

The Mechanics of Good Fortune

On Intergenerational Mobility during the Second Industrial Revolution

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Abstract

To what extent and how can individuals in occupations that are beneficially affected by structural transformations transmit their gains in socio-economic status to their offspring? To address this question, we complement US full count census data with newly digitized data on the local supply of secondary education and occupational income in the late nineteenth century. We analyze the case of machinists whose occupation experienced a relative labor demand spike during the Second Industrial Revolution (1870-1914), resulting in higher income, job stability and less occupational downgrading. Using matching and fixed effects regressions, we document that the (grand)sons of men who were machinists in 1870 held occupations with significantly higher earnings than the (grand)sons of comparable non-machinists. The higher earnings of machinists' sons mainly stemmed from parental investment in their education. However, this effect is absent for those sons who were already too old to attend high school when the relative income of machinists started to rise. Additionally, the sons of initially rural machinists benefited from rural-to-urban migration. Our results are robust to controlling for family-fixed effects (comparing machinists to their non-machinist brothers), pre-1870 spatial sorting, and a rich set of next-door neighbor and grandparental characteristics.

Key words: Intergenerational Transmission, Second Industrial Revolution, Rural-Urban Migration, Investment in Education

JEL Codes: I24, J62, N31

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1 Introduction

Structural transformations have a profound impact on the career and socio-economic status of most people. In particular, recent waves of robotization or trade shocks changed the structure of the labor market and the life of millions of workers depending on their industry or occupation (Acemoglu and Restrepo, 2019; 2020; Autor et al., 2016; Dauth et al., 2021a; b; Graetz and Michaels, 2018; Humlum, 2019; Traiberman, 2019). However, since these shocks are very recent and the grandchildren of affected workers have not even been born yet, we can merely speculate how the offspring of demanded tech workers or of displaced manufacturing workers might fare in the very long run. Therefore, in this paper we go back in time to study the effect of an arguably equally disrupting time period on the labor market: the Second Industrial Revolution (ca. 1870-1914). We try to understand *to what extent* and *how* members of a particularly demanded occupation - machinists¹ - could pass on their gains in socio-economic status to later generations in the United States.

To the best of our knowledge, this is the first work which documents the persistence in income gains caused by a labor market shock on the grandchildren of affected individuals, i.e. over three generations. Moreover, we also shed light on the mechanisms which underlie the documented intergenerational persistence: internal migration and increased (secondary) education. We show that these two channels may account for the entire positive effect of machinists on their offspring's earnings, though their relative importance depends on initial urban status: the offspring of initially rural machinists gained from more schooling as well as from internal migration to more urban areas, whereas the channel of internal migration does not play a significant role for the sons of urban machinists.

Using the US full count census, we can overcome the main hurdle to intergenerational studies: the scarcity of available data connecting generations. We exploit this data set leveraging the strengths of two, complementary estimation methods: propensity score matching and fixed effects regression. Our empirical strategy amounts to comparing the post-1870 outcomes of machinists to those of non-machinists, who were observationally very similar to machinists before the onset of the Second Industrial Revolution. Next, we identify the offspring of these individuals and investigate their outcomes as well. In our baseline strategy, we use personal and residential characteristics from the census as controls and complement them with occupation-based education (Song et al., 2020) and novel earnings scores. These earnings scores, constructed based on U.S. Department of Labor (1900), are another contribution of this paper as we are the first ones to calculate state-specific earnings scores for a large number of occupations before 1890.

In this paper, we document that machinists could pass on their relative gains in socio-economic status to their (grand)sons. First, we find that machinists, whose occupation experienced a relative labor demand boom starting in the 1870s, enjoyed higher earnings and occupational stability, and were more likely to live in urban places after 1870. As explained in Section 2, the surge in demand for machinists resulted from innovations leading to mechanization and the rapid spread of factory production methods in the US. Therefore, much demanded machinists could avoid switching to lower-paying, often agricultural occupations during the volatile business cycle

¹Workers in charge of installing and maintaining machinery.

of the Gilded Age. Thus, besides a relative wage improvement, the identified occupational earnings gains are driven by less occupational downgrading rather than occupational upward mobility, which could be suggestive of unobserved ability. Second, the sons of machinists held occupations with 5-12 log-points higher real or nominal earnings scores than the sons of comparable non-machinists in 1900.² Finally, a significant positive effect is estimated on the individual- or occupation-level income of grandsons in 1940, seventy years after 1870.

Next, we shed light on the mechanisms behind the documented intergenerational transmission. For the sons of initially rural machinists, the positive earnings effect partly stems from a higher probability of living in an urban area as an adult (urban wage premium). To quantify the approximate size of earnings gains originating from rural-to-urban migration, we multiply the differential likelihood of urban status with an earnings score-based estimate of the rural-to-urban migration premium pertaining to the early-twentieth-century United States (Ward, forthcoming).

Additionally, machinists' sons benefited from parental investment in their education irrespective of initial urban status, receiving approx. 0.35 more years of (mainly secondary) schooling. To study the role of education in explaining the earnings effect, we simply combine our years of schooling point estimate with a returns to schooling estimate in Goldin and Katz (2000). Moreover, by exploiting a newly digitized, county-level data set on high school provision, we establish that the positive earnings and especially schooling effects on the sons of machinists increased in county-level private high school provision.³ This complementarity between a machinist father and local high school supply was especially strong when free-of-charge public high school supply was limited and private schools had a high teacher-student ratio. On the other hand, gains from private high schools decreased if these schools could be attended at a low price and put emphasis on scientific education in their curricula (e.g., mechanical drawing), the type of knowledge which the sons of machinists could more easily acquire at home. This suggests that passing on scientific knowledge in an informal way, within a family also helped machinist's sons succeed. Furthermore, the estimated positive effects on machinists' sons declined in public high school provision as well. This empirical result is consistent with financially more constrained non-machinist parents (Becker and Tomes, 1979; 1986). Last, we estimate a coefficient on education which is not significantly different from zero for sons who were older than ten years in 1870, suggesting that machinists were not differentially more likely to invest in the education of their sons before 1870.⁴

Apart from heterogeneity exercises, we conduct a series of robustness checks to mitigate concerns that the identified positive effects can be explained by (the transmission of) the machinists' unobserved ability. Arguably the most convincing robustness checks are regressions containing family-fixed effects, i.e. comparing machinists to their own brothers.⁵ The results from this specification, which controls for the similar environment of upbringing and inherited genes, are qualitatively and quantitatively similar to those obtained from the baseline analysis. In

²After correcting for the bias which stems from mismeasurement. See Section 6.4.

³We show that this effect is driven by the medium-level tuition fee. At this cost level, education was less affordable for rival boys but not prohibitively costly for machinists.

⁴In accordance with the literature documenting dynamic complementarities in the production of human capital (see Heckman and Cunha, 2007), it was arguably already too late to invest in their education when the relative earnings of machinists started to rise.

⁵This robustness test can only be conducted for the generation of machinists themselves because we run into sample size limitations for later generations.

addition, the lack of correlation - both within and across families - between the machinist indicator and standard (historical) proxies of unobserved ability (e.g., number of children or spousal literacy - measured in 1870) suggests that machinist fathers were not more able compared to their brothers or comparable peers.

We demonstrate that the results are not driven by occupation-state or census division-level pre-trends (e.g., changes in the employment share, probability of switching to agriculture, etc. in the 1850s and 1860s), and are insensitive to which specific occupations are the dominant "control occupations". Moreover, our preferred propensity score matching strategy eliminates initially large differences in the overwhelming majority of characteristics of wives, fathers and next-door-neighbors between machinists and non-machinists - even *without* matching on these characteristics. We also establish that similarly aged sons of younger and older machinists experienced similar positive effects, indicating the absence of early sorting into the machinist occupation by more talented individuals. Additionally, the inclusion of birth state-destination county (1870)-fixed effects makes it very unlikely that the results reflect spatial sorting prior to 1870.

Related literature This work is closely connected to the literature which examines the effect of parental labor market shocks on affected children. Exploiting layoffs, [Hilger \(2016\)](#) and [Mörk et al. \(2020\)](#) find at most very small negative effects on the education and adult earnings of affected children.⁶ As both papers point out, these might be the consequence of a generous welfare state offsetting otherwise reduced parental spending on education. A more accurate comparison to our setting might come from papers that focus on less developed countries with a rather weak welfare state or low-income (financially constrained) families. These papers tend to find that changes in parental income - not necessarily induced by job loss - do matter for the offspring (see, e.g., [Aizer et al., 2016](#); [Akee et al., 2010](#); [Dahl and Lochner, 2012](#); [Di Maio and Nisticò, 2019](#); [Løken et al., 2012](#); [Manoli and Turner, 2018](#)). Surveying the literature, [Cooper and Stewart \(2017\)](#) conclude that there is "*strong evidence that income has causal effects on a wide range of children's outcomes, especially in households on low incomes*", whereas wealth shocks do not seem to have substantial effects on children either in a historical ([Bleakley and Ferrie, 2016](#)) or in a modern context ([Cesarini et al., 2017](#)). Additionally, there is a large literature documenting the role of credit constraints and grants, mostly for college education.⁷ Our contribution is to show that the effect of labor market shocks may persist even for the offspring in the second generation. In addition, we pin down mechanisms which lead to the documented intergenerational persistence. These are not well-understood even in the modern context and, to the best of our knowledge, have not been studied in a historical context yet.

This paper also speaks to a literature which seeks to identify the determinants of intergenerational mobility in the 19th-20th century United States. [Parman \(2011\)](#) demonstrates that children from high-income families

⁶Early papers tend to exploit mass layoffs or factory closures, and find mixed effects on schooling and future earnings of children affected by parental job loss ([Bratberg et al., 2008](#); [Coelli, 2011](#); [Oreopoulos et al., 2008](#); [Rege et al., 2011](#)). However, [Hilger \(2016\)](#) argues that many early findings on large, negative effects might be driven by the assortative matching of low-quality workers and low-quality firms leading to selection into layoffs or closure. [Løken \(2010\)](#), exploiting the oil boom in Norway as a permanent income shock, finds no effect on children either.

⁷There is ample evidence that credit constraints and grants for schooling matter even in modern contexts and in many developed countries. The early literature is summarized in [Lochner and Monge-Naranjo \(2012\)](#), see also [Bettinger et al. \(2019\)](#), [Castleman and B. T. Long \(2016\)](#), [Denning et al. \(2019\)](#), [Fack and Grenet \(2015\)](#), [Hai and Heckman \(2017\)](#), [Lee and Seshadri \(2019\)](#), [Molina and Rivadeneyra \(2021\)](#), [Solis \(2017\)](#), and [Wright \(2021\)](#).

benefited disproportionately more from improving public high school availability in Iowa at the turn of the 20th century, resulting in a higher intergenerational income elasticity. However, we find a negative association between public high school supply and the relative gains of machinists' sons, in line with [Solon \(2004\)](#) and [Olivetti and Paserman \(2015\)](#). Since parents of similar socio-economic background tend to have similar preferences over education ([Boneva and Rauh, 2018](#)), we believe that comparing machinist fathers to fathers in other middle-class occupations might eliminate the effect uncovered by [Parman \(2011\)](#) in our case. In a comparison of migrating to non-migrating brothers, [Ward \(forthcoming\)](#) finds that rural-urban migration was an important contributor to upward mobility in the early-twentieth-century US, particularly so for people from the poorest households. This finding is in line with our results on the importance of urban place of living for initially rural machinists' sons. Furthermore, [Olivetti and Paserman \(2015\)](#) and [Song et al. \(2020\)](#) show that industrialization was a major determinant of a relatively low intergenerational mobility around 1900. Our case study of machinists aligns well with this view and suggests highly persistent positive effects on their offspring.

By analyzing the effect of a change in occupational labor demand on machinists themselves, this work is also connected to a fast growing literature which investigates the effect of technology-induced occupational labor demand changes on affected individuals. Papers studying the impact of automation or robotization typically find that robots decrease the employment share of lower-skilled production workers and benefit workers in occupations with complementary tasks - just as early machines did to machinists ([Acemoglu and Restrepo, 2020](#); [Dauth et al., 2021b](#); [Graetz and Michaels, 2018](#); [Humlum, 2019](#)). Focusing on the automation of telephone operation, [Feigenbaum and Gross \(2020\)](#) find that incumbent telephone operators bore most of the losses: they were more likely to be in lower-paying occupations or left the labor force entirely after automation started. However, growth in middle-skill jobs absorbed the labor supply of later generations. Using exceptionally disaggregated Swedish data on occupations, [Edin et al. \(2019\)](#) show that those facing occupational decline lost about 2-5 percent of mean cumulative earnings and were less likely to remain in their starting occupations - the mirror image of what we estimate in the US for machinists. Additionally, Swedish earnings losses are partly accounted for by reduced employment and increased time spent in unemployment and retraining. Our contribution to this literature lies in analyzing a different time period, mainly the Second Industrial Revolution, in detail.

The paper is structured as follows. First, Section 2 discusses the historical background, then, Section 3 addresses questions related to data sources and sample construction. Section 4 presents the empirical strategy while Section 5 contains the main results. Thereafter, the reader may find a battery of robustness exercises and a discussion of a non-classical measurement error in Section 6. Finally, Section 7 concludes.

2 Historical background

The machinist occupation was born in the First Industrial Revolution in the United Kingdom, but members of this occupation played an important role in innovative activities in the United States in the early nineteenth century as well ([Kelly et al., 2020](#); [Meisenzahl and Mokyr, 2011](#); [Sokoloff and Khan, 1990](#)). Nevertheless, professional

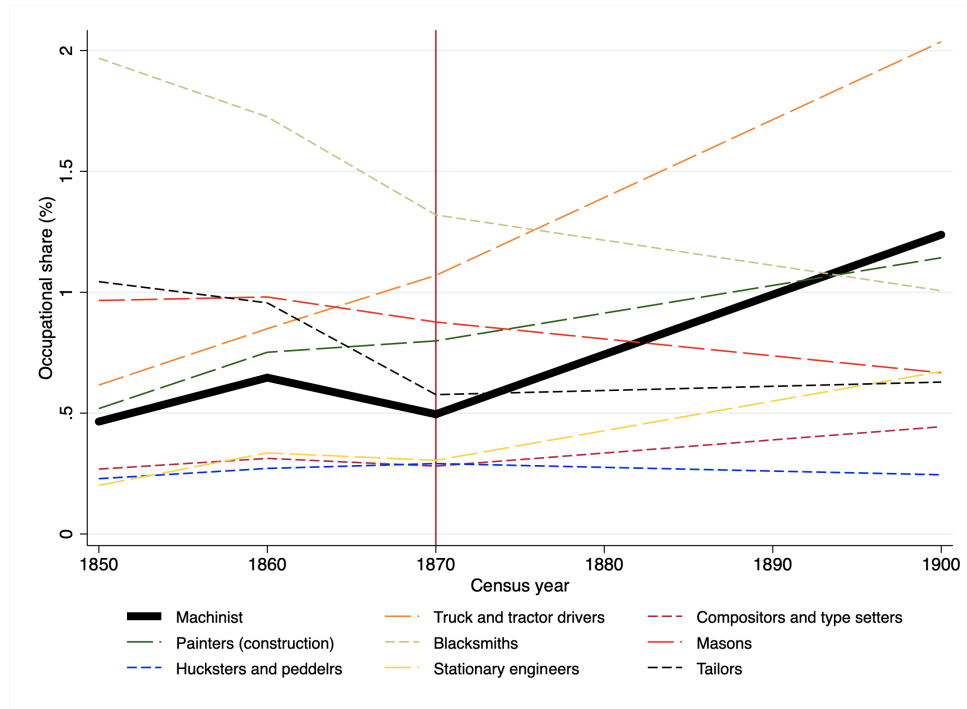


Figure 1: The evolution of occupational employment shares over time

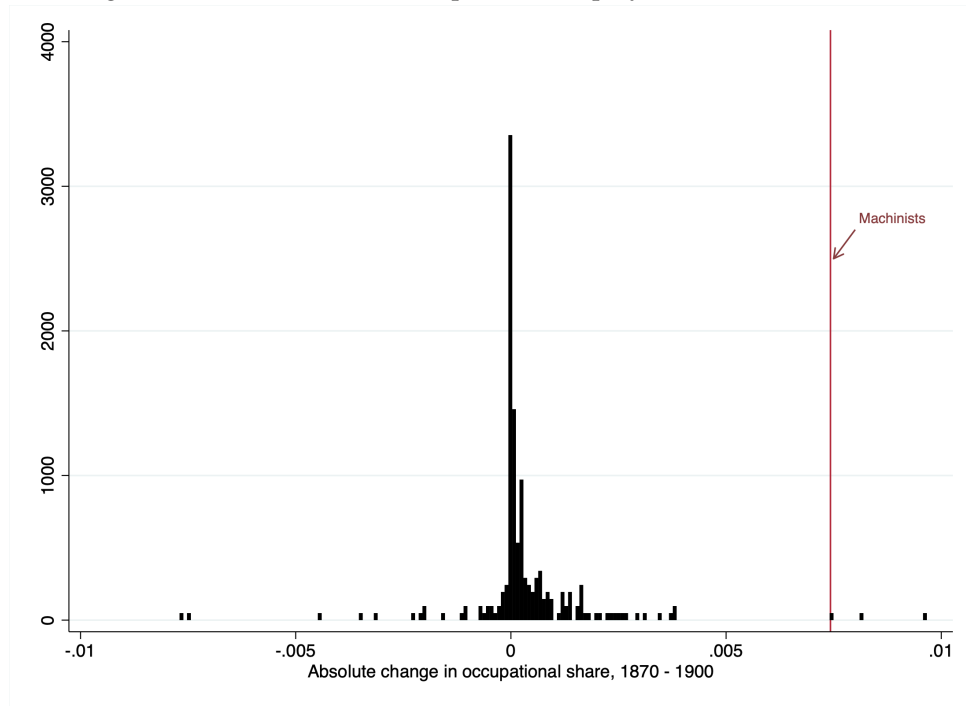


Figure 2: Histogram of occupational employment changes

Notes. **Figure 1:** the sample includes all males aged 16-65 who did not give a non-occupational response in the full count census in a given year. The number of workers in each occupation is divided by the total number of workers in 1850, 1860, 1870 and 1900. Harmonized occupations (1950) are used. Therefore, people classified as 'Truck and tractor drivers' were predominantly teamsters in the nineteenth century. **Figure 2:** the same sample used as in Figure 1. Only the employment of 'Mine operatives and laborers' and 'Truck and tractor drivers' grew faster than that of machinists out of the narrowly defined (i.e., not 'not elsewhere classified') occupations (see Section 3.1).

engineers had taken over this inventive role by the mid-nineteenth century, even before the Second Industrial Revolution started (Hanlon, 2021; Maloney and Valencia Caicedo, 2020). Thus, the assembly and maintenance of industrial machinery was left as the task of most machinists (U.S. Department of Labor, 1899). People could enter this occupation through the helper system, a type of informal apprenticeship. This meant initially simple operations followed by a sequence of more demanding tasks as they gained experience next to senior machinists. Additionally, the division of labor among American machinists reached a substantially higher level compared to the UK, resulting in a relatively lower skill requirement and making a cross-country earnings comparison of machinists almost impossible (Rosenbloom, 2002). In spite of reduced skill requirements in the US, machinists remained a part of the so-called "labor aristocracy" alongside other skilled craftsmen, for instance, blacksmiths, carpenters, conductors, masons, painters or plumbers (Dawson, 1979; Rosenbloom, 2002).

While at-scale factory production was limited to the textile industry until the Civil War, the situation changed rapidly after the onset of the Second Industrial Revolution around 1870. Mechanization and factory production methods spread swiftly across a wide range of industries, led by steel and chemicals production, and was supercharged by the utilization of electricity and novel ways of transportation (e.g., the railway; Mokyr, 1999; Rosenbloom, 2002). American manufacturing harnessed steam engines with a total capacity of approx. 1.000 thousand HP in 1870. This figure exponentially increased to almost 9.000 thousand by 1900 (Rosenberg and Trajtenberg, 2004). The average establishment size stagnated between 1849 and 1869, but it experienced a historically unprecedented growth in the 1870s and 1880s as production concentrated in factories (O'Brien, 1988).

As assembling, setting up and maintaining the machinery were the main tasks of machinists, they were found in a wide range of mechanized sectors by the end of the 19th century (U.S. Department of Labor, 1899): brush, buttonmold, canned corn, cigarette, faucet, female shoes, ingrain carpet, needle or teaspoon production, etc.. The sudden need for expertise to handle machines in these sectors led to a fast rising demand for machinists. The change in their employment share, which could barely outpace the growth of the labor force and was similar to that of some other craftsmen prior to 1870, experienced a steep acceleration (see Figure 1). As a result, their number almost doubled between 1870 and 1880, and a five-fold increase is registered in the full count census between 1870 and 1900. The US population merely doubled in these three decades. Thus, the expansion of the machinist occupation surpassed practically any other major group of craftsmen.

Despite the outstanding growth in their number, machinists did not experience a relative earnings decline. On the contrary, their relative earnings increased compared to most occupations from the early 1870s to the 1880s, and relative earnings gains seem to have disappeared only by the end of the century to some extent (see Table 1 and Section 5).⁸ Taken together, the substantial employment expansion and relative earnings growth are consistent with a positive labor demand shock induced by the Second Industrial Revolution - relative to most other middle-skilled occupations.

⁸One potential cause behind the disappearance of earnings gains as measured by occupational earnings scores is the following. While the machinist occupation was growing, it started to employ relatively more young, less experienced workers. Thus, a declining average experience level might have pushed the occupational earnings level down.

Table 1: Occupational earnings (1850-1892; in 1890 dollars)

Occupation	Yearly earnings score					Growth (%)			Growth (Massachusetts)	
	1850	1860	1870-72	1879-1881	1890-92	1850-1872	1872-1880	1872-1892	1872-1880	1872-1892
Blacksmith	453	462	541	427	523	19	-21	-3		1
Bricklayer		457	684	671	895		-2	31	-25	13
Cabinetmaker			400	430	487		7	22	-12	14
Carpenter	376	389	478	422	492	27	-12	3	-7	14
Locomotive engineer	568	542	654	758	874	15	16	34	6	49
Locomotive fireman	330	310	356	367	488	8	3	37	19	31
Machinist	414	430	445	473	530	8	6	19	11	22
Mason	398	459	580	535	734	46	-8	27	5	37
Painter	455	417	447	528	460	-2	18	3	3	12
Pattern maker	407	435	544	474	618	34	-13	14	3	44
Plasterer	429	414	613	625	766	43	2	25		
Shoemaker			456	380	454		-17	0		
Stone cutter		438	733	640	858		-13	17	-24	9
Teamster	364	290	344	369	447	-6	7	30	12	45
Watchman	269	270	290	288	362	8	-1	25	7	16

Note: the data source is [U.S. Department of Labor \(1900\)](#). Occupations are not harmonized. Earnings are converted to 1890 dollars using inflation values from [measuringworth.com](#). Every yearly earnings score is constructed as follows. First, all state-year daily wage observations are collected which are based on at least ten individuals. For 1870-1872, 1879-1881 and 1890-1892, we take the state-year observation with the largest number of individuals. Second, the conversion of daily wage rates to yearly earnings is described in Appendix B.2. Finally, the values presented are the weighted averages of state-level scores. The weights are the number of individuals who contributed to the average wage calculation in every state. The last two columns contain only observations from Massachusetts.

3 Data

The main data sources for this work are various waves of the US full count census between 1850 and 1940 ([Ruggles et al., 2021](#)). This data set is complemented with i) novel, state- and time-varying earnings scores pre-1900 (Section 3.2); ii) newly digitized measures of county-level high school provision around 1880 (Appendix B.3); iii) the occupational education rank of [Song et al. \(2020\)](#); and iv) some development-related county characteristics from the NHGIS ([Manson et al., 2021](#)).

3.1 Linking historical censuses

Analyzing intergenerational mobility necessitates linking individuals over time across distinct waves of the full count census. In this paper, we start out with the census conducted in 1870 to find the fathers (first generation - G₁), whose offspring we follow in later decades and whose male parent (i.e. the grandfather - G₀) we find in earlier decades in subsequent parts of this analysis.⁹

A few major restrictions are made on the 1870 full father (G₁) sample. Exclusively fathers who were between 20 and 40 years old are included for two reasons. First, teenager workers tend to have transient occupations ([Papageorgiou, 2014](#)). Second, relatively old workers did not live with their kids anymore (the only way to identify family relationships) and were often not alive in 1900, the year chosen for the analysis of their long-run outcomes.¹⁰ Furthermore, we exclude every individual with a non-occupational response or outlier wealth (personal property or real estate value above the 99th percentile). Individuals who held an agricultural occupation (farmer, farm manager/foreman/laborer), reported certain apprenticeship, or their harmonized occupation was a type of "not elsewhere classified" (e.g., 'Clerical and kindred workers (n.e.c.)') are also omitted. These restrictions are important

⁹The paper is limited to the analysis of male observations since the surname change of women upon marriage makes their linking over time impossible.

¹⁰The 1890 census records were burnt in a fire.

because farmers had completely different characteristics compared to non-agricultural workers. Additionally, apprenticeships could obviously not be the final occupation of young adults. Finally, loosely classified occupations make the use of occupational education ranks or earnings scores less reliable if not impossible.

As a next step, fathers are linked to their own 1900 observation. An individual is considered linked if at least one of the two conservative linking methods offered by [Abramitzky et al. \(2020\)](#) yields a match.¹¹ These linking methods have a particularly low false positive ratio ([Bailey et al., 2020](#)). Thus, we can avoid erroneously linking observations between two different people which helps us reduce the attenuation bias at the expense of a reduced sample size. Importantly, this linking rule is used for *every* linking in the entire paper.

In 1870, we can identify sons (G₂) who lived with their father and link them to 1900 and 1940, separately. Exclusively sons who were at most 20 years old in 1870 are included. Then, we link these sons between 1870-1900 and 1870-1910, and find their kids in the respective end year in order to identify grandsons (G₃). As a final step, we link grandsons found in 1900/1910 to 1940.

In Section 6.1 and 6.3, we use the characteristics of grandfathers (G₀) in 1860. To do so, we link fathers back to 1850 and 1860. If a grandfather is only found in 1850 (e.g., because he already lived separately from the father in 1860), we link him forward to 1860 in order to obtain grandfathers' characteristics from the exact same year.

3.2 Occupational earnings scores for the late nineteenth century

One of our contributions is providing novel, state-specific earnings score estimates for the late-nineteenth-century United States. There are at least three reasons why these measures are crucial for this project. First, the traditional approach used in the literature - generating occupational income scores based on income reported in the 1940 census and using them in earlier decades - has been shown to perform more poorly the earlier it is applied prior to 1940 ([Inwood et al., 2019](#); [Saavedra and Twinam, 2020](#)). Especially for periods when relative wages are changing rapidly, [Inwood et al. \(2019\)](#) recommend constructing earnings scores based on data from the studied time period, even if the sample might not be representative. Second, a considerable share of education received was informal in the 19th century (e.g., apprenticeships; see [Goldin and Katz, 2008](#); [Kelly et al., 2020](#); [Meisenzahl and Mokyr, 2011](#)). Therefore, while we can control for the (formal) education percentile rank devised by [Song et al. \(2020\)](#), we might not be able to capture the full difference in occupational human capital across occupations with this measure. However, earnings scores combined with the educational rank might very well capture the actual level of human capital implied by the sum of formal and informal education. Third, even the labor market of the north-eastern part of the United States (New England, Middle Atlantic, East-North Central), where most of the machinists lived, was not integrated until the 1880s and the difference between the north-eastern and Pacific (or southern) regions persisted even longer ([Rosenbloom, 1996; 1998](#)). [Kaboski and Logan \(2011\)](#) also find spatially-varying returns to education in the United States in the early twentieth century. Consequently, applying the same earnings score to a certain occupation all over the United States could lead to inaccurate conclusions. To the best of our knowledge,

¹¹The conservative linking methods provided by [Abramitzky et al. \(2020\)](#) require matches be unique by name and birthplace within a five-year age band.

Table 2: Summary statistics of fathers (G1 in 1870)

Variable	Mean	Difference (machinists - non-machinists)	
	(non-machinists)	Raw difference	Conditional on state-fixed effects
Age (in years)	34,1	-0,5 [0.076] ***	-0,5 [0.089] ***
Literate (Yes=1)	0,92	0,05 [0.0082] ***	0,04 [0.0075] ***
Education rank of occupation (Song et al., 2020)	50,4	4,3 [1.182] ***	4,5 [0.968] ***
Value of real estates (in 1870 dollars)	793,4	-128,0 [32.218] ***	-74,9 [14.424] ***
Value of personal property (livestock, jewels, bonds, etc.; in 1870 dollars)	350,9	-87,7 [15.703] ***	-85,9 [26.635] ***
Both parents native born (Yes=1)	0,58	-0,10 [0.025] ***	-0.10 [0.0229] ***
Both parents foreign born (Yes=1)	0,37	0,08 [0.0226] ***	0,07 [0.0218] ***
Immigrant - UK or Ireland (Yes=1)	0,16	0,12 [0.0181] ***	0,1 [0.0173] ***
Immigrant - Germany (Yes=1)	0,15	-0,04 [0.0128] ***	-0,02 [0.009] *
Urban place of living (Yes=1)	0,44	0,34 [0.0195] ***	0,26 [0.0285] ***
Population of place of living	7553 ²	33543 [14867] **	27692 [13249] **
New England (Yes=1)	0,13	0,16 [0.0731] **	-
Middle Atlantic (Yes=1)	0,32	0,04 [0.051]	-
East-north Central (Yes=1)	0,28	-0,1 [0.0354] **	-
West-north Central (Yes=1)	0,09	-0,04 [0.0220] *	-
South (Yes=1)	0,15	-0,05 [0.0249] **	-
West and Pacific (Yes=1)	0,03	-0,01 [0.0119]	-

Note: robust standard errors clustered at the state level (1870) in brackets. The summary statistics presented pertain to the final, total sample used in Table 5 and A6. The raw difference between means of machinists and non-machinists is the coefficient on the machinist dummy in an OLS regression with a constant and the dummy. This OLS regression also includes state-fixed effects (1870) in the last column. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

all existing earnings scores data sets for the late nineteenth century provide a single score for each occupation and pertain to the last decade of the 19th century (Preston and Haines, 1991; Sobek, 1996). Hence, we proceed to construct our own measure of state-specific occupational earnings for the 1870s and 1880s.

In this section, we outline the main steps of calculating these earnings scores. The interested reader can find detailed information and the discussion of the underlying assumptions in Appendix B.2. The source of our occupational earnings information is U.S. Department of Labor (1900). For many occupations,¹² we digitized the average daily wage found in 1870-72 (the 1872 score), 1879-81 (the 1880 score), 1890-92 (the 1892 score) in every state. In case of multiple observations within a three-year period, we digitized the daily wage which was calculated based on the largest number of observations. Then, daily wages were converted to yearly earnings scores and 1890 dollars. In this way, the earnings scores could be calculated for many large, low- and medium-skilled occupations. The income of high-skilled occupations (e.g., lawyers or physicians) was imputed by combining the earnings scores provided by Sobek (1996) with our own earnings scores.

The previously described steps provide *nominal* earnings scores. However, it is well-known that the costs of living differed significantly between urban and rural areas, and across states (Koffsky, 1949; Stecker, 1937). Hence, we also calculated *real* earnings scores adjusting for these price differences following Collins and Wanamaker (2014) (see Appendix B.2 for more details).

3.3 Summary statistics

Machinists were not the "representative agents" of the US economy. As it can clearly be seen from Table 2, most of their observables differed from the rest of the population. Machinist fathers in our analysis were slightly younger, more educated, less wealthy, more likely to be immigrants (especially of English ancestry) and lived in more urban,

¹²Besides machinists, the focus was on occupations i) which are in the control group in a large number in 1870 following propensity score matching, and ii) which played a large role in the economy later (i.e., important possible occupations for fathers or sons in 1900).

larger places than non-machinists in 1870. Since they were concentrated in the New England and Middle Atlantic census divisions, one might want to disentangle the effect of spatial distribution from other causes of significant difference. Therefore, differences in means are also presented after netting out state-fixed effects. Nonetheless, machinists seem to exhibit similar, though somewhat smaller differences in characteristics within states.

4 Empirical strategy

In this section, we describe our two, complementary empirical strategies: propensity score matching and fixed effects regressions.

4.1 Propensity score matching

Our primary empirical strategy is propensity score matching on many observable characteristics of fathers in 1870 (Austin, 2011; Ho et al., 2007; Leuven and Sianesi, 2003; Rosenbaum and Rubin, 1983). This estimation strategy amounts to estimating each individual’s probability of being a machinist in a logit regression as a first step. For every machinist father, the five non-machinist fathers with the closest estimated probability are chosen as control observations with replacement.¹³ Then, we compare the outcomes of machinist fathers and of their offspring to the outcomes of matched control fathers (and of their offspring) in the resulting sample. The relatively small share of machinists in the full sample implies that there are many potential control observations, making our setting particularly well-suited for matching. The aim of matching is to reduce the correlation between the machinist dummy, which indicates if a father was a machinist in 1870, and the control variables. The full list of these control variables is shown in Appendix B.1. In short, we include i) personal characteristics (e.g., age, literacy, proxies of migration background, education rank of occupation, etc.); ii) place of living characteristics (e.g., urban dummy, state-fixed effects, measures of county-level industrialization, etc.); and iii) state-occupational level features constructed pre-1870 (e.g., probability of job switching or migration). Importantly, 1870 was the last historical census wave in which detailed information was collected on personal wealth: the value of real estates and personal property (the contemporary dollar value of all stocks, bonds, mortgages, notes, livestock, plate, jewels, and furniture owned by the respondent), separately.¹⁴ Interactions and squares of many background characteristics are also included to match the distribution of these covariates more closely (Ho et al., 2007; Imai et al., 2008).

The main advantage of matching is that by reducing the correlation between the explanatory variable of interest and observables, such as personal wealth or urban status, we considerably reduce the influence of correlated unobservables. For example, the wealth proxies are most likely correlated with individual talent and

¹³Additionally, we use a caliper of 0.01 and restrict the analysis to the common support of machinist and non-machinist fathers. This never results in losing more than ten treated observations in the main analysis. In a few analyses of later generations, we use ten instead of five neighbors because of the small sample size but this change is always duly noted.

¹⁴Wealth at a young age is an even better predictor of future wealth than parental wealth, and a good proxy for intergenerational correlation in savings behaviour and additional transfers from parents (Boserup et al., 2018).

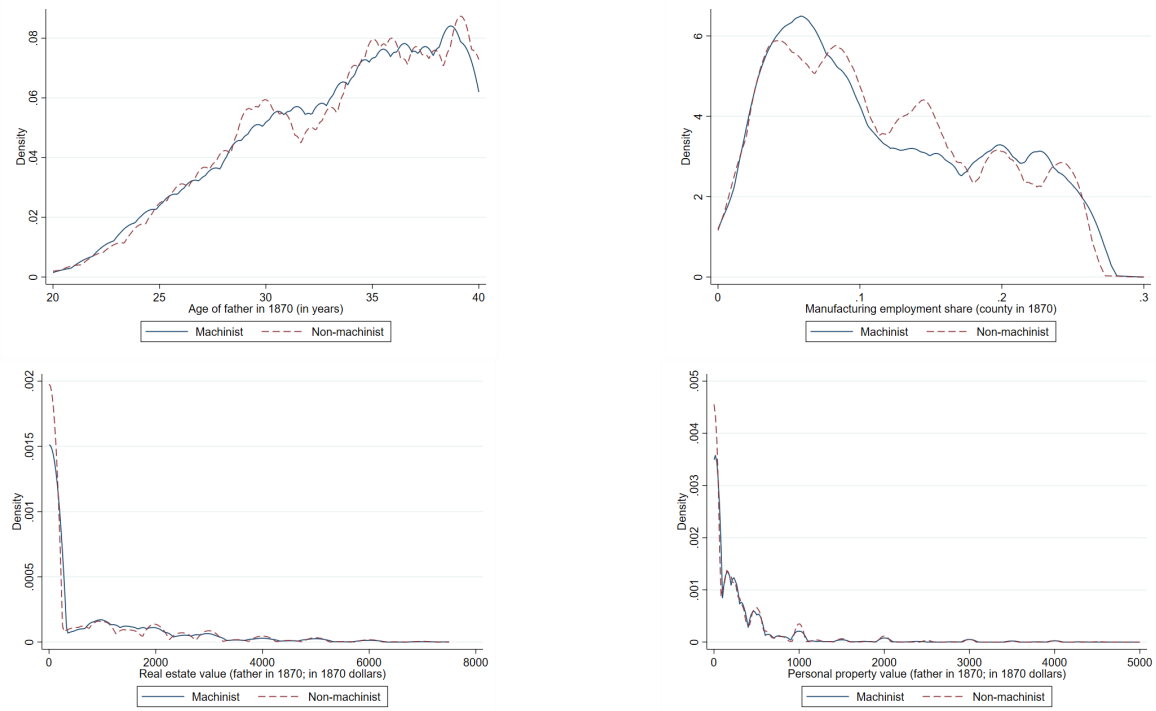


Figure 3: The histogram of some continuous characteristics after matching

Notes: the figures are created after the propensity score matching which generates Table 5. They depict the density of certain continuous control variables for machinists and matched non-machinists using weights obtained from the matching process.

family heritage, or the urban status can capture many urban (dis)amenities. Furthermore, matching diminishes our own discretion over how to control for a given background characteristic (Ho et al., 2007).¹⁵

The main limitation of using matching in our setting is that the full count census does not provide individual-level information on earnings and education before 1940. To overcome this lack of data, occupation-based characteristics are used. For education, the occupational education percentile rank of Song et al. (2020) is included. This is a percentile rank (0-100) based on the average occupational years of (formal) schooling in a person's birth cohort.¹⁶ For income, which is probably more volatile over time than the education requirement of most occupations, we use our own state-level real earnings score constructed for 1870-72. The latter is exclusively included in the analyses of income-related outcomes because its inclusion reduces the sample size along with the precision of the estimation without significantly changing the coefficients on non-pecuniary outcome variables. In fact, the real earnings score tends to be somewhat *lower* for machinists than for matched non-machinist control observations when it is not included in the list of control variables.¹⁷

¹⁵The application of propensity score matching in this paper is mostly immune to the criticism of King and Nielsen (2019) for several reasons: i) contrary to their claim that matching often increases imbalance compared to the unmatched sample, we transparently show that matching decreases it in our application; ii) the large sample makes the "propensity score matching paradox" less likely to appear; and iii) even though a caliper is used, the number of unmatched and, consequently, dropped machinists is always one-digit.

¹⁶For fathers and sons, we use the earliest available birth cohort around 1880 whose birth percentile rank is based on detailed years of schooling data and not merely on literacy. For grandsons, we use the percentile of the birth cohort around 1900.

¹⁷Machinists have an average score of \$500, while the matched (unmatched) control average is \$530 (\$582) in Table 5. Notice that this imbalance works *against* our findings.

In practice, propensity score matching works well in this setting and the correlation between observables and the machinist dummy, which is highly significant for most cases (Table 2), vanishes. Apart from similar means, the whole distribution of control covariates is closely matched (see Figure 3). However, the mean of a small subset of variables remains significantly different in some cases. The typical example is urban status: while machinists tend to be significantly *more* urban compared to the full sample, they are somewhat *less* urban in the matched one. Nevertheless, the standardized difference lies below 10% (the upper bar for tolerable difference - [Austin, 2011](#)) even in this case.¹⁸ To avoid any bias from such residual differences, we include every control variable (their main effects) which has a significantly different mean at 5% in a regression after matching.¹⁹ The other reason for running a regression on the matched sample instead of reporting the immediate outcome of matching is to construct clustered, more conservative standard errors at the state level.

The occupations with the most matched control observations are presented in Table A1. While the role of carpenters, truck and tractor drivers, and shoemakers is relatively large, none of them exceeds 10% of the control observations. We also show in Section 6 that their omission does not affect the results in any meaningful way. It must be emphasized that we use harmonized occupational codes provided by IPUMS as it is usually done in the literature. Therefore, the category 'Truck and tractor drivers' mainly consists of teamsters, draymen and hackmen in the 19th century.

4.2 Fixed effects regression

Despite the appealing features of propensity score matching, it precludes the inclusion of numerous fixed effects for two reasons: the algorithm occasionally does not converge when including county or county-urban status-fixed effects, and the small size of the matched subsample makes the estimation of fixed effects very imprecise. Another problem with matching is that it does not allow for weighting, so the sample cannot be weighted to make it representative of the US population (more details in Section 6.2). To address these issues, we also present some results using fixed effects regressions.

In our fixed effects regressions, exactly the same baseline controls are included as in matching in addition to county-fixed effects (1870).²⁰ Therefore, the offspring of machinists are compared to the offspring of non-machinists who lived in the exact same county in 1870 and had similar paternal (G1) observables. To bring this analysis in spirit closer to matching, fathers whose occupation is below the 25th or above the 85th educational rank percentile are omitted from the analysis (the rank of machinists is the 55th). In this way, the very low-skilled (e.g., lumbermen or miners) and high-skilled (e.g., architects or lawyers) fathers are not in the sample so that we can focus on the "middle class". Another advantage of fixed effects regressions is that they allow us to precisely estimate interaction

¹⁸In Table 3, the difference (machinist minus non-machinist) between the probability of urban place of living in 1870 is 34% before matching and -4% after matching. In this particular application, the mean (median) standardized bias is 15.4 (9.2) before matching and 2.2 (1.3) after matching.

¹⁹We are aware of the "balance test fallacy" coined by [Ho et al. \(2007\)](#) and [Imai et al. \(2008\)](#), who discourage researchers to use the significance of difference between means as a balancing threshold. However, we find in practice that the inclusion of significantly different (p-value below five percent) characteristics matters to a very limited extent and the inclusion of non-significantly different variables does not have any effect on the estimation.

²⁰We use the *reghdfe* package in Stata by [Correia \(2016\)](#).

terms between the machinist dummy and other variables as well.

Formally, the regression specification takes the following form:

$$y_{s,f,c,1900} = \beta \cdot \text{Machinist}_{f,1870} + \gamma \cdot x'_{f,1870} + \delta_{c,1870} + \epsilon_{s,f,c,1900} \quad (1)$$

where $y_{s,f,c,1900}$ represents an outcome variable for son s of father f measured in 1900 (e.g., a binary variable if the son held an agricultural occupation). The explanatory variable of interest is $\text{Machinist}_{f,1870}$, which equals one if the father was a machinist in 1870. County-fixed effects ($\delta_{c,1870}$) and all paternal baseline controls ($x_{f,1870}$) are also included. Reassuringly, the effects on main outcomes estimated by propensity score matching and fixed effects regressions tend to be quantitatively and qualitatively very similar.

In order to get a consistent estimate of β , the error term, $\epsilon_{s,f,c,1900}$, must be uncorrelated with the machinist dummy conditional on our predetermined controls. Thus, the main concern about the validity of the empirical strategy is that particularly talented fathers sorted into the machinist occupation before 1870 in an unobserved way, causing omitted variable bias. To alleviate this concern, we present many heterogeneity and robustness checks in Sections 5.2 and 6. These empirical exercises suggest that (a within-family intergenerational transmission of) unobserved ability is not driving our results.

5 Main results

In the first part of this section, key results establishing the gains of the machinist occupation post-1870 and the intergenerational transmission between machinists and their (grand)sons are presented. To elaborate on mechanisms of transmission, we conduct some heterogeneity exercises in the second part.

5.1 Long-term effects and intergenerational transmission

Fathers (G1) between 1870 and 1900 Table 3 contains the main, non-pecuniary outcomes for our linked 1870-1900 father sample using propensity score matching. The first column shows that machinists were 8.7 percentage points (0.2 standard deviation) less likely to switch their occupation. This coefficient can be decomposed into switching to different types of jobs. In particular, roughly one-third of the total effect stemmed from a lower likelihood of switching to an agricultural job (Column 2), while the rest can be attributed to a less likely change for another non-agricultural occupation (Column 3). We interpret the lower likelihood of leaving the initial occupation as the first sign of a beneficial effect on machinists post-1870. Namely, there is an extensive literature which documents the large costs of occupation switching in many contexts (e.g., [Artuç et al., 2010](#); [Cortes and Gallipoli, 2018](#); [Dix-Carneiro, 2014](#); [Kambourov and Manovskii, 2009](#); [Sanders and Taber, 2012](#); [Traiberman, 2019](#)). This literature suggests that machinists lost less lifetime earnings caused by the costly accumulation of occupation- or task-specific human capital due to their lower likelihood of changing their occupation.

Internal migration has been established as a pre-eminent way to upward mobility in the studied time period

Table 3: Main outcomes - fathers (G1; 1870-1900)

	Occupational change [(1) = (2)+(3)]			Migration (Yes=1)		Place of living (1900 - Yes=1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any occupation (Yes=1)	Agricultural	Non-agricultural	Within-state	Across states	Higher population than in 1870	Urban
Machinist (G1)	-0.087*** (0.010)	-0.033*** (0.006)	-0.054*** (0.012)	0.006 (0.007)	0.011 (0.008)	0.058*** (0.012)	0.073*** (0.010)
Mean of outcome	0.77	0.19	0.57	0.20	0.37	0.46	0.50
Standard deviation of outcome	0.42	0.39	0.49	0.40	0.48	0.50	0.50
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	18811	18811	18811	18811	18811	18811	18811
Number of clusters	50	50	50	50	50	50	50

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 3902 matched machinist fathers. The outcome variable is a binary variable which equals one if the father changed occupation (Col. 1), changed occupation and the new occupation is agricultural (Col. 2 - farmer, farm manager/foreman/laborer) or non-agricultural (Col. 3), migrated within-state across counties (Col. 4) or across states (Col. 5), his place of residence fell into a larger SIZEPL category in 1900 than in 1870 (Col. 6), he lived in an urban place in 1900 (Col. 7). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(J. Long and Ferrie, 2007; 2013; Ward, forthcoming). However, no evidence is found on a differential probability of migration within or across states (Columns 4-5). We further elaborate on migration *destinations* in Table A2. First, we decompose the insignificant migration differential and find that machinist fathers tended to migrate significantly more (less) to urban (rural) places. Second, we also establish that initially rural machinists were particularly more likely to move to urban areas and initially urban machinists were less likely to migrate to rural areas. These effects can clearly be seen in Columns 6-7 of Table 3 as well: machinist fathers lived in more populous and more urban places by 1900 (both effects stronger than 0.1 standard deviation). The urban environment could provide them and their offspring with better opportunities in a period when urbanization and growth were tightly intertwined.

Next, we direct our attention to analyze the effect on occupational earnings scores. In Columns 1-2 of Table 4, we assume that fathers held the same occupation and lived in the same place in 1880 as in 1870. We do so because an additional linking to 1880 would come at the expense of a large sample size reduction. The coefficients suggest that machinist fathers experienced a relative increase of 8-9 log-points in their earnings score.²¹ This finding is unsurprising since it is documented in Table 1 that the relative wage of the machinist occupation increased compared to most other occupations in this time period. While the magnitude of the effect is substantial (0.25 s.d.), we treat it as an *upper* bound on the actual effect because control fathers could switch their occupation or place of living in order to reduce the relative earnings gap. Therefore, we also linked fathers between 1870 and 1880 (instead of 1900) and, thus, allowed for occupation and place of living change in Table A3. As expected, the estimated earnings effect is somewhat smaller (6-7 log-points) but still significantly positive.

In the last four columns of Table 4, we use the occupation and state of living of fathers in 1900 to construct outcome variables. The first observation is that both the nominal and real earnings score gains expectedly declined compared to 1880. The second observation is that using the widely-used earnings scores (Preston and Haines, 1991; Sobek, 1996) results in a larger coefficient compared to our own nominal score.²² We suspect that this

²¹The same coefficient on the nominal and real earnings score is mechanical. Since we assume that fathers do not change their occupation, state and urban status between 1870 and 1880, only the nominal wage change of the given occupation matters in this calculation.

²²Preston and Haines (1991) do not provide an earnings score for owner-occupier farmers and calculate earnings scores based on an urban sample in the Cost of Living survey. Sobek (1996) instead calculates an unweighted average of all distinct earnings scores for every occupation.

Table 4: Measures of economic status (medium- and long-run) - fathers (G1)

	State-occupation in 1870		State-occupation in 1900			
	(1) State-level nominal log-score (1880)	(2) State-level real log-score (1880)	(3) Sobek log-score	(4) State-level nominal log-score (1892)	(5) State-level real log-score (1892)	(6) Preston-Haines log-score
Machinist (G1)	0.085* (0.048)	0.085* (0.048)	0.067*** (0.009)	0.034*** (0.011)	0.019 (0.012)	0.084*** (0.012)
Mean of outcome	6.11	6.25	6.21	6.25	6.37	6.49
Standard deviation of outcome	0.33	0.34	0.56	0.44	0.43	0.37
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	16124	16124	11573	11573	11573	9895
Number of clusters	32	32	47	47	47	50

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1870 in Col. 1-2; 1900 in Col. 3-6) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 3820 (Col. 1-2), 2669 (Col. 3-5) and 2334 (Col. 6) matched machinist fathers. The outcome variable is the state-level nominal and real log-score (Col. 1 and 2.) merged to the state and occupation of fathers in 1870; the Sobek, state-level nominal and real, and Preston-Haines log-score (Col. 3-6) merged to the state and occupation of fathers in 1900. Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

discrepancy partly stems from the treatment of agricultural workers. In particular, the ratio between the score of farm laborers and other laborers is substantially lower in Sobek (1996) or Preston and Haines (1991) than in the case of our scores. Knowing that machinists were significantly less likely to switch to agricultural occupations, assigning lower scores to agricultural jobs amplifies the relative earnings gains of machinists. We believe that our scores might be more accurate since Alston and Hatton (1991) or Hatton and Williamson (1991) show that a large part of the gap in nominal earnings between farm and common laborers can be explained by more in-kind benefits (especially the value of accommodation) for the former group. As explained in Appendix B.2, we calculate farm laborers' remuneration based on daily wages *without* accommodation which brings the ratio between the earnings of farm and common laborers close to those reported in Alston and Hatton (1991) and Hatton and Williamson (1991), and takes into account the monetary value of accommodation. Nevertheless, the 3.5-8 log-points higher nominal earnings scores do not account for the fact that machinist fathers were more likely to reside in more populous, urban places in 1900 - implying higher consumer prices. When these differences in cost of living are adjusted for, the estimated positive effect becomes insignificant (Column 5). In other words, the real gains of initially machinist fathers were arbitrated away in the (very) long run.

We further investigate the effect on earnings scores in Table A4, focusing on individuals who changed their occupation between 1870 and 1900. The main takeaway of this table is that, besides the increasing relative wage of the machinist occupation, the relative earnings gain of initially machinist fathers was the result of a six percentage points lower likelihood of switching to an occupation with considerably lower earnings rather than differential upward mobility. In our interpretation, non-machinists lost their occupations more frequently in the turbulent times of the Gilded Age, when recurrent busts in the aftermath of panics characterized an overall robust growth. As breadwinners of their family, they had to find an alternative, potentially lower-paying (agricultural) occupation in the absence of generous unemployment benefits. This interpretation is consistent with Boone and Wilse-Samson (2019) who show that movement to farms served as a source of migratory insurance during the Great Depression. Moreover, the fact that machinists were not more likely to switch to managerial jobs or becoming proprietors (Column 6) supports our claim that the improved outcomes for themselves and their offspring were not the result

Table 5: Main outcomes - sons (G2; 1900)

	Occupational characteristics		Migration (Yes=1)		Place of living			Personal characteristics		
	(1) Agricultural occ. (Yes=1)	(2) Education rank	(3) Within-state	(4) Across states	(5) Urban (Yes=1)	(6) Higher population than in 1870 (Yes=1)	(7) Manuf. emp. per capita (county)	(8) # of children	(9) Married (Yes=1)	(10) Owning house (Yes=1)
Machinist (G1)	-0.028*** (0.007)	2.403*** (0.745)	-0.009 (0.011)	0.043*** (0.012)	0.045*** (0.011)	0.033** (0.016)	0.004 (0.003)	-0.080** (0.038)	0.005 (0.010)	0.000 (0.014)
Mean of outcome	0.19	49.68	0.30	0.31	0.55	0.52	0.09	1.70	0.77	0.43
Standard deviation of outcome	0.39	28.25	0.46	0.46	0.50	0.50	0.07	1.86	0.42	0.50
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	8745	8745	8745	8745	8745	8745	8745	8745	8745	8745
Number of clusters	45	45	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 1842 matched machinist sons. The outcome variable is an agricultural occupation indicator (Col. 1 - farmer, farm manager/foreman/laborer), the education rank of occupation (Col. 2), a binary variable which equals one if the son migrated within-state across counties (Col. 3) or across states (Col. 4) between 1870 and 1900, a binary variable which equals one if the son lived in an urban place in 1900 (Col. 5) or his place of residence fell into a larger *SIZEPL* category in 1900 than in 1870 (Col. 6), manufacturing employment per capita (as % of total county population; Col. 7), the number of children in the household (Col. 8), a marriage status (Col. 9) and house ownership (Col. 10) indicator. Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of unobserved talent.

The outcomes of sons (G2) The next question we answer is if the benefits of fathers could be transmitted to their sons. The main, non-pecuniary outcomes are presented in Table 5. Similarly to their fathers, sons were significantly less likely to hold an agricultural occupation (Column 1). Furthermore, they held occupations which had significantly higher education ranks (almost +0.1 s.d.). Whereas the latter finding simply suggests that machinists' sons held occupations with on average more educated peers, we can estimate *individual-level* schooling using the 1940 census. We linked sons between 1870 and 1940 to this end. The results in Table A5 show that machinists' sons had indeed 0.21 years more schooling. The effect is mainly the result of a 3.6 percentage points (+0.1 s.d.) higher likelihood of having some secondary education, meanwhile the effect on university education is a tightly estimated zero. The secondary school coefficient should be treated as a *lower* bound on the actual effect since the beneficial effect of education on longevity could lead to endogenous attrition. Thus, as sons were at least seventy years old in 1940, the less educated control sons might have been more likely to pass away before 1940, leading to a downward bias in the estimated coefficient.

In line with the higher level of educational attainment, we find a significantly higher probability of long-distance migration for sons between 1870 and 1900 (+0.1 s.d. - Column 4 in Table 5; Malamud and Wozniak, 2012; Rosenbloom and Sundstrom, 2003; Wozniak, 2010). This foreshadows our findings on higher earnings because the migration premium increased in distance in this time period (Ward, forthcoming). The effect on a higher probability of home and larger cities persists, though it slowly starts to fade away compared to the first generation.²³ In fact, the difference in the manufacturing employment share of the county of living in 1900 is insignificant. This suggests a certain convergence in the type of place of living across the sons of machinists and non-machinists. Finally, we uncover some evidence that the more educated sons of machinists had fewer kids, perhaps because they faced higher opportunity costs of raising children (Ager et al., 2020). The effect on marriage probability and house ownership is insignificant.

The pattern of the earnings effect for sons is similar to the paternal one: the well-known nominal scores having a more positive coefficient than our own score, and a diminished coefficient once across-state and rural-urban price differences are accounted for (Table 6).

²³For instance, the positive effect of an urban place of living drops by 60% in magnitude, from 1.5 to 0.9 standard deviations.

Table 6: Measures of economic status - sons (G2; 1900)

	(1) Sobek log-score	(2) State-level nominal log-score	(3) State-level real log-score	(4) State-level real score (level)	(5) Preston-Haines log-score
Machinist (G1)	0.070*** (0.014)	0.042*** (0.011)	0.032*** (0.010)	15.208** (6.452)	0.069*** (0.011)
Mean of outcome	6.23	6.23	6.35	628.89	6.45
Standard deviation of outcome	0.53	0.43	0.41	294.05	0.39
Unbalanced controls	Yes	Yes	Yes	Yes	Yes
Sample size	6812	6812	6812	6812	6687
Number of clusters	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 1548 (1514 in Col. 5) matched machinist sons. The outcome variable is the Sobek, state-level nominal and real log-score (Col. 1-3), the state-level real score in levels (Col. 4) and the Preston-Haines log-score (Col. 5). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Main outcomes - grandsons (G3; 1940)

	Occupational characteristics				Education		Personal characteristics		
	(1) Agricultural occ. (Yes=1)	(2) Weeks worked	(3) Self-employed (Yes=1)	(4) Education rank	(5) # of grades completed	(6) More than primary education (Yes=1)	(7) # of children	(8) Married (Yes=1)	(9) Owned a house (Yes=1)
Machinist (G1)	-0.024** (0.011)	0.865** (0.408)	0.013 (0.014)	1.902*** (0.666)	0.312*** (0.113)	0.047** (0.020)	-0.002 (0.061)	0.016 (0.012)	0.023 (0.022)
Mean of outcome	0.10	44.29	0.22	31.95	9.90	0.54	1.52	0.87	0.53
Standard deviation of outcome	0.30	14.33	0.42	19.04	3.26	0.50	1.66	0.33	0.50
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	6383	6383	6383	6383	6383	6383	6383	6383	6383
Number of clusters	49	49	49	49	49	49	49	49	49

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1940) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 969 matched machinist grandsons. We use ten matched control observations instead of five owing to the small number of machinist grandsons. The outcome variable is an agricultural occupation indicator (Col. 1 - farmer, farm manager/foreman/laborer), the number of weeks worked (Col. 2), a self-employed status indicator (Col. 3), education rank of occupation (Col. 4), highest grade of schooling (Col. 5 - winsorized at the 99th percentile in the final sample), a binary variable which equals one if the highest grade of schooling is at least 9 years (Col. 6), number of children in the household (Col. 7), a marriage status (Col. 8) and house ownership (Col. 9) indicator. Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The outcomes of grandsons (G3) Table 7 documents the main, non-pecuniary outcomes for machinists' grandsons. The set of possible outcomes is richer thanks to the increased data collection effort in the 1940 census. First, we learn that even the grandsons were less likely to be engaged in an agricultural occupation, they worked more weeks, but did not have a differential likelihood of self-employment (Columns 1-3). The availability of individual-level educational attainment allows us to compare the magnitude of the effect on the occupational education rank and on the highest grade of individual-level schooling (Columns 4 and 5). Reassuringly, both variables imply a very similar, positive magnitude: 0.1 standard deviation. This comparison corroborates our entire analysis because we seem to approximate actual education very closely with occupation-level average education scores. We also see that machinist grandsons were almost five percentage points more likely to have completed at least primary school. However, we do not find any significant effect on the number of children, marriage probability and house ownership indicator (though the signs of the coefficients are in the expected direction).

The final piece of main results concerns the income of grandsons (Table 8). The first four columns include wage earner as well as self-employed grandsons. As self-employed individuals did not report their income, we impute it following the best practice in the literature (see Appendix B.4.3). The results are clear: both the nominal and the real wage effect are positive and significant. This conclusion becomes even stronger when we focus

Table 8: Measures of income - grandsons (G3; 1940)

	Self-employed & wage workers				Wage workers			
	(1) Log-wage (nominal)	(2) Log-wage (real)	(3) Wage (level)	(4) Non-wage income (Yes=1)	(5) Log-wage (nominal)	(6) Log-wage (real)	(7) Wage (level)	(8) Non-wage income (Yes=1)
Machinist (G1)	0.068** (0.034)	0.061* (0.034)	112.774** (52.069)	-0.009 (0.012)	0.082** (0.036)	0.074** (0.037)	117.019** (55.541)	-0.026** (0.011)
Mean of outcome	7.26	0.20	1855.92	0.30	7.21	0.15	1771.97	0.17
Standard deviation of outcome	0.82	0.81	1278.30	0.46	0.82	0.81	1207.89	0.38
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	6212	6212	6212	6212	5244	5244	5244	5244
Number of clusters	49	49	49	49	49	49	49	49

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1940) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 908 (746 in Col. 5-8) matched machinist grandsons. We use ten matched control observations instead of five owing to the small number of machinist grandsons. The outcome variable is the log of reported nominal wage (Col. 1 and 5 - winsorized at the 95th percentile in the final sample), the log of reported real wage (Col. 2 and 6 - winsorized at the 95th percentile in the final sample), the level of reported nominal wage (Col. 3 and 7 - winsorized at the 95th percentile in the final sample), and a meaningful non-wage income indicator (Col. 4 and 8. - more than \$50). The sample includes wage workers as well as self-employed people reporting non-zero wage in Columns 1-4, while it is restricted to wage earners in Columns 5-8. The imputation of self-employed income is described in Appendix B.4.3. Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

exclusively on wage earners whose wages do not require imputation (Columns 5-8).

In conclusion, we document large and significant gains for the sons and grandsons of machinists in terms of education- and income-related outcomes even after seventy years. The implied limited level of intergenerational mobility²⁴ is consistent with a large literature which demonstrates that intergenerational mobility was indeed low and declined at the turn of the twentieth century in the United States (J. Long and Ferrie, 2013; Olivetti and Paserman, 2015; Song et al., 2020; Ward, 2019). Therefore, the initial gains of machinist fathers dissipated slowly over time and generations.

5.2 Mechanisms behind the intergenerational transmission

The goal of this section is to understand the mechanism behind the intergenerational transmission of the improved socio-economic status of machinist fathers to their offspring. We focus on the transmission from fathers to sons since the small sample size for grandsons does not let us draw robust conclusions.

Secondary education as a pathway to upward mobility Education meant at most primary schooling for the overwhelming majority of young people in the late-nineteenth-century United States: merely nine percent of American youth had high school diploma even in 1910. This share only moderately increased from the 1870s until the start of the so-called High School Movement in the 1900s. In the studied time period, high schools were mostly attended by the children of the (upper)-middle class. To a lesser extent, farmers or manual workers also sent their offspring to study as they saw high school education as a way out of a rural life and physical toil for their children. Rural areas maintained mostly private high schools and only cities could afford to finance public high schools. Private secondary schools regularly charged a tuition fee and non-residents were expected to pay a boarding fee (cost of accommodation) as well, meanwhile public institutions normally did not demand any payment. Nevertheless, the role of public schools remained inferior to private institutions until the 1890s. Therefore, in the absence of strictly implemented compulsory schooling laws for secondary schooling, it mainly

²⁴Around 65% of the earnings gains of fathers (Column 1 in Table A3) were transmitted to their sons (Column 2 in Table 6), which is consistent with the values reported in Ward (2019).

Table 9: Heterogeneity by the level of private tuition fee - sons (G2; 1900)

	Education rank						Other outcomes (medium fee)	
	(1) Full sample	(2) Low tuition fee (<25th percentile)	(3) Medium tuition fee (25th-75th percentile)	(4) High tuition fee (>75th percentile)	(5) Medium tuition fee (below median public HS)	(6) Medium tuition fee (teacher-pupil ratio>0.04)	(7) Max. primary education (% in occupation)	(8) Sobek log-score
Machinist (G1)	4.207*** (0.928)	6.454*** (1.491)	4.000*** (1.270)	3.143 (1.900)	4.369** (2.074)	1.477 (1.625)	-2.067** (0.869)	0.091*** (0.024)
Private high school (%) x Machinist (G1)	0.893* (0.512)	-2.225 (1.543)	3.574*** (0.973)	0.292 (0.854)	4.803*** (1.278)	4.368*** (1.191)	-2.145*** (0.794)	0.035* (0.020)
Manufacturing emp. (%) x Machinist (G1)	0.058 (0.491)	0.094 (0.978)	0.526 (0.637)	-0.131 (1.124)	0.510 (1.088)	1.979** (0.926)	-0.390 (0.477)	-0.016 (0.013)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	39570	9871	19742	9957	6455	13511	19742	19181
Number of clusters	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons who were not older than ten years in 1870, and whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. The sample is additionally restricted to sons who lived in a county in 1870 i) with private tuition fee below the 25th percentile (Col. 2) / between the 25th and 75th percentiles (Col. 3 and 5-8) / above the 75th percentile (Col. 4); ii) with below median public high school share (male public high school students as % of 14-20 year-old males in the county in 1880 - Col. 5); iii) with an average teacher-pupil ratio above 0.04 in private schools. The outcome variable is the education rank of occupation (Col. 1-6), the share of workers who had at most primary education in the son's occupation (Col. 7) and the Sobek log-score (Col. 8). The share of private high school students and of manufacturing employment (as % of county population in 1870) are winsorized at the 99th percentile and standardized. Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

depended on their parents' income and preferences if the sons of machinists and their peers received post-primary education (e.g., [Goldin, 1998](#); [Goldin and Katz, 2000; 2008](#); [Lingwall, 2010](#); [Tyack, 1974](#)).

We documented earlier that machinist fathers experienced occupational stability and higher earnings in the period when most sons in the sample reached high school age around 1880. Thus, they could afford to educate their sons more easily. Indeed, we present evidence consistent with a complementarity between local private secondary school provision and parental income. Additionally, it has been demonstrated that parents of similar socio-economic status tend to have similar preferences over the schooling of their kids ([Boneva and Rauh, 2018](#)). Thus, we do not expect these (unobserved) preferences to drive the findings. Moreover, our subsequent findings are inconsistent with preferences for more schooling at every tuition fee (cost of education) level.²⁵

First, the effect of private high school provision is studied by interacting the machinist main effect with the share of boys who attended high school in the county. We assume that sons still lived in the county where they were located in 1870 when they reached high school age. In addition, only those sons are included who were not older than ten years in 1870, so that they were not too old to benefit from secondary education and reached high school age around 1880 - the year which our schooling measure corresponds to. Every specification includes an interaction with the county-level share of manufacturing employment as well, so that we can avoid that the results are driven by the known negative association between high schooling and industrialization ([Goldin and Katz, 1999](#)). Both the high school provision and industrialization proxy are standardized in the full sample. This means that the coefficients can be interpreted as the effect of one standard deviation increase in the given variable.

The results are presented in Table 9. When the tuition fee was neither too cheap (so the main cost of schooling was the foregone wage and practically everyone could attend high school; Column 2) nor prohibitively expensive even for machinists (Column 4), the sons of machinists benefited from the increased availability of private high schools. At mean private high school provision, a machinist son had an occupation with a four percentiles higher education rank. If he instead grew up in a county with a one standard deviation lower high school provision, the entire positive effect might have vanished (Column 3). To strengthen our increased parental investment interpretation, we show that the identified positive coefficient on the interaction term is driven by counties which had low public high school provision, i.e. boys could mainly pursue secondary education at

²⁵The entire schooling data collection and preparation process is described in Appendix B.3.

private schools as public high school provision was very limited (Column 5). Additionally, Column 6 establishes that the coefficient on the interaction term is particularly large across counties with high-quality private secondary schools (high teacher-pupil ratio - [Card and Krueger, 1992](#); [Chetty et al., 2014](#)). The last two columns show that the complementarity between a machinist father (income effect) and local private high school provision also manifests itself for other relevant outcomes. Better private high school provision at medium tuition fee level led to machinists' sons having fewer people with at most primary education in their occupation ([Song et al., 2020](#)) and a larger increase in their earnings score.

Second, we study the effect of public secondary education supply in large cities (at least 7,500 inhabitants in 1880). In the standard model of [Becker and Tomes \(1986\)](#), the effect of a higher income of machinist fathers allowing their sons to stay in school longer should be diminishing in the expansion of the mostly free of charge, public high school system. This happens if parents faced restrictions to borrowing or savings and public schooling purely substituted for private schooling.²⁶ The test of this hypothesis is presented in Table 10. At mean public high school provision, an urban machinist's son had a three percentage points higher occupational education rank compared to sons of non-machinists. However, half of this relative gain was lost in counties with one standard deviation higher public high school provision (e.g., Akron, OH, Hartford, CT or Richmond, VA). Compared to these places, the gains of machinists' sons were three times larger in cities with one standard deviation below the mean (e.g., Indianapolis, IN, Jersey City, NJ or Joliet, IL). Columns 2 and 3 show that the other two outcomes of interest were influenced by expanding public secondary schools in a similar way. Column 4 establishes that the expansion of public schools particularly mattered under medium private tuition fee, in line with the previous analysis of private high schools. Exclusively urban sons were included in the estimation so far, even though [Goldin and Katz \(2008\)](#) write that township public schools sometimes educated the youth of the urban center as well as those of nearby rural communities. Therefore, the sample is expanded with rural sons within the county of large cities in Column 5. The interaction coefficient becomes somewhat smaller, suggesting that the effect is driven by the urban subsample who grew up in the physical proximity of schools. In Column 6, cities with more than 100,000 inhabitants are excluded from the sample which makes the interaction term even larger in magnitude.

The extent of local public high schooling was influenced by other factors than the level of industrialization (high opportunity cost of staying in school in industrialized counties) as well. Wealthier, more equal and stable communities tended to be associated with a more abundant public high school supply. Using proxies following [Goldin and Katz \(1999\)](#), we demonstrate that our interaction with public schooling does not capture, for instance, the beneficial effect of wealthier residents who, in turn, were willing to invest in public schools. In Column 7, two wealth proxies are included, but the coefficient of interest remains practically unaffected. We use the wealth share of the top 1% of residents as a proxy for wealth inequality and the share of elderly people to capture the stability of the local community in Column 8. Interestingly, the machinist effect seems to increase in local wealth inequality. We suspect that this effect is attributable to the presence of wealthy factory owners who

²⁶[Goldin and Katz \(2008\)](#) report that the tuition fee itself was on average 5% of the gross earnings of skilled workers. The boarding fee could double or triple the costs. The recent empirical evidence on credit constraints is discussed in the introduction.

Table 10: Heterogeneity by the supply of public schooling - sons (G2; 1900)

	Full urban sample			Dependent variable: education rank				
	(1) Education rank	(2) Max. primary education (% in occupation)	(3) Sobek log-score	(4) Medium tuition fee	(5) Rural and urban county population	(6) No large cities	(7) Wealth proxies	(8) Inequality and old population
Machinist (G1)	3.033*** (1.068)	-1.770*** (0.610)	0.029 (0.018)	4.557*** (1.640)	3.179*** (0.793)	2.724* (1.396)	2.804** (1.168)	1.991* (1.060)
Public high school (%) x Machinist (G1)	-1.644*** (0.588)	1.031*** (0.359)	-0.022* (0.012)	-2.587*** (0.743)	-1.302** (0.522)	-2.379*** (0.769)	-1.475*** (0.540)	-1.940*** (0.526)
Manufacturing emp. (%) x Machinist (G1)	0.534 (0.559)	-0.183 (0.303)	0.011 (0.012)	-0.296 (0.783)	0.703 (0.511)	0.438 (0.826)	0.819 (0.674)	-0.822 (0.972)
Agricultural production per agric. worker x Machinist (G1)							2.234 (1.496)	
Wealth per capita x Machinist (G1)							-0.613 (1.061)	
Top 1% share of wealth x Machinist (G1)								4.781** (1.863)
Elderly population (%; above 65 y.o.) x Machinist (G1)								2.239* (1.317)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	19393	19393	18767	9901	26555	12424	19393	19393
Number of clusters	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons who were not older than ten years in 1870, whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870, and who lived in places classified as urban in 1870. Additionally, the sample is restricted to sons in counties with medium tuition fee (Column 4 - see Table 9), is expanded to include rural sons as well within a county (Column 5), and does not include sons in cities with more than 100,000 inhabitants in 1870 (Column 6). The outcome variable is the education rank of occupation (Col. 1 and 4-8), the share of workers who had at most primary education in the son's occupation (Col. 2) and the Sobek log-score (Col. 3). The share of public high school students (as % of 12-20 years old males in the county in 1880), the share of manufacturing employment (as % of county population in 1870), the agricultural production per agricultural worker (the estimated yearly, county-level agricultural production is from [Manson et al. \(2021\)](#)), while the number of agricultural workers is from the 1870 full count census - agricultural workers are farmers, farm managers, foremen and laborers), the wealth per capita (calculated as the total wealth in a county - the sum of real estates and personal property - divided by county population in 1870), the top 1% share of wealth (calculated as the county-level wealth - sum of real estate and personal property - share of the richest one percent in 1870; only males who were above 16 years old), and share of elderly people (share of people older than 65 years in the 1870 full count census) are winsorized at the 99th percentile and standardized. Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

utilized modern, mechanized production methods in their establishments, requiring the intensive involvement of machinists. Alternatively, intergenerational mobility might have simply been lower in counties with higher concentration of wealth ([Chetty and Hendren, 2018b](#); [Chetty et al., 2014](#)), which could make the catch-up of non-machinists' sons more difficult. Nonetheless, the interaction with public high schooling becomes even more negative in this column.²⁷ We conclude that the provision of public high schooling could dampen the difference between the offspring of machinists and non-machinists, in line with [Becker and Tomes \(1986\)](#). This result also suggests that machinist families did not have particularly strong preferences for education, since otherwise they could have sent their sons to college using money saved from substituting private with free public high school education or, simply, let their sons stay in public high school longer.

Third, being able to decipher blueprints, having some elementary knowledge of algebra or chemistry, and mechanical drawing skills were all valuable on the labor market in the late nineteenth century ([Goldin and Katz, 2000](#); [2008](#)). The sons of machinists could easily learn many of these skills from their fathers, thereby gaining some advantage outside formal schooling - which we call the information channel. However, schools increasingly started to incorporate scientific subjects into their curriculum which may have decreased the benefits of machinists' sons. To test this hypothesis, we use the Reports of the Commissioner of Education. These volumes contain relevant information - if the given school taught mechanical drawing or had a chemical laboratory - on two types of private high schools: institutions for secondary instruction and preparatory schools. We calculate the share of high school students whose school replied with a yes to any of the two questions. The underlying assumption is that these institutions put an emphasis on technical education in their curriculum. The interaction between technical education at school and a machinist father is estimated in Table 11. In line with our hypothesis, offering

²⁷The highly significant interaction term in Column 3 of Table 9 also survives the inclusion of these control interactions.

Table 11: Information channel - sons (G2; 1900)

	Full sample	Low tuition fee (below city median)	Low tuition fee & population > 5,000 (city in 1870)		
	(1)	(2)	(3)	(4)	(5)
	Education rank	Education rank	Education rank	Max. primary education (% in occupation)	Sobek log-score
Machinist (G1)	4.502*** (0.787)	5.691*** (0.894)	3.846** (1.449)	-2.277** (0.979)	0.059* (0.032)
Technical education (% of HS students) x Machinist (G1)	-0.643 (0.652)	-1.852** (0.781)	-3.489** (1.458)	1.616* (0.945)	-0.049** (0.019)
Manufacturing emp. (%) x Machinist (G1)	-0.065 (0.539)	-0.621 (0.888)	0.862 (1.499)	-0.479 (0.868)	0.013 (0.028)
Baseline controls	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes
Sample size	34475	20746	8327	8327	8070
Number of clusters	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons who were not older than ten years in 1870 and whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. The sample is additionally restricted to sons who lived in 1870 i) in a county with private tuition fee below the median of cities (places with more than 5,000 inhabitants in 1870 - Col. 2-5) and ii) in places with more than 5,000 inhabitants in 1870 (Col. 5). The outcome variable is the education rank of occupation (Col. 1-3), the share of workers who had at most primary education in the son's occupation (Col. 4) and the Sobek log-score (Col. 5). The share of technical education (% of private high school students - institutions for secondary instruction or preparatory schools - whose school had a chemical laboratory or taught mechanical drawing) and of manufacturing employment (as % of county population in 1870) are winsorized at the 99th percentile and standardized. Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

technical education decreased the relative gains accruing to machinists' sons, but only if private high schools were accessible to "rival" boys too (relatively low tuition fee; Column 2). Moreover, this effect is particularly strong in cities, where the benefits of technical skills could be reaped in manufacturing production, as opposed to rural areas and is also present for other potential outcomes (Columns 3-5).

A decomposition of gains in earnings A simple goal is set in this subsection: understanding and quantifying to what extent the earnings effects of machinists' sons are driven by rural-urban differences and education.

First, we split the nominal earnings effect between sons who resided in villages (settlements with less than 5,000 inhabitants) and in cities in 1870. The first column of Table 12 shows that rural machinists' sons had significantly larger earnings gains: they had on average 4.7 log-points higher nominal earnings scores relative to city-dweller machinists' sons. A possible explanation is that the sons of rural machinists, being more educated than their peers, migrated to urban places more intensively or they migrated with their father to these areas and, thereby, had access to better paying urban occupations (see Tables 3 and A2). Machinists' sons in cities, on the other hand, could have experienced a relative urban premium only if initially urban non-machinists' sons would have left cities for rural areas.²⁸

In line with the previous interpretation, Column 2 shows that the entire higher probability of urban place of living effect can be attributed to sons of initially rural machinists. We can use the differential probability between the offspring of rural and city-dweller machinists to calculate the differential earnings effect which can be explained by rural-urban earnings differences. Ward (forthcoming) estimates that rural-to-urban migration led to a 30 log-point increase in the log-earnings score in the early-twentieth-century United States. Assuming that this figure accurately describes the average gains of machinists' sons derived from rural-to-urban migration, we can conclude that the majority of the 4.7 log-points difference can be explained by the differential relative probability of urban status ($2.6 = 0.085 \cdot 30$).

²⁸ Additionally, the magnitude of earnings losses from urban-to-rural migration was significantly smaller than gains from rural-to-urban migration (Ward, forthcoming).

In Column 3, the sample is restricted to villagers' sons who lived in counties with medium level tuition fee (see Table 9). In line with anecdotal evidence in [Goldin and Katz \(2000\)](#) and [Goldin and Katz \(2008\)](#), we find that the (secondary) education of rural sons was indeed the pathway to urban life. At mean private high school provision, the son of a villager machinist was 12% more likely to live in an urban place three decades later than a comparable non-machinist's son. However, this effect increases by 70% when private high school provision increases by one standard deviation. We believe that this result lends support to the interpretation that machinists' sons ended up in urban places at least partly because they were more educated.

Second, we want to understand to what extent the rest of the machinist effect ($2.5 = 7.2 - 4.7$) can be explained by returns to education. In unreported results, we establish that less than the half of the 0.21-year-longer schooling (see Column 1 in Table A5) stemmed from longer primary schooling, while the majority was the result of secondary schooling. Taking the returns to schooling estimates of [Goldin and Katz \(2000\)](#), we calculate that two-thirds ($1.7 = \text{return to high school} + \text{return to primary school} = 10.3\% \cdot 0.13 + 4.8\% \cdot 0.08$) of the remaining machinist effect was the result of more years of schooling.²⁹ Considering that [Goldin and Katz \(2000\)](#) argue that their returns estimated in Iowa (1915) might be a lower bound on returns to education and that our estimated 0.21-year-longer schooling might be a lower bound too (owing to endogenous attrition), we can attribute practically the entire remaining earnings effect to returns to schooling.³⁰

5.3 Fathers in other demanded occupations

The main results section is closed by looking at the sons of fathers in other occupations which were already present around 1870 and also received a boost from technological innovations during the Second Industrial Revolution (see [Mokyr, 1999](#)).

The first such occupational group contains fathers who were chemists, engineers (mainly civil or mechanical), or telegraph operators - all white-collar jobs. Column 2 in Table 13 shows that their sons might have experienced even larger benefits, as measured by the education rank of occupation, than the sons of machinists. The conclusion is similar for the point estimate of the log-earnings score (Column 6), though this coefficient is imprecisely estimated, potentially owing to the small sample size.

Subsequent columns investigate the effect on the sons of two other, relatively lower-skilled groups of workers: employees of the railways (for instance, locomotive engineers or firemen) and operatives of the metal industry (smeltermen, heaters, etc.). Interestingly, we do not find evidence on any significant effect on their sons using our baseline matching estimation. While the explanation of the missing effect is beyond the scope of this work, we suspect that the labor market competition stemming from masses of low-skilled, European immigrants might have affected these lower-skilled workers more severely. Thus, the labor supply could more easily match the rising

²⁹We cannot analyze a heterogeneous years of schooling effect by initial urban status owing to the small number of sons in the 1940 sample.

³⁰The returns to education of [Goldin and Katz \(2000\)](#) combine within and across occupations gains, whereas earnings scores-based estimates can exclusively capture the latter. The estimates of [Feigenbaum and Tan \(2020\)](#) - those based on income scores measured before the Great Compression ([Goldin and Margo, 1992](#)) - indicate that 60-70% of the effect of a year of education on individual wages is captured in the effect on occupational earnings scores (4.4% vs 2.6-3.1%; see Tables 7 and A.9 of [Feigenbaum and Tan, 2020](#)).

Table 12: The urban-rural gap in the earnings effect (G2; 1900)

	State-level nominal log-score (1892)	Urban place of living (Yes=1)	
	(1) Full sample	(2) Full sample	(3) Villagers & medium tuition fee
Machinist (G1)	0.072*** (0.019)	0.099*** (0.023)	0.124*** (0.032)
City (1870) x Machinist (G1)	-0.047* (0.025)	-0.085*** (0.030)	
Private high school (%) x Machinist (G1)			0.088*** (0.026)
Manufacturing emp. (%) x Machinist (G1)			-0.037 (0.024)
Baseline controls	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes
Sample size	45605	45605	6542
Number of clusters	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons who were not older than ten years in 1870 and whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. The sample is additionally restricted to sons who lived in a place with less than 5,000 inhabitants and in a county with private tuition fee between the 25th and 75th percentiles in 1870 (see Table 9). The outcome variable is state-level nominal log-score (Col. 1) and an indicator for an urban place of living in 1900 (Col. 2-3). The specifications in Columns 1-2 also include a city indicator which equals to one if a son lived in a place with more than 5,000 inhabitants in 1870. Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

demand in these occupations.

Table 13: Sons of fathers in other occupations (G2; 1900)

	Education rank				Sobek log-score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Machinist (G1)	2.403*** (0.745)				0.041*** (0.013)			
White-collar occupation boosted by the Second Ind. Rev. (G1)		4.959*** (1.757)				0.055 (0.038)		
Employee of railways (G1)			-0.300 (1.531)				0.030 (0.021)	
Metal industry operative (G1)				-0.517 (1.539)				-0.051 (0.040)
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	8745	1643	3317	2893	8424	1448	3020	1453
Number of clusters	45	45	43	43	45	42	44	41

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The final sample includes 1842, 200, 763, 665, 2779, 175, 746, 628 (Col. 1-8, respectively) matched sons of machinists. To mitigate the imprecision caused by the small number of treated observations, ten controls are chosen for the sons of white-collar workers instead of the usual five. The outcome variable is the education rank of occupation (Col. 1-4) and the Sobek log-score (Col. 5-8). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. The Sobek score merged to the occupation of fathers (1870) is also included in the propensity score matching in Columns 5-8. White-collar occupations boosted by the Second Industrial Revolution are: chemists, engineers (IPUMS's harmonized OCC1950 code between 41 and 49), and telegraph and telephone operators. Railway employees are: brakemen, locomotive engineers, locomotive firemen and switchmen. Metal industry operatives are: filers, furnacemen, heaters, grinders, polishers and smeltermen. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Robustness checks

We discuss the robustness of our main findings below, implementing modifications in our baseline matching or regression estimations. In most cases, we concentrate on the effect on the two crucial outcomes of sons for the sake of brevity (the education rank and urban place of living indicator in 1900). However, we deviate from these outcomes in a few specifications owing to sample size considerations and, instead, look at some outcomes of fathers.

6.1 Robustness checks using matching

Occupational employment pre-trends Facing occupational choice in their teenager years, fathers could elicit information about the future of certain occupations from their employment growth. For instance, the employment share of sailors was on a constant decline after the spread of steamships, indicating a gloomy future for prospective sailors. If machinists followed a relatively faster employment growth path compared to baseline control occupations, the identified positive effects could be the result of better foresight (and correlated talent) of machinist fathers, or simply the result of pre-trends leading to better occupation-level outcomes even in the absence of the Second Industrial Revolution. To assess this potential bias, we constructed the changes in the employment share of occupations at the census division level for the 1850s and 1860s (see Appendix B.4.1). These two measures are also included in the propensity score matching implemented in Columns 1 and 2 of Table A8. In comparison with Columns 2 and 5 of Table 5, both coefficients (insignificantly) increase in magnitude. Consequently, differential employment growth trends in the decades when fathers chose their occupation cannot explain our findings.

Manufacturing control occupations One might be concerned that workers of the manufacturing sector might have been more open-minded to modernity than people employed in more traditional sectors and predisposed to

benefit from the overarching industrial and urban transformation in the late-nineteenth-century US. If this was the case, our matching estimation would be upward biased as the baseline control group contains many workers outside of manufacturing as well (e.g., carpenters or teamsters). Therefore, the sample is restricted to fathers who were employed in durable or non-durable manufacturing in Columns 3 and 4 in Table A8. However, this restriction causes no meaningful change in the coefficients of interest.

Maternal observables As more than 95% of mothers were not active on the labor market in our sample in 1870, we cannot use occupation-based measures of their socio-economic status. However, next to maternal age, we constructed an indicator variable if the mother was native born and if she was literate. Our baseline matching strategy in Section 5 balances our sample on maternal age and nativity even without including them as controls, but it is significantly more likely that a machinist's son had a more literate mother (2.6 percentage points difference - which amounts to a 9.6% standardized difference). We assess if more educated mothers drive our results in Columns 5 and 6 (Table A8), where we match on the three maternal observables as well. Once again, our findings are not affected by this change in the baseline specification.

Influential control occupations A particular concern could be an influential role played by the largest control occupations (Table A1). The interpretation of our findings would be profoundly different if the results were driven by a certain small group of control occupations. Therefore, we exclude fathers employed in the three largest control occupations - exclusively these three have a larger than five percent share among matched controls - from the pool of potential control individuals in last two columns of Table A8. The omission of these occupations, which provide approximately one-quarter of the control individuals in the baseline matching, does not influence the results in any significant way.³¹

The role of next-door neighbors The important effect of the neighborhood where kids grow up is well-established both in current and historical US context (see e.g., [Abramitzky et al., 2021](#); [Chetty and Hendren, 2018a](#); [b](#); [Chetty et al., 2014](#); [Durlauf, 2004](#); [Galster, 2012](#); [Ward, 2020](#)). In our (subsequent) regression analysis, at most county-fixed effects can be included to capture the effect of growing up in the same neighborhood. However, within-county residential segregation along ethnic ([Eriksson and Ward, 2019](#)) or other socio-economic lines calls into question whether neighborhoods should be defined at the county-level.

To demonstrate that our results are not driven by machinists residing in more prosperous neighborhoods, we exploit the fact that next-door neighbors can be identified in the full count census. We construct the average value of personal property, real estate value, occupational education rank, literacy and foreign-born status of the closest household heads in 1870 (see Appendix B.4.4 for details). Reassuringly, our baseline matching strategy balances on these initially significantly different characteristics even without their inclusion (e.g., in the estimation

³¹While the omission of the largest control occupations does not matter for our results, if control occupations experienced an employment decline or rise in 1870-1900 does matter. Restricting control occupations only to those which experienced an increasing (decreasing) employment share in these decades would result in different coefficients: 1.6 (4.3) for the educational rank and 0.034 (0.084) for the urban status indicator. In our baseline matching strategy, the average employment share change of the matched control group is approximately zero (unreported results).

in Table 5 - not reported). Thus, machinists tend to have very similar neighbors compared to matched control observations. Therefore, we believe that omitted differences in neighborhood quality cannot drive the findings.

The role of grandparental (Go) characteristics Grandfathers (Go) could influence our results and their interpretation in many ways. For instance, grandfathers with better foresight could nudge fathers to choose an occupation that was expected to be prosperous or to leave agriculture. Additionally, if machinists had significantly richer or more educated parents, this could introduce a more mundane form of omitted variable bias into the empirical analysis. All these reasons make the linking of fathers (G1) to their fathers (grandfathers; Go) important. In the resulting sample, we can assess the difference in coefficients with and without controlling for a large number of grandparental observables measured in 1860. Before doing so, we acknowledge that our sample might be selected since the parents of most foreign-born individuals did not live in the United States and some grandfathers might have died before 1860. However, the resemblance of coefficients estimated in Tables 3 and A9 suggests that the degree of this selection is not severe.³²

First, we investigate how well the baseline matching strategy performs *without* explicitly balancing the sample on grandparental observables. The fact that the age, wealth (both real estate and personal property), urban status, population of place of living, and steel and iron industry dummy of grandfathers are significantly different before, but not significantly different after matching lends credibility to our estimation strategy. Furthermore, even when the difference cannot be eliminated in the case of certain remaining variables, it shrinks substantially. For instance, non-machinist grandfathers are fifteen percentage points more likely to have an agricultural occupation initially. This gap is reduced to five percentage points with a p-value of 1%. Nonetheless, there are several variables which are still highly significantly different, the most prominent one being the indicator variable of a machinist grandfather.

Second, Table A9 reports the results with and without controlling for grandparental characteristics (see the notes below the table for the full list). It can be observed that the inclusion of these Go background variables in the matching procedure does not change the results. Consequently, we can conclude that the main findings are not driven by grandparental observables.

6.2 Robustness checks using regressions

As a validation step before presenting the full set of robustness checks with fixed effects regressions, the baseline results for sons are estimated using these regressions instead of matching. The comparison of Tables 5 and 6 to Tables A6 and A7 reveals that the two estimation methods produce very similar coefficients which are not significantly different from each other.

³²The sole qualitatively different result is long-distance migration. Unlike the baseline analysis, where it is insignificantly positive, the coefficient becomes significantly positive at 5% in the new sample.

Spatial sorting before 1870 Even though we can include county-fixed effects in our regressions, individuals who resided in a certain county in 1870 might have still been different in their migration history. Ideally, people who were born in a given county should not be compared to people who migrated there. Since the Second Industrial Revolution does not have a well-defined starting date, it could be the case that, when only county-fixed (1870) effects are used, in-migrated machinists with a good instinct to spot places with a growth potential are compared to locals who happened to be born there.³³

Therefore, more detailed fixed effects are specified to tackle the possible spatial sorting prior to 1870. To do so, we generate fixed effects combining state of birth (country of birth for the foreign-born), county of living in 1870, an urban status indicator in 1870, and an indicator variable for above median age of the father. For instance, if a 28 year-old machinist was born in South Carolina, but then moved to the rural part of Erie county (NY), we are going to compare him to individuals with exactly the same migration history and below median age. Consequently, we will cease to compare individuals to all other locals in 1870. The underlying assumption is that individuals sharing the same migration history had very similar information and keenness to migrate. While the coefficients in Table A10 (Columns 1-2) somewhat decrease compared to Table A6, a large part of this insignificant difference is attributable to a slightly different, reduced sample.³⁴ This sample size reduction is the result of our narrowly defined fixed effects as we lose observations in less densely populated, rural areas or with a peculiar migration history. Finally, we can conclude that spatial sorting preceding the 1870s does not drive our results.

Additional state-occupation level pre-trends Our baseline matching strategy contains merely two occupation-state level characteristics (probability of migration and occupation change in the 1860s) because the matching algorithm would not converge if many more were added. This limitation is simply the result of the occupation-based "treatment". However, many other similar variables can be included in fixed effects regressions. To this end, we calculated the two aforementioned variables for the 1850s, and added the average change in the urban status indicator and the probability of switching to an agricultural occupation for every occupation in the 1850s and 1860s (see Appendix B.4.2). The absence of any significant change after the inclusion of these control variables in Table A10 shows that the results are not outcomes of spatially-varying, occupation-level pre-trends.

Weighting for a representative sample The implementation of propensity score matching does not allow us to use any kind of weights. However, it is a well-known issue in the literature using the full count census that linking across different census waves might engender a non-representative sample. Therefore, we calculated the widely used inverse proportional weights to make the sample representative of the US population around 1870 (see Appendix B.5 for the details), then applied them in Columns 5-6 of Table A10. One can clearly see that our regression estimation without weighting produces coefficients very close to these new estimates. Therefore, we believe that our results accurately reflect the US population at the onset of the Second the Industrial Revolution.

³³Klein and Crafts (2020) argue that in the early-twentieth-century United States "technological progress accelerated at this time but its progress was quite erratic and the development of new technologies and industrial locations was unpredictable."

³⁴Results with the new sample but without the new fixed effects are available upon request.

Restricting the set of control occupations The baseline regression estimation includes all fathers whose occupation is above the 25th but below the 85th educational rank percentile. In the last robustness exercise reported in Table A10, we further restrict the sample of fathers to the 45th-65th educational rank percentiles. No significant change ensues aside from a slight drop in the coefficients.

Old and young fathers/sons Before occupations start to grow rapidly or are about to decline, there is much uncertainty about their future. More forward-looking and able individuals might have anticipated the eventual rise of machinists and took up this occupation early on. This type of sorting would imply that more positive effects should be observed for the sons of older machinists. Table A12 presents a comparison of the effect on sons depending on the age of the father. The age of sons is restricted between 0 and 5 in 1870 because otherwise older fathers have substantially older kids who, in turn, grew up in different years. Reassuringly, we do not find any significant difference between the sons of older and younger machinists when the sample is split by the age of the median machinist father.

Dynamic complementarity in the production of human capital is a well-established finding in the literature of education economics (see [Caucutt and Lochner, 2020](#); [Heckman and Cunha, 2007](#); [Lee and Seshadri, 2019](#)). This implies that those sons of machinists who were relatively old in 1870 should have experienced a relatively smaller increase in their level of education compared to the younger ones because they lacked complementary education investments during their early childhood. We investigate this question in the last column of Table A12. Confirming the theoretical prediction, machinists' sons who were older than ten years around the onset of the Second Industrial Revolution did not enjoy any gains in education (proxied by the education rank) in comparison with sons of similar, non-machinist workers.

6.3 Grandfather-fixed effects

Our arguably most important robustness checks are regressions in which grandfather-fixed effects are included. In other words, we compare machinists to their non-machinist brother(s). In this way, we can eliminate concerns related to machinists growing up in more advantaged families (unobservables not captured by the job, place of living or wealth of the grandfather) or inheriting a particular genetics, which helps them succeed in life (see [Mogstad and Torsvik \(2021\)](#) for a recent survey on this topic). To eliminate within-family differences in talent across siblings, we still control for many of their personal characteristics in 1870: county of living, education rank, literacy or wealth. Our regression specification thus takes the following form:

$$y_{f,c,g,1900} = \beta \cdot \text{Machinist}_{f,1870} + \gamma \cdot x'_{f,1870} + \delta_{c,1870} + \kappa_g + \epsilon_{f,c,g,1900} \quad (2)$$

where the fixed effect for grandfather g of father f appears as a new control variable (κ_g). Unfortunately, we can only apply this estimation strategy for fathers' outcomes due to sample size limitations. Moreover, the baseline sample must be extended in two ways even for fathers. First, we include all fathers who were between 16

Table 14: Within-family estimation - fathers (G1; 1870-1900)

	Occupational change (Yes=1)				Other outcomes		
	(1) Baseline	(2) Baseline	(3) 20-40 y.o.	(4) Unrestricted sample	(5) Urban in 1900 (Yes=1)	(6) Migration (within-state; Yes=1)	(7) Migration (across states; Yes=1)
Machinist (G1)	-0.107*** (0.037)	-0.138** (0.062)	-0.133* (0.079)	-0.204*** (0.041)	0.116** (0.050)	-0.027 (0.043)	0.066 (0.049)
Grandfather (Go)-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	22799	22799	17793	69221	22799	22799	22799
R ²	0.24	0.64	0.65	0.67	0.72	0.62	0.65

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are multiway clustered at the grandfather-county (1900) level. None of the specifications is weighted. The sample includes all fathers who held an occupation between the 24.7th and 84.7th education rank percentiles in 1870 - except for Column 4 which includes all fathers irrespective of the education rank of their occupation. In every column, the age of included fathers is between 16 and 50 years (inclusive) - except for Column 3 where the age is restricted between 20 and 40 years (inclusive). The outcome variable is a binary variable which equals one if i) the father changed occupation between 1870 and 1900 (Col. 1-4), ii) the father lived in an urban place in 1900 (Col. 5); iii) the father migrated within-state across counties (Col. 6) or across states (Col. 7). Personal controls included in the regressions are (all measured in 1870): the education rank of occupation, urban status and literacy indicator, age (in years), value of real estate and personal property, number of inhabitants in the place of living and a farmer-farm manager-farm foreman indicator. The interactions of the urban indicator, size of place of living, two wealth measures, education rank and age are also included. The squared size of place of living, wealth measures and age are included as well. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and 50 years old (originally 20-40). Second, loosely defined occupations are not omitted anymore (e.g., *Clerical and kindred workers (n.e.c.)*). Nevertheless, the baseline regression sample restriction is still implemented and we only include fathers whose occupation had an occupational education rank between 25th and 85th percentiles, thereby excluding farm laborers, fishermen but even high-skilled individuals such as bookkeepers or physicians.

The results of this estimation are shown in Table 14. The comparison of Columns 1 and 2 shows that the inclusion of grandfather-fixed effects does not significantly change the coefficient of interest in spite of a forty percentage-point increase in the R^2 . This suggests that machinist fathers were significantly less likely to change their occupation even compared to their non-machinist brothers. In Column 3, the age of fathers is restricted to the original 20-40 range. The point estimate is practically unchanged but less precisely estimated owing to the sample size reduction. Next, we include all brothers irrespective of their education rank in Column 4. This produces an even larger coefficient than the initially estimated one in Column 2. Other outcomes of fathers are presented in Columns 5-7. The same conclusion can be drawn quantitatively and qualitatively as before (see Tables 3 and A9): a substantial positive likelihood of living in an urban place and (if anything) a positive probability to migrate across states.

The within-family estimation can greatly reduce the role of certain confounding unobservables, but it cannot entirely eliminate differences stemming from the different ability of brothers. In our previous analysis, we already made two steps to reduce their role. First, analogously to [Feigenbaum and Tan \(2020\)](#), who restrict their sample to small years of education differences between twins, brothers holding occupations with the lowest and highest education ranks were excluded. The underlying assumption is that brothers with more similar education ranks are more likely to be similar in terms of unobservables as well. Second, the included personal characteristics (for instance, the two wealth measures, the education rank of occupation or literacy dummy) should already capture a certain degree of differences in ability. To further reduce the likelihood that the results are driven by unobserved ability, we borrow from the literature which estimates returns to schooling using twins (e.g., [Ashenfelter and Rouse, 1998](#); [Feigenbaum and Tan, 2020](#)). They argue that some observable variables - marriage

status,³⁵ spousal education, number of kids, etc. - are correlated with ability. In Table A11, we demonstrate that none of these variables are correlated with the machinist dummy. Perhaps even more importantly, specifications *without* grandfather-fixed effects show no significant association either.

6.4 Correcting measurement error and magnitude comparison

It is well-known that the misreporting of binary independent variables produces a non-classical measurement error in regression estimations because the measurement error is mechanically negatively correlated with the correctly measured value (see e.g., [Aigner, 1973](#); [Bingley and Martinello, 2017](#); [Dupraz and Ferrara, 2021](#)). Consequently, the OLS estimate is a lower bound on the consistent coefficient normally. The relationship between the correct coefficient and inconsistent OLS estimate is the following:

$$\text{plim } \hat{\beta}_{OLS} = \beta \cdot (1 - p - q) \quad (3)$$

where β is the consistent coefficient, p is the share of false positives (among fathers classified as machinists, $p\%$ were incorrectly classified as one), and q is the share of false negatives (among fathers classified as non-machinists, $q\%$ were actually machinists).

In our case, q can be set equal to zero owing to the small share of machinists in the whole sample. A non-negligible p can be the result of two, distinct measurement errors. First, a machinist observation might be linked to a non-machinist one when we link across census waves. For conservative linking methods used in this paper, [Bailey et al. \(2020\)](#) estimate a false positive ratio of 10-15%. Second, even if we could perfectly link individuals to their own observations over time, the misreporting of occupations can cause measurement error. [Ward \(2019\)](#) shows that around one-third of respondents misreported their occupation in the full count census, relying on a census re-enumeration in Saint Louis in 1880.³⁶ Therefore, we believe that assuming $p \approx 40\%$ might capture the true extent of false positives.

Using the previously introduced formula, one can see that the OLS coefficient is assumed to be downward biased by a factor of 0.6 (=1-0.4). Under this assumption, the consistently estimated effects are around 66.67% larger than the earlier OLS estimates. This implies that a machinist's son had on average a four percentiles higher education rank (Col. 2 of Table 5), 0.35 years more of schooling (Col. 1 of Table A5), and a seven log-points higher nominal earnings score (Col. 2 of Table 6) than a son of a comparable but non-machinist father.

7 Conclusion

In this paper, we investigate to what extent and how winners of structural transformations can transmit their gains in socio-economic status to their offspring. Combining full count census data with newly digitized data sources, we

³⁵In the absence of a separate census question on marriage status in 1870, a father is imputed to be married if the age of the spouse is known.

³⁶If a reported machinist was more than 66.67% likely to actually hold the machinist occupation, the magnitude of the adjustment factor declines along with p .

establish that machinists, whose occupation experienced a relative labor demand spike in the United States during the Second Industrial Revolution, experienced relatively higher income and job stability. Relying on propensity score matching and fixed effects regressions, we document that the (grand)sons of machinists were significantly better-off in terms of earnings-related outcomes than (grand)sons of observationally similar non-machinists. In addition, the main contribution of this work is pinning down the mechanism which underlies the documented intergenerational transmission. We find that the sons of rural machinists benefited from rural-to-urban migration and parental investment in their education, while the sons of urban machinists mostly gained from the latter channel. A wide range of robustness checks show that the results are unlikely to be driven by the (transmitted) unobserved ability of machinist fathers.

In conclusion, the main mechanisms behind intergenerational mobility seem to have changed little over more than a century: the opportunities offered by high-quality urban neighborhoods (see Chetty and Hendren, 2018a; b; Chetty et al., 2014; Durlauf, 2004; Galster, 2012; Laliberté, 2021) and by high educational attainment guarantee a higher socio-economic status in the age of telegraphs as well as of smartphones. We also show that expanding public schools could equally well reduce inequality stemming from financially constrained parents in the past as nowadays (Dobbie and Fryer, 2011; Duflo, 2001; Lucas and Mbiti, 2012; Neilson and Zimmerman, 2014; Wantchekon et al., 2015). Taken together, our results suggest that the effects of current transformations in the labor market, such as automation, might be passed on to later generations, but to a lesser extent due to today's considerably more expanded public education and unemployment benefit system (allowing for less occupational downgrading) - especially if people are allowed to move to places offering better economic prospects.

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A Additional empirical results

Table A1: Top control occupations

Top control occupations (OCC1950)	% of all control observations
Carpenters	9,92%
Truck and tractor drivers	7,68%
Shoemakers	6,16%
Painters (construction)	4,17%
Blacksmiths	4,15%
Masons	3,34%
Hucksters and peddlers	2,94%
Stationary engineers	2,91%
Tailors	2,58%
Molders (metal)	2,36%
Bookkeepers	2,24%
Compositors and typesetters	2,10%
Meat cutters	1,99%
Stone cutters	1,86%
Clergymen	1,64%

Note: the results presented in this table pertain to the propensity score matching in Table 5.

Table A2: Migration destination decomposition - fathers (G1; 1900)

	Migration (within and across states) [(1)=(2)+(3)]			Urban destination [(2)=(4)+(5)]		Rural destination [(3)=(6)+(7)]	
	(1) Any destination	(2) Urban destination	(3) Rural destination	(4) Urban in 1870	(5) Rural in 1870	(6) Urban in 1870	(7) Rural in 1870
Machinist (G1)	0.017 (0.011)	0.037*** (0.009)	-0.020*** (0.007)	0.016 (0.011)	0.021*** (0.004)	-0.013* (0.006)	-0.007** (0.003)
Mean of outcome	0.58	0.28	0.30	0.15	0.13	0.13	0.17
Standard deviation of outcome	0.49	0.45	0.46	0.36	0.33	0.33	0.38
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	18811	18811	18811	18811	18811	18811	18811
Number of clusters	50	50	50	50	50	50	50

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights gained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The outcome variable is a binary variable which equals one if the father migrated between 1870 and 1900 (across or within states; Col. 1), if he migrated and was found in an urban (Col. 2) or rural (Col. 3) place of living in 1900, if he migrated to an urban destination by 1900 and lived in an urban (Col. 4) or rural (Col. 5) place of living in 1870, if he migrated to a rural destination by 1900 and lived in an urban (Col. 5) or rural (Col. 6) place of living in 1870. Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Measures of economic status - fathers (G1; 1880)

	(1) State-level nominal log-score (1880)	(2) State-level real log-score (1880)
Machinist (G1)	0.065** (0.025)	0.057** (0.023)
Mean of outcome	6.07	6.20
Standard deviation of outcome	0.42	0.42
Unbalanced controls	Yes	Yes
Sample size	19120	19120
Number of clusters	47	47

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1880) level. All specifications are weighted by weights gained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 4428 matched machinist fathers. The outcome variable is the state-level nominal and real log-score (Col. 1 and 2). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Measures of occupational income and mobility - occupation switcher fathers (G1; 1870-1900)

	Earnings scores (state-occupation in 1900)					Occupational mobility measures (1900)	
	(1) Sobek log-score	(2) State-level nominal log-score (1892)	(3) State-level real log-score (1892)	(4) Higher-paying occupation (+\$150 or more; Yes=1)	(5) Lower-paying occupation (-\$150 or less; Yes=1)	(6) Manager/official/proprietor (Yes=1)	(7) Siegel's prestige log-score
Machinist (G1)	0.036** (0.014)	0.032** (0.012)	0.026** (0.013)	0.004 (0.018)	-0.060*** (0.016)	-0.015 (0.010)	0.028** (0.012)
Mean of outcome	6.07	6.17	6.30	0.22	0.34	0.13	3.56
Standard deviation of outcome	0.60	0.45	0.43	0.42	0.47	0.33	0.36
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	7337	7337	7337	7337	7337	7337	7337
Number of clusters	47	47	47	47	47	47	47

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights obtained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 1578 matched machinist fathers and non-machinist fathers who did not hold the same occupation in 1870 and 1900. The outcome variable is the Sobek, state-level nominal and real log-score (Col. 1-3), an indicator variable if the state-level real log-score was at least \$150 higher (Col. 4) or lower (Col. 5) in 1900 than in 1870, an indicator variable if the father held a managerial/proprietor occupation (Col. 6; OCC1950 code=290), and Siegel's prestige log-score (Col. 7). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Measures of education and wealth - sons (G2; 1940)

	(1) Highest grade completed	(2) Some primary education (years < 9; Yes=1)	(3) Some secondary education (9 <= years <=12; Yes=1)	(4) Some university education (12 < years; Yes=1)	(5) Owned a house (Yes=1)
Machinist (G1)	0.209** (0.091)	-0.032** (0.012)	0.036*** (0.010)	-0.004 (0.009)	0.018 (0.019)
Mean of outcome	7.91	0.75	0.17	0.08	0.68
Standard deviation of outcome	3.42	0.44	0.38	0.28	0.47
Unbalanced controls	Yes	Yes	Yes	Yes	Yes
Sample size	7543	7543	7543	7543	7543
Number of clusters	49	49	49	49	49

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1940) level. All specifications are weighted by weights gained from propensity score matching described in the main text. The summary statistics reported are unweighted and pertain to the full estimation sample before matching. The final sample includes 919 matched machinist sons. We use ten matched control observations instead of five owing to the small number of machinist sons. The outcome variable is the highest grade of schooling completed (Col. 1 - winsorized at the 99th percentile), a binary variable which equals one if i) the years of schooling is below nine years (Col. 2), ii) the years of schooling is between nine and twelve years (Col. 3), or iii) the years of schooling is more than twelve years (Col. 4), and an indicator variable for house ownership (Col. 5). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Main outcomes - sons (G2; 1900)

	Occupational characteristics		Migration (Yes=1)		Place of living (1900)			Personal characteristics		
	(1) Agricultural occ. (Yes=1)	(2) Education rank	(3) Within-state	(4) Across states	(5) Urban (Yes=1)	(6) Higher population than in 1870 (Yes=1)	(7) Manuf. emp. per capita (county)	(8) # of children	(9) Married (Yes=1)	(10) Owning house (Yes=1)
Machinist (G1)	-0.025*** (0.007)	3.368*** (0.648)	-0.014 (0.010)	0.048*** (0.011)	0.044*** (0.009)	0.035*** (0.012)	0.003 (0.002)	-0.051 (0.040)	0.002 (0.009)	-0.001 (0.011)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	63857	63857	63857	63857	63857	63857	63857	63857	63857	63857
Number of clusters	45	45	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. The outcome variable is an agricultural occupation indicator (Col. 1 - farmer, farm manager/foreman/laborer), the education rank of occupation (Col. 2), a binary variable which equals one if the son migrated within-state across counties (Col. 3) or across states (Col. 4) between 1870 and 1900, a binary variable which equals one if the son lived in an urban place in 1900 (Col. 5) or his place of residence fell into a larger SIZEPL category in 1900 than in 1870 (Col. 6), manufacturing employment per capita (as % of total county population), the number of children in the household (Col. 8), a marriage status (Col. 9) and house ownership (Col. 10) indicator. Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Measures of economic status - sons (G2; 1900)

	(1) Sobek log-score	(2) State-level nominal log-score	(3) State-level real log-score	(4) State-level real score (level)	(5) Preston-Haines log-score
Machinist (G1)	0.060*** (0.011)	0.040*** (0.010)	0.030*** (0.010)	15.170** (6.406)	0.075*** (0.009)
Baseline controls	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes
Sample size	49268	44553	44553	44553	40331
Number of clusters	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. The outcome variable is the Sobek, state-level nominal and real log-score (Col. 1-3), the state-level real score in levels (Col. 4) and the Preston-Haines log-score (Col. 5). Baseline controls are described in Appendix B.1. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Robustness checks - sons (G2; 1900)

	Occupational pre-trends		Manufacturing control occs		Maternal characteristics		Top 3 control occs excluded	
	(1) Education rank	(2) Urban (Yes=1)	(3) Education rank	(4) Urban (Yes=1)	(5) Education rank	(6) Urban (Yes=1)	(7) Education rank	(8) Urban (Yes=1)
Machinist (G1)	4.133*** (1.010)	0.074*** (0.015)	2.913** (1.210)	0.050*** (0.015)	3.153*** (0.830)	0.050*** (0.013)	2.107*** (0.641)	0.052*** (0.012)
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	7015	7015	3111	3111	8904	8904	8570	8570
Number of clusters	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights gained from propensity score matching described in the main text. The sample includes 1842 (Col. 1-2, 7-8), 1794 (Col. 3-4) and 1823 (Col. 5-6) matched machinist sons. The outcome variable is the education rank of occupation (every odd column) or an urban place of living indicator (every even column). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. In Columns 1-2, changes in the employment share of father's occupation (measured in percentage points and calculated for fathers' 1870 census division) between 1850-1860 and 1860-1870 are also included in the matching process (see Appendix B.4.1 for more details). In Columns 3-4, exclusively those fathers are included who worked in durable or non-durable manufacturing in 1870. In this specification, the matching process chooses a single control father owing to the reduction in the number of potential control occupations. In Columns 5-6, the matching process balances the sample on maternal characteristics (1870): a literacy and a native-born status indicator, and her age in 1870 (in years). In Columns 7-8, carpenter, truck & tractor driver and shoemaker fathers are excluded from the control group in matching. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Main outcomes - fathers (G1; 1870-1900)

	Occupational change (Yes=1)		Agricultural occupation in 1900 (Yes=1)		Migration (across states) (Yes=1)		Urban in 1900 (Yes=1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Machinist (G1)	-0.098*** (0.015)	-0.110*** (0.016)	-0.037*** (0.010)	-0.030*** (0.009)	0.038** (0.015)	0.030** (0.012)	0.073*** (0.017)	0.070*** (0.016)
Unbalanced controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grandfather (Go) controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample size	5429	5456	5429	5456	5429	5456	5429	5456
Number of clusters	48	49	48	49	48	49	48	49

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. All specifications are weighted by weights gained from propensity score matching described in the main text (every even column matches on grandfather controls as well). The sample includes 1120 (1116 with grandfather controls included) matched machinist fathers. The outcome variable is a binary variable which equals one if i) the father changed occupation (Col. 1-2) and the new occupation is agricultural (Col. 3-4 - farmer, farm manager/foreman/laborer), ii) he migrated across states between 1870 and 1900 (Col. 5-6), iii) he lived in an urban place in 1900 (Col. 7-8). Unbalanced controls included in the regressions are characteristics whose mean between machinist and control fathers is still significantly different at 5% after matching. Grandfather controls are (all measured in 1860): a literacy indicator, age (measured in years), an indicator if the father lived in the same state in 1870 as the grandfather in 1860 but in a different county, an indicator if the father lived in a different state in 1870 from the grandfather in 1860, indicator variables for the grandfather holding an agricultural (farmer, farm manager/foreman/laborer) or manufacturing (durable or non-durable manufacturing) occupation, indicator variables if the grandfather worked for the railways (railroad conductor, locomotive engineer, locomotive fireman, brakeman, switchman) / in the metal industry (molder, structural metal worker, furnaceman, heater, filer, grinder, polisher, roller, tinsmith and coppersmith) / in the chemical industry (IND1950: Cement, concrete, gypsum and plaster products; Miscellaneous chemicals and allied products; Petroleum refining; Miscellaneous petroleum and coal products; Rubber products) / in the steel and iron industry (IND1950: Blast furnaces, steel works, and rolling mills; Other primary iron and steel industries; Fabricated steel products) / in machinery (IND1950: Agricultural machinery and tractors; Office and store machines and devices; Miscellaneous machinery; Electrical machinery, equipment, and supplies) / as a machinist, an indicator for urban status, the number of inhabitants in the place of living (SIZEPL), and the value of personal property and real estates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Robustness checks with regressions - sons (G2; 1900)

	Spatial sorting pre-1870		State-occupation pre-trends		Weighting		Restricted control occs	
	(1) Education rank	(2) Urban (Yes=1)	(3) Education rank	(4) Urban (Yes=1)	(5) Education rank	(6) Urban (Yes=1)	(7) Education rank	(8) Urban (Yes=1)
Machinist (G1)	2.587*** (0.898)	0.037*** (0.012)	2.782*** (0.678)	0.035*** (0.009)	3.166*** (0.730)	0.039*** (0.010)	3.162*** (0.655)	0.039*** (0.011)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Detailed fixed effects	Yes	Yes	No	No	No	No	No	No
Sample size	55770	55770	61796	61796	63857	63857	52046	52046
Number of clusters	45	45	45	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted - except for Columns 5-6, where we use inverse proportional weights (see Appendix B.5 for details). The sample includes all sons whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870 - except for Columns 7-8, where these cutoffs are 44.7 and 64.7, respectively. The outcome variable is the educational rank of occupation (every odd column) or a binary variable which equals one if the son lived in an urban place in 1900 (every even column). Baseline controls are described in Appendix B.1. Detailed fixed effects are generated by interacting the state of birth (county for the foreign-born) indicator, the county of residence indicator (1870), an urban place of living indicator (1870), and an indicator if the father was at least 34 years old in 1870. In Columns 3-4, state-occupation level measures of migration (within and across states jointly), occupation change probability, change in urban status and the probability of switching for an agricultural occupation (farmer, farm manager/foreman/laborer) are included for the 1850s and 1860s (see Appendix B.4.2 for details). Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Ability bias - fathers (G1 in 1870)

	Having a child (Yes=1)		Number of children		Having a spouse (Yes=1)		Literate spouse (Yes=1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Machinist (G1)	-0.005 (0.010)	0.041 (0.042)	-0.002 (0.024)	0.053 (0.095)	-0.005 (0.009)	0.018 (0.042)	0.003 (0.005)	-0.037 (0.036)
Grandfather (Go)-fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	159716	22799	159716	22799	159716	22799	90473	9581
R ²	0.38	0.73	0.41	0.76	0.43	0.77	0.41	0.76

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are multiway clustered at the grandfather-county (1900) level. None of the specifications is weighted. The sample includes all fathers who held an occupation between the 24.7th and 84.7th education rank percentiles in 1870. In every column, the age of included fathers is between 16 and 50 years (inclusive). The outcome variable is i) a binary variable which equals one if the father had at least one child in 1870 (Col. 1-2), ii) the number of children in 1870 (Col. 3-4); iii)-iv) a binary variable which equals one if the father had a spouse (Col. 5-6) and, conditional on having a wife, she was literate (Col. 7-8). Personal controls included in the regressions are (all measured in 1870): the education rank of occupation, urban status and literacy indicator, age (in years), value of real estate and personal property, number of inhabitants in the place of living and a farmer-farm manager-farm foreman indicator. The interactions of the urban indicator, size of place of living, two wealth measures, education rank and age are also included. The squared size of place of living, wealth measures and age are included as well. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Robustness checks by the age of fathers and sons - sons (G2 in 1900)

	Sons (0-5 y.o.) of young fathers (<33 y.o.; G1)		Sons (0-5 y.o.) of old fathers (≥33 y.o.; G1)		Old and young sons
	(1) Education rank	(2) Urban (Yes=1)	(3) Education rank	(4) Urban (Yes=1)	(5) Education rank
Machinist (G1)	4.176*** (1.397)	0.041** (0.020)	3.616*** (1.106)	0.044** (0.020)	4.001*** (0.695)
Machinist (G1) × 1(son (G2) older than 10 y.o. in 1870)					-4.098*** (1.323)
Baseline controls	Yes	Yes	Yes	Yes	Yes
County-fixed effects (1870)	Yes	Yes	Yes	Yes	Yes
Sample size	16338	16338	18266	18266	63857
Number of clusters	45	45	45	45	45

Note: OLS regression coefficients with standard errors in parentheses. Standard errors are clustered at the state (1900) level. None of the specifications is weighted. The sample includes all sons i) whose father held an occupation between the 24.7th and 84.7th education rank percentiles in 1870; and ii) who were not older than five years in 1870 (Col. 1-4). The outcome variable is the educational rank of occupation (Col. 1,3,5) or a binary variable which equals one if the son lived in an urban place in 1900 (Col. 2,4). Column 5 includes the son age indicator as a main effect separately. Baseline controls are described in Appendix B.1. The estimation includes only fathers who were younger (older) than thirty-three years in 1870 in Columns 1-2 (3-4). Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Data Appendix

B.1 Controls used in propensity score matching

The controls used in the baseline propensity score matching are the following. For every father in the census in 1870, we measure:

- Personal characteristics: age (in years), literacy (can read and write, yes=1), foreign-born dummy (yes=1), native-born dummy (yes=1), dummies for the UK (yes=1) and for Germany (yes=1) as country of birth;
- Occupational characteristics: education rank of occupation (percentile rank; [Song et al., 2020](#)),³⁷ occupation-state level migration and probability of occupation change between 1860 and 1870 (see Appendix B.4.2);
- Measures of individual wealth: value of personal property and real estates (separately);
- Characteristics of place of residence: urban status (yes=1), size category of place of living,³⁸ dummy for living in the state of birth (yes=1) and state-fixed effects.

Moreover, we use the pairwise interactions of the following six variables: real estate, personal property, age, urban dummy, population of place of living, education rank. We also include the square of the two wealth measures, the age and the population of place of living. Finally, we include several county characteristics downloaded from the NHGIS ([Manson et al., 2021](#); for 1870): the share of manufacturing employment (% of total population), manufacturing output per capita, manufacturing output per manufacturing wage earners, the share of steam engine-provided engine power (% of steam engine- and water-driven engine-provided total). We refer to these controls jointly as *baseline controls*.

B.2 The construction of state-level earnings scores

The source of state-level earnings data is the *Fifteenth Annual Report of the Commissioner of Labor* which reports daily average wages for US states and other countries mainly for years in the second half of the 19th century. We sought to find a close match for every occupation i) which has a large role as control occupation for machinist fathers in 1870, or ii) which is a common occupation across fathers or sons in 1900.

We checked all relevant state-occupation pairs for 1870-72, 1879-1881 and 1890-1892, and digitized every entry in which at least ten individuals were used for average wage calculation. In case of multiple entries within any of the three-year time spans for a given state, we chose the average wage which was based on the largest number of wage reporting individuals. We exclusively included entries for males. The ending years of 1872, 1881 and 1892 were chosen because they preceded the Panics of 1873 and 1893, and the Depression of 1882-85. In a few cases, we deviated from our baseline data collection strategy to improve our sample. For miners, census-based,

³⁷We use the first available rank which is constructed for those born around 1880.

³⁸We converted the original *SIZEPL* variable into actual population numbers using the midpoint of every interval. The first and last categories are defined using half the length of the second and penultimate intervals, respectively.

daily average wages were used from 1889 for the 1892 income score because the number of reporting states and observations used for average wage calculation were undoubtedly superior to other publications between 1890-1892. For farm laborers, data were digitized from the *Ninety-ninth Bulletin of U.S. Department of Agriculture (Wages of Farm Labor)*. Daily wages were digitized *without* board (accommodation) to reduce the gap in in-kind compensation between agricultural and manufacturing laborers (Alston and Hatton, 1991, Hatton and Williamson, 1991). For the 1892 score, the number of occupations available is increased in our sample by using the publication titled *The slums of Baltimore, Chicago, New York, and Philadelphia: prepared in compliance with a joint resolution of the Congress of the United States (1892)*. We take mostly occupations in services³⁹ for Maryland, Illinois, New York and Pennsylvania.

Next, daily wages were converted to yearly earnings following Sobek (1996). We assumed 245 days of work for the majority of occupations, 225 days for building trades (bricklayers, cabinetmakers, carpenters, masons, painters, plasterers) and farm laborers, 270 days for clerical occupations (bookkeepers, clerks, telegraph operators).⁴⁰ For farm labor, we assumed 30 days of harvest wages and 195 (=225-30) days of non-harvest wages.⁴¹ We multiplied the yearly earnings of farm labor by the ratio of farmer-to-farm labor score in Sobek (1996) to compute earnings scores for owner-occupier farmers.

The main limitation of our earnings score is that the earnings of high-skilled workers, for instance, lawyers or physicians, cannot be observed. To solve this problem, the following imputation procedure is set up. First, we took the earnings scores of the fifteen occupations (TOP15) for which we have the most state-year level observations.⁴² Afterwards, we calculated the earnings scores of missing, predominantly white-collar occupations⁴³ by multiplying our earnings scores for the available TOP15 occupations with the ratio of the Sobek score of the missing occupation and each of the TOP15 occupations. Then, we took the unweighted average of the implied earnings scores which constitutes our earnings score for missing occupations. We calculated this average only if at least eight of the fifteen (more than half) occupations were available for the case of a given state-year pair in order to reduce measurement error. The main assumption underlying this imputation procedure is that the ratio of earnings scores found in Sobek (1996) around 1890 is the same across states (for our 1892 score), or the same across states and time (for our 1872 and 1880 scores). Reassuringly, Katz and Margo (2014) find that the skilled artisans-to-clerks earnings ratio remained stable between the 1840s and 1880s. The debate if the earnings of higher-skilled workers differed across states more or less than the earnings of production workers or craftsmen has not been settled yet

³⁹These occupations are: barbers, bartenders, watchmen, policemen, detectives, agents (n.e.c), clerks, longshoremen, hucksters, salesmen (n.e.c). We included an observation if the average wage could be calculated using at least ten individuals.

⁴⁰The slum report provides weekly wages. Following Sobek (1996), we assumed 45 weeks worked apart from clerical jobs, where 48 weeks are assumed.

⁴¹Unlike Sobek (1996), we did not assume 245 days of work for farm laborers because it gave rise to a tendency of *nominal* farm laborer wages surpassing laborer wages. This would be inconsistent with existing evidence (Alston and Hatton, 1991; Hatton and Williamson, 1991). Our ratio between farm laborer to laborer nominal earnings scores is really close to the estimates found in the literature which takes into account the pecuniary value of in-kind remuneration as well. Moreover, the similar length of (un)employment spells between farm workers and workers in building trades is also consistent with Engerman and Goldin (1991).

⁴²These occupations are: blacksmiths, boilermakers, cabinetmakers, carpenters, compositors, engineers (locomotive), firemen (locomotive), laborers (n.e.c.), machinists, molders, painters, pattern makers, plumbers, stone cutters and teamsters.

⁴³These occupations are: operatives (n.e.c.), managers, physicians, lawyers, meat cutters, clergymen, pharmacists, policemen, insurance agents, foremen (n.e.c.), teachers (n.e.c.), craftsmen (n.e.c.), fishermen, engineers (civil and mechanical separately), accountants, chemists, draftsmen, editors, funeral directors, musicians, ship officers, stenographers, real estate agents, janitors, waiters, gardeners and sailors. For 1872 and 1880, the list also includes barbers, bartenders, agents (n.e.c.) and hucksters.

(see [Goldin, 1998](#); [Rosenbloom, 1990](#); [1996](#); [2002](#); [Sundstrom and Rosenbloom, 1993](#)). Therefore, applying the Sobek score ratio-implied premia for earlier decades might not introduce a large measurement error since most of our TOP15 benchmark occupations are classified as artisans/craftsmen and we mostly impute the wages of white-collar workers.⁴⁴

Another empirical barrier is that some harmonized occupations have many potential matches in our earnings score data. For instance, we had to aggregate the earnings score of miners of coal, iron or zinc into a single score for miners. The affected occupations are brickmasons (bricklayers and masons), railroad conductors (freight, passenger or not specified), miners (coal, iron, lead and zinc), spinners and weavers (cotton or woolen goods). Our state-year level earnings score for these harmonized occupations is defined as the observation-weighted average earnings score (of "subcategories").⁴⁵ As a last step, missing earnings scores were imputed with the unweighted average of states within a given census division whenever it was possible.

The estimates of [measuringworth.com](#) were used to convert all earnings scores into 1890 dollars. The 1872/1880 earnings scores were multiplied by 0.75/0.89.

The conversion of nominal earnings scores to real scores requires state-level and urban-rural price differences. Nominal earnings (1872, 1880 and 1892) were deflated by the state-level price index of [Haines \(1989\)](#), and nominal wages (1940) by the cost of living measures reported in [Stecker \(1937\)](#). As [Haines \(1989\)](#) and [Stecker \(1937\)](#) do not contain information on all states, we use the price index of a neighboring state in the case of missing values (the actual pairs are available upon request).⁴⁶ We inflate earnings scores in places with less than 25,000 inhabitants by 1.192 (1872, 1880 and 1892 - [Hatton and Williamson, 1991](#)) and by 1.205 (1940 - [Williamson and Lindert, 1980](#)) to account for urban-rural price differences. In doing so, we follow the best practice in earlier literature (e.g., [Collins and Wanamaker, 2014](#)).

B.3 The construction of schooling supply measures

The source of our high school supply proxies are different *Reports of the Commissioner of Education*. We followed a distinct data collection strategy for private and public high schools.

Private high schools We refer to institutions for secondary instruction, preparatory schools, commercial and business colleges (excluding evening schooling), preparatory departments of colleges and universities, and schools of science as private high school.

First, all available data on private high schools were digitized from the 1880 Report. If a school was reported

⁴⁴For 1872, we need an additional step because there are no data on the wages of miners, shoemakers and tailors who play an important part in the control group of machinists. To impute their wages, we follow our procedure described in the main text with one exception. Instead of using the ratio of Sobek scores, we calculate our observation-weighted, US-level earnings score in 1880 for the TOP15 occupations as well as for miners, shoemakers and tailors. We use the ratio of these earnings scores to implement the imputation procedure in order to diminish the potential effect of the Second Industrial Revolution on relative wages over time.

⁴⁵Additionally, we included furnacemen in foundries or in the gas industry as furnacemen, and lumbermen can be lumber handlers, lumber pilers or wood choppers as well. Two of the different "subcategories" of furnacemen or lumbermen never coincided within a state-year cell. Thus, there was no need to calculate observation-weighted averages.

⁴⁶[Stecker \(1937\)](#) reports cost of living for more than one city in some states. We calculated the unweighted average of cost of living in cities within those states.

as not replying to the query of the Commissioner's office, we tried to find it in the 1882 Report. Different types of schools were expected to report different data, so the following pieces of information could be digitized:

- Institutions for secondary instruction: number of teachers and students (split by gender), tuition fee, dummy whether mechanical drawing is taught, dummy if they had a chemical laboratory;
- Preparatory schools: number of teachers and students,⁴⁷ tuition fee and dummy if they had a chemical laboratory;
- Commercial and business colleges: number of teachers and students⁴⁸ (split by gender), and tuition fee;
- Preparatory departments of colleges and universities: number of teachers and students (split by gender);
- Schools of science: number of teachers and students (split by gender).

If tuition fees were not reported for the entire scholastic year (but for a term or month), a 40-week (10-month) long scholastic year was assumed which was the most common length. The children of residents sometimes did not have to pay the tuition fee. In such cases, the tuition fee is set equal to zero. In the next step, schools were matched to counties (1870) one-by-one using their reported location (post office).

Public high schools The data collection process for public high schools is more complex. While detailed statistics were reported for private high schools starting from the 1870s, no school-level information is available on public high schools until 1890. Moreover, the year of establishment is solely recorded in the Reports published in the mid-1900s.

To circumvent these data limitations, we adopted the following data collection strategy. First, we restricted our attention to schools in cities which had a population of 7,500 in 1880 since municipality/school name changes between 1890 and the mid-1900s would be an insurmountable barrier to data collection considering the number of public schools. Then, we turned to Reports of the mid-1900s for the list of public high schools which were established in these cities until 1880. Next, all available data were digitized on these high schools in the 1890/91 Report. If a school did not report despite being established pre-1880, we searched for it in the 1892/93 Report. For high schools which existed in 1890 but had no establishment year, we searched the web to gather information about their establishment year. As a result of this process, we obtained information on the number of teachers and students (split by gender) in public high schools around 1890. We believe that this value should be strongly positively correlated with its counterpart in 1880 since high school completion rates started their rapid increase only after the turn of the century (Goldin, 1998; Goldin and Katz, 2008). One might also argue that in the largest, fastest growing cities schools might have been split between 1880 and 1890 and, consequently, we underestimate the true extent of high school provision. Nonetheless, we show in the relevant analysis that our results are robust to the omission of these metropolises.

⁴⁷Preparatory schools are not included in our high school student shares since we do not know the exact number of male students.

⁴⁸We digitized the number of students in day education if it was available. Otherwise, the missing value was imputed with the number of all students including evening schooling.

Imputation of missing values Before the creation of the final measures of schooling supply, missing values for the six school types had to be imputed. We followed the same procedure for all of them (except for public high schools - see the last paragraph of this section). First, if the number of all students was missing, we used the unweighted average of the same type of schools within-state (if there were less than ten such schools, then within-census division). The number of male students was imputed using the unweighted share of males in the same type of schools within-state (if there were less than ten such schools, then within-census division) and multiplying it by the (imputed) number of all students. The number of teachers was imputed similarly - the unweighted average of the same type of schools within-state (if there were less than ten such schools, then within-census division). Finally, a missing tuition fee was imputed as the number of students-weighted tuition fee within-state (if there were less than ten schools of the underlying type, then within-census division).

The aggregation of school-level measures to the county level amounts to a simple summation of the number of students and teachers, and taking the weighted average (by number of students) in case of the tuition fee. The five different private school types were pooled together before summation. The share of private and public high school students was calculated as the number of male students divided by the number of males aged 14-20 in a given county in 1880. The teacher-pupil ratio is defined as the student-weighted ratio of teachers to all students (male and female) at each school. The share of students having technical education (at institutions for secondary instruction or preparatory schools) was constructed as follows. All students who were at a school which offered mechanical drawing or had a chemical laboratory were indicated as having technical education. The sum of these students is divided by the total number of students at the county level.

For public high schools, the strong dependence of school size on local population necessitated a different imputation strategy. First, we ran the following regression:

$$y_{c,s} = \beta \cdot Population_{c,1880} + \gamma \cdot Population_{c,1880}^2 + f_{state(c)} + \epsilon_{c,s} \quad (4)$$

where $y_{c,s}$ is the number of students or teachers in city c and public high school s . City population and its squared form (population figures are from the Report of the Commissioner of Education in 1880), and the state-fixed effects produce an $R^2 \approx 0.5$. This model is used to impute the missing number of students and teachers if a public high school already existed before 1880. To split the number of students by gender, the average gender ratio is used within state - if at least ten public high schools have non-missing data -, otherwise the average of public high schools in the census division.

B.4 Construction of other variables

B.4.1 Occupational employment growth until 1870

The full count censuses of 1850, 1860 and 1870 are used to compute changes in employment shares in the period preceding the Second Industrial Revolution. In all three years, we dropped individuals who were not between 16

and 65 years old and gave a non-occupational response (*OCC1950* codes larger than 978). Then, we calculated the share of every harmonized occupation for each census division. Last, we created the differences between 1850-60 and 1860-70, and merged them to fathers in 1870 based on their occupation and census division.

B.4.2 Occupation-state level measures in the pre-period

We computed several occupation-state level measures based on the 1850, 1860 and 1870 full count censuses. To do so, the census was first restricted to individuals between 16 and 40 years old. We assigned to every state the occupational level i) probability of changing occupation; ii) probability of migration (changing county or state), iii) average change in the urban status dummy, iv) probability of having agricultural occupation at the end of the decade - based on individuals who at the beginning of the decade (1850s and 1860s) lived in the given state.

B.4.3 Imputing self-employed income

We followed the literature in imputing the income of self-employed individuals in the 1940 full count census (see e.g., [Collins and Wanamaker, 2017](#); [Ward, forthcoming](#)). As a first step, a sample of male self-employed workers was created in the 1960 5% census. We calculated the ratio between the total income and wage income for these individuals. Finally, the wage of self-employed individuals with non-zero reported wage in 1940 was inflated by the median of the calculated ratio (1.89). The main assumption of this imputation is that the ratio remained constant between 1940 and 1960.

For self-employed individuals, who reported zero wage earned in 1940, we use the median total income obtained from the 1960 census after a conversion from 1960 to 1940 dollars and conditional on reporting more than 50 weeks worked. We calculated the median separately for the agricultural (*OCC1950*: 100, 123, 810, 820, 830, 840) and non-agricultural self-employed.

B.4.4 Characteristics of next-door neighbors

To calculate observable measures for next-door neighbors, we first took every household head from the 1870 census. This data set is sorted, so neighbors appear next to each other. To every household head we assigned its ten closest neighbors, i.e. the five household heads right before and after a given person. Afterwards, the real estate and personal property values were winsorized at the 1st and 99th percentiles. Finally, the average of observable neighbor characteristics was computed and they were assigned to the 1870 full count census. We use the occupational education rank estimated for the 1880 (earliest) cohort by [Song et al. \(2020\)](#) for all neighbors. Literacy (foreign-born status) is measured as a dummy which is set equal to one if a given neighbor could read and write (was born outside the US).

B.5 Inverse proportional weights

To create inverse proportional weights for the sons' sample, sons were linked between 1870 and 1900 with the two conservative linking methods developed by [Abramitzky et al. \(2020\)](#). First, we merged the full count census of 1900 to the crosswalk, keeping matched as well as unmatched observations. Next, we also merged this data set with the 1870 full count census. If an observation could not be matched with any of the two conservative linking methods, we considered it unmatched and generated a variable which was set to zero for this case (one otherwise). Then, we used this binary variable as an outcome of a probit regression on age bins (following the code provided by [Abramitzky et al., 2020](#)), an urban place of living indicator, the population size category of place of living (*SIZEPL*) and census division-fixed effects - all measured in 1900. Finally, the inverse proportional weight for every single matched observation was calculated based on the following formula: $(1 - \hat{p}) / \hat{p}$, where \hat{p} is the predicted probability of a successful match. We set the weight equal to zero for observations which were unmatched.