National Service and the Great Leveling:
New evidence on midcentury American inequality

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Abstract

This paper extends US distributional national accounts, and explains midcentury wage compression with a natural experiment. New estimates from archival tax and survey data show that the relative gains of the New Deal era were more durable for the upper middle class than for the poorest — except in wages, where bottom-50% compression was sharp and lasting. To explain this, I link the 1940 and 1950 full-count Censuses to WWII enlistment records and exploit a discontinuity in draft eligibility: fathers before Pearl Harbor were largely exempt from service. The causal wage premium from service was strongest for working-class men.

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1 Introduction

This investigation re-examines long-run trends in income distribution in the 20th century United States by returning to new and previously unused archival data with a new method for statistical inference, and I explain these new trends with a natural experiment that captures the World War II veteran wage premium.

After Piketty and Saez (2003) tracked US income inequality over the 20th century with tax and survey data, Piketty, Saez, and Zucman (2018) harmonized long-run macroeconomic data with similar data, to provide an estimate of the full national income distribution, including all sources of income in the national accounts. These and related studies show that income and wealth inequality has risen to a level not seen since the early 20th century.

This paper extends the detail of these estimates to the earlier era, and attempts to explain the results. Where the existing distributional national accounts begin in 1962 (for the *full* income distribution and not only the top 10 percent), this paper provides estimates back to 1913.[1] Where several studies have attempted to explain the midcentury decline in income inequality, this paper contributes causal evidence on a new explanation: World War II service itself contributed to productivity gains for middle-class wage earners.

Among complementary hypotheses on trade, technology and education,[2] we can test one further hypothesis: World War II service itself helps explain wage growth, and particularly the upward economic mobility of the working class. For those who served, there was a significant wage premium, one which can be interpreted causally by way of a natural experiment on the probability of being drafted.

[1] While Piketty et al. (2018) had estimated the full percentile distribution of US income shares since 1962, this had not yet been done for the years prior to 1962. Nor had this been done for the income from wages and salaries, which is the most significant source of total fiscal income, and especially for middle-class households. From there, I scale up the inference from fiscal income to pre-tax distributional national accounts. For a more thorough discussion of the income concepts used in this paper, please refer to Blanchet, Chancel, Flores, and Morgan (2021).

[2] Several explanations for postwar wage compression have been explored in previous studies, including perhaps most notably a 'race between education and technology' (in the eponymous monograph of Goldin & Katz, 2008), in which skills growth matched technology and jobs during much of the postwar era before lagging since; and a progressive tax code during the postwar era which has flattened since the 1980s (Piketty, 2014). Lindert and Williamson (2016) pointed to demographic factors (a slowdown in labor supply growth); trade policy (a relatively closed domestic economy and lack of international competition); financial sector retrenchment (due to tight regulation); technological change (favoring unskilled workers); and returns to education and skill (with unparalleled increases in human capital attainment). Most recently, Artola Blanco and Gómez-Blanco (2024) confirmed the compression in the postwar wage distribution, but also found a composition effect as salaried workers turned to self-employed.

Exploiting a discontinuity in the probability of being drafted[3]—fathers before 7 December 1941 were largely exempted from the draft—I show that the birthdate of a man's eldest child predicts his service in WWII, and consequently predicts his wages in 1950.

World War II veterans experienced a significant wage premium in the postwar economy, compared to peers who did not serve—and so did the veterans whose first child was born just after Pearl Harbor, compared to the counterfactual (pre-Pearl Harbor) fathers who did not serve. We can interpret this as a causal effect of WWII service on wages. The effect was greatest for those who served in their early twenties; for those who served longer; and for those who came from working-class backgrounds.

2 Data and Method

Before this study, the paucity of pre-1962 data had inhibited analysis of early 20th century changes across the whole of the income distribution. However, as total income (national accounts) data was not missing for these years, and since top decile income tax data was not missing, it was possible to estimate the 'top 10' vs. 'bottom 90' percent split in the income distribution.

Using the generalized Pareto interpolation method from Blanchet, Fournier, and Piketty (2022), I harmonize long-forgotten survey data (Goldsmith, 1951) with administrative tax tabulations (e.g., SOI, 1950), to extend estimates of the lower 90% income distribution to the World War II period and before. That is, I complement the Piketty and Saez (2003) and Piketty et al. (2018) benchmark series on top shares with data even for the working class. This harmonized series, with the Goldsmith series integrating to SOI tax data, creates a unified long-run series on the United States income distributions.

Of course, the advantage of this new historical dataset goes beyond a replication study with more detailed data. As discussed in section 3 below, the new income distribution estimates show the postwar wage compression—which I explain with a new investigation of the WWII veteran wage premium.

[3] Angrist and Krueger (1994) follow a similar IV intuition to argue that the veteran wage premium was illusory, strictly a question of selection into service. However, their instrumental variable (to predict the probability of being drafted without otherwise predicting wages) is the soldier's quarter of birth. As such, its identifying variation occurs only among those who turned 18 years old between 1944 and 1946, as the war (and the probability of being drafted at one's 18th birthday) declined. [Note that Barrera, Ferrara, Fishback, and Heggeness (2025) also rely on this intuition.] As I will try to demonstrate, the LATE on teenagers and on those who enlisted very late in the war (or after it ended) misses the treatment effect on those for whom service mattered more. Older soldiers served for longer, and their service may have occurred at a more propitious age.

To understand the egalitarianism of the postwar era, and in particular the wage compression that was among its defining characteristics, I investigate an old hypothesis with new evidence.

I re-open this inquiry, with new data and a new instrument, and test the hypothesis of a causal effect of WWII service on postwar wages.

The new data is the newly available 1950 complete-count decennial United States Census, which I link to the 1940 version of the same (by way of the Integrated Public Use Microdata Series, IPUMS; Ruggles et al., 2024).[4] In fact, the IPUMS 1940 data is also linkable to the WWII Army Enlistment Dataset from the innovative CenSoc project (Goldstein et al., 2023). I link this dataset of army veterans and the circumstances of their enlistment, to the linked 1940 and 1950 full-count Census wage distributions from IPUMS.[5]

The new instrument exploits a discontinuity in draft probability around a 'fatherhood deadline' of 7 December 1941. This setup relies on a similar intuition to that of Angrist and Krueger (1994) and Barrera et al. (2025), as a natural experiment with an exogenous predictor for the probability of service, where the instrument for service would only affect later earnings by way of this channel. However, whereas their instrument examined teenagers whose 18th birthday occurred as the probability of being drafted decreased from 1944 through 1946, this instrument is valid for all men eligible to be drafted during the war, although I prefer to restrict the sample to a narrower event window of fatherhood before and after Pearl Harbor. Arguably, men who become fathers may have different earnings potential from men who do not. And men who became fathers later in life (or closer to 1950) may differ in unobserved ways from men who became fathers earlier in life (or closer to 1935). However, men whose eldest child was born just after Pearl Harbor are unlikely to differ significantly from men whose eldest child was born just before Pearl Harbor. Their most salient difference would be in the probability of being drafted.

The original Selective Training and Service Act of September 1940, authorizing the draft via the Selective Service System, provided for the 'dependency deferment' (Class III-A) of all registrants with dependent children. As the army expanded, amendments tightened the criteria for deferment (Selective Service System, 1947). In particular, after the attack on Pearl Harbor of 7 December 1941 (and declaration of war the next day), the February 1942 edition of the Selective Service System regulations stated that "no registrant shall be deferred

[4]The 1950 full-count Census became publicly available in January 2024 (see link).

[5]Unfortunately, these wage distributions are top-coded at $5,000 and $10,000 in 1940 and 1950 current USD, respectively. (And they only cover wages, with limited coverage of any other income or wealth concepts, with many missing values that we assume are missing at random.) As such, even the full-count Census data provides limited information about the top of the wage distribution (and less or no information about other incomes). However, I assume that the relative rankings along the wage distribution are unbiased, and that the orders of magnitude on wage amounts are reliable, as well.

by placing him in Class III-A... if he acquired such status on or after December 8, 1941" (Selective Service System, 1947).

An April 1943 memorandum to local draft boards clarified "the order in which men 18 through 37 years of age would be called for induction" (i.e., the draft order):

> (1) single men with no dependents,
> (2) single men with collateral dependents [i.e., dependents other than children],
> (3) married men with wives only, and
> (4) men with children. (Selective Service System, 1946)

The memorandum also quoted the Code of Federal Regulations that defined dependency deferral (i.e., CFR Title 32, Chapter V, Section 622):

> 622.31. Class III-A: Man with child or children deferred by reason of maintaining bona fide family relationship... In Class III-A shall be retained or placed any registrant who with his child or children maintains a bona fide family relationship in their home, provided such status was acquired prior to December 8, 1941. (Selective Service System, 1946)

Subsequently, Public Law 197 of December 1943 "prohibited the induction of fathers until all nonfathers in the country had been processed for induction," where:

> The term "father" shall include only a person who was married prior to December 8, 1941, who has maintained a bona fide family relationship with his family since that date, and who has a child as defined in this section. ...
>
> The term "child" as used in these regulations means a legitimate child born prior to September 15, 1942, a stepchild, adopted child, foster child, or a person who is in the relationship of child to the registrant, who became such prior to December 8, 1941, who is less than 18 years of age, or who by reason of mental or physical defects is incapable of self-support, who is unmarried, and with whom the registrant has maintained a bona fide family relationship in their home since December 7, 1941. (Selective Service System, 1947)

Note that these later criteria for deferment eligibility actually required fathers to have been married prior to 8 December 1941, and allowed for 'pre-Pearl Harbor' children to be born up to 15 September 1942 (i.e., with conception prior to Pearl Harbor). In practice, we do not observe in the 1950 Census whether men were married before Pearl Harbor, only their marital status in 1950.[6] And that there is some ambiguity in whether the child for dependency deferment eligibility was born prior to 7 December 1941 or prior to 15 September 1942, is made slightly less relevant by the fact that our data was measured on 1 April 1950 (Census Day) and only asked 'age' not date of birth. The probability of service conditional on eldest child's age, then, represents a 'fuzzy' discontinuity (see Lee & Lemieux, 2010).

[6] And occasionally the duration of marriage, but with a preponderance of missing values. Of course, we also only observe surviving children who remained in the household through 1950. I take the eldest child's age as the instrument, and control for marital status in 1950.

We can study study the event window with respect to 1 April 1942: If a man's eldest child was at least 8 years old on 1 April 1950, then we count him a 'pre-Pearl Harbor' father; if less than 8 on 1 April 1950, then a 'post-Pearl Harbor' father.

Among fathers in the 1950 Census, the probability of having served in WWII increased dramatically around the 8-year-old eldest-child age cutoff. But there are non-compliers on both sides of this running variable (age of eldest child)—i.e., some pre-Pearl Harbor fathers were drafted; and some post-Pearl Harbor fathers were not drafted—so, again, the instrument represents a fuzzy discontinuity.

Since it is possible to observe directly whether Census respondents served in World War II,[7] it is also possible to estimate the local average treatment effect (LATE) and not merely the intent-to-treat (ITT) effect. The linked CenSoc data—although it is a relatively small sample of the 1940 full-count Census (and of that linked to the 1950 full-count Census)—allows us to observe: (i) whether the veteran was drafted or volunteered; (ii) whether they were drafted from civilian status rather than a military-adjacent occupation, e.g., National Guard service; and (iii) their date of enlistment (a proxy for their length of service until the end of the war).

In sum, the OLS model is a standard Mincer (1958) earnings function:

$$y_{1950} = \beta_1 \mathit{educ} + \beta_2 \mathit{exper} + \beta_3 \mathit{exper}^2 + \alpha \mathit{WWIIservice} + \xi X + \epsilon \qquad (1)$$

where (log) wages in 1950, y_{1950}, is regressed on years of *education* and *experience* (and its square term). The parameter α represents the impact of service in World War II on wages, and is the parameter of interest. X is a vector of controls including dummies for age cohort, race, urban location in 1950, and marital status in 1950.

However, one might worry about selection into service (à la Angrist & Krueger, 1994; Barrera et al., 2025; Collins & Zimran, 2025), whether because individuals with higher latent earnings potential may have disproportionately volunteered, or because the military would reject (not draft) individuals who were "physically, mentally, or morally unfit for service" (Class IV-F; Selective Service System, 1950), whom we may imagine had a lower latent earnings potential. Among married fathers, less than 2% were placed into this category Class IV-F (compared to approximately 20% of the larger population of registrants), which suggests that fathers represent a particularly valid subsample from among whom to instrument for the probability of being drafted.

[7] So-called 'sample-line individuals,' randomly one per household, were asked a battery of extra questions in the 1950 US Census, and among these (when the respondent was male and of age) whether they had served in World War II.

The probability to serve in World War II, then, can be instrumented in the following first-stage equation:

$$WWIIservice = \gamma_1 educ + \gamma_2 exper + \gamma_3 exper^2 + \theta Z + \zeta X + u \tag{2}$$

where Z is the 1 April 1942 eldest-child age cutoff for pre- vs. post-Pearl Harbor fatherhood, and covariates are the same as above.

Beyond these cross-section regressions with only 1950 Census data, the linked 1940 Census (and CenSoc) datasets allow for a panel fixed-effects design, albeit with the sample size reduced to those who were also observed in 1940 (and in army enlistment records)—and particularly their 1940 wages or their father's.

The panel fixed-effects model can be written as follows:

$$y_{it} = \lambda WWIIservice_{it} + \delta_i + \pi_t + \phi X_{it} + e_{it} \tag{3}$$

where y_{it} is the difference between 1940 and 1950 outcomes; *WWIIservice* can be instrumented as in equations 1 and 2 above; δ_i and π_t are individual and time fixed effects, respectively; and most of the controls above are unnecessary because constant within individuals over time.[8]

The outcome variable y_{it} can be interpreted as a change in log wages,[9] or instead, the model can be specified as a rank-rank regression (Chetty, Hendren, Kline, Saez, & Turner, 2014), where the outcome of interest is a change in rank of within-cohort wage distributions. For 1950 Census respondents linked to the the 1940 Census but whose 1940 wages are not reported (e.g., because they were not yet in the labor force), it is occasionally possible to link their father's within-cohort wage distribution ranking in 1940.

3 Results

From the harmonized Goldsmith (1951) and SOI (1950) data series discussed above, it is possible to estimate the shape of the income distribution below the 90th percentile. Immediately apparent is the dramatic rise of top incomes in the pre-World War II era, and its fall in the wartime and post-war era, but these tectonic shifts predominantly affected the upper middle-class households in the

[8] I can control for the move between rural and urban locations, between 1940 and 1950.

[9] Or, more specifically, as the log of the 1950 to 1940 wage ratio:

$log(y_{1950}) - log(y_{1940}) = log(\frac{y_{1950}}{y_{1940}})$

50th to 90th percentiles of the income distribution. The income share of the lower 50% of households fell during the Great Depression and rose during and after the war, but the magnitude of these changes was not as large as that for the upper middle 40% of households, in the total income distribution.

Beyond the data contribution to see the total income distribution for all individuals and all incomes,[10] we can now look inside the bottom 90% of the *wage* income distribution, as well, to show the evolution of wage income accruing to the poorest 50% of earners, over the entire midcentury period. In the wage distribution, we see a sharp and durable increase in wage income to the bottom 50% during and after World War II.

To explain this, I demonstrate a causal impact of World War II service on wages. Table 1 shows the OLS regression of wages on education and experience, with or without controls, in cross-section for the 1950 full-count Census (among sample-line individuals who were asked about their WWII-veteran status, and for whom both wages and control variables were nonmissing). Model (2) adds WWII-veteran status to the canonical Mincer model in columns (1), while Model (3) interacts veteran status with education, to test the impact of the GI Bill education subsidies available only to veterans. However, 1950 is likely too early a timeframe to observe the wage impact of GI Bill-subsidized education investment, as the returning veterans who took advantage of the program would have still been in school, or still too early within the education-experience tradeoff to experience the benefit of the former. Nonetheless, the 1950 wage premium for veterans is unmistakeable in Model (2)—and if anything amplified in Model (3) controlling for those veterans who had returned to school by 1950.

The question, of course, is whether the OLS impact of WWII service on wages is causal, or merely a correlation of earnings potential with selection into service.

Table 2 introduces the instrumental variable of eldest-child's birthdate with respect to Pearl Harbor, according to several event windows from one year to seven years. (In the 1-year window, fathers whose eldest child was born between 2 April 1941 and 1 April 1942 are compared to fathers whose eldest child was born between 2 April 1942 and 1 April 1943. In the 7-year window, the extreme dates are widened to 2 April 1935 and 1 April 1949, respectively.[11]) World II service is predicted according to logistic regression and linear probability models, with and without controls, and the variation across specifications hardly matters. Even controlling for age, the 7-year window around 1 April 1942 has the highest predictive power, and the greatest difference between the probability of service for post- vs. pre-Pearl Harbor fathers. Overall, of the 3.9 million men in our

[10] All data is available online at wid.world.

[11] Regardless of the width of the event window, the event date remains 1 April 1942, as measured on 1 April 1950.

sample,[12] 47.7% served in World War II. Only 21.2% of pre-Pearl Harbor fathers served, although the statistic increases to 25.6% among those whose eldest child was born within 7 years before Pearl Harbor. In the one-year window around 1 April 1942 (eldest child aged 8 years on 1 April 1950), 36.2% of pre-Pearl Harbor fathers served, while nearly 50% served among 1-year post-Pearl Harbor fathers (eldest child aged 7 years on 1 April 1950), or 43.3% on average. For sample size, intuition, and clarity, the 3-year window[13]is the preferred specification, but results do not change much according to the choice of event window for the instrument.

Of course, it is unlikely that the impact of service on earnings would be equal for all individuals. Table 3 presents the results by age cohort, according to men's ages in 1946 when the draft ended. In Model (1) of this table, the headline OLS results for all men age 18-42 in 1946 correspond to the OLS results in Table 1. In Model (2), we see the headline IV results for the three-year window.[14] Heterogeneous impacts by cohort are then examined with separate regressions for each age group. The age group 18-20 in 1946 corresponds to the sample studied by Angrist and Krueger (1994) and Barrera et al. (2025), so it no surprise that the effect size is minimal (and, with controls, actually reverses sign)—although the positive sign for the instrument on fatherhood [if particularly weak among teenagers, for whom fatherhood was rare] hints at a different story than the result of instrumenting on quarter-of-birth.[15]

The most dramatic impact of service is observed among men aged 21-25 in 1946, both in OLS and IV estimates. While the OLS impact for this group

[12]That is, men aged 18-42 in 1946 who responded to the 1950 full-count US Census as 'sample-line individuals,' with information on their veteran status as well as wages and control variables.

[13]For robustness, the three-year window is examined more closely in Supplementary Appendix Tables A1 and A2. The first shows that nearly 55% of post-Pearl Harbor fathers (within three years) served in World War II, against only 32% of pre-Pearl Harbor fathers (within three years). The latter were nearly 2.5 years older, although their wages (and within-cohort wage percentile) were very similar. (Note that I control for age cohorts in these regressions, as well.) Likely reflecting this age difference, pre-Pearl Harbor fathers were more likely to have their own wage observed in (and linked from) the 1940 Census, whereas post-Pearl Harbor fathers were more likely to have their father's wage observed in (linked from) the 1940 Census, implying that they were more likely to have been members of their parents' household at the time of the 1940 Census.

Meanwhile, with the same logic as Table 2 on the first-stage estimates, Appendix Table A2 presents the second-stage IV estimates for each of the event windows discussed. If we look at the 3-year window, with controls, and take the effect sizes literally, service in World War II is associated with a 6% increase in 1950 wages. The tighter 1-year event window (still with controls) actually predicts a larger effect size, on the order of a 10% increase in 1950 wages.

[14]These results also correspond to the results in Supplementary Appendix Table A2, which shows parallel headline IV results for other widths of the event window, including 1 or 5 or 7 years.

[15]In Angrist and Krueger (1994), the sign for the IV estimate actually turned negative (while the sign of the OLS estimate was positive), suggesting that young draftees at the end of the war experienced an earnings tradeoff relative to a counterfactual education or work experience.

is marginally more important for this cohort than for subsequent cohorts, the IV result is staggering. Taken at face value, service in the war led to a more than 100% increase in postwar wages, for post-Pearl Harbor fathers compared to pre-Pearl Harbor fathers. This was also the cohort most likely to serve in the war, in general. However, we might believe that there is some unobserved heterogeneity among those within the 21- to 25-year-old age group at 1946, who became fathers between 1939 and 1942 (between their ages 14 and 21 at the extremes) rather than between 1942 and 1945 (their ages 17 and 24 at the extremes), even controlling for age.

The next cohort, ages 26-30 at 1946—while perhaps less impressionable to a 'formative' life experience building human capital than their 21- to 25-year-old comrades, and more vulnerable to a mid-career interruption or tradeoff from service—may represent a group for whom pre-Pearl Harbor fatherhood in the 3-year birthdate window (between ages 19 to 26) is less different from post-Pearl Harbor fatherhood in the 3-year birthdate window (between ages 22 and 29);[16]or, more specifically, that the individuals themselves ('early' vs. 'late' fathers) would be more similar, such that the change in their probability of getting drafted is an even cleaner natural experiment. For this group, World War II service is estimated to generate at least an 18% increase in 1950 wages.

For individuals age 31 and over in 1946, the OLS effect decreases and the IV effect disappears. We might conclude that the impact of World War II service was less for this group, whether due to a larger tradeoff in the interruption of veterans' existing career trajectories, or due to or a mismatch between the type of 'training' that service represents and the stage of life of these soldiers, or due to another factor.

Nonetheless, for a sizeable subsample of veterans who served in the war during their early and mid-twenties, the experience proved an unmistakable stimulus to subsequent earnings capacity.

In fact, the effect was greatest for those in the lower half of the wage distribution of 1940. Table 4 shows the same cross-section OLS and IV results, but restricted to those whose 1940 position in the wage distribution (or their father's) could be linked to the 1950 wages.

In the IV estimates here, in Panel A, for those who found themselves in the bottom 50% of the 1940 wage distribution (for their age), WWII service is associated with a 10% increase in 1950 wages. There is no discernible effect for

[16]A note on the age ranges in parentheses here (and above):
By construction, a man aged 26-30 (or 21-25) on 1 April 1946 who had become a father *before* 1 April 1942, could not have become a father after his age 26 (or his age 21) four years prior, so for pre-Pearl Harbor fathers the age-at-fatherhood range is 19-26 (or 14-21).
In the same way, a man aged 26-30 (or 21-25) on 1 April 1946 who had become a father *after* 1 April 1942, could not have become a father before his age 22 (or his age 17) four years prior, so for post-Pearl Harbor fathers the age-at-fatherhood range is 22-29 (or 17-24).

individuals who were in the top half of the 1940 wage distribution, although the OLS suggests even a negative impact. In Panel B, an even greater effect is observed among those whose father's position was bottom 50% in 1940 (age-specific wage distributions), although in Panel B some relevant effects exist (in IV, if not OLS) even for those whose father was higher in the 1940 wage distribution.

Finally, Table 5, Panel A, shows the results of panel regressions among the linked subsample from 1940 and 1950 full-count US Censuses. With individual fixed effects that difference out any time-invariant individual characteristics, the quasi-experiment on date of fatherhood around Pearl Harbor is, in principle, even more appealing—even if it does restrict the sample to those whose wages were observed in 1940 and linked to the 1950 full-count US Census. The effect size in the results of Table 5 is even larger than those of the previous tables above (e.g., Table 3, or Table A2 in the Supplementary Appendix), which would seem to confirm the experimental result of the impact of WWII service on wages.

Panel B restricts the sample even further, by excluding any veterans who were not drafted from civilian occupations. (In this case, the treatment is tightened to focus only those who did not volunteer for service, and were not affiliated in any military-adjacent occupation prior to their induction.) That is, the link to the CenSoc database on WWII army enlistees allows to pinpoint those veterans who were drafted ("selectees") from a "civilian" source, and all other veterans (whether observed as volunteer enlistees, or unobserved in CenSoc data) can be excluded from the estimate.[17] After restricting treatment to civilian draftees, the estimate of WWII's impact rises to a more than 100% increase in 1950 over 1940 wages.

For these civilian draftees, further evidence from CenSoc in Panel C indicates that length of service mattered—i.e., the intensity of the 'treatment.' Within the CenSoc sample of WWII veterans drafted from civilian occupations, longer service predicted higher 1950 wages. The 3-year window of fatherhood around Pearl Harbor is an effective IV even in this case, because it predicts a deferment of service and therefore less service time overall, even when we are restricting the sample to men who were drafted and did serve. If we take the effect sizes at face value, every three months of wartime service was associated with a more than 1% increase in 1950 wages.

A lasting wage compression defined the postwar era. The bottom 50% wage share increased by more than 50% during the war years, and remained level for a generation.

[17] Unfortunately, draft classifications are not observed, nor does CenSoc provide any data on the men who did not enlist, so we cannot exclude men who were classified as 'unfit' by their local draft boards. However, as discussed in section 2 above, this represents less than 2% of fathers (Selective Service System, 1947).

The results of this paper suggest that the midcentury decrease in inequality was not (or not only) a 'leveling' of top incomes. Rather, the results remind us that this was an era defined by new possibilities for the working class. Part of that is a human capital story. And national service in World War II constituted a type of training whose impact mattered more for postwar wages than either education or experience—and particularly for those who started at the bottom of the wage distribution.

What type of training this service constituted—whether 'soft' skills (communication, leadership, discipline), or 'hard' skills (engineering, technology, manufacturing), or something else—cannot be established with certainty by the analysis in this paper. However, we can probably rule out the 'signaling' hypotheses that wartime service was only valuable for its reputation effects, or a network, or a later hiring preference. It is unlikely that these would have been in place so soon after the war as to show themselves in 1950 wage data. If anything, those who did not serve may have had access to stronger networks in civilian life, uninterrupted by service.

Similarly, we can also rule out that the effect of service on wages was only a matter of education. By 1950 the effect of the GI Bill on wages would not have appeared yet, and in fact veterans' (subsidized) education investment may have been negatively correlated with wages in 1950.

And finally, with this new estimate we can also rule out that pure selection bias explains veterans' productivity. The veteran wage premium is similar in IV to OLS, but can be interpreted causally.

Under these conditions, the demonstrated wage premium for World War II veterans—an effect size estimated higher among civilian draftees than among all enlistees; and with increasing returns on the intensive margin (duration of wartime service)—suggests a formative experience that, for many, translated directly into postwar earnings.

Whatever else the costs and benefits of World War II, its impact on productivity and human capital is notable. More than an unrepeatable set of circumstances in history, the economic mobility of the greatest generation offers an example for our time. National service can serve the country and the citizen at once.

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Tables

Table 1: OLS results: Impact of WWII service on (log) wages, 1950

	(1)		(2)		(3)	
education	0.0287***	0.0163***	0.0284***	0.0161***	0.0331***	0.0189***
	(0.000115)	(0.000166)	(0.000115)	(0.000166)	(0.000161)	(0.000199)
experience	0.0671***	0.0569***	0.0657***	0.0574***	0.0640***	0.0525***
	(0.000293)	(0.000710)	(0.000294)	(0.000710)	(0.000296)	(0.000734)
experience^2	-0.00153***	-0.00184***	-0.00145***	-0.00184***	-0.00140***	-0.00171***
	(0.00000827)	(0.0000196)	(0.00000840)	(0.0000196)	(0.00000847)	(0.0000203)
WWII veteran			0.0660***	0.0302***	0.155***	0.0836***
			(0.000983)	(0.00101)	(0.00229)	(0.00230)
education · 1(WWII vet)					-0.00976***	-0.00593***
					(0.000225)	(0.000229)
controls		✓		✓		✓
R-squared	0.033	0.063	0.034	0.063	0.035	0.064
N	3,892,814	3,892,814	3,892,814	3,892,814	3,892,814	3,892,814

Notes: Model (1) is the Mincer (1958) model regressing log wages on years of education, years of experience, and years of experience squared. Model (2) includes World War II service as a covariate. Model (3) tests the effect of World War II service interacted with education, as in the postwar G.I. Bill (or prewar human capital). Each model is estimated with and without control variables. Controls include dummies for age; race (nonwhite); urban vs. rural location in 1950; and marital status in 1950. Robust standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 2: First-stage results: Impact of eldest child's birthdate on probability of WWII service

	(1) Logistic regression		(2) Linear probability		% WWII vets	*N*
all men age 18-42 in 1946					**47.7%**	**3,892,814**
					o/w pre-Pearl Harbor fathers:	
pre-Pearl Harbor fathers					**21.2%**	**1,156,184**
eldest child's birthdate, wrt Pearl Harbor						
7-year window	**1.142*** **	**1.064*** **	**0.249*** **	**0.221*** **	**49.6%**	**1,835,592**
	(0.00382)	(0.00388)	(0.000812)	(0.000814)	*o/w pre-Pearl Harbor fathers:*	
R-squared			0.230	0.266	**25.6%**	**732,606**
F-statistic			94053.5	74026.5		
5-year window	**0.940*** **	**0.892*** **	**0.205*** **	**0.186*** **	**46.8%**	**1,291,680**
	(0.00421)	(0.00429)	(0.000906)	(0.000901)	*o/w pre-Pearl Harbor fathers:*	
R-squared			0.194	0.226	**28.2%**	**555,557**
F-statistic			51053.6	42792.9		
3-year window	**0.668*** **	**0.650*** **	**0.144*** **	**0.135*** **	**44.1%**	**763,650**
	(0.00515)	(0.00525)	(0.00111)	(0.00110)	*o/w pre-Pearl Harbor fathers:*	
R-squared			0.161	0.188	**31.7%**	**353,911**
F-statistic			16924.8	15260.4		
1-year window	**0.452*** **	**0.451*** **	**0.0970*** **	**0.0937*** **	**43.3%**	**287,777**
	(0.00813)	(0.00826)	(0.00174)	(0.00171)	*o/w pre-Pearl Harbor fathers:*	
R-squared			0.139	0.164	**36.2%**	**134,554**
F-statistic			3124.0	2999.5		
controls		✓		✓		

Notes: Model (1) presents the logistic regression of WWII service on eldest-child birthdate windows with respect to Pearl Harbor (7 December 1941)—or more specifically 1 April 1942, eight years prior to Census Day 1950. Model (2) presents the linear probability model of the same. For example, the '3-year window' compares the probability of WWII service of [1 = fathers whose eldest child was born between 2 April 1942 and 1 April 1945 (post-Pearl Harbor fathers)] vs. [0 = fathers whose eldest child was born between 2 April 1939 and 1 April 1942 (pre-Pearl Harbor fathers, approximately)]. All specifications include covariates for education and experience (and its square term), à la Mincer (1958). Both models are estimated with and without control variables. Controls include dummies for age; race (nonwhite); urban vs. rural location in 1950; and marital status in 1950. Robust standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 3: Life cycle timing of national service: OLS and IV results by age cohort

	(1)				(2)		
	OLS		*N*	% WWII vets	IV		*N*
men age 18-42 in 1946	0.0660***	0.0302***	3,892,814	47.7%	0.137***	0.0605***	763,650
	(0.000983)	(0.00101)			(0.0145)	(0.0153)	
F-statistic					16924.8	15260.4	
age 18-20	0.0441***	-0.0131***	419,765	48.6%	1.461	1.068	9,245
	(0.00284)	(0.00286)			(1.469)	(2.566)	
F-statistic					2.189	0.604	
age 21-25	0.125***	0.0687***	819,221	74.7%	1.408***	1.066**	105,837
	(0.00226)	(0.00225)			(0.259)	(0.455)	
F-statistic					74.04	21.00	
age 26-30	0.0846***	0.0481***	808,965	65.8%	0.359***	0.184***	263,450
	(0.00210)	(0.00207)			(0.0326)	(0.0348)	
F-statistic					3020.9	2575.9	
age 31-35	0.0250***	0.0158***	776,405	38.5%	0.0289	-0.0116	230,330
	(0.00207)	(0.00205)			(0.0181)	(0.0181)	
F-statistic					11060.5	10915.1	
age 36-42	-0.00191	0.00273	1,068,458	19.5%	-0.0137	-0.00613	154,788
	(0.00223)	(0.00222)			(0.0324)	(0.0321)	
F-statistic					4866.1	4869.9	
controls		✓				✓	

Notes: Model (1) presents the second set of OLS results from Table 1, regressing (log) wages on WWII service, with separate regressions estimated for each age cohort. Model (2) does the same but disaggregates the results of Table A2, where the eldest-child birthdate '3-year window' (with respect to 1 April 1942) instruments for the probability of WWII service. All specifications include covariates for education and experience (and its square term), à la Mincer (1958). Both models are estimated with and without control variables. Controls include dummies for age; race (nonwhite); urban vs. rural location in 1950; and marital status in 1950. Robust standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 4: Service and mobility: Results by 1940 wage bracket

	OLS			IV		
	(1)		*N*	(2)		*N*
1940 wage distribution ranking						
linked sample, all	-0.00244	0.00204	1,074,241	0.0664***	0.0180	256,946
	(0.00200)	(0.00200)		(0.0226)	(0.0228)	
F-statistic				7074.8	6976.2	
p0p50	0.0293***	0.0203***	428,831	0.234***	0.107***	89,709
	(0.00309)	(0.00308)		(0.0382)	(0.0387)	
F-statistic				2347.4	2250.1	
p50p90	-0.0202***	-0.00791***	495,806	0.0602*	0.0371	122,595
	(0.00286)	(0.00289)		(0.0311)	(0.0309)	
F-statistic				3574.5	3638.0	
p90p100	-0.0230***	-0.00994*	149,604	0.0357	0.0517	44,639
	(0.00578)	(0.00586)		(0.0641)	(0.0659)	
F-statistic				1021.9	982.4	
father's 1940 wage distribution ranking						
linked sample, all	0.0511***	0.0202***	494,622	0.506***	0.344***	71,635
	(0.00269)	(0.00279)		(0.0720)	(0.0769)	
F-statistic				648.2	560.1	
p0p50	0.0805***	0.0424***	207,673	0.528***	0.349**	30,903
	(0.00406)	(0.00417)		(0.132)	(0.147)	
F-statistic				188.1	148.8	
p50p90	0.00713*	-0.0123***	237,024	0.341***	0.237**	33,876
	(0.00387)	(0.00403)		(0.0897)	(0.0942)	
F-statistic				390.6	356.4	
p90p100	0.0285***	-0.0151	49,925	0.485**	0.418*	6,856
	(0.00933)	(0.00987)		(0.234)	(0.238)	
F-statistic				73.94	71.99	
controls		✓			✓	

Notes: Model (1) regresses log 1950 wages on WWII service, with separate regressions estimated within each 1940 wage bracket: p0p50 (bottom 50% of wage earners); p50p90 (middle 40%); and p90p100 (top 10%). The 1940 wage corresponds either to the 1950 Census individual (whose 1950 wages are the dependent variable) in Panel A, or to his father in Panel B—but in both cases taking the within-cohort income distribution rank, from age-specific 1940 wage distributions. Model (2) does the same as the OLS model but uses the eldest-child birthdate '3-year window' (with respect to 1 April 1942) to instrument for the probability of WWII service. All specifications include covariates for education and experience (and its square term), à la Mincer (1958). Both models are estimated with and without control variables. Controls include dummies for age; race (nonwhite); urban vs. rural location in 1950; and marital status in 1950. Robust standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 5: Panel fixed effects: Effect of WWII service on (log) wages, 1940-50

	OLS			IV		
	(1)	(2)	*N*	(3)	(4)	*N*
Panel A: Panel fixed-effects						
WWII service	0.111***	0.108***	*1,055,967*	0.858***	0.853***	*251,987*
	(0.00261)	(0.00260)		(0.0282)	(0.0281)	
Panel B: Restrict to civilian draftees						
WWII civilian draftees	0.210***	0.205***	*784,727*	1.128***	1.120***	*181,237*
	(0.00445)	(0.00443)		(0.0493)	(0.0492)	
Panel C: Wartime service (in months)						
WWII service time	0.00214***	0.00200***	*85,259*	0.00337***	0.00354***	*20,529*
[civilian draftees]	(0.000292)	(0.000291)		(0.00124)	(0.00123)	
controls		✓			✓	

Notes: This table shows the results of panel regressions of the difference in 1950 vs. 1940 (log) wages, for men aged 18-42 in 1946 whose records are linked between 1940 and 1950 full-count US Censuses. Panel A presents OLS and IV results (where WWII service is instrumented as above, using the '3-year window' of eldest-child birthdate around Pearl Harbor). Panel B restricts the treated sample (veterans) to those who were observed as 'selectees' from 'civilian' occupations in the CenSoc database (Goldstein et al., 2023), linked to the 1940 full-count US Census in that study and also to the 1950 in this study. Panel C estimates, for these draftees, the marginal effect of one additional month of service from the date of enlistment to the end of the war on 2 September 1945. All specifications include covariates for education and experience (and its square term), à la Mincer (1958). Models are estimated with and without controlling for a rural-urban move between 1940 and 1950. Previous controls (and other time-invariant individual characteristics) are differenced out via individual fixed effects. Standard errors in parentheses are clustered at the level of the individual. * p<0.10 ** p<0.05 *** p<0.01.

Supplementary Appendix

Appendix A Additional Figures and Tables

Table A1: Balance table for 3-year window of eldest child birthdate

	eldest child birthdate		
	pre-Pearl Harbor	post-Pearl Harbor	*N*
Panel A: WWII service, wages and covariates in 1950 full-count US Census			
new fathers age 18-42 in 1946	353,911	409,739	763,650
served in WWII	31.7%	54.8%	
age in 1946	31.3	28.8	
years of education (1950)	9.0	9.3	
married (1950)	99.0%	99.2%	
urban (1950)	60.5%	62.5%	
nonwhite	7.1%	6.7%	
wage in 1950 (current USD)	2,786	2,759	
within-cohort wage percentile	57.8	58.6	
Panel B: Wages (and parent wages) in 1940 full-count US Census			
linked wage in 1940	42.8%	25.7%	256,946
wage in 1940 (current USD)	1,055	902	
within-cohort wage percentile	61.2	58.1	
linked father's wage in 1940	6.2%	12.2%	71,635
father's wage in 1940 (current USD)	1,143	1,304	
father's within-cohort wage percentile	51.5	54.2	

Notes: The '3-year window' subsample compares fathers' eldest child's birthdate: [1 = fathers whose eldest child was born between 2 April 1942 and 1 April 1945 (post-Pearl Harbor fathers)] vs. [0 = fathers whose eldest child was born between 2 April 1939 and 1 April 1942 (pre-Pearl Harbor fathers, approximately)]. For the variables presented in this table, all p-values testing between-group difference in means are significant at $p<0.0005$.

Table A2: IV results: Impact of WWII service on (log) wages, 1950

birthdate window, wrt Pearl Harbor			*N*
7-year window	0.131***	0.0584***	1,835,592
	(0.00614)	(0.00689)	
F-statistic	94053.5	74026.5	
5-year window	0.128***	0.0564***	1,291,680
	(0.00834)	(0.00910)	
F-statistic	51053.6	42792.9	
3-year window (benchmark)	0.137***	0.0605***	763,650
	(0.0145)	(0.0153)	
F-statistic	16924.8	15260.4	
1-year window	0.166***	0.102***	287,777
	(0.0339)	(0.0347)	
F-statistic	3124.0	2999.5	
controls		✓	

Notes: The model presented here corresponds to model (2) in Tables 1 and 2 above—and instruments for the probability of WWII service via the eldest child's birthdate, with results for several birthdate windows around Pearl Harbor (December 7, 1941). For example, the '3-year window' compares the probability of WWII service of [1 = fathers whose eldest child was born between April 2, 1942 and April 1, 1945 (post-Pearl Harbor fathers)] vs. [0 = fathers whose eldest child was born between April 2, 1939 and April 1, 1942 (pre-Pearl Harbor fathers, approximately)]. Both specifications include covariates for education and experience (and its square term), à la Mincer (1958). The second is estimated with control variables. Controls include dummies for age; race (nonwhite); urban vs. rural location in 1950; and marital status in 1950. Robust standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01.