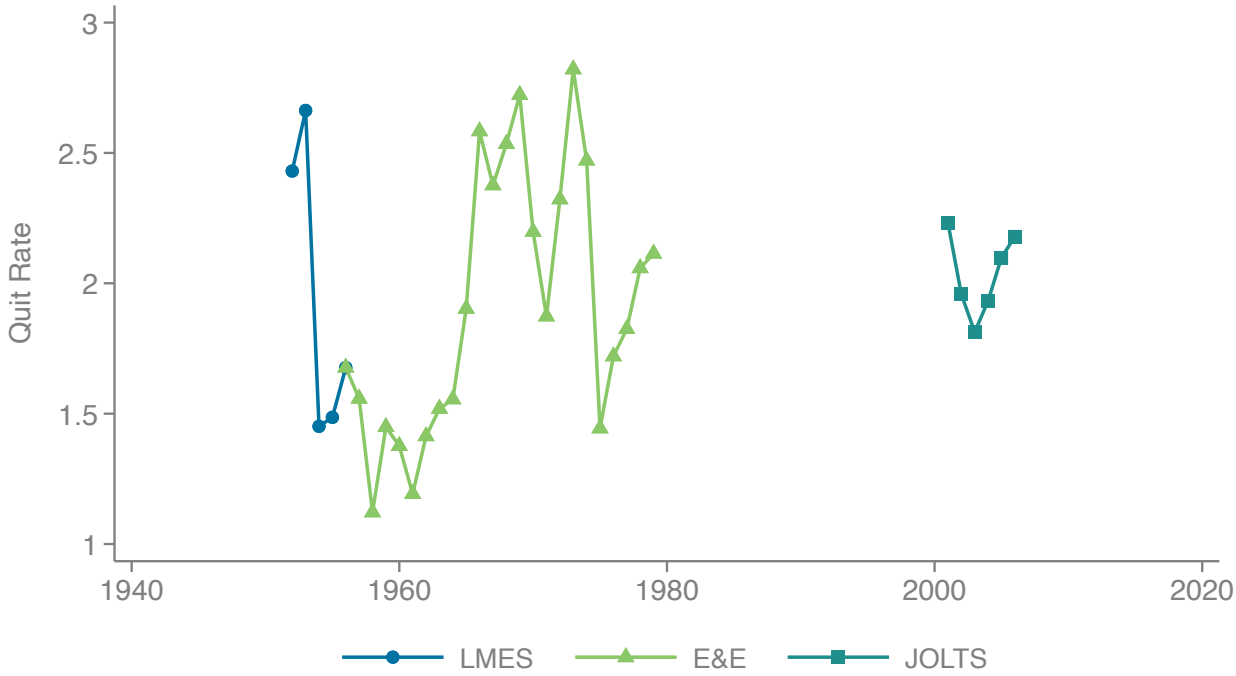
Appendix Figure B.8: Total R&D Procurement



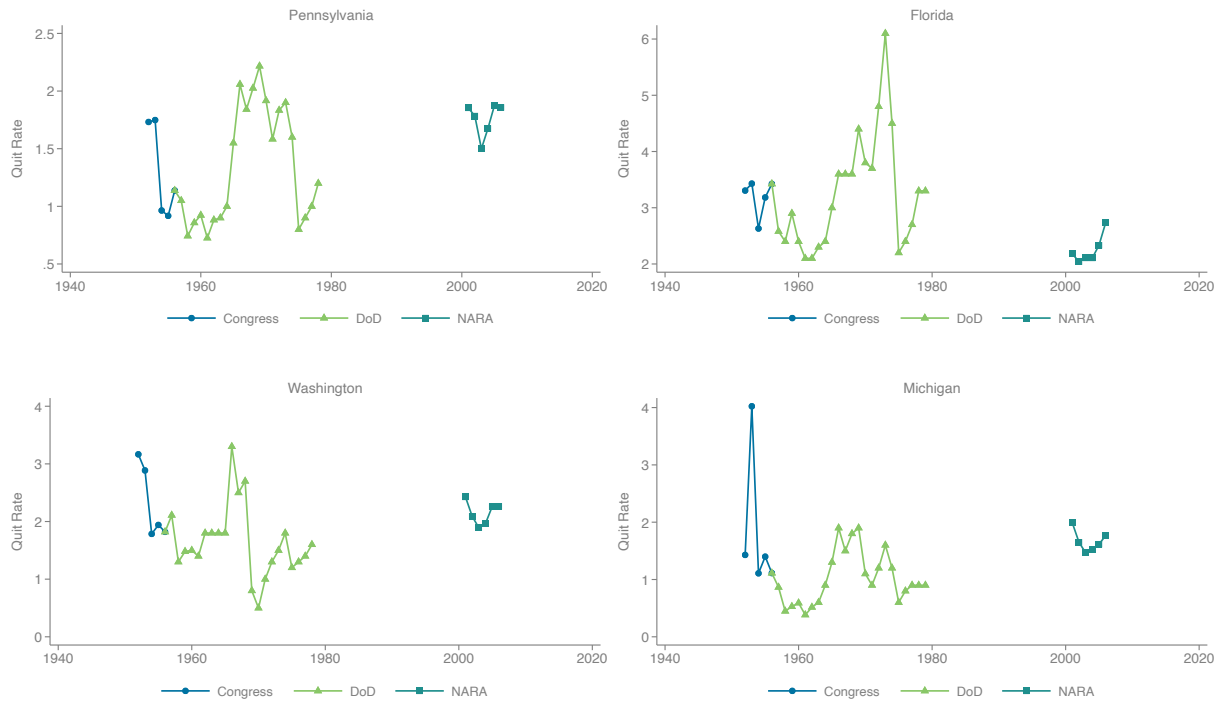
Appendix Figure B.9: Total R&D Procurement by State



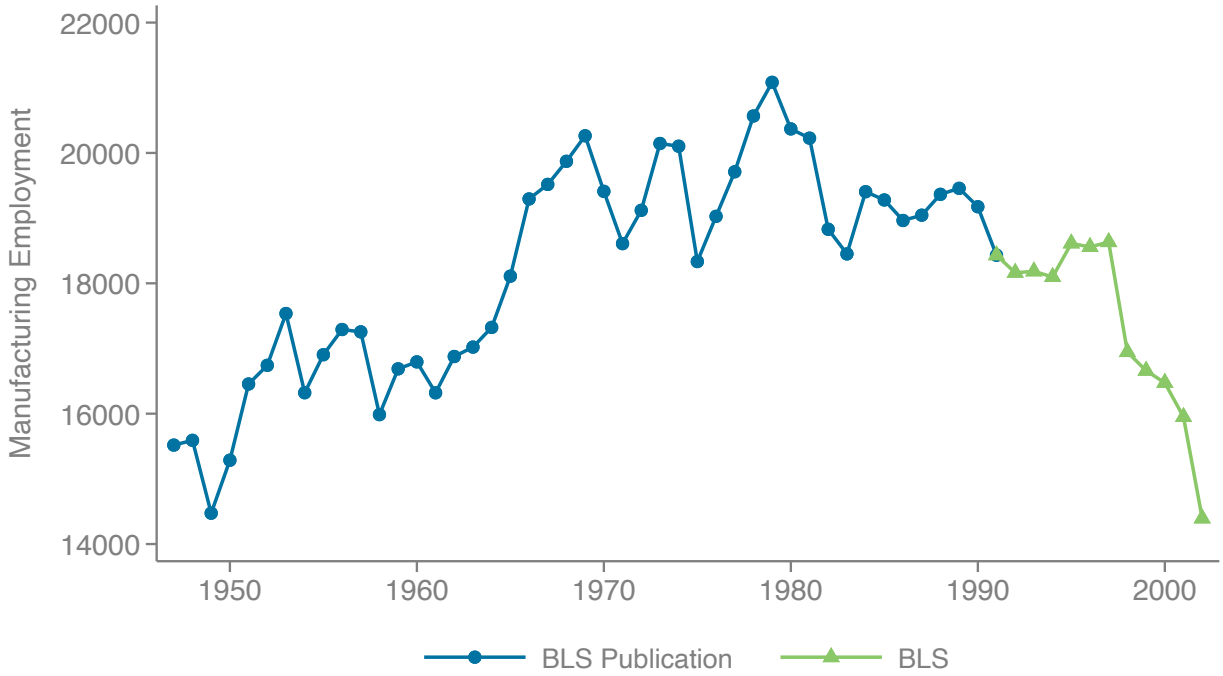
Appendix Figure B.10: Quit Rate



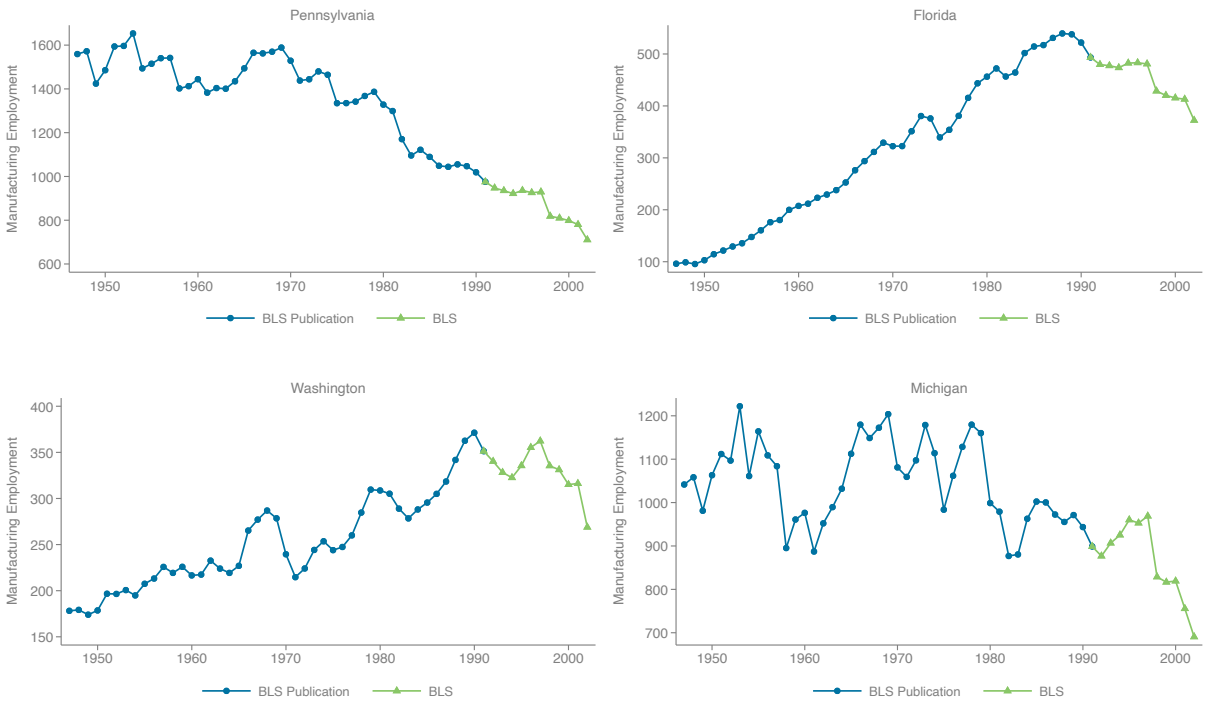
Appendix Figure B.11: Quit Rate by State



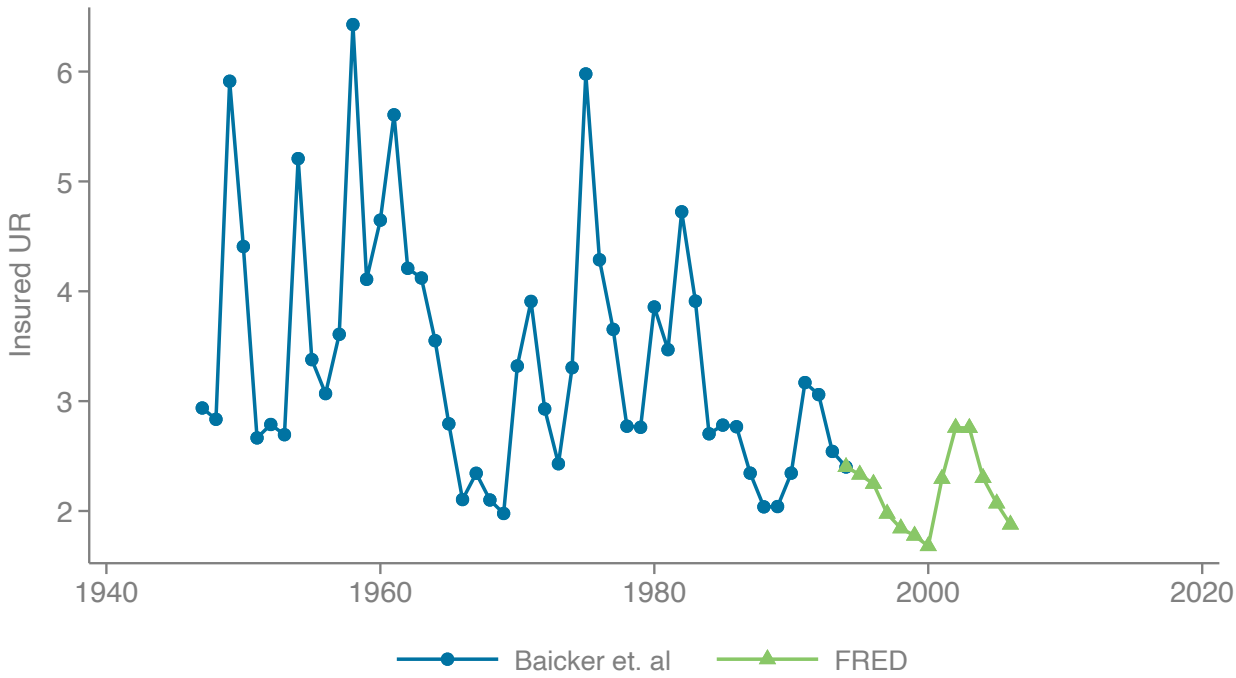
Appendix Figure B.12: Manufacturing Employment



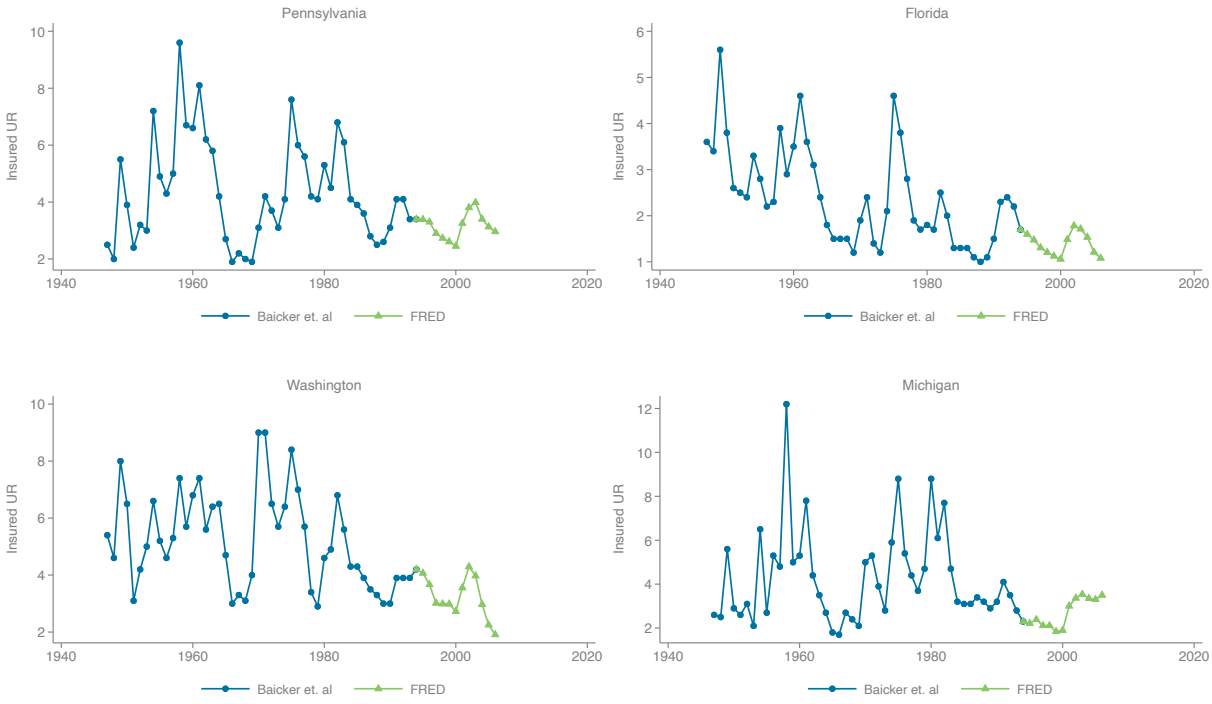
Appendix Figure B.13: Manufacturing Employment by State



Appendix Figure B.14: Insured Unemployment Rate



Appendix Figure B.15: Insured Unemployment Rate by State



Appendix C. "Share Filed" and the Bottom of the Earnings Distribution

In this section we discuss why we believe that the share of adults filing a tax return in a state tracks movements in the bottom of the earnings distribution. In Table C.1, we display the tax filing thresholds and the minimum tax brackets for each decade from 1950 to 1990. The filing threshold, which determines whether a tax unit must file a tax return with the IRS, is generally around the 10th–20th percentile of the family income distribution in the Census. Therefore, changes in the share of adults filing a tax return in a state should be related to the share of tax units earning above or below the 10th–20th percentile of the national family income distribution. This should be especially true between 1950–1970 when there were fewer incentives to file a tax return when you earn less than the filing threshold (e.g. to claim EITC).

We test this relationship empirically in Figure C.1–Figure C.5 where we plot the share of families earning above the filing threshold in a state against the share of adults filing a tax return in that state from 1950–1990.[29] The share of adults filing explains a substantial amount of the variation in share of families earning above the filing threshold or the minimum tax bracket between 1950–1970 ($R^2 = 0.62 - 0.80$). The relationship is weaker between 1980–1990 ($R^2 = 0.05 - 0.46$) and for the share of families earning above the 90th percentile of the national family income distribution ($R^2 = 0.07 - 0.68$).

Finally, we show that the share of adults filing is strongly correlated with state-level inequality in the bottom half of the distribution between 1950–1970 in Figure C.1 – Figure C.5. The share filing measure explains a substantial share of the variation in state-level 90–10 inequality between 1950–1970 ($R^2 = 0.22 - 0.58$) and this is entirely driven by 10–50 inequality ($R^2 = 0.30 - 0.58$) as compared to 90–50 inequality ($R^2 = 0.04 - 0.39$). There is no relationship between share filing and inequality between 1980–1990 consistent with what we found earlier and the greater incentives to file a tax return for those earning below the filing threshold.

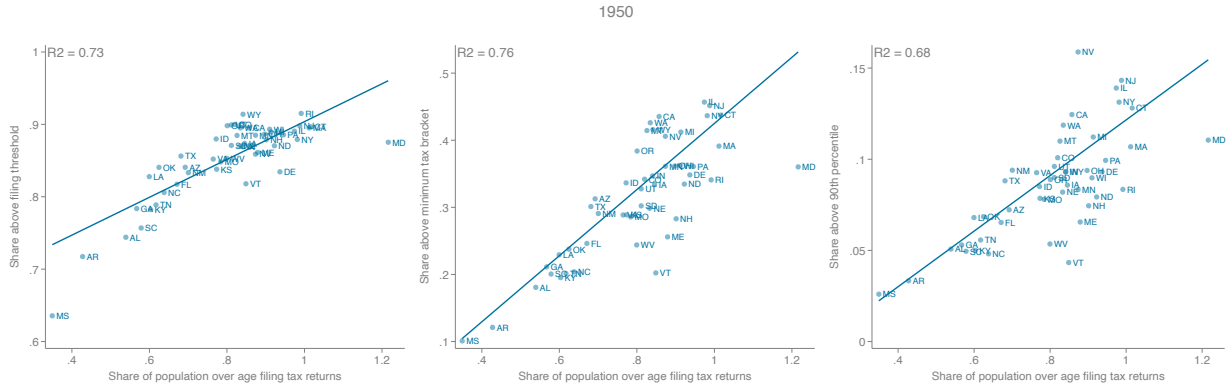
[29] Each family in the Census is assigned a tax unit type of single, head of household, married (jointly), and widowed based on the family composition and assigned a corresponding filing threshold. Share of adults filing is the number of tax returns filed in the state divided by the adult population in the state from the World Inequality Database.

Appendix Table C.1: Filing Thresholds and Minimum Tax Brackets

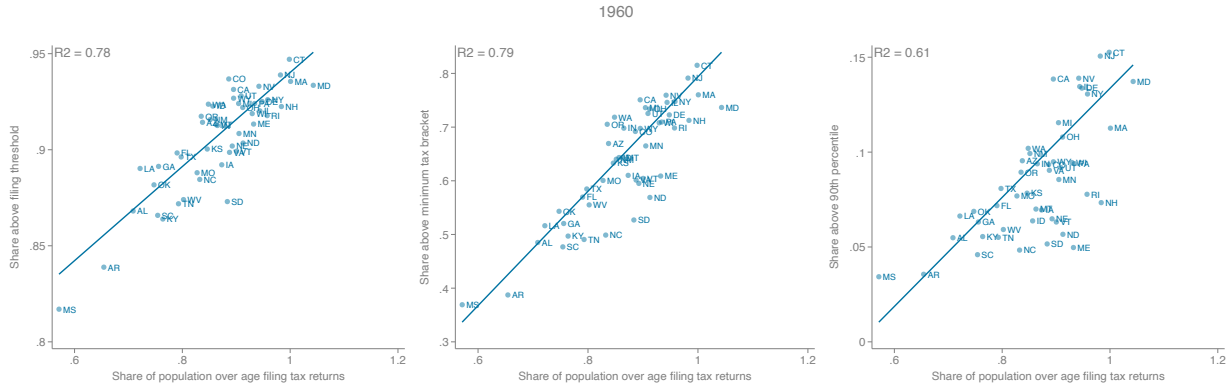
	Single Filer		Married Filing Jointly		Married Filing Separate	
	Under 65	Over 65	Under 65	Over 65	Under 65	Over 65
				1950		
Filing threshold	600	600	600	600	600	600
	[14]	[14]	[14]	[14]	[14]	[14]
Min. tax bracket	2,000	2,000	4,000	4,000	2,000	2,000
	[35]	[35]	[72]	[72]	[35]	[35]
				1960		
Filing threshold	600	1,200	600	1,200	600	1,200
	[6]	[13]	[6]	[13]	[6]	[13]
Min. tax bracket	2,000	2,000	4,000	4,000	2,000	2,000
	[20]	[20]	[39]	[39]	[20]	[20]
				1970		
Filing threshold	1,700	2,300	2,300	3,500	600	600
	[11]	[15]	[15]	[23]	[4]	[4]
Min. tax bracket	500	500	1,000	1,000	500	500
	[3]	[3]	[6]	[6]	[3]	[3]
				1980		
Filing threshold	3,300	4,300	5,400	7,400	1,000	1,000
	[9]	[13]	[17]	[24]	[3]	[3]
Min. tax bracket	2,300	2,300	3,400	3,400	1,700	1,700
	[5]	[5]	[10]	[10]	[4]	[4]
				1990		
Filing threshold	5,300	6,100	9,550	10,850	2,050	2,050
	[8]	[10]	[17]	[20]	[3]	[3]
Min. tax bracket	19,450	19,450	32,450	32,450	16,225	16,225
	[36]	[36]	[58]	[58]	[30]	[30]

Notes: This table displays filing thresholds and minimum tax brackets from 1950–1990 for three common tax filing groups (head of household and widowed are omitted). Percentiles are in brackets underneath each value based on family incomes reported in the Census. The filing threshold for married filing jointly depends on whether each spouse is older than 65, for simplicity we show the value when both are under 65 and both are over 65.

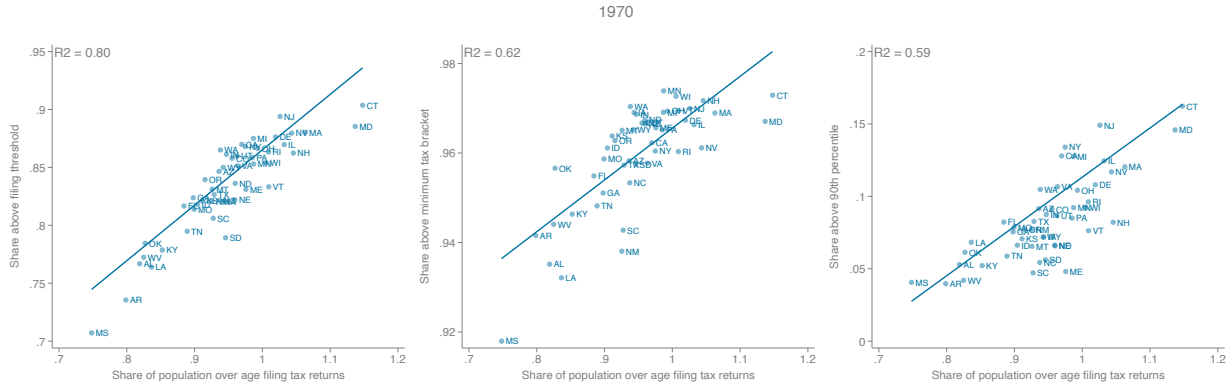
Appendix Figure C.1: Correlation between Share Filing Returns and Tax Thresholds 1950



Appendix Figure C.2: Correlation between Share Filing Returns and Tax Thresholds 1960

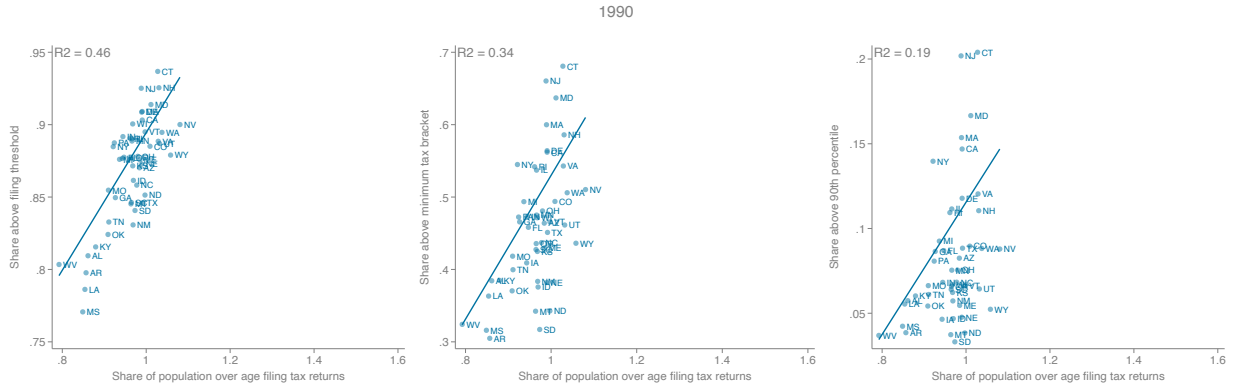


Appendix Figure C.3: Correlation between Share Filing Returns and Tax Thresholds 1970



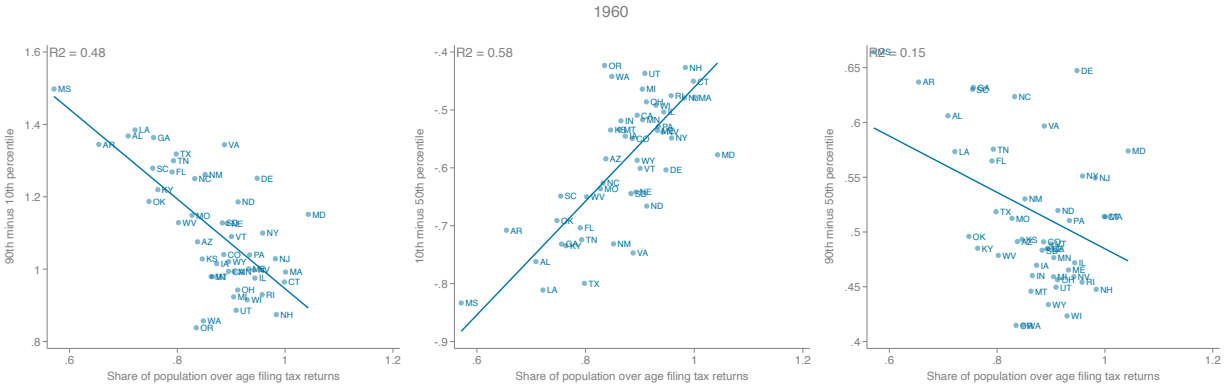
Appendix Figure C.4: Correlation between Share Filing Returns and Tax Thresholds 1980

Appendix Figure C.5: Correlation between Share Filing Returns and Tax Thresholds 1990

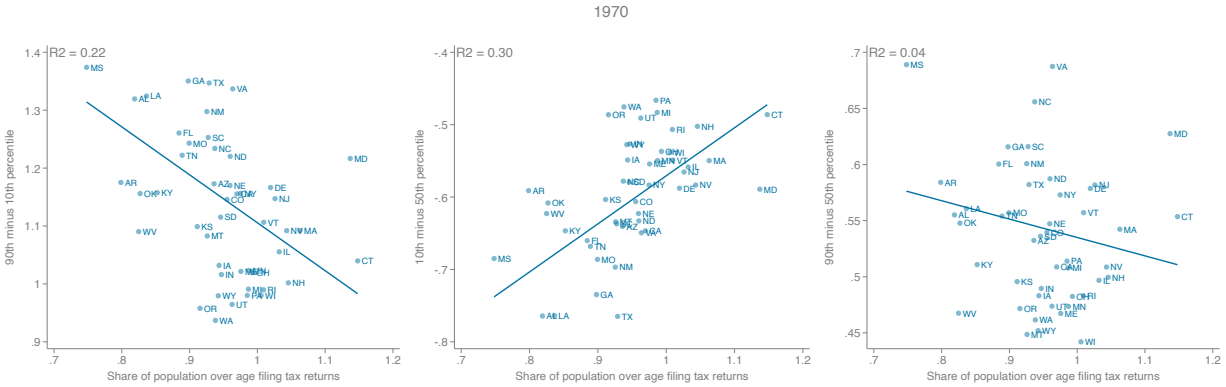


Appendix Figure C.6: Correlation between Share Filing Returns and Inequality 1950

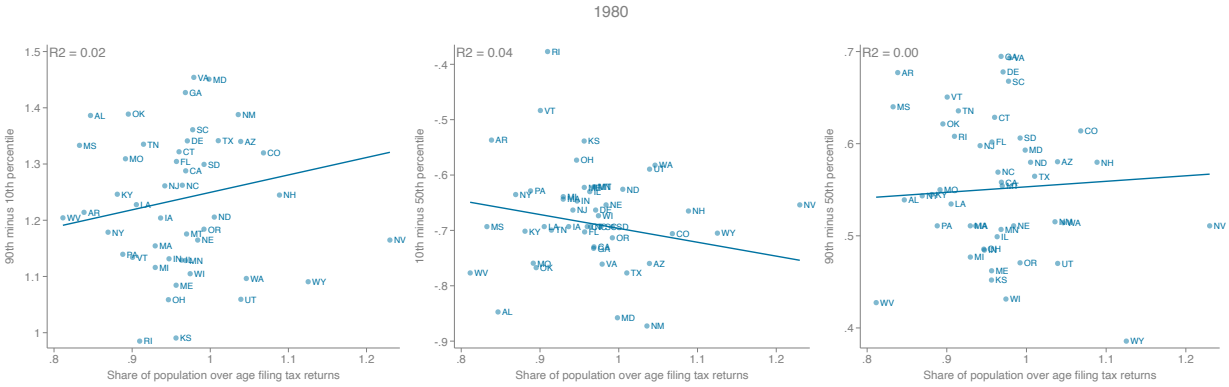
Appendix Figure C.7: Correlation between Share Filing Returns and Inequality 1960



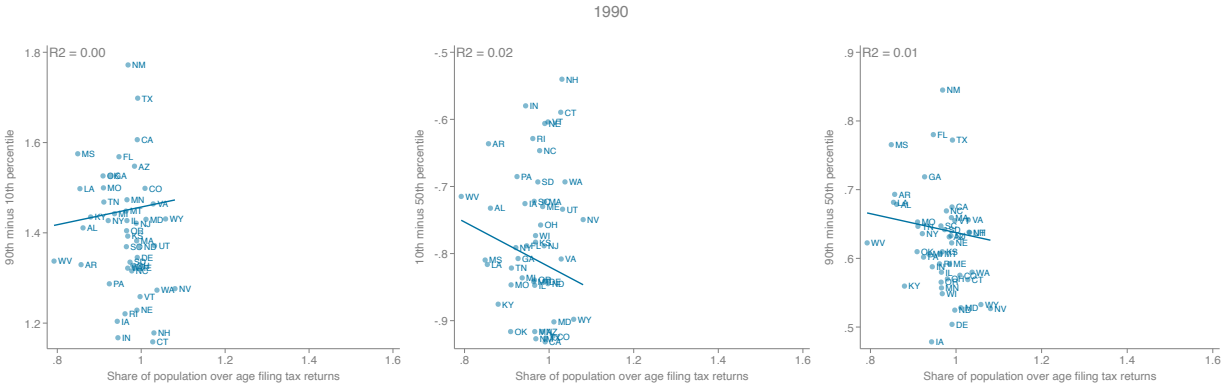
Appendix Figure C.8: Correlation between Share Filing Returns and Inequality 1970



Appendix Figure C.9: Correlation between Share Filing Returns and Inequality 1980



Appendix Figure C.10: Correlation between Share Filing Returns and Inequality 1990



Appendix D. Firm Analysis Appendix

D.1. Top 100 Data

We collect data from Congressional Reports that compile lists of the Top X (usually 100) companies recieving prime contract awards. These lists contain information on the firms rank based on prime contracts received as well as the dollar amount of contracts awarded. These lists are used to construct our treatment variables defined below. The lists cover various periods and are as follows:

1. Top 100 firms from July 1950 to June 1953
2. Top 100 firms from July 1950 to June 1951 (first 11 months)
3. A subset of 28 firms from the Top 100 from July 1950 to June 1952
4. Top 100 firms from July 1950 to December 1954
5. Top 100 firms from July 1953 to December 1954
6. Top 100 firms from January 1955 to December 1955
7. Top 100 firms for each fiscal year from 1957 to 1961

We adjust periods covering more than year so that they approximate a fiscal year e.g. for (5) we divide by 12/18. Using these lists we construct implied procurement amounts and ranks for the following years:

- 1952: Difference between (3) and (2)
- 1953: Difference between (1) and (3)
- 1954: (5) appended by the difference between (4) and (1)

D.2. Construction of Main Variables

Treated and Control Firms. Treated firms are firms that ever appear in a Top 100 list during our period of interest (1951–1961). Control firms are firms that never appear in any Top 100 list during this period. There are 79 firms in our sample, 40 treated firms and 39 control firms.

Market Capitalization. We use firm-month level data on market capitalization from 1947–1961 from the Center for Research in Security Prices (CRSP). Market capitalization is defined as the share price times the number of shares outstanding in each month. Fiscal year level market capitalization is constructed as the average market capitalization over the months in the fiscal year. Fiscal year is defined as the fiscal year variable from Compustat which indicates the month in which the fiscal year ends for each firm. For firms that are missing fiscal year, we assume the fiscal year corresponds to the calendar year which is true for the majority of the firms in our sample.

Executive Compensation. We use executive compensation data from Frydman and Saks (2010) covering 1947–1961, which are collected from 10-K reports available at the Baker Library of the Harvard Business School. We compute annual average remuneration across all executives at the firm, which is defined as salary plus bonus pay. We assume that the annual compensation corresponds to the firms fiscal year and should correspond to our other data measured at the firm fiscal year.

Ratio. We construct a compensation ratio measure by dividing executive pay by market capitalization.

Employment and Revenue. We measure employment and revenue from 1950–1961 using a combination of data from Compustat as well as data collected from archival Annual Reports for the treated firms in our sample. We focus on measuring U.S. employment. To determine whether Compustat reports U.S. employment we use the annual reports. If Compustat provides a different employment series, we prioritize the U.S. employment reported in the Annual Reports. When Annual Reports are not available or do not specify the geographic scope of employment, we rely on Compsutat data without knowing the geographic scope (this is always the case for the control firms in our sample). For revenue, Annual Reports complement the Compustat data by filling in missing values.

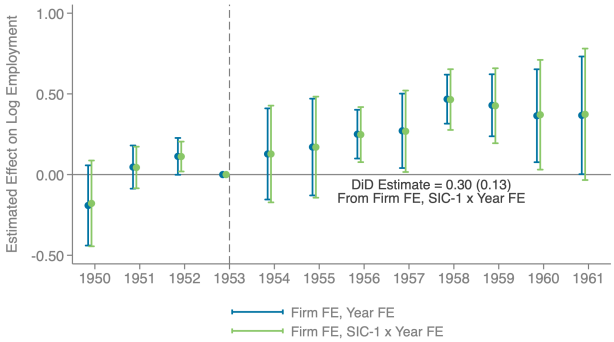
Procurement. We measure procurement for control and treated firms separately. For control firms (i.e. firms that did not appear in the Top 100 lists between 1951–1961) we estimate their procurement values using detailed NARA data from 1966. We measure each control firm's share of total US procurement in 1966 from the NARA data. We then apply this share to each year's total US procurement between 1951–1961 (excluding 1956 which is excluded because we have no Top 100 list in that year). For the small number of firms that cannot be linked to the NARA data we assign them the procurement share of the 500th ranked firm in the 1966 data. For treated firms we observe procurement values in the Top 100 lists. We do not impute procurement values for treated firms in years where we do not observe them in the Top 100 lists. This explains the differences in sample sizes between rank-based (always observed for all firms) and procurement based regressions.

Top Rank Indicators. We create indicator variables corresponding to whether or not a firm belongs to the Top 10 firms receiving procurement for each year between 1951 and 1961, excluding 1956 which has no Top 100 list). Control firms (firms that never appear in a Top 100 list) are always assigned a value of zero. Treated firms are assigned a value of one for each year that they appear in the Top 100 and a value of zero otherwise. We only observe the exact Top 100 ranking between 1951–1953 in 1951. We impute the indicator values between 1952 and 1953 as follows:

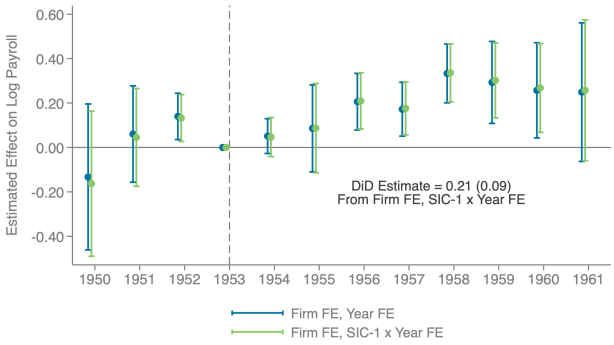
- If the firms rank for 1951, 1951-1952, and 1951-1953 are all outside the Top 10, we assume they were outside the Top 10 for both 1952 and 1953.
- If the firms rank for 1951, 1951-1952, and 1951-1953 indicate the firm was ranked in the Top 10 for all these years, we assume they remained in the Top 10 for both 1952 and 1953.

Appendix Figure D.1: Effects of firm-level missile shift on firm-level employment and payroll

(a) Employment



(b) Payroll



Both panels show coefficients from equation 3, where the outcome in Panel (a) is log employment, from Compustat, and the outcome in Panel (b) is log payroll, collected from the subsample for which we could find payroll in the annual reports. Samples are limited for firms for which we have at least 10 observations in the sample window.

- If the ranks for 1951 and 1951-1952 both show the firm in the Top 10, we assume they were in the top 10 in 1952. If the firm enters the top 10 between 1951-1952 but is not in the Top 10 in 1951, we assume they were in the Top 10 in 1952. Likewise, if there is a decline (in top 10 in 1951 but not between 1951-1952) we assume they were not in the top 10 in 1952.
- We follow a similar logic for 1953 using the 1951-1952 ranks and the 1951-1953 ranks.

This procedure is also used for creating Top 20 indicators.