Online Appendix

# Intergenerational Mobility in American History: Accounting for Race and Measurement Error

Zachary Ward*

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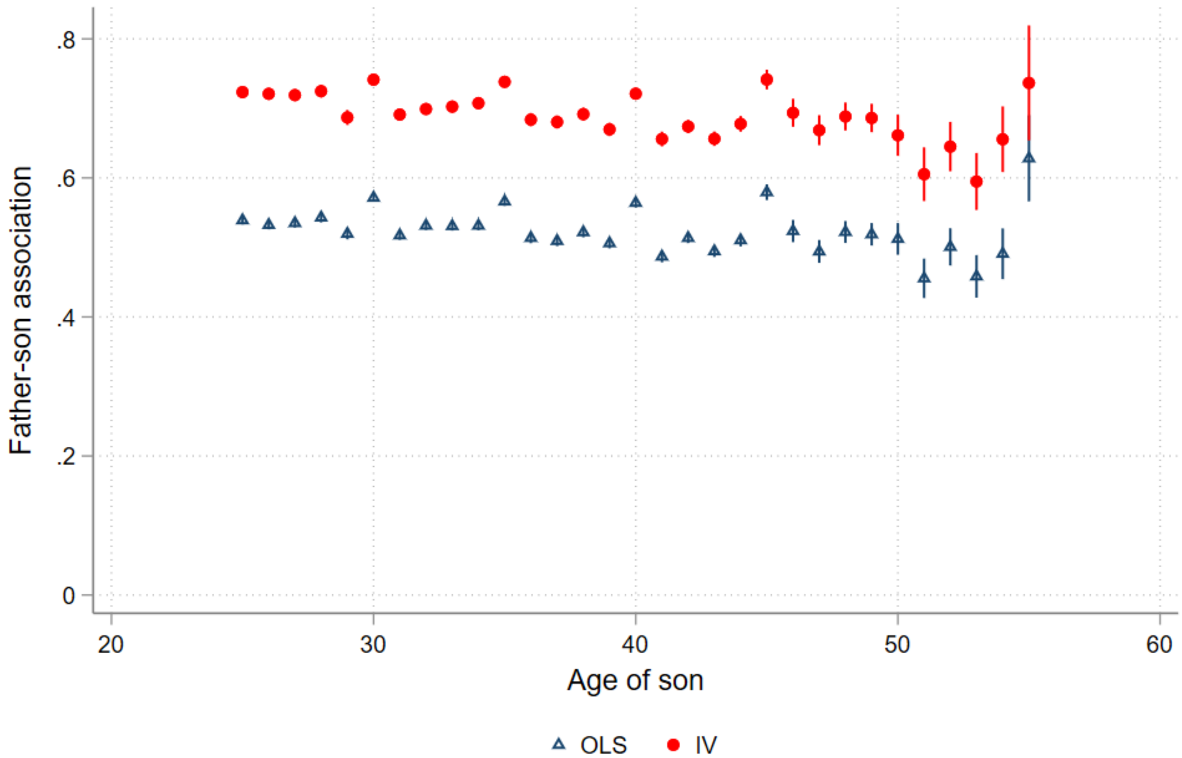
*email: Zach.A.Ward@gmail.com. Department of Economics, 1621 S 3rd St, Hankamer School of Business, Baylor University, Waco, TX, 76710. The collection of PSID data used in this study was partly supported by the National Institutes of Health under grant number R01 HD069609 and R01 AG040213, and the National Science Foundation under award numbers SES 1157698 and 1623684.

**Table A1.** Historical relative mobility estimates from linked datasets

| Paper | Linked years | Estimate | Mobility metric | Population (male unless noted) | Imputed Outcome | Outcome imputed by | Imputation from year |
|---|---|---|---|---|---|---|---|
| Song et al. (2020) | 1850-1880 | 0.19-0.24 | IGC | White | Lit./educ. rank | Occ. and birth cohort | 1850-1940 |
| Saavedra and Twinam (2019) | 1850-1880 | 0.46 | IGE | White | Income | Occ., sex, age, race, state, and industry | 1950 |
| Craig et al., (2019) | 1850-1880 | 0.24 | IGE | Males, MA marriage | Wealth | Occupation | 1870 |
| Craig et al., (2019) | 1850-1880 | 0.19 | IGE | Females, MA marriage | Wealth | Occupation | 1870 |
| Collins and Wanamaker (2021) | 1880-1900 | 0.43 | Rank | White | Earnings | Occ., race and region | 1940 |
| Collins and Wanamaker (2021) | 1880-1900 | 0.68 | Rank | Black | Earnings | Occ., race and region | 1940 |
| Ward (2020) | 1880-1910 | 0.50 | IGE | White, 2nd gen | Income | Occupation | 1890, 1950 |
| Abramitzky et al. (2021) | 1880-1910 | 0.27 | Rank | White, 2nd gen | Income | Occ., race, state, age and country | 1940 |
| Abramitzky et al. (2021) | 1880-1910 | 0.36 | Rank | White, 3rd-plus gen | Income | Occ., race, state, age and country | 1940 |
| Song et al. (2020) | 1880-1910 | 0.30-0.33 | IGC | White | Lit./educ. rank | Occ. and birth cohort | 1850-1940 |
| Saavedra and Twinam (2019) | 1880-1910 | 0.40 | IGE | White | Income | Occ., sex, age, race, state, and industry | 1950 |
| Saavedra and Twinam (2019) | 1880-1910 | 0.60 | IGE | Black | Income | Occ., sex, age, race, state, and industry | 1950 |
| Craig et al., (2019) | 1880-1910 | 0.20 | IGE | Males, MA marriage | Wealth | Occupation | 1870 |
| Craig et al., (2019) | 1880-1910 | 0.17 | IGE | Females, MA marriage | Wealth | Occupation | 1870 |
| Collins and Wanamaker (2021) | 1910-1930 | 0.40 | Rank | White | Earnings | Occ., race and region | 1940 |
| Collins and Wanamaker (2021) | 1910-1930 | 0.54 | Rank | Black | Earnings | Occ., race and region | 1940 |
| Ward (2020) | 1910-1940 | 0.28 | IGE | White, 3rd gen | Income | Occupation | 1890, 1950 |
| Abramitzky et al. (2021) | 1910-1940 | 0.32 | Rank | White, 2nd gen | Earnings | Occ., race, state, age and birthplace | 1940 |
| Abramitzky et al. (2021) | 1910-1940 | 0.36 | Rank | White, 3rd-plus gen | Earnings | Occ., race, state, age and birthplace | 1940 |
| Kosack and Ward (2020) | 1910-1940 | 0.38 | Rank | White | Earnings | Occ., race and region | 1940 |
| Kosack and Ward (2020) | 1910-1940 | 0.32 | Rank | Black | Earnings | Occ., race and region | 1940 |
| Song et al. (2020) | 1910-1940 | 0.32-0.31 | IGC | White | Lit./educ.rank | Occ. and birth cohort | 1850-1940 |
| Feigenbaum (2018) | 1915-1940 | 0.21 | IGE | 1915 Iowans | Earnings | Actual number | 1915, 1940 |
| Feigenbaum (2018) | 1915-1940 | 0.17 | Rank | 1915 Iowans | Earnings | Actual number | 1915, 1940 |
| Parman (2011) | 1915-1940 | 0.11 | IGE | 1915 Iowans | Earnings | Actual number | 1915, 1940 |
| Bailey et al. (2020) | various-1940 | 0.23 | IGE | Born in Ohio or NC | Earnings | Actual number | 1940 |

Notes: Selected estimates from literature for those who reports an elasticity, correlation or association from a regression of son's outcome on the father's outcome. More mobility estimates appear in some of these papers, but the point of the table is that all estimated associations are much lower than this paper's estimates.

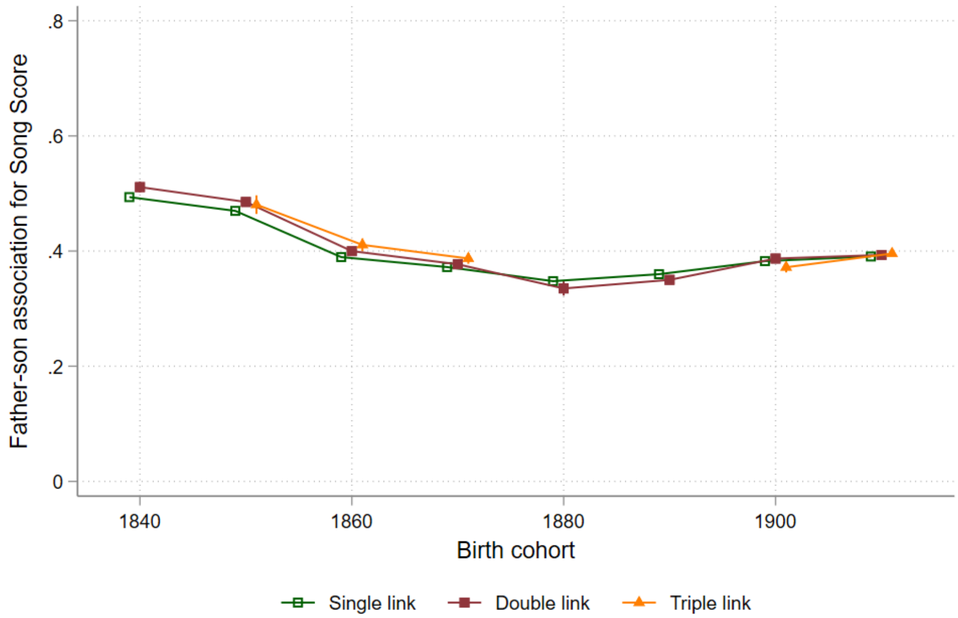
**Figure A1.** Father-son association does not vary much by age of son



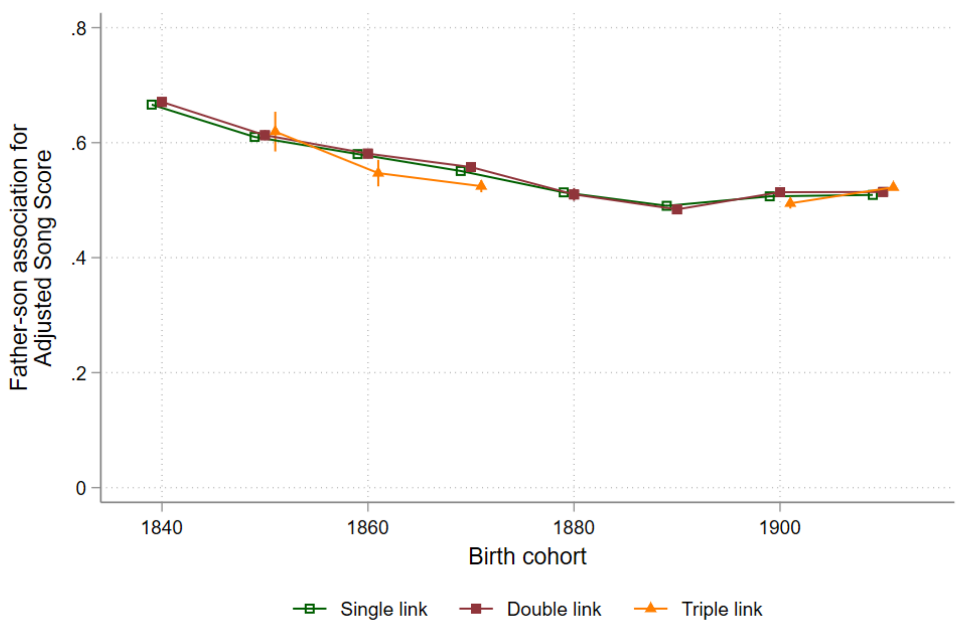

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. The above associations plot the father-son association that is separately estimated by the age of the son. The adjusted Song score is used to measure status.

**Figure A2.** Linking multiple times does not produce substantially different mobility estimates

Panel A. White-only sample

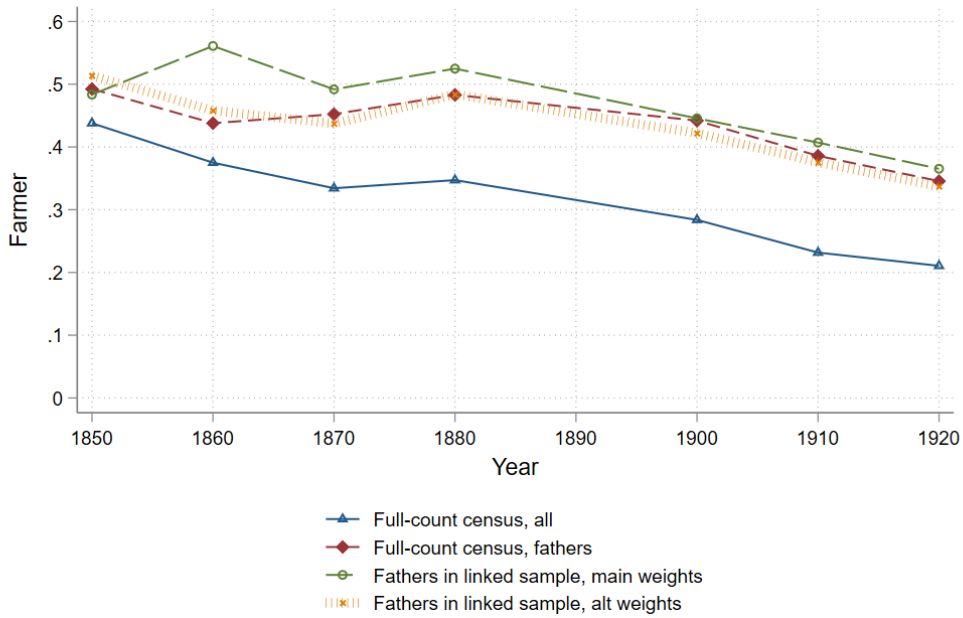



Panel B. Black and white sample



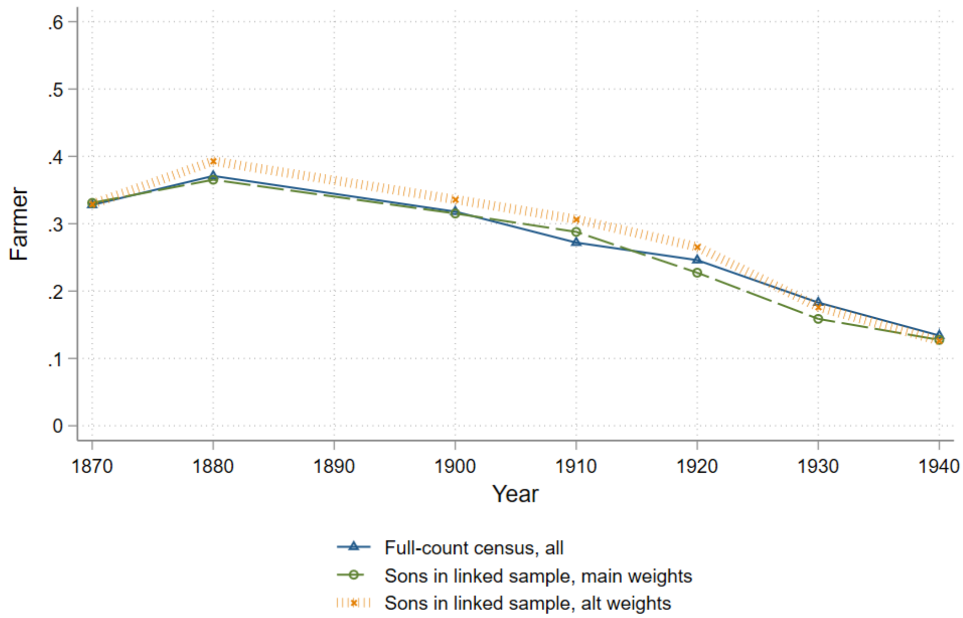

Notes: Linked samples between 1850-1940. Each figure shows the association between one son observation and one father observation. While the regression is the same across estimates (OLS), the underlying sample changes. The linked sample either observes the father one (single link), two (double link), or three times (triple link). No averaging is done for the above estimates. Note that the triple-linked sample is subsample of the double-linked sample, and the double-linked sample is a subset of the single-linked sample. Triple-linked estimates are missing for the 1840, 1880 and 1890 cohort because fathers cannot be linked to the 1840 or 1890 censuses. Each sample is weighted separately to match population characteristics.

**Figure A3.** Fraction of farmers compared to full-count data
Panel A. Characteristics of fathers observed as adults



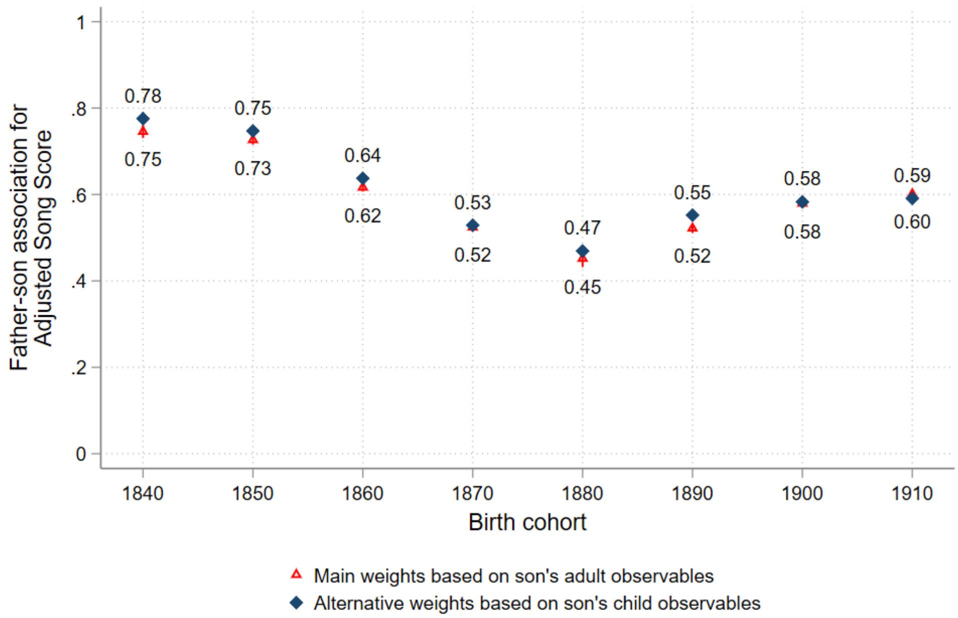

Notes: Data are either from a linked sample of fathers and sons, or the full-count cross-sections between 1850 and 1940. Panel A plots the fraction of each dataset that are farmers with respect to the father's year of observation in the linked data, while Panel B (next page) plots the fraction farmers with respect to the son's year of observation in the linked data. The "full-count, all" lines are the fraction farmers in the full-count census, where the sample is limited the same age range as in the linked sample. The "full-count census, fathers" line plots the fraction farmers for cross-sectional sample where the individual also has a 0-14 year old son in the household. The "fathers in linked sample, main weights" uses inverse proportional weights to make the sample representative of the *son's* adult outcome. The "fathers in linked sample, alt weights" uses inverse proportional weights to make the sample representative of the *father's* adult outcome (or, equivalently, the son's child outcome). This figure shows that fathers are more farmer-heavy than the overall population, and that the main weights do not completely pin down the fraction farmer correctly for *fathers*. However, using alternative weights that does capture the correct share of farmer fathers does not lead to substantially different mobility estimates (see Figure A4).

Figure A3 continued.
Panel B. Characteristics of sons observed as adults

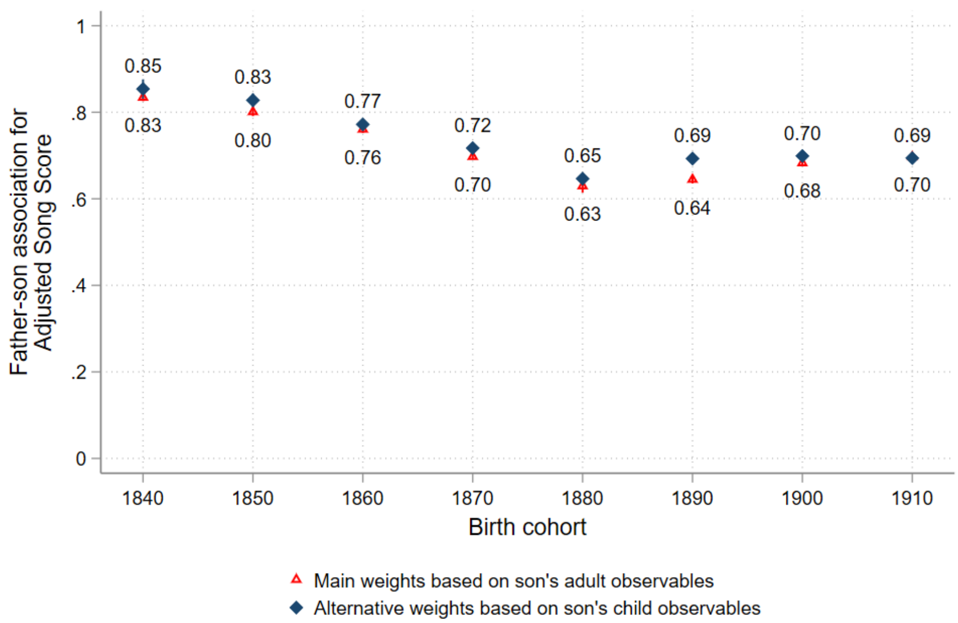



Notes: Data are either from a linked sample of fathers and sons, or the full-count cross-sections between 1850 and 1940. Panel A (prior page) plots the fraction of each dataset that are farmers with respect to the father's year of observation in the linked data, while Panel B plots the fraction farmers with respect to the son's year of observation in the linked data. The "full-count, all" lines are the fraction farmers in the full-count census, where the sample is limited the same age range as in the linked sample. The "Sons in linked sample, main weights" uses inverse proportional weights to make the sample representative of the son's adult outcome. The "Sons in linked sample, alt weights" uses inverse proportional weights to make the sample representative of the *father's* adult outcome.

**Figure A4.** Trend in relative mobility when using weights based on the father's observation
Panel A. White-only population




Panel B. Black and white



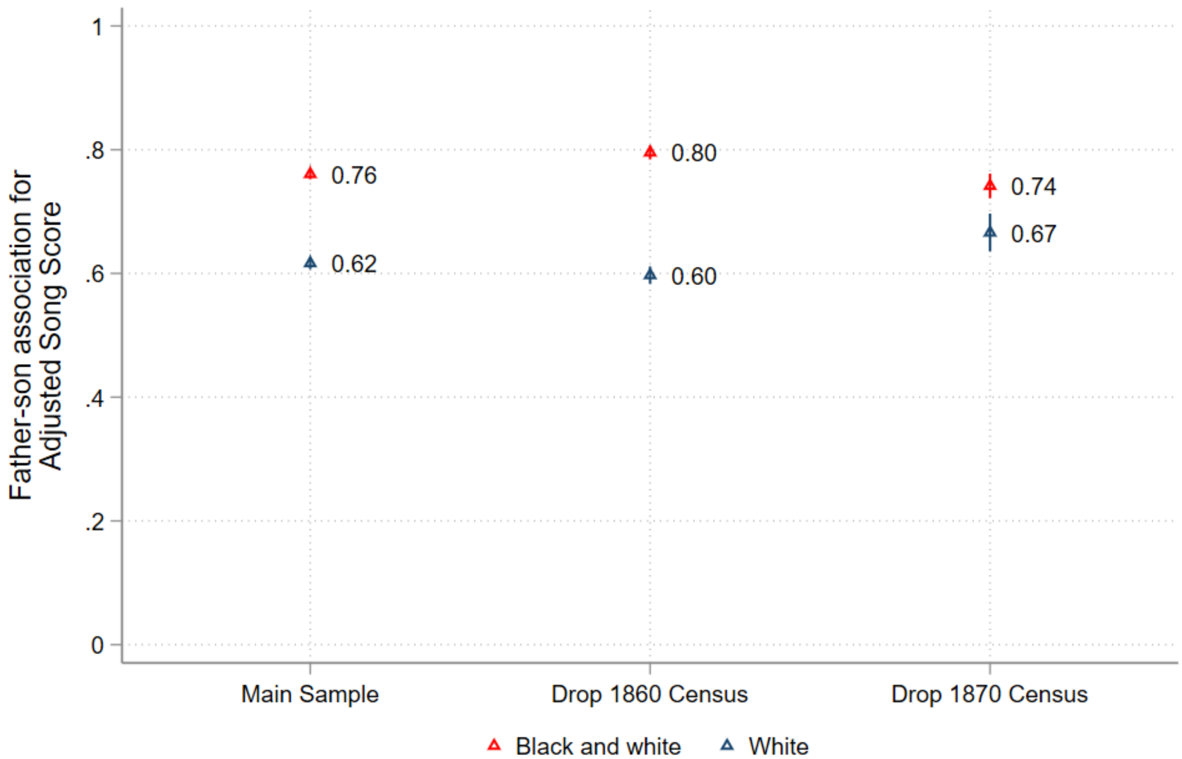

Notes: Data are either from a linked sample of fathers and sons between 1850 and 1940. Main weights are the preferred weights throughout the main paper and are based on the sons' adult outcome (or the second census for the link between the son's child and adult outcome). Alternative weights are weighted based on the sons' child outcome (or the first census for the link between the son's child and adult outcome).

**Figure A5.** Trend in mobility based on different weights for the size of the family
Panel A. White population


Panel B. Black and white population

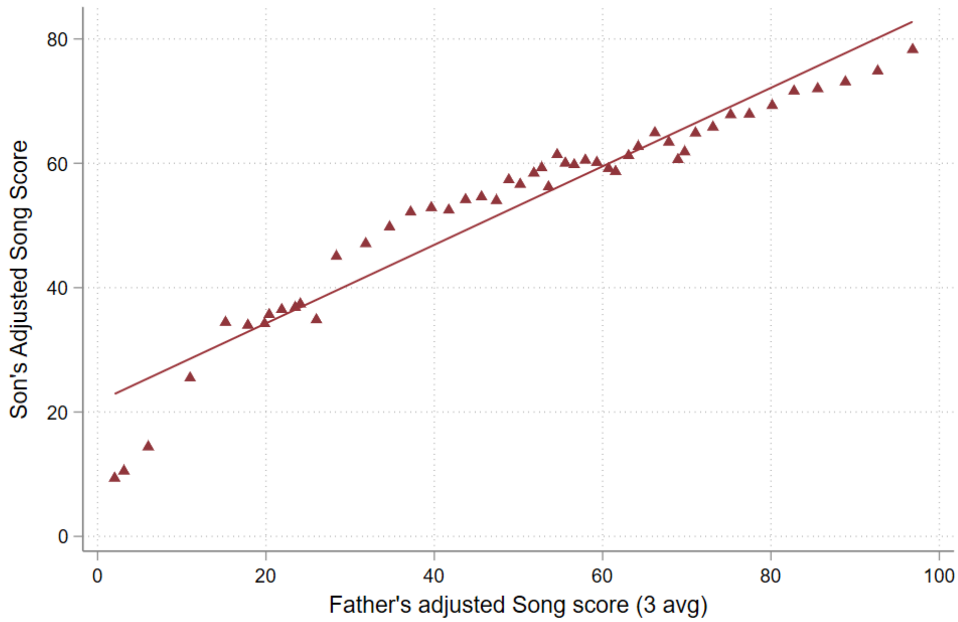

Notes: Data are from a linked sample of fathers and sons between 1850 and 1940. Main weights are the preferred weights throughout the main paper, such that each observation is a father-son pair. Note that the father can show up multiple times if there are multiple sons. The estimates that "Adjust weights to the inverse of family size" multiply the weight by the inverse of the size of the family, which down weights larger family.

**Figure A6.** Mobility **e**stimates for the 1860 birth cohort are not affected when "burning" the 1860 or 1870 Censuses

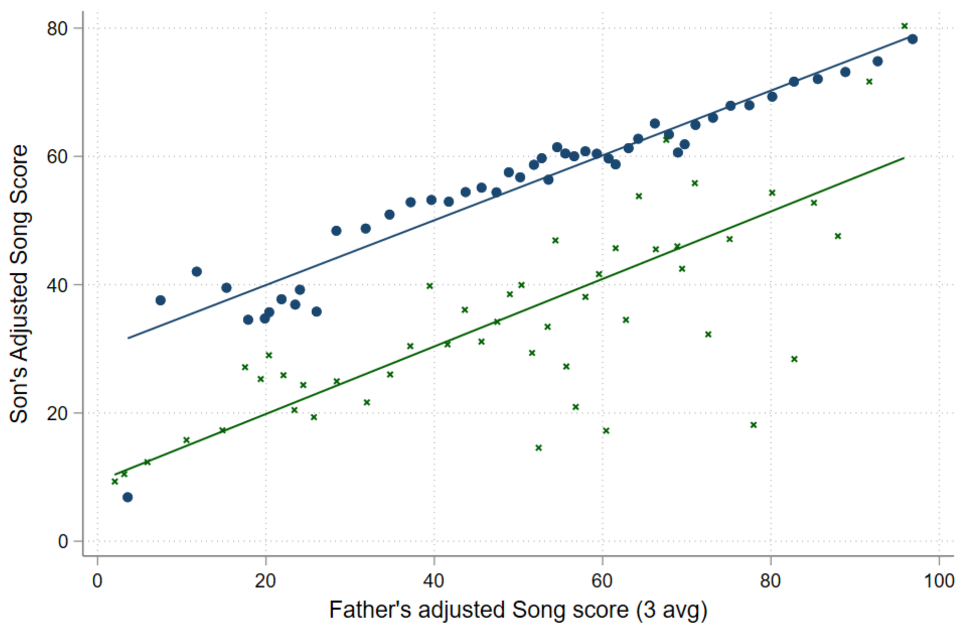



Notes: Data are from a linked sample of fathers and sons across US Censuses. This figure tests whether estimates for the 1860 birth cohort (rounded from 1855-1864 birth years) differ when "burning" nearby censuses. This check is meant to determine whether the burning of 1890 Census manuscripts matter for estimates. The "Main Sample" uses all available data and are the main IV estimates in the paper. The "Drop 1860 Census" estimates assumes that the 1860 Census is not available, and thus estimates for the 1860 birth cohort are based on 6-14 year-old children observed in the 1870 census. The "Drop 1870 Census" assume that the 1870 Census is burned, and thus estimates are based on 0-5-year-old children observed in the 1860 Census. Since "Main Sample" point coefficients are similar to those when dropping the 1860 or 1870 Censuses, the results suggest that missing a single cross-sectional census does not strongly alter mobility estimates.

**Figure A7.** Father-son association of adjusted Song score is approximately linear
Panel A. Pooled

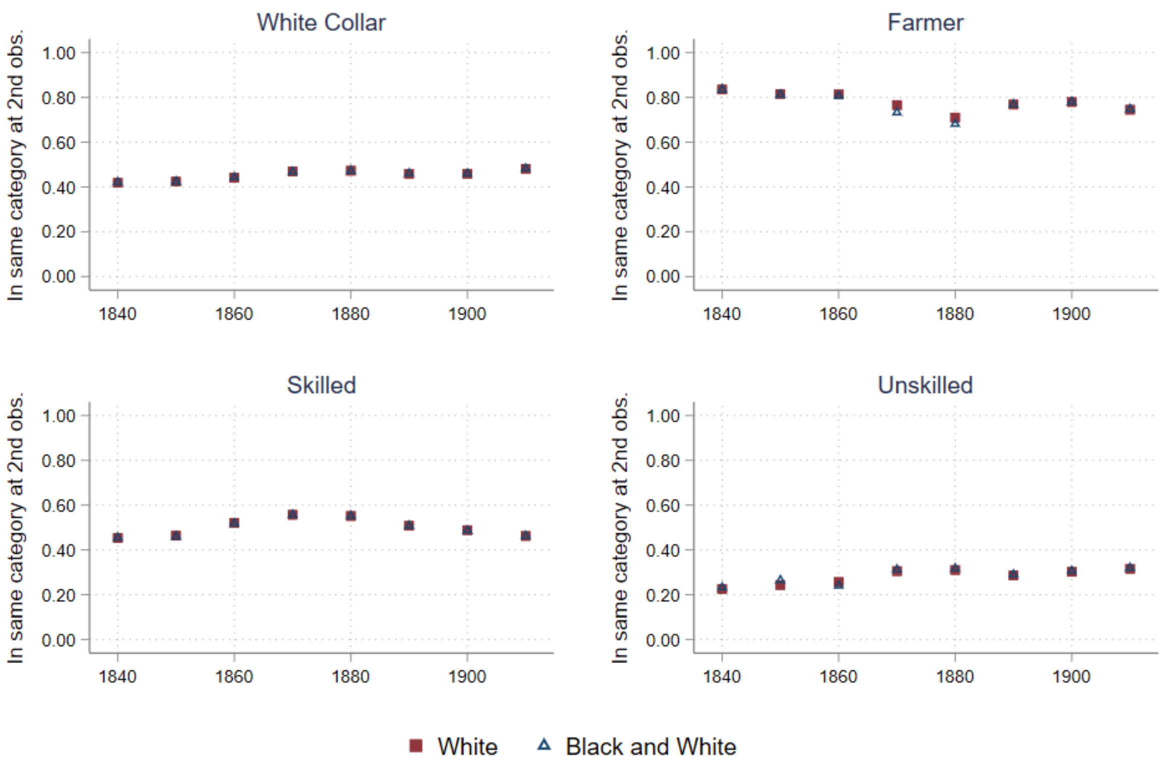



Panel B. Within race




Source: Data are from 1850-1940 linked US censuses.
Notes: The figure shows the binscatter plot when using the average of three father observations. The adjusted Song score assigns a 0-100 measure based on a ranking of the mean literacy rate/education level by occupation, race, region and birth cohort (See Appendix C and Song et al. 2020).

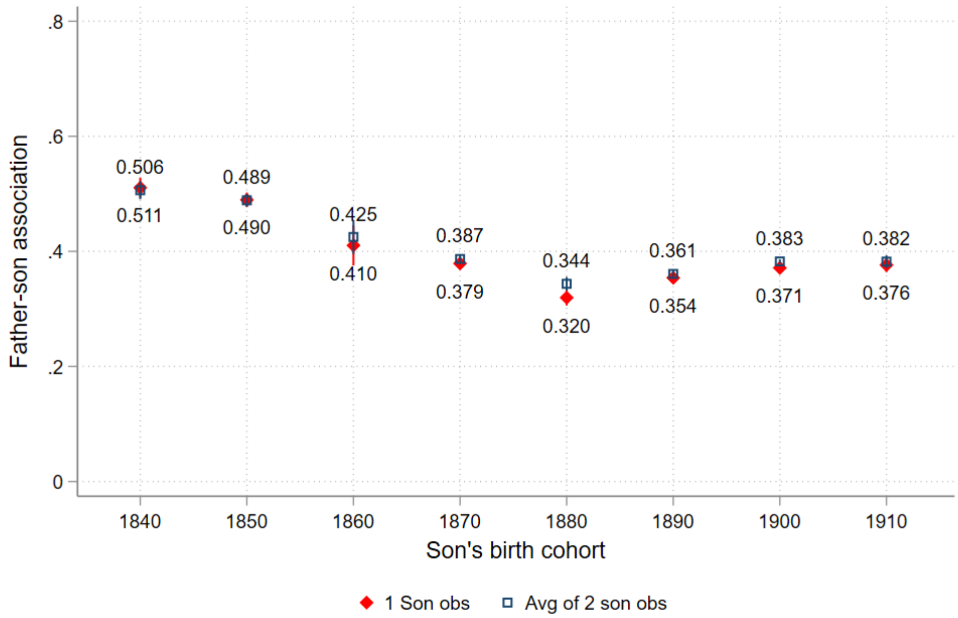
**Figure A8.** Intragenerational associations of belonging to an occupation category



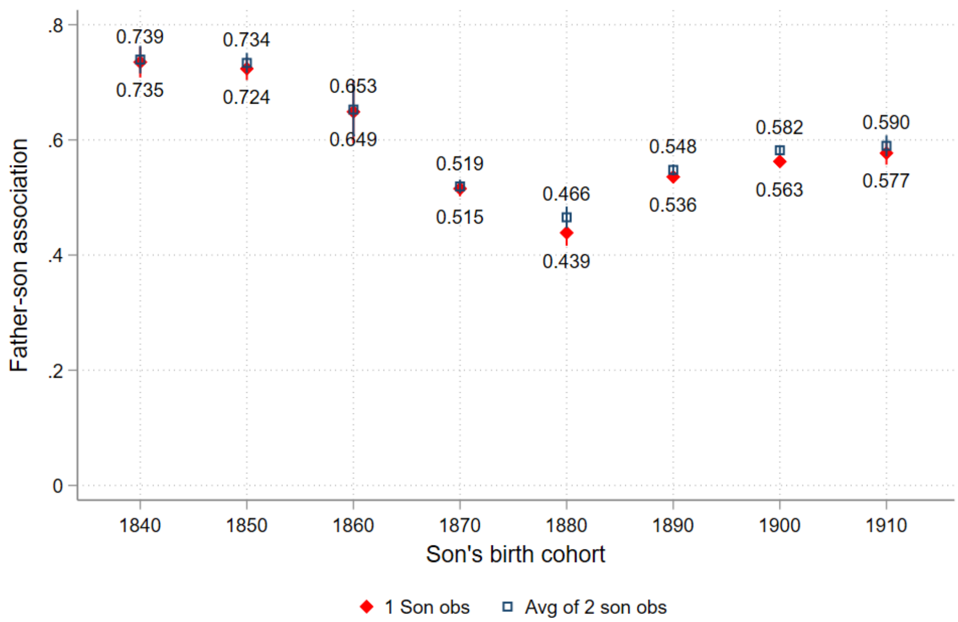

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. White-collar occupations are professional (*occ1950* codes: 0-99), managers (200-299), clerical (300-399), and sales (400-499). Farmers are owners and tenants, as well as farm managers. Unskilled are operatives (600-699), Service workers (700-799), farm laborers and laborers (800-970). Skilled are Craftsmen (500-599).

**Figure A9.** Averaging the son's outcome does not strongly influence associations

Panel A. OLS estimates

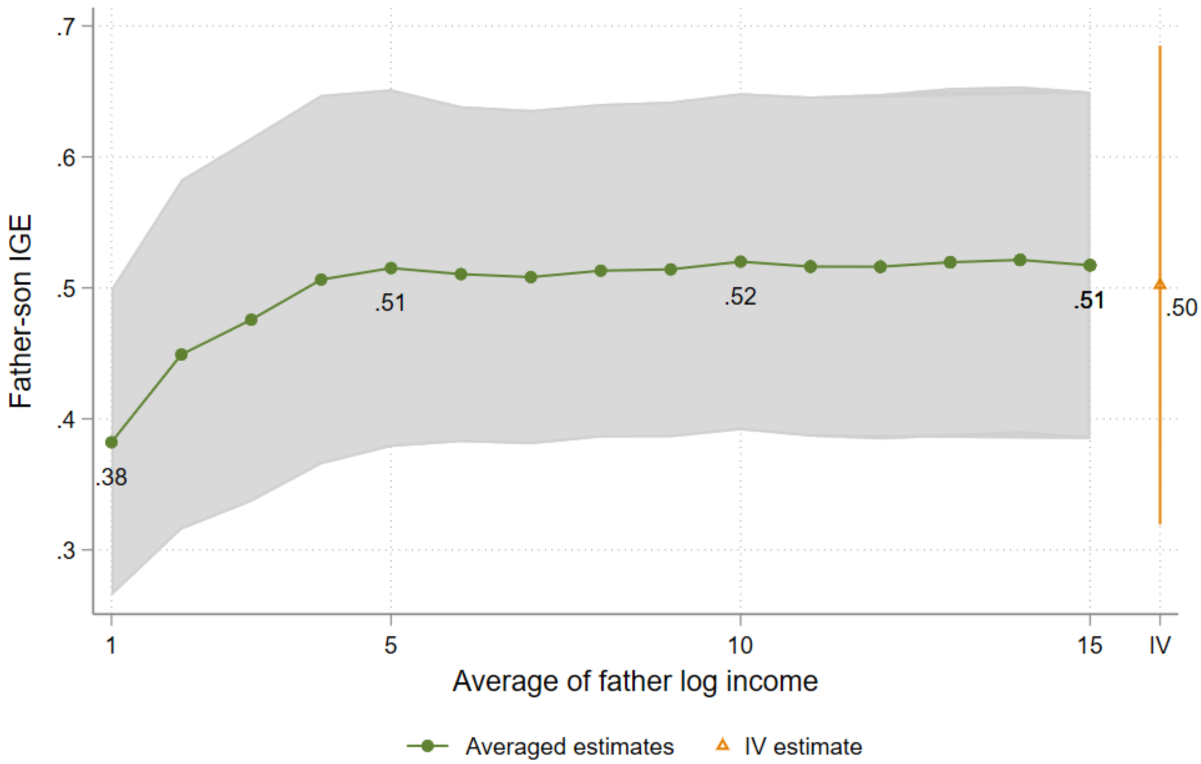



Panel B. IV estimates




Notes: Data are a linked sample of white fathers and sons from the 1850-1940 United States Censuses. The dependent variables in this figure is the average of the son's Adjusted Song score across two observations. Therefore, estimates are for the subsample of the main data where a son was successfully linked between childhood and adulthood two times. Similar to the father sample, I only keep sons who are observed 10 years apart.

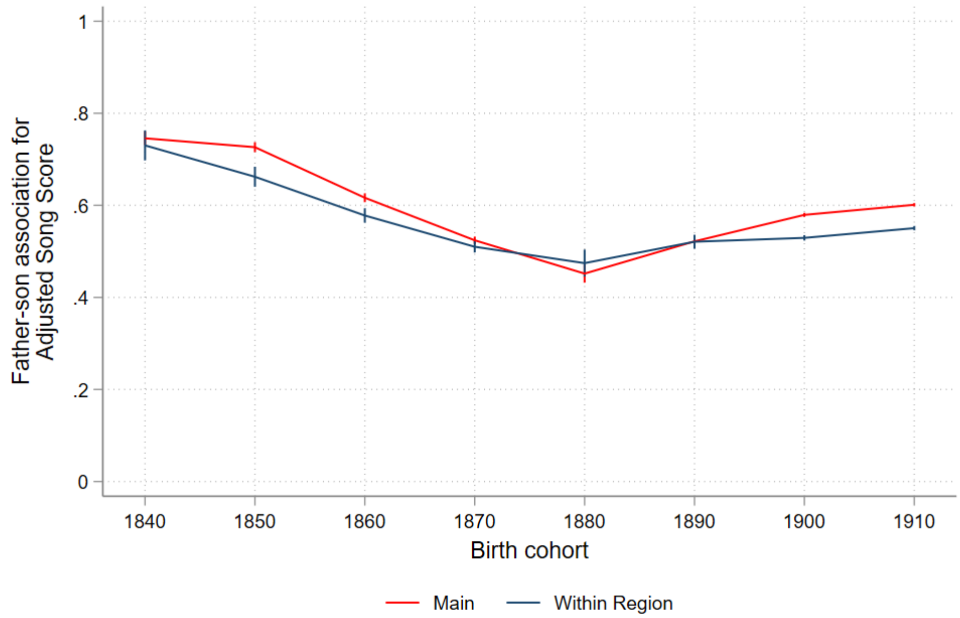
**Figure A10.** IV estimates of the IGE are similar to averaging the father's income more than five times



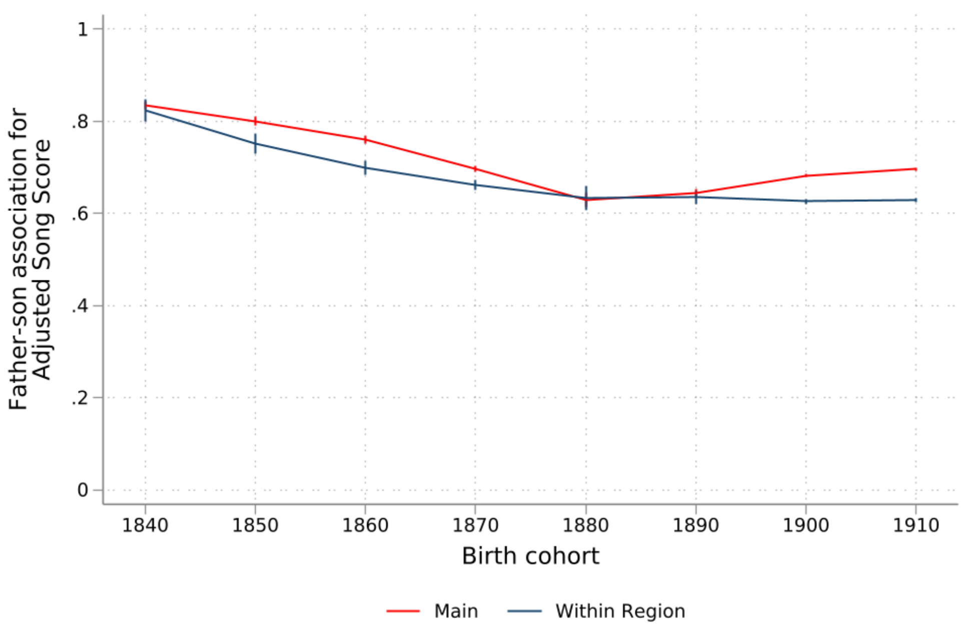

Notes: Data are from the Panel Study of Income Dynamics. The sample is of 1,492 fathers and sons where the father has at least 15 labor income observations. Each point comes from a different regression, either from the log of the average father's labor income, or from instrumenting the father's log labor income with an observation that is between 3-20 years away (with 10 years away being the preferred distance). The single-year estimate comes when the father is observed closest to age 40. The method of averaging then uses the next closest observation to this "prime" age, typically moving plus one and then minus one year away from age 40. The sample is weighted in the way discussed in Appendix K, where inverse probability weights are created after pooling the PSID with the CPS and predicting selection into the PSID sample based on age, race, and broad occupation category. 95% confidence intervals are plotted.

**Figure A11.** Estimates controlling for parental region.

Panel A. White population

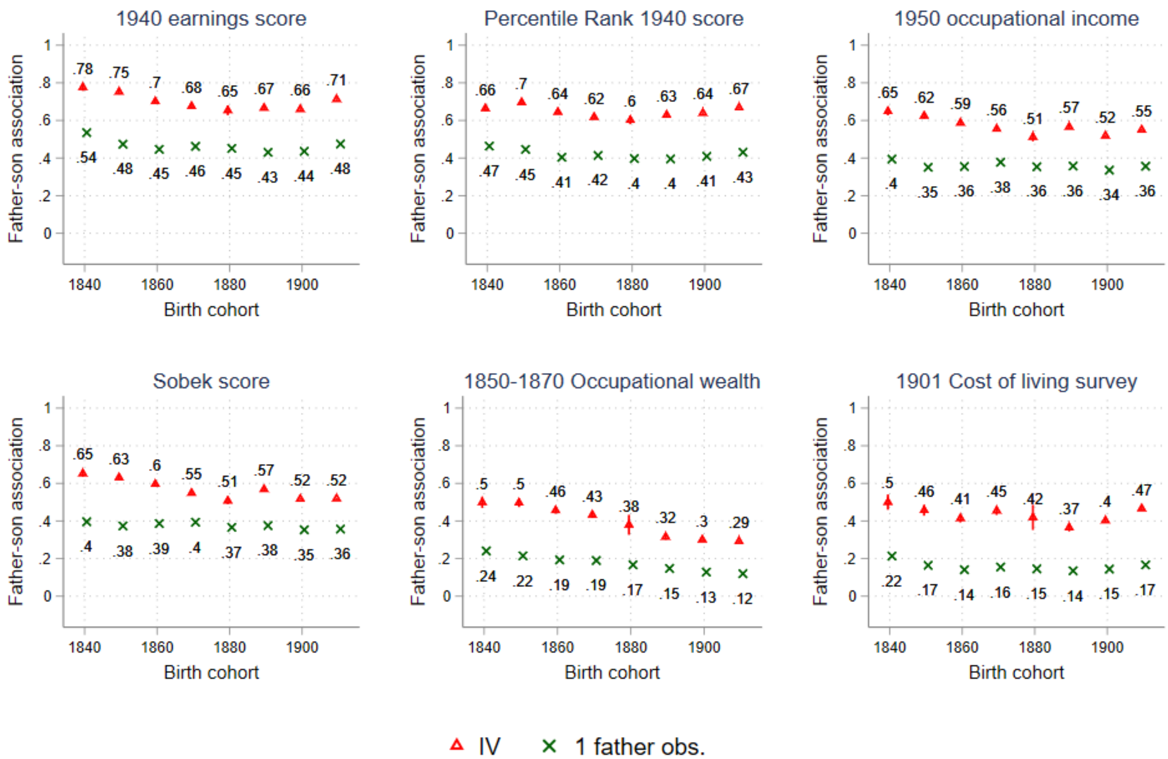



Panel B. Black and white




Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. The "Main" estimates are the preferred point estimates in the paper. They come from regressing the son's status on the father's status, after instrumenting the father's status with a second observation. The "Within Region" estimate control for the parent's region of residence.

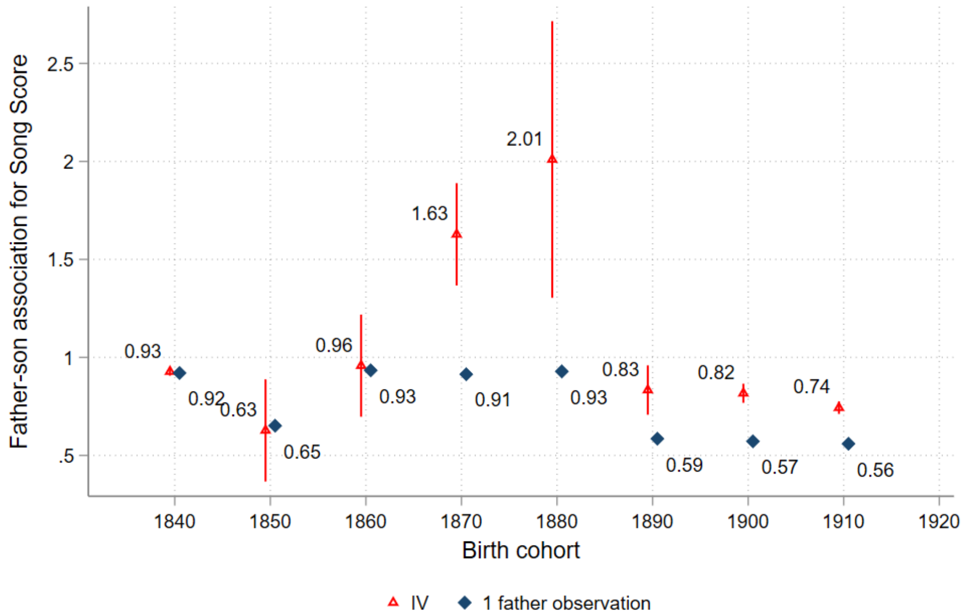
**Figure A12.** Measurement error matters for other status measures that are based on occupation



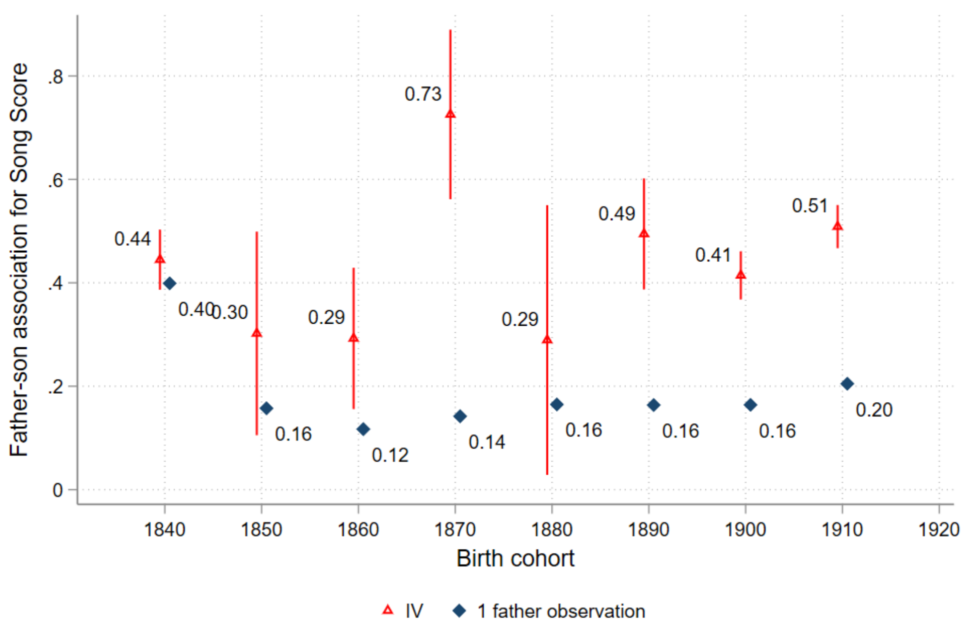

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. The 1940 earnings score is mostly based on 1940 wage income by occupation, race and region, though adjustments are made for self-employed workers and farmers (see Collins and Wanamaker (forthcoming) for a similar score and Appendix C from Kosack and Ward (2020)). The 1950 occupational income score is based on the *occscore* variable from IPUMS. The Sobek score is based on 1890 income information from Sobek (1996). The occupational wealth score is based on 1850-1870 wealth (real estate plus personal property when available) by occupation. The 1901 Cost of Living Survey score is based on a survey of urban, married workers salary by occupation (see Abramitzky, Boustan and Eriksson 2012). Besides the percentile ranked 1940 score, all other measures are logged.

**Figure A13.** Trend in within-Black mobility

Panel A. Adjusted Song score (status by occupation, race and region)

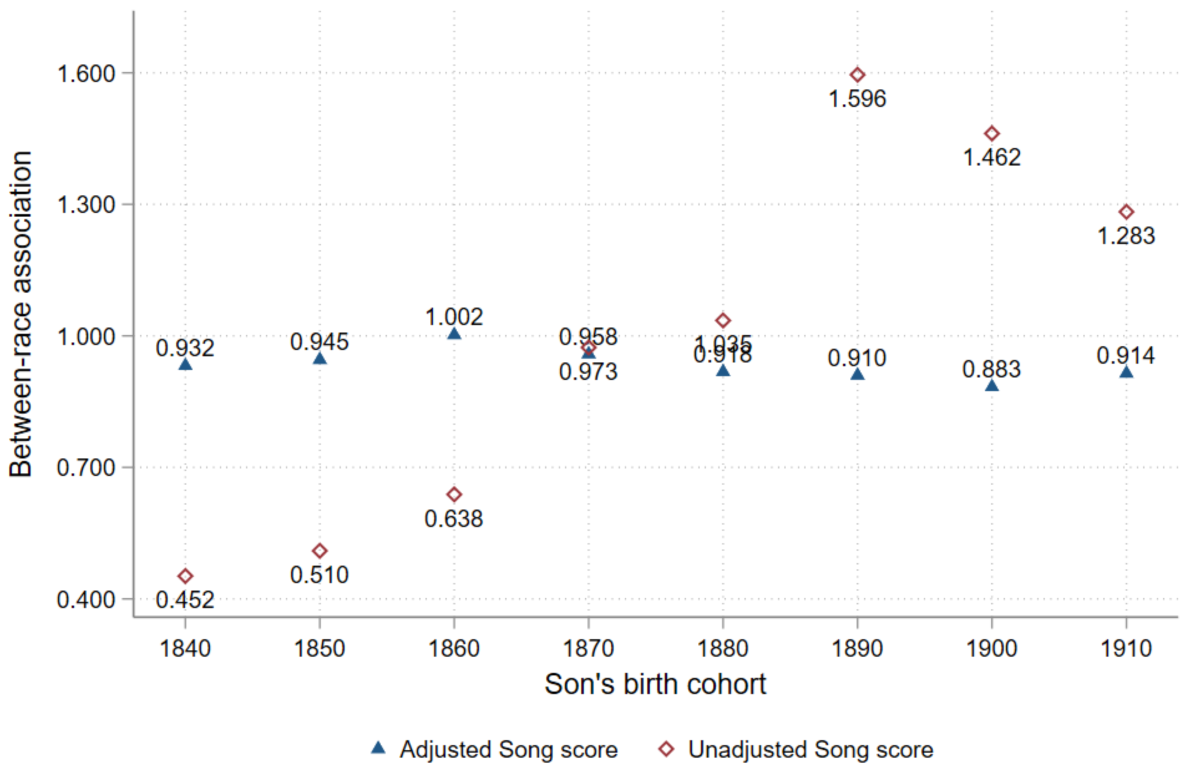



Panel B. Song score (occupation-only status)




Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. The sample is limited to Black sons. Note that associations can be above one because most Black families are concentrated in the bottom of the distribution (less than 25th percentile).

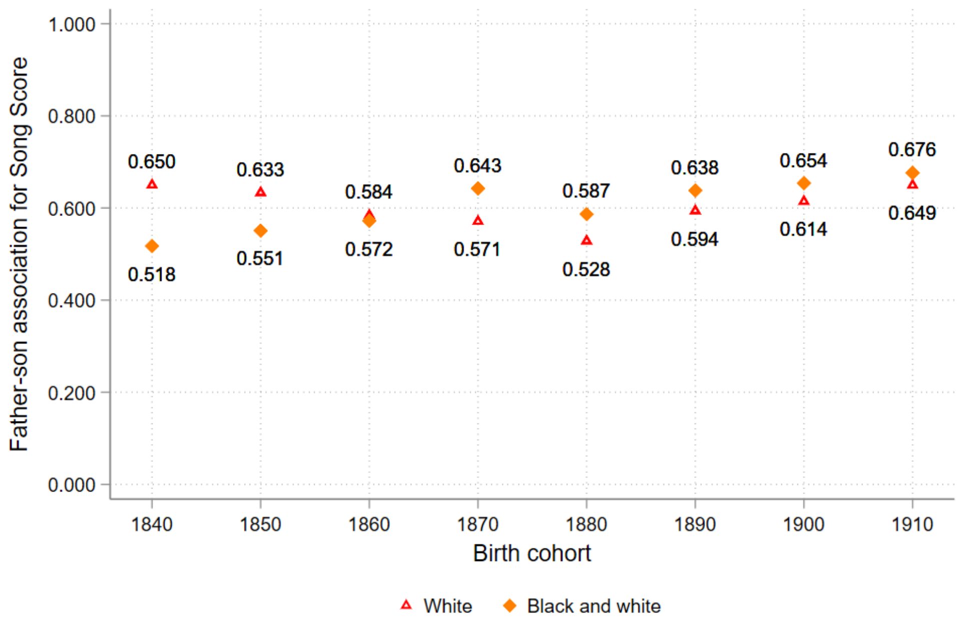
**Figure A14.** Between-race associations



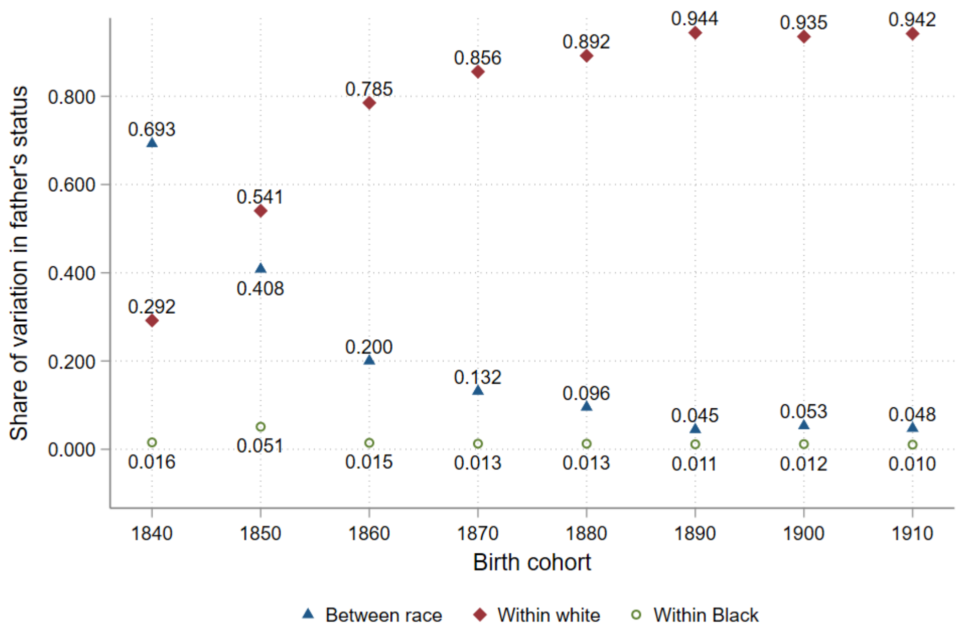

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. The results are from a regression of the son's status, averaged by race, on the father's status, averaged by race. This results captures the persistence of Black-white gaps across generations.

**Figure A15.** Father-son associations increase after including Black families

Panel A. Status measure does not adjust for within-occupation differences by race and region

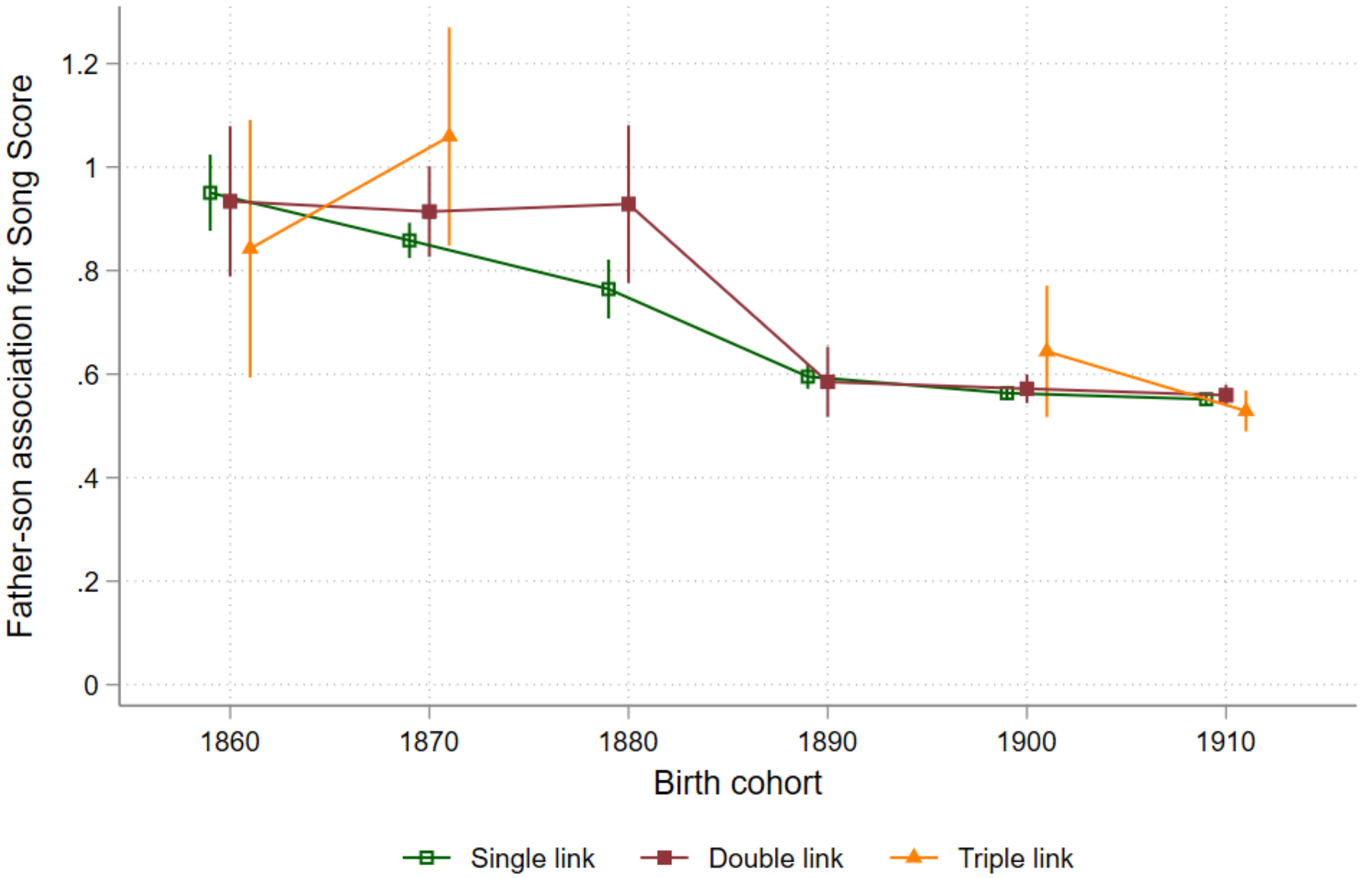



Panel B. Within-between decomposition of variation



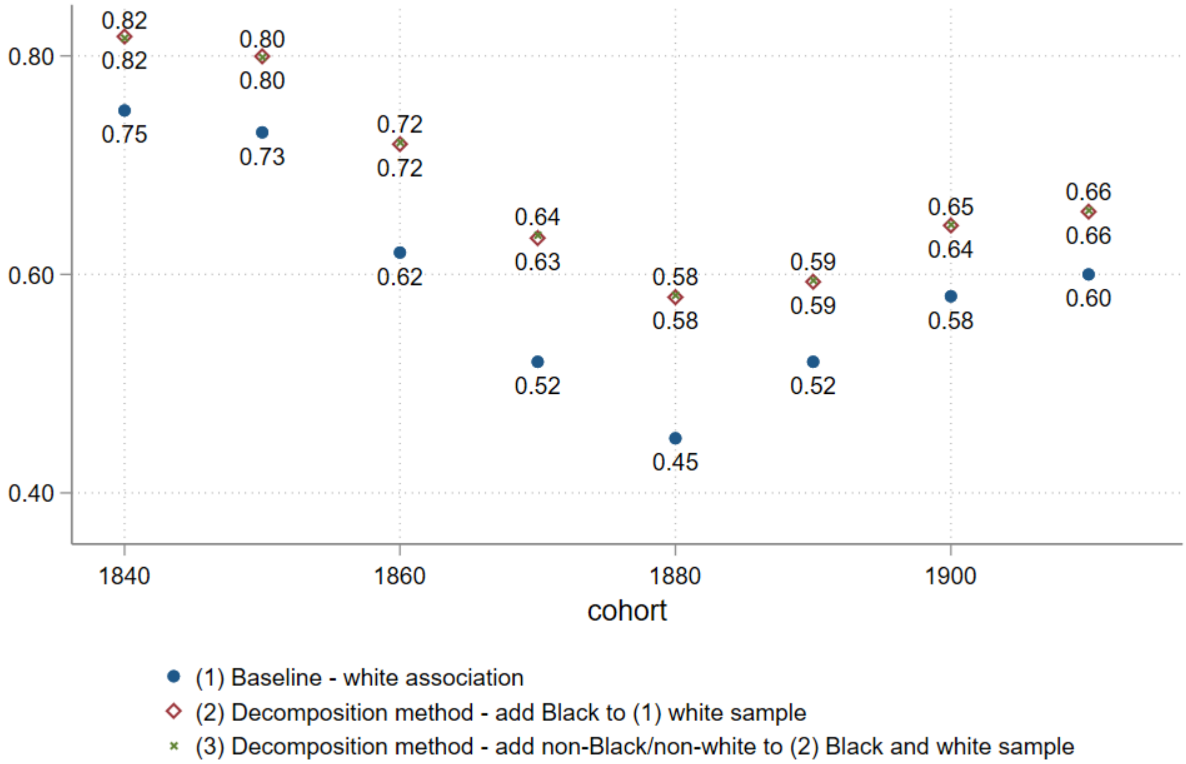

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. Panel A shows IV estimates of the father-son association based on a sample of white fathers and sons, or a pooled sample of white and Black fathers and sons. Panel A measures status based on the human capital level by occupation. Panel B plots the within-share and between-shares of variation in the linked data. The classical measurement error formula is used to eliminate error when calculating shares of variation.

**Figure A16.** Linking multiple times does not strongly influence Black estimates, except for the 1880 cohort




Notes: This figure shows the trend in relative mobility estimates between parent and child when using a sample that is single linked (one father and one son observation), double linked (two father and one son), or triple linked (three father and one son). Weights are created separately for each sample. The regression is the same across all samples, where the point estimate is a association between the son's adult status and a single father's status, with no averaging or instrumenting. The results show that mobility estimates are similar for post-1890 birth cohorts. They vary for 1860-1880 birth cohorts but are only statistically different for the 1880 cohort.

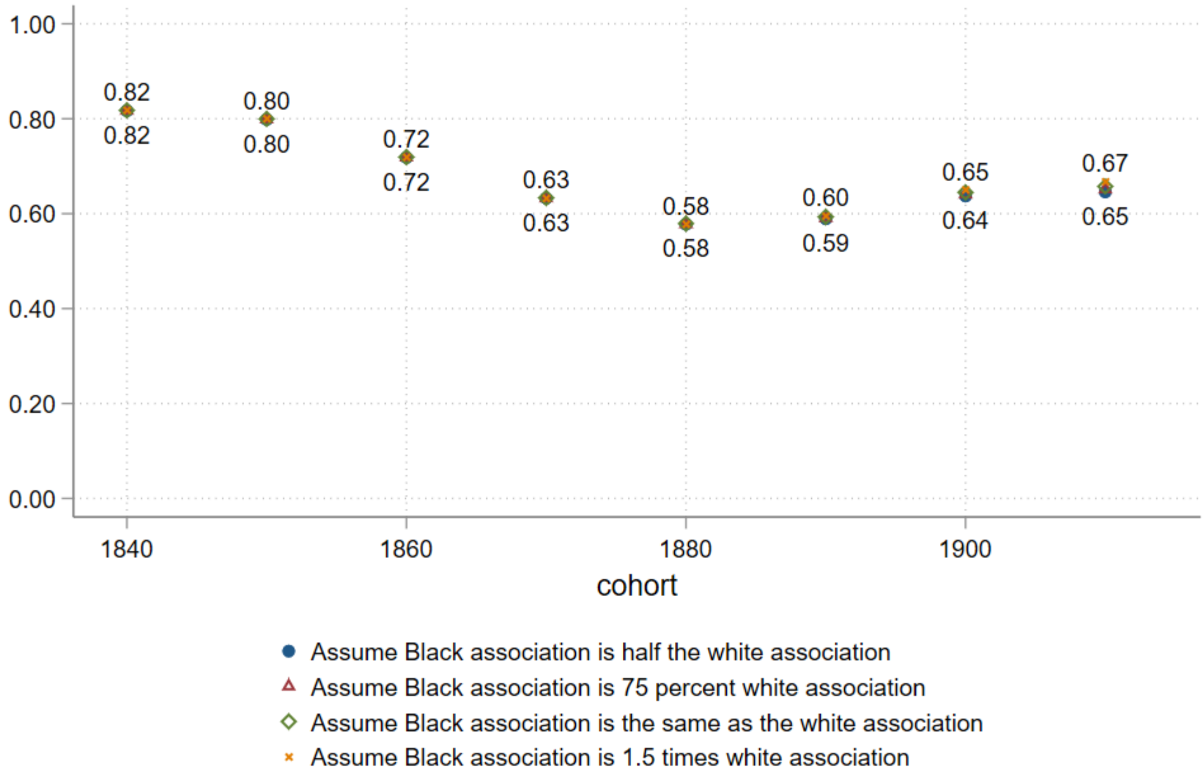
**Figure A17.** Estimates of mobility based on the within-between decomposition
Panel A. Including non-Black minorities




Notes: see next page

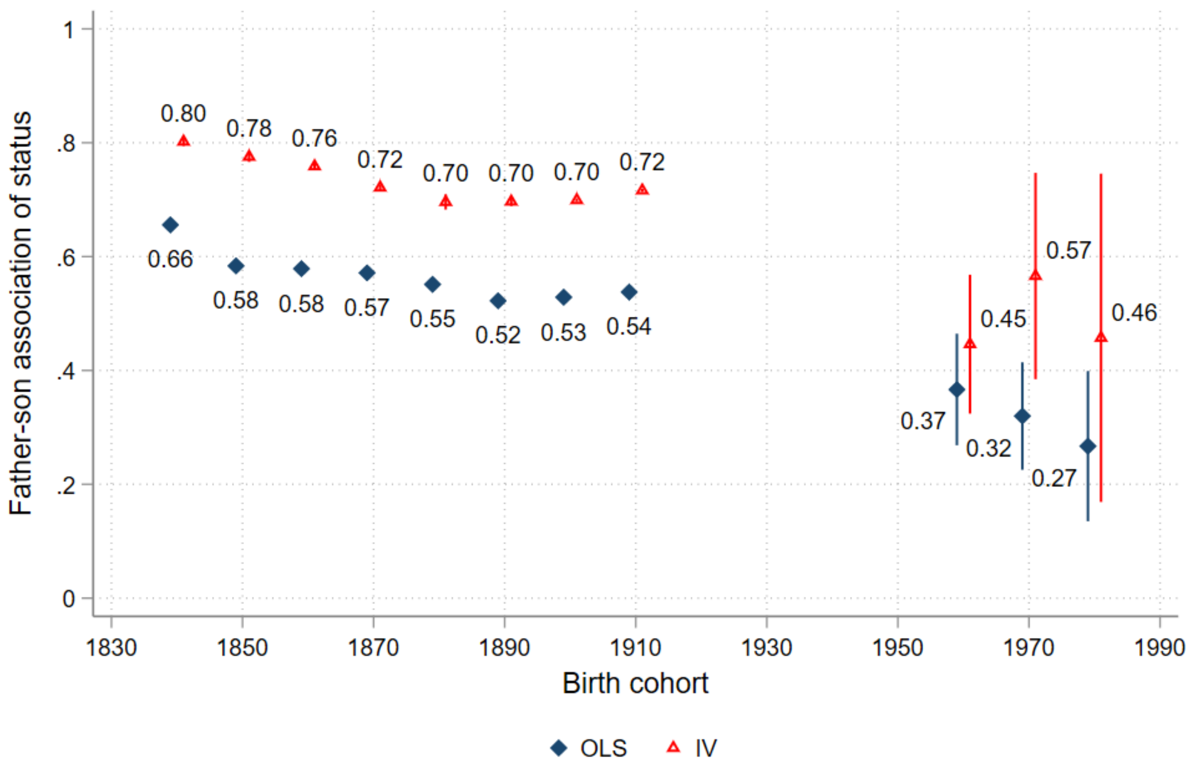
**Figure A17 (continued)**

Panel B. Various assumptions of within-Black mobility




Notes: Data are from the 1850-1940 full-count United States Censuses (Ruggles et al. 2020). The within-between decomposition $\hat{\beta}_1 = \sum_{g=1}^{G} \theta^g \hat{\beta}_1^g + \theta^b \hat{\beta}_1^b$ is used to estimate $\hat{\beta}_1$ for the population. The within-white, within-Black and within-other (Asian or American Indian) shares of variation $\theta^{white}$, $\theta^{Black}$, $\theta^{other}$ are directly measured in cross-sectional census data under the assumption that generations are 30 years apart. For example, for the son's 1910 birth cohort, $\theta^{white}$, $\theta^{Black}$ and $\theta^{other}$ is the share of variation for those born in the 1880 cohort and are fathers. The next part of the within-between formula, $\hat{\beta}_1^{white}$, $\hat{\beta}_1^{Black}$ and $\hat{\beta}_1^{other}$ are unobserved in cross-section data. I take the IV estimates of $\hat{\beta}_1^{white}$ from the linked data, and then assume $\hat{\beta}_1^{white} = \hat{\beta}_1^{Black} = \hat{\beta}_1^{other}$ in Panel A, or $\hat{\beta}_1^{Black} = a\hat{\beta}_1^{white}$ in panel B, where a=0.5, 0.75, 1 or 1.5. The between-race share of variation $\theta^b$ is measured directly in the cross section. Finally, $\hat{\beta}_1^b$ is also directly measured via a regression of the average status by race in the son's generation on the average status in the father's generation, under the assumption that generations are 30 years apart. There are two main points to this figure. First, Panel A shows that not including other races in the linked data (Asian or American Indian) does not influence population estimates. Second, Panel B shows that estimates of within-Black mobility, which may be biased in the linked data, do not strongly influence overall mobility estimates.

**Figure A18.** Long-run trend in mobility based on a percentile ranking of imputed earnings




Notes: Data are from the 1850-1940 United States Censuses and the PSID. Instead of measuring status via a percentile ranking of human capital by occupation/race/region, I measure it using a percentile ranking of imputed earnings by occupation/race/region. For 1850-1940 data, imputed earnings are based mostly on the 1940 Census, similar to Collins and Wanamaker (2022). For post-1940 data, imputed earnings are based income by occupation/race/region for the nearest decade, which is taken from the 1950-2000 Census samples from IPUMS, and the 2010 and 2019 ACS (Ruggles et al. 2020).

**Appendix B.** Details on weighting and representativeness

*Weighting process*

Since historical censuses do not contain unique identifiers, linking algorithms match people across censuses by stable characteristics: first name, last name, year of birth, race, and place of birth. However, many people are not linked across censuses, whether due to common names or messy underlying data. Ultimately, linked samples are unrepresentative of the underlying population. In this section, I discuss how I weigh the linked samples to address this problem and provide more detail about the linked sample's representativeness.

I weight the linked sample with inverse propensity weights that are custom created for the linked sample. This method is discussed in more detail in Bailey, Cole, and Massey (2020), but the general process is as follows:

(1) For each decade where the son's adult outcome is observed, I pool the linked sample with the full-count census. For example, if the son's adult outcome is observed in 1880, I pool the linked sample of adult sons observed in 1880 with the 1880 full-count census. I ensure the population includes the same age ranges as the linked sample.[1]

(2) I estimate a probit model to predict who is in the linked sample. The probit uses the following variables:
   - Black indicator variable
   - Age (10-year bins) and its interaction with the Black variable
   - Occupation category (White collar, semi-skilled, farmer, low-skilled) and its interaction with the Black variable
   - Region of residence (North, South, West, or Midwest) and its interaction with the Black variable

[1] One issue with weighting the foreign-born is that the linked sample includes only child arrivals (children 0-14 linked from their childhood household), while the full-count censuses include both child arrivals and adult arrivals. For the 1900-1930 Censuses, I drop adult arrivals using information on the year of immigration. For the other censuses, I randomly drop roughly 70 percent of the foreign-born population such that the share foreign-born for adults reflects the share foreign-born of 0-14 children in prior birth cohorts. Specifically, for the 1870 census, I match the share foreign-born of adults to the share foreign-born of 0-14 year-olds in the 1850 census; for the 1880 Census, I match it to the pooled 1850 and 1860 Censuses, and for the 1940 Census, I match it to the pooled 1900, 1910 and 1920 Censuses. This method is inexact, but the within-between decomposition suggests that including foreign-born children does not matter much for results.

- Whether one lives in a different state from birth

(3) I calculate the predicted probabilities of being linked based on observable characteristics $\hat{p}$. I also check whether the distribution of predicted probabilities overlaps between the linked and non-linked samples to ensure that the linked sample does not capture extremely unusual individuals. For the probability distributions, see Figure B1.

(4) The weights used for the analysis are calculated as $\left(\frac{1-\hat{p}}{\hat{p}}\right)\left(\frac{q}{1-q}\right)$ where q is the share of the population that is linked.

(5) I check the representativeness of the linked sample relative to the underlying population (Tables B1-B4).

Before reweighting, Tables B1-B4 show that the linked samples are unrepresentative of the population. First, the linked samples tend to underrepresent the Black population, and also the Southern population. For example, except for the 1880 census (which includes the appended data of formerly enslaved children), the share of Black adults in the linked sample is about 30 percent of the share in the population sample. Those who live in the South are also underrepresented, while those who live in the Midwest are overrepresented. Finally, white-collar workers are overrepresented while unskilled workers are underrepresented. Weighting via inverse propensity weights addresses these issues, but it does not address any potential unrepresentativeness of the dataset.

*The effect of weighting on mobility estimates*

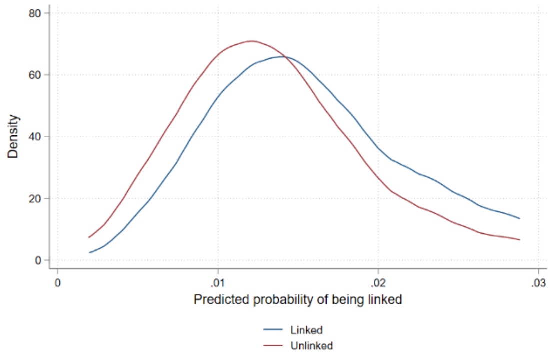
Figures B2 (OLS) and B3 (IV) show that weighting tends to increase the father-son association. In each Figure, Panel A shows how weighting influences father-son associations for the double-linked sample, while Panel B shows how weighting influences the associations for the triple-linked sample. For example, the OLS association for the *unweighted* 1860 birth cohort is 0.45, while it is 0.58 for the *weighted* data – an increase of 29 percent. The unweighted association for the triple-linked data is even lower than the double-linked data at 0.41 (Figure B2, Panel B). Since the triple-linked data has a smaller father-son association, linking more times (and *not* weighting) attenuates estimates. However, weighting appears to address the attenuation from more links: the weighted estimates for the triple-linked data is 0.58 and the double-linked data is 0.55.

This result is shown more clearly in Figure A2, where linking more times does not strongly influence mobility estimates if one weights the data.

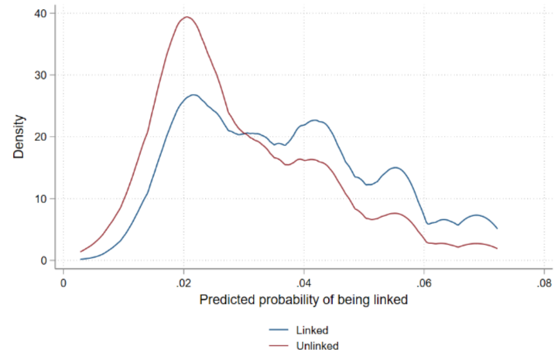
The main reason why weighting increases the father-son association is that the Black share of the sample is too small in the unweighted data. Since Black fathers are less likely to be linked than white fathers, multiple links further reduce the share Black in the data. Using race as a variable to predict successful linkage in the probit model addresses this issue and ensures that the Black share of the sample reflects the population. If one limits the sample to white families, then weighting has a smaller influence on mobility estimates (Figures B4 and B5). Further, there is no clear pattern where weighting increases or decreases the father-son association.

**Figure B1.** Overlap of predicted probability of being in the linked sample

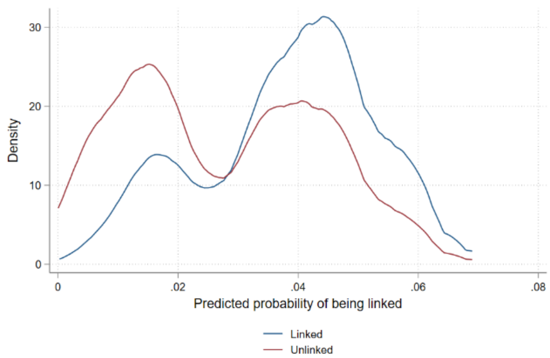
A. 1870 Census



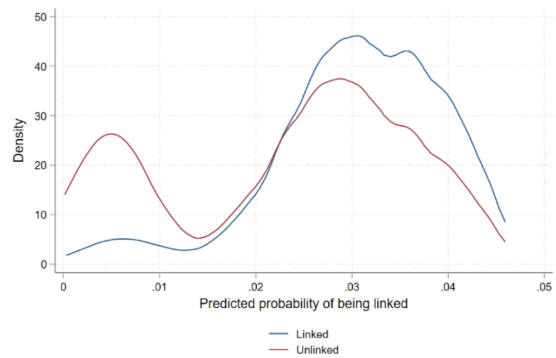

B. 1880 Census



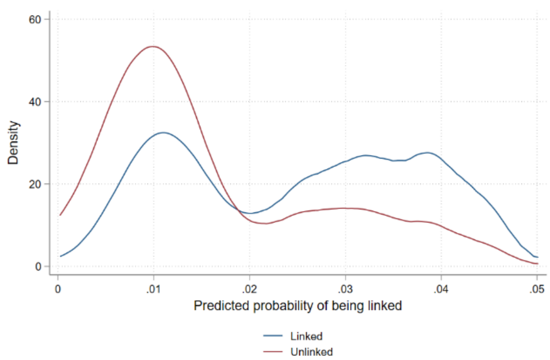

C. 1900 Census




D. 1910 Census




E. 1920 census




F. 1930 Census

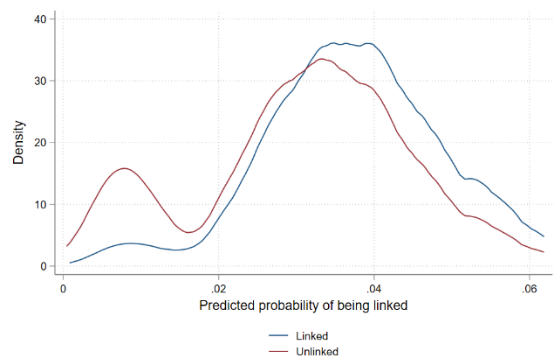
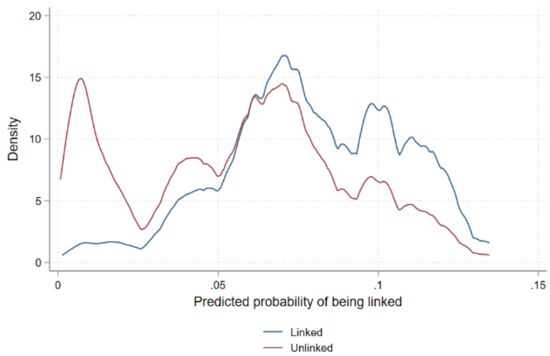


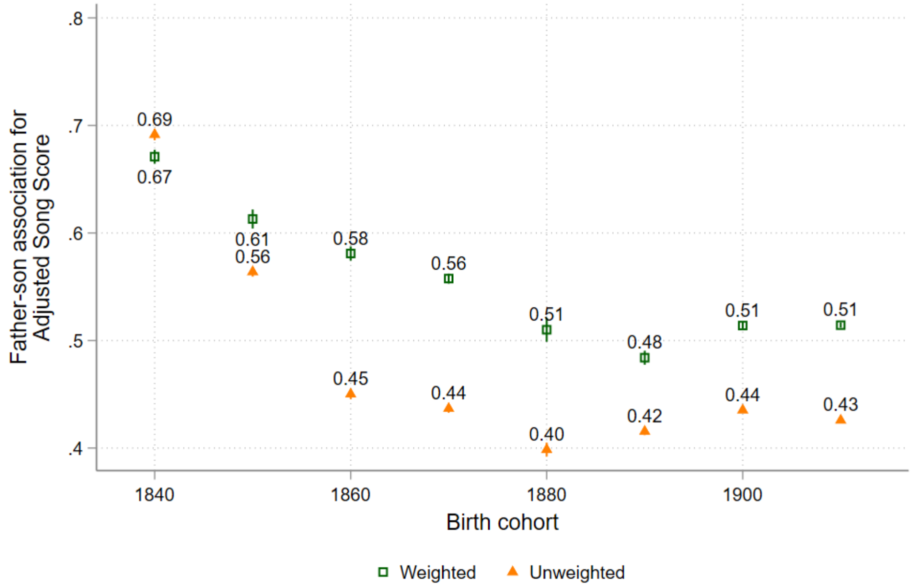
**Figure B1 (continued).** Overlap of predicted probability of being in the linked sample (continued)
G. 1940 Census



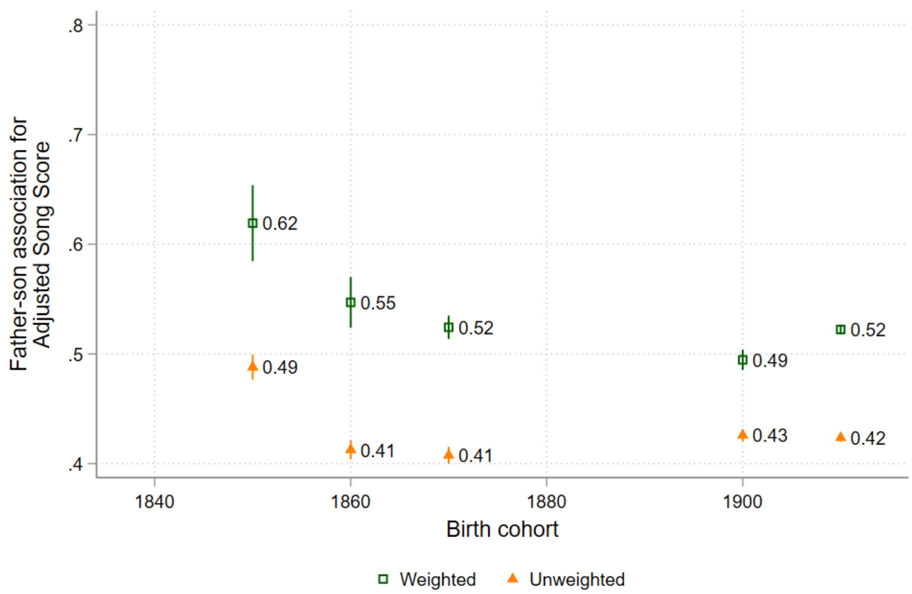

Notes: Each panel show the kernel densities for the predicted probability of being linked versus being not linked, based on observable characteristics.

**Figure B2.** Weighting generally increases father-sons associations, OLS

Panel A. Double linked



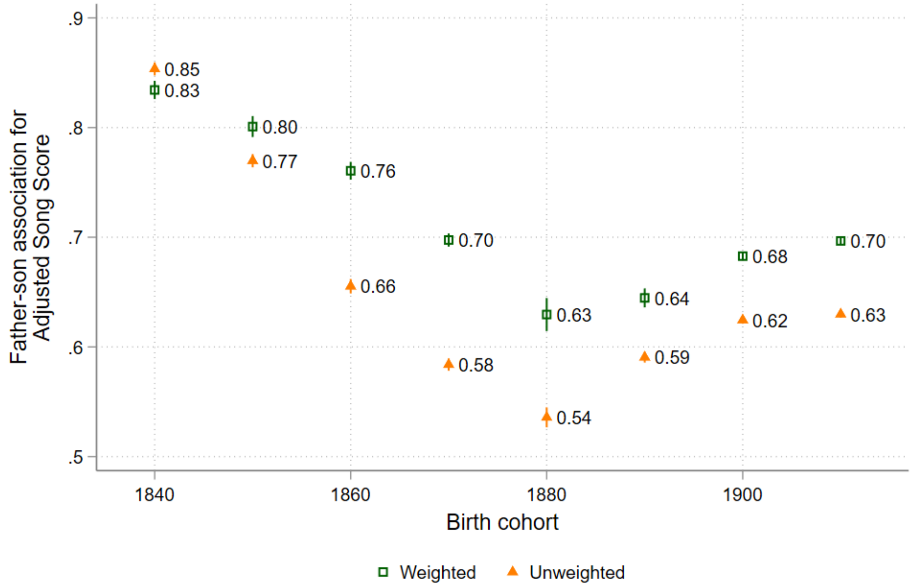

Panel B. Triple linked



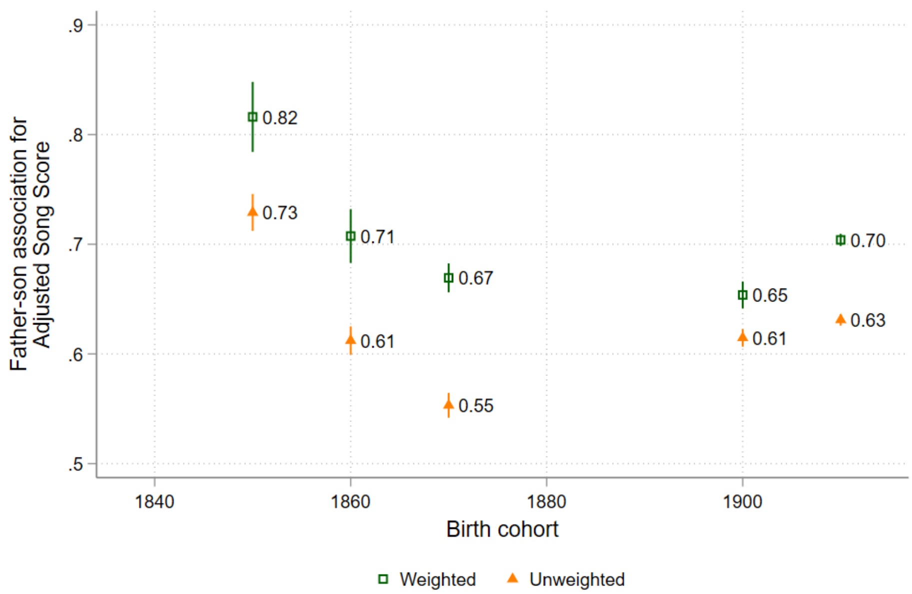

Notes: Data are from a linked sample of father and sons between the 1850 and 1940 United States Censuses. All estimates are based on an OLS regression of one son observation on one father. The sample changes by whether it is weighted or not. In Panel A, estimates are from the double-linked sample, where the father is observed twice. In Panel B, estimates are from the triple-linked sample. The point of this figure is that weighting influences estimates. Note that the weighted estimates in Panel A are similar to the weighted estimates in Panel B.

**Figure B3.** Weighting generally increases father-sons associations, IV

Panel A. Double linked



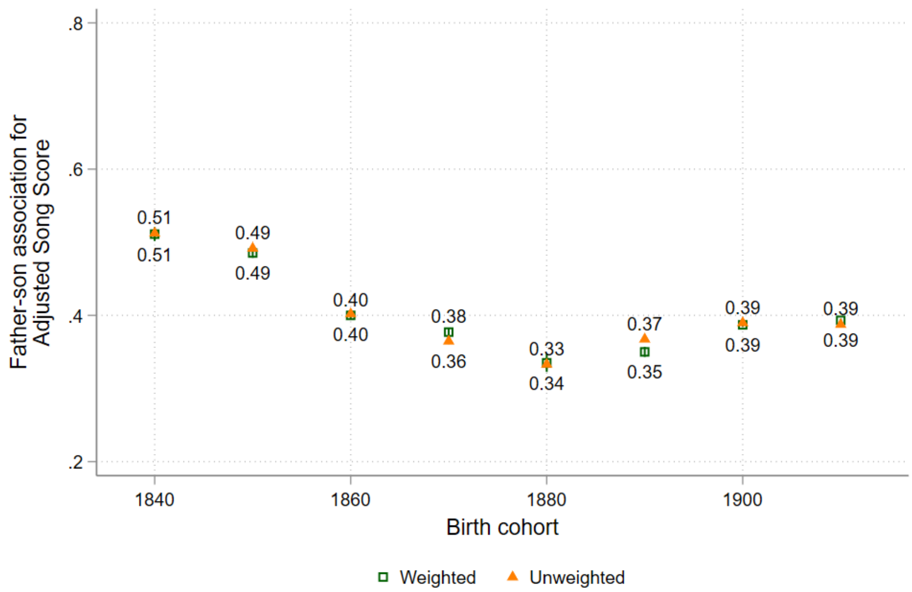

Panel B. Triple linked



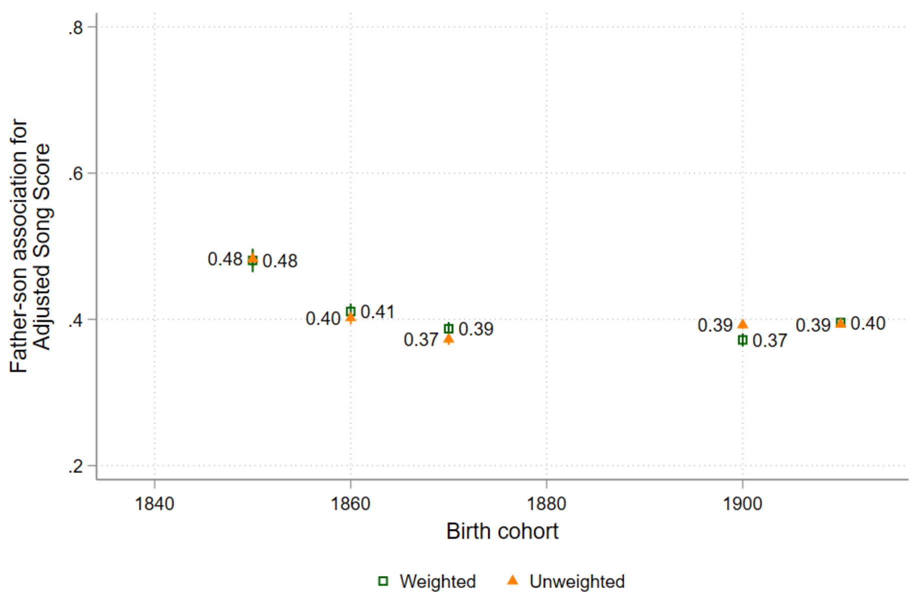

Notes: Data are from a linked sample of father and sons between the 1850 and 1940 United States Censuses. All estimates are from an IV regression whether one father observation is instrumented with a second. The sample changes by whether it is weighted or not. In Panel A, estimates are from the double-linked sample, where the father is observed twice. In Panel B, estimates are from the triple-linked sample. The point of this figure is that weighting influences estimates. Note that the weighted estimates in Panel A are similar to the weighted estimates in Panel B.

**Figure B4.** Weighting has a smaller influence on the white-only sample, OLS

Panel A. Double linked



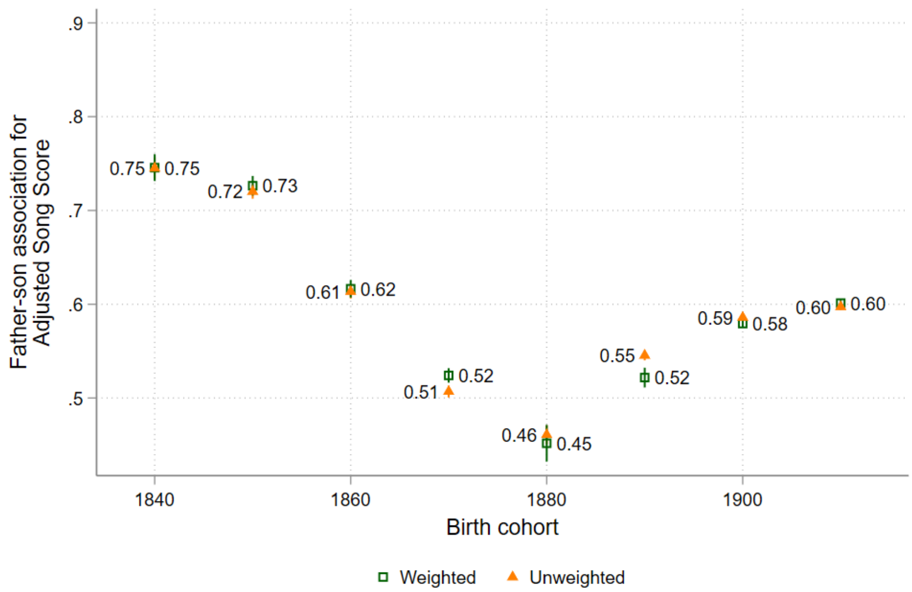

Panel B. Triple linked



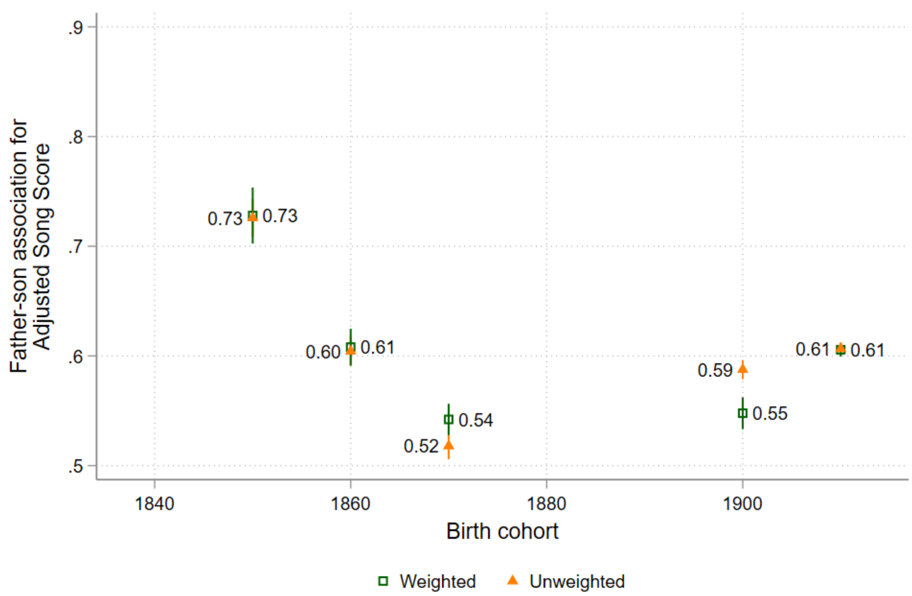

Notes: Data are from a linked sample of white father and sons between the 1850 and 1940 United States Censuses. All estimates are based on an OLS regression of one son observation on one father. The sample changes by whether it is weighted or not. In Panel A, estimates are from the double-linked sample, where the father is observed twice. In Panel B, estimates are from the triple-linked sample. The point of this figure is that weighting influences estimates. Note that the weighted estimates in Panel A are similar to the weighted estimates in Panel B.

**Figure B5.** Weighting has a smaller influence on the white-only sample, IV

Panel A. Double linked




Panel B. Triple linked




Notes: Data are from a linked sample of white father and sons between the 1850 and 1940 United States Censuses. All estimates are from an IV regression whether one father observation is instrumented with a second. The sample changes by whether it is weighted or not. In Panel A, estimates are from the double-linked sample, where the father is observed twice. In Panel B, estimates are from the triple-linked sample. The point of this figure is that weighting influences estimates. Note that the weighted estimates in Panel A are similar to the weighted estimates in Panel B.

**Table B1**. Representativeness of sons observed in 1870 and 1880

| | 1870 | | | 1880 | | |
|---|---|---|---|---|---|---|
| | population | link weight | link unweight | population | link weight | link unweight |
| Black | .147 | .148 | .243 | .137 | .138 | .084 |
| | (.354) | (.355) | (.428) | (.344) | (.345) | (.278) |
| Age | 30.047 | 29.676 | 29.433 | 33.746 | 33.290 | 33.260 |
| | (3.529) | (2.996) | (3.031) | (6.271) | (5.870) | (5.173) |
| Live in Northeast | .291 | .291 | .308 | .276 | .271 | .360 |
| | (.454) | (.454) | (.462) | (.447) | (.444) | (.480) |
| Live in Midwest | .329 | .328 | .275 | .335 | .334 | .340 |
| | (.469) | (.469) | (.446) | (.472) | (.471) | (.473) |
| Live in South | .343 | .343 | .401 | .345 | .349 | .274 |
| | (.474) | (.475) | (.490) | (.475) | (.476) | (.446) |
| Live in West | .035 | .035 | .014 | .043 | .044 | .024 |
| | (.186) | (.186) | (.120) | (.203) | (.206) | (.153) |
| Interstate mover | .573 | .574 | .652 | .584 | .577 | .668 |
| | (.494) | (.494) | (.476) | (.492) | (.493) | (.470) |
| White Collar | .136 | .137 | .141 | .152 | .153 | .179 |
| | (.343) | (.344) | (.348) | (.359) | (.360) | (.383) |
| Farmer | .332 | .335 | .325 | .371 | .367 | .414 |
| | (.471) | (.472) | (.468) | (.483) | (.482) | (.492) |
| Unskilled | .297 | .295 | .332 | .248 | .248 | .191 |
| | (.457) | (.456) | (.471) | (.432) | (.431) | (.393) |
| Skilled | .232 | .231 | .199 | .227 | .230 | .214 |
| | (.422) | (.421) | (.399) | (.419) | (.421) | (.410) |
| Observations | 2420913 | 33094 | 33094 | 5379072 | 166581 | 166581 |

Notes: Representativeness of the linked data with respect to the adult son's observation.

**Table B2.** Representativeness of sons observed in 1900 and 1910

| | 1900 | | | 1910 | | |
|---|---|---|---|---|---|---|
| | population | link unweight | link weight | population | link unweight | link weight |
| Black | .117 | .114 | .023 | .115 | .114 | .029 |
| | (.322) | (.318) | (.151) | (.319) | (.318) | (.167) |
| Age | 37.205 | 37.174 | 38.340 | 39.312 | 39.549 | 40.086 |
| | (8.631) | (8.413) | (7.186) | (7.862) | (7.242) | (6.347) |
| Live in Northeast | .258 | .256 | .305 | .250 | .248 | .283 |
| | (.437) | (.436) | (.460) | (.433) | (.432) | (.450) |
| Live in Midwest | .354 | .356 | .397 | .333 | .328 | .390 |
| | (.478) | (.478) | (.489) | (.471) | (.469) | (.487) |
| Live in South | .326 | .325 | .235 | .329 | .336 | .234 |
| | (.468) | (.468) | (.424) | (.469) | (.472) | (.423) |
| Live in West | .061 | .062 | .061 | .086 | .086 | .091 |
| | (.240) | (.241) | (.240) | (.281) | (.281) | (.288) |
| Interstate mover | .618 | .619 | .679 | .608 | .613 | .662 |
| | (.485) | (.485) | (.466) | (.488) | (.487) | (.472) |
| White Collar | .184 | .184 | .227 | .227 | .224 | .283 |
| | (.387) | (.388) | (.419) | (.418) | (.417) | (.450) |
| Farmer | .318 | .308 | .354 | .285 | .281 | .309 |
| | (.465) | (.461) | (.478) | (.451) | (.449) | (.462) |
| Unskilled | .244 | .248 | .178 | .225 | .228 | .160 |
| | (.430) | (.432) | (.382) | (.418) | (.419) | (.367) |
| Skilled | .252 | .258 | .239 | .261 | .265 | .245 |
| | (.434) | (.437) | (.426) | (.439) | (.441) | (.430) |
| Observations | 10,736,552 | 313159 | 313159 | 12,171,028 | 296842 | 296842 |

Notes: Representativeness of the linked data with respect to the adult son's observation.

**Table B3.** Representativeness of sons observed in 1920 and 1930

| | 1920 | | | 1930 | | |
|---|---|---|---|---|---|---|
| | population | link unweight | link weight | population | link unweight | link weight |
| Black | .112 | .109 | .030 | .106 | .105 | .026 |
| | (.316) | (.312) | (.172) | (.308) | (.307) | (.159) |
| Age | 37.861 | 36.880 | 40.452 | 34.712 | 34.503 | 34.209 |
| | (8.623) | (8.494) | (7.999) | (6.239) | (5.865) | (5.523) |
| Live in Northeast | .247 | .243 | .269 | .255 | .255 | .239 |
| | (.431) | (.428) | (.443) | (.435) | (.436) | (.426) |
| Live in Midwest | .336 | .332 | .392 | .331 | .330 | .420 |
| | (.472) | (.471) | (.488) | (.470) | (.470) | (.493) |
| Live in South | .325 | .332 | .232 | .311 | .312 | .224 |
| | (.468) | (.471) | (.422) | (.463) | (.463) | (.417) |
| Live in West | .090 | .091 | .105 | .101 | .101 | .114 |
| | (.287) | (.288) | (.307) | (.302) | (.302) | (.318) |
| Interstate mover | .626 | .611 | .680 | .632 | .632 | .699 |
| | (.483) | (.487) | (.466) | (.482) | (.482) | (.458) |
| White Collar | .236 | .233 | .287 | .283 | .286 | .328 |
| | (.425) | (.422) | (.452) | (.450) | (.452) | (.469) |
| Farmer | .246 | .237 | .276 | .164 | .161 | .184 |
| | (.430) | (.425) | (.447) | (.370) | (.368) | (.388) |
| Unskilled | .211 | .213 | .156 | .223 | .219 | .170 |
| | (.408) | (.409) | (.362) | (.416) | (.413) | (.376) |
| Skilled | .305 | .315 | .280 | .328 | .332 | .316 |
| | (.460) | (.464) | (.449) | (.469) | (.471) | (.464) |
| Observations | 16,766,637 | 281,017 | 281,017 | 16,003,734 | 519,920 | 519,920 |

Notes: Representativeness of the linked data with respect to the adult son's observation.

**Table B4.** Representativeness of sons observed in 1940

| | population | 1940<br>link unweight | link weight |
|---|---|---|---|
| Black | .095 | .094 | .022 |
| | (.294) | (.292) | (.147) |
| Age | 38.120 | 37.617 | 34.204 |
| | (8.721) | (8.358) | (6.290) |
| Live in Northeast | .254 | .255 | .225 |
| | (.435) | (.436) | (.418) |
| Live in Midwest | .316 | .304 | .411 |
| | (.465) | (.460) | (.492) |
| Live in South | .317 | .332 | .221 |
| | (.465) | (.470) | (.415) |
| Live in west | .110 | .107 | .141 |
| | (.314) | (.310) | (.348) |
| Interstate mover | .676 | .669 | .721 |
| | (.467) | (.470) | (.448) |
| White Collar | .288 | .291 | .327 |
| | (.452) | (.454) | (.469) |
| Farmer | .134 | .129 | .134 |
| | (.341) | (.335) | (.341) |
| Unskilled | .234 | .233 | .190 |
| | (.423) | (.423) | (.392) |
| Skilled | .342 | .346 | .347 |
| | (.474) | (.475) | (.476) |
| Observations | 23,745,187 | 1,509,227 | 1,509,227 |

Notes: Representativeness of the linked data with respect to the adult son's observation.

**Appendix C.** Details on measuring status in historical census data

A main limitation of the historical censuses is that it lacks income or earnings data. A common workaround is to impute earnings based on occupation, race, and region. However, detailed information on earnings is unavailable until the 1940 census. Projecting 1940 earnings on data from the mid-19th century may badly measure status differences across occupations; for example, if the relative status of farmers has declined over time. Song et al. (2020) overcome this problem by measuring status based on human capital by occupation. The advantage is that human capital is measured with literacy in censuses between 1850 and 1930, and with educational attainment in censuses 1940 and after. Therefore, the literacy rate by occupation will capture the change in relative status for occupations. Indeed, Song et al. (2020) show that many high-status occupations in the 1850s have lost status over time.

For this paper, I re-create the Song et al. (2020) measure (which I call the "Song score"). While their score is available online[2], I update their score in three ways. First, while they mostly use full-count data to estimate literacy rates by occupation before 1930, the 1860 and 1870 censuses were not available. Since then, the full-count data has been released, so I use the 1860 and 1870 full-count censuses. The full-count data allow me to more accurately measure literacy differences across occupations (and especially when later by occupation/race/region). Note that I also use the full-count data to estimate the educational attainment by occupation in the 1940 census.

The second and more important update is to account for racial and regional differences within occupation. Racial and regional inequality was high, especially before World War II. Ignoring these gaps within occupation would understate inequality in the past. For example, Southern farm laborers had lower literacy rates than Northern farm laborers; moreover, Black farm laborers had a lower literacy rate than white farm laborers.

A third update is to include the enslaved population in the estimates. First, while enslaved males were engaged in a variety of "occupations" or tasks, these tasks are unobserved, so I assume that those enslaved held the same "occupation" of slave. Second, the enslaved population is unobserved in the free population schedules from 1850 and 1860, so I create a synthetic enslaved

[2] See https://osf.io/6c58f/.

population in 1850 and 1860.[3] I then append this synthetic data of enslaved individuals to the 1850 and 1860 censuses before calculating the Song score. The enslaved population is approximately 14 percent of the non-slave population in 1850, and 13 percent of the non-slave population in 1860. I also make the simplifying assumption that the enslaved population has the lowest literate rate of any "occupation"/race/region cell. Enslaved individuals are also assigned the "South" census region.

The process of creating the score is as follows: pool 25 to 64-year-old males from the different censuses and samples together, calculate the mean literacy/education rate by occupation/region/race/cohort (or just by occupation/cohort for the unadjusted version), and then percentile rank the cells by cohort. The percentile ranks are then smoothed across cohorts to address sharp switches in ranks from measurement error.[4] The occupations in this measure are not the 3-digit occ1950 codes but instead 70 "microclass" occupations which Song et al. (2020) argue are more comparable across the 19th and 20th centuries. For the years 1930 and before, I use the full-count censuses. For years after 1930, I use the full-count 1940 Census, 1 percent samples of the 1960 and 1970 censuses, 5 percent samples of the 1980 and 1990 censuses, and the 2000, 2010, and 2017 ACS.

Note that despite the measure being a percentile ranking, the score ranks occupations in the *population* rather than in the *linked* data. This difference is small but important, partially because the linked data targets the population of fathers. Also, ranking within the linked dataset ensures the father-son rank-rank slope is the same as the rank-rank correlation (Chetty et al. 2014). These differences are also important when averaging father occupations to account for measurement error: averaging the ranks reduces variation while ranking the average does not. Due to these differences, and to avoid confusion with a percentile ranking of the linked data as in Chetty et al. (2014), I refer to the base measure as the "Song score" rather than a rank.

---

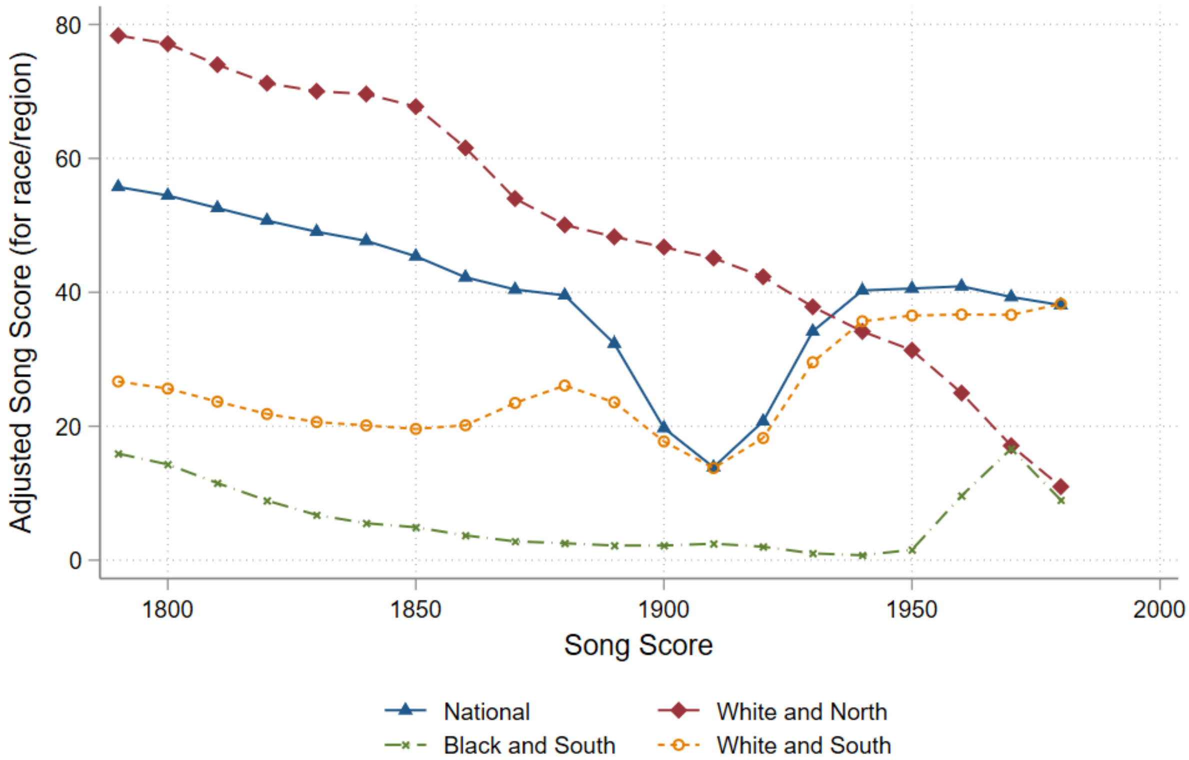
[3] I take a 14 percent sample of the non-enslaved population from the 1850 full-count and 13 percent sample from the 1860 full count, change the race to Black, the region to South, and literacy rate to zero. Since the Song score ranks occupation cells by cohort, a zero-literacy rate ensures the slave "occupation" will be the lowest ranked.

[4] The smoothed rank for cohort $t$ is 0.25 times the unsmoothed rank from cohort $t-1$, 0.50 times the unsmoothed rank from cohort $t$, and 0.25 times the unsmoothed rank from cohort $t+1$. For the first cohort and last cohort, I use 0.75 times the rank of the cohort and 0.25 times the rank of the next or previous cohort.

To provide an idea of this score, Figure C1 plots the Song score for farmers. Two key patterns emerge. First, the national score for farmers misses significant variation across race and region for 19$^{th}$-century cohorts. For the adjusted score and the 1800s birth cohort, Northern white farmers are near the 80$^{th}$ percentile while Southern Black farmers are near the 10$^{th}$ percentile. However, the national rank for farmers was around the 55th percentile. Second, there was significant convergence by the 1980 birth cohort. These results that occupational scores miss wide regional and racial economic gaps were important in the 19$^{th}$ and early 20$^{th}$ centuries, but have grown less important over time.

Figure C2 confirms that there is important variation across race and region within an occupational cell. The figure shows a scatter plot between the unadjusted Song score on the horizontal axis and the adjusted Song score on the vertical. For example, for an unadjusted score that is located under the 5$^{th}$ percentile, the adjusted Song score has a range from 0 to 45. Similarly, many high-status occupations for the national score drop to less than 20 percent for the adjusted score. I rely on the adjusted score for most of the analysis.

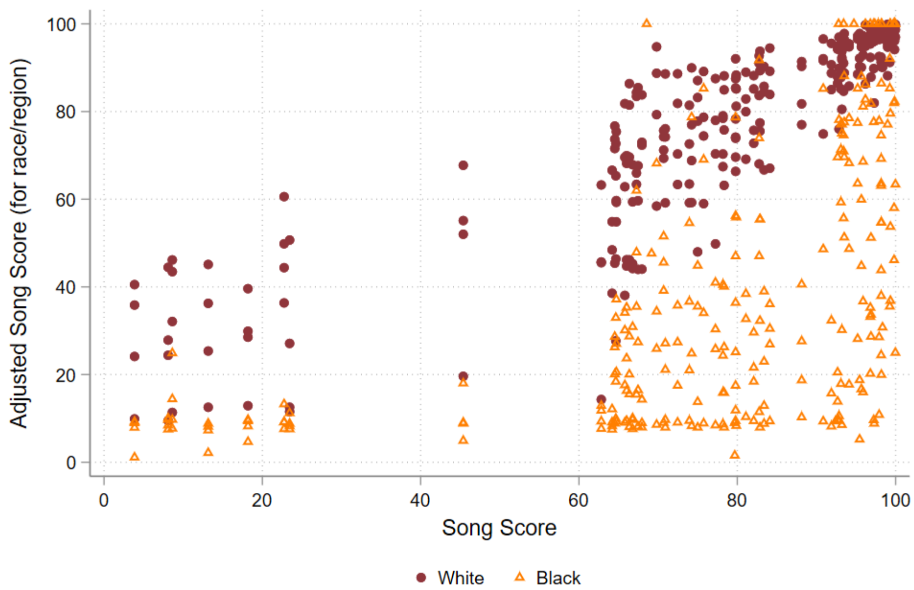
**Figure C1**. Unadjusted and Adjusted Song scores for farmers between 1800 and 1980 cohorts



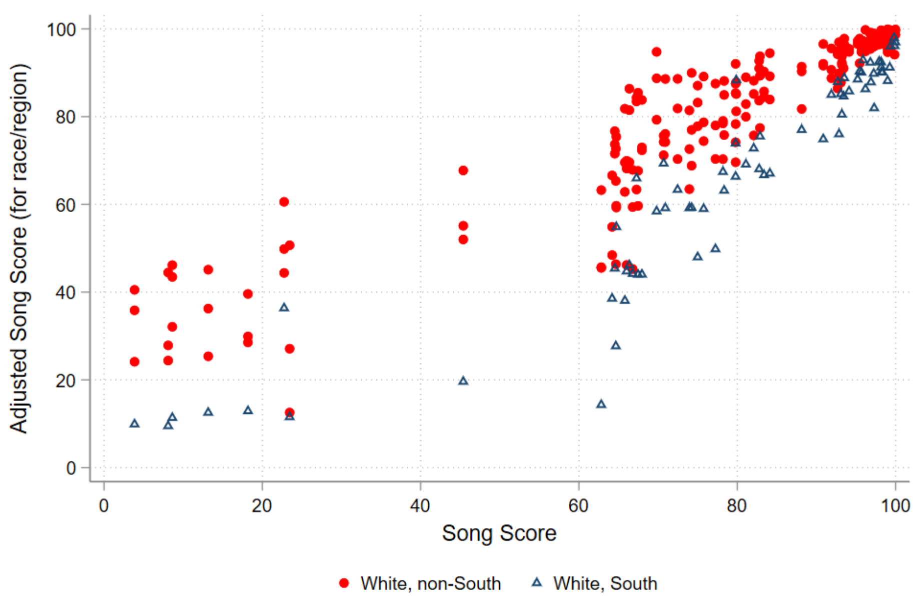

Notes: The "National" line is the unadjusted Song score. The other lines are the adjusted Song scores for different race and region groups.

**Figure C2.** Unadjusted and Adjusted Song scores for the 1850 birth cohort

*Panel A. White v. Black scores*




*Panel B. South v. non-South score for whites*




Notes: For each unadjusted Song score, there is wide variation in the adjusted Song score. That is, occupational-based status measures miss variation within occupation by race and region.

**Appendix D.** Comparison of estimates to Song et al. (2020)

The paper that is most similar to this paper is Song et al. (2020). However, our results differ, where Song et al. (2020) find a decline in relative mobility between the 19$^{th}$ century and today, while I find an increase in mobility. In this appendix, I explicitly list the differences between our approaches to clarify why our mobility estimates differ from one another. While including Black males in the sample, using a status measure that accurately captures racial inequality, and accounting for measurement error are the primary differences, there are more subtle differences that can matter for mobility estimates.

*Historical data*

Table D1 lists the differences between Song et al.'s (2020) approach and my approach to the historical data. One key difference is the construction of the historical data. First, the linking methods vary across papers. Song et al. (2020) use a variation of the Ferrie (1996) method to link censuses. The method is described in Long and Ferrie (2013, Online Appendix 2), but the essential process is as follows. Potential links are searched for across censuses after blocking on the Soundex version of the first name, Soundex version of the last name, birthplace, and parental birthplace. Potential links must be within three years of birth across censuses. Given this set of potential links, the chosen link is the one with minimum string distance for the first and last names, as measured with the SAS SPEDIS function. Only unique links are kept. The algorithm I use in this paper differs in some important ways. First, names are not standardized by any phonetic algorithm since standardizing names may lead to false positives (Bailey et al. 2020). Second, I do not block on parental birthplace. However, I do require race to match across censuses since race is highly stable (Price et al. 2021). Third, I only use links that have unique first name/last name strings within plus/minus two years of birth. This restriction is what the Census Linking Project calls a "conservative" link, or what Bailey et al. (2020) term "robustness." The reason why it is conservative is that it reduces false positives, but also lowers the linking rate.

In addition to a different linking methodology, our data structure varies in other ways. First, I link my data up to three times, though a double-linked dataset is used for the primary estimates. Second, we use different census years. While my paper uses every available census between 1850 and 1940, and every possible 20-, 30-, or 40- year link from childhood to adulthood, Song et al. (2020) use 30-year links (1850-1880, 1880-1910, and 1910-1940). Third, the historical data in

Song et al. (2020) links 0-20-year-old children forward to the next census, while I link 0-14-year olds.[5]

Third, the regression used to estimate mobility differs in a few ways. First, I weight my data to be representative of observables, while Song et al. (2020) use unweighted data for the historical analysis. Second, my primary estimates are for the *association* between father and son, while Song et al. (2020) focus on the *correlation*. Third, I include a quartic in age for the father and son to account for lifecycle effects, while Song et al. (2020) do not.

Do these differences, besides accounting for race and measurement error, matter for estimates? First, the linking method appears to matter (see Figure D1). If one estimates the same specification across the white male data in this paper and from Song et al. (2020), then estimates differ. One reason why estimates could differ is if false positives were more likely for one algorithm than the other (Bailey et al. 2020). However, there is no clear-cut pattern where one estimate is higher than the other. For example, the Song et al. (2020) estimates are higher for the 1860 birth cohort (0.30 v. 0.24), but lower for the 1910 birth cohort (0.31 v 0.37).[6] However, the 1860 cohort is the largest estimate where the Song et al. (2020) data is higher than in my data. The age structure of the samples may explain this difference. Song et al.'s (2020) 1860 birth cohort (1856-1865) consists of 15-20-year-olds in the 1880 Census, while my data consists of 6-14-year-old in the 1870 census and 0-5-year-olds in the 1860 censuses. It may be that intergenerational persistence is different for 15-20-year olds observed in the household than the overall 15-20-year-old population, especially since older-aged children tended to be in farming households (Xie and Killewald 2013). This possibility is difficult to test explicitly since if I restrict the Song et al. (2020) data to 0-14-year-old children to match my sample, then there are no estimates for the 1860 or 1890 cohorts.

Besides the linking method, the number of links does not appear to matter for estimates, nor does using specific census years. Figure A3 shows that mobility estimates for single-linked

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[5] Song et al. (2020, Appendix S1) note that their data is "typically aged 0 to 17-year-olds" in the first census (pg. 2).

[6] Besides the linking method, one reason for the difference in estimates for the 1860 birth cohort could be due to the age structure of the samples. Song et al.'s (2020) 1860 birth cohort (1856-1865) consists of 15-20-year-olds in the 1880 Census, while my data consists of 6-14-year-old in the 1870 census and 0-5-year-olds in the 1860 censuses. It may be that intergenerational persistence is different for 15-20-year olds observed in the household than the overall 15-20-year-old population, especially since older-aged children tended to be in farming households (Xie and Killewald 2013). This possibility is difficult to test explicitly since if I restrict the Song et al. (2020) data to 0-14-year-old children to match my sample, then there are no estimates for the 1860 or 1890 cohorts.

data are similar to double-linked or triple-linked data, as long as one weights the data to be representative of observables. The main way that the use of different censuses would influence estimates is if the age of the son varies across datasets. For example, Song et al. always use 30-year links, which places most sons between 30-50 years old; the sons in my data are 25-55 years old.[7] However, Figure A1 shows that the age of the son does not strongly influence estimates.

Weighting the data to be representative is important for mobility estimates, but the impact on estimates is larger when the data include both Black and white males. When the data include both races, Figures B2 and B3 show that weighted persistence estimates can be 30 percent higher than unweighted estimates. However, when the sample is only of white families (like the Song et al. 2020 data), then the difference between weighted and unweighted estimates is smaller. In fact, in contrast to weighting increasing estimates for Black and white data, weighting mostly decreases estimates for white-only data (Figures B4 and B5). Overall, weighting explains some of the reason why white estimates in this paper differ from Song et al. (2020) but is not as important as the linking methodology.

Song et al (2020) prefer the father-son correlation ($\widehat{\beta_1} = \frac{Cov(y_{father}, y_{son})}{\sqrt{Var(y_{father})Var(y_{son})}}$) over the father-son association ($\widehat{\beta_1} = \frac{Cov(y_{father}, y_{son})}{Var(y_{father})}$). The reason why I prefer the father-son association is because standard measurement error techniques (classical measurement error, instrumental variables) more easily handle error in the father's status rather than the son's. Estimates of the father-son correlation can differ from the father-son association; however, it does not change the overall result that relative mobility was higher today than in the past (see Appendix E).

Finally, one difference between our estimates is that I include lifecycle controls in the data, while Song et al. (2020) do not. However, controlling for a quartic in the father's and son's age does not strongly influence estimates (see Figure D2).

[7] The fathers in my data set are between 25-65 years old. While Song et al.'s note and their data for 50-95-year-old fathers, the number of older-aged fathers are unreasonably high in their data (i.e., the average age of the father is 71.6, but the average age of the son is 8.6). Based on this observation, I assume that they added 30 years to the father's age due to the 30-year linking process, such that their data is actually of 20-65-year-old fathers.

*Modern-day data*

Besides these issues in the historical data, the modern-day data differs across our papers (see Table D2). Song et al. (2020) compile a large number of sources to estimate mobility for birth cohorts after 1910, which include the Panel Study of Income Dynamics (PSID), the National Longitudinal Survey of Youth 1979 (NLSY79), the National Longitudinal Survey of Older and Younger Men (NSLM), the General Social Survey (GSS), the Wisconsin Longitudinal Survey (WLS), the Occupational Changes in a Generation surveys (OCG), the Survey of Income and Program Participation (SIPP), and the National Survey of Families and Households (NSFH). In addition to these surveys, Song et al (2020) use modern-day linked data from the Census Bureau, which links the 1940 Census, the 1973-1990 Current Population Survey Annual Social and Economic Supplement (CPS ASEC), the 2000 Census Long Form, and the 2001-2015 American Community Survey (ACS). In contrast, I only use the PSID. The reason why I use a much more limited dataset is that it is the only one that is publicly accessible and has multiple father occupation observations.

Another way in which my data differs from Song et al. (2020) is that I only use observations in the PSID where the father's occupation is observed twice. In contrast, Song et al. (2020) use recalls of the father's occupation, which is available for more observations. Recalls may be more error-prone, which would attenuate estimates. At the same time, observing multiple father occupations may lead to a selected sample, which produces different estimates.

Ultimately, differences in data do not matter for relative mobility estimates between the 1960 and 1980 birth cohorts. Figure D3 shows that point estimates are similar across the survey data in Song et al. (2020) and this paper. However, one difference is that the estimates from Song et al. (2020) are much more precise, since they pool other data sources together. Note that I cannot test differences with the restricted-access data. However, that data has lower father-son associations than the survey data, which is consistent with mobility improving over time.

**Table D1.** Differences and similarities in historical mobility estimates between this paper and Song et al. (2020).

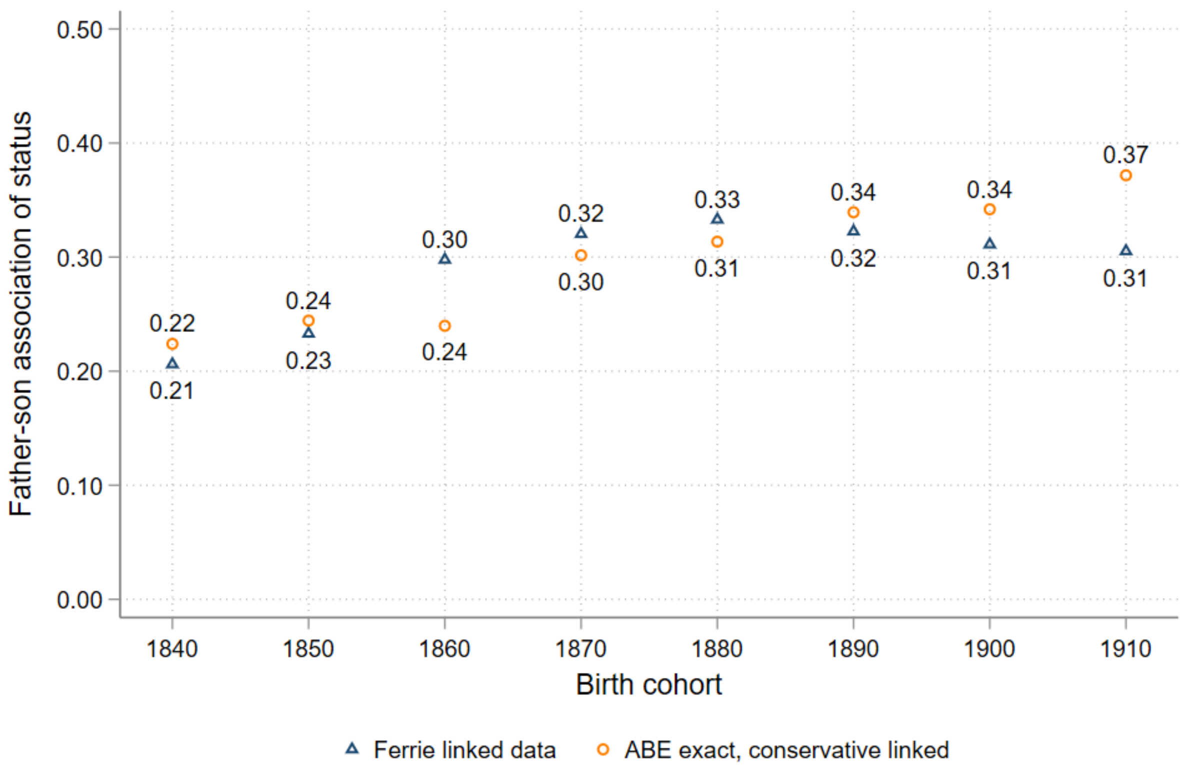
| Key research choices: | Ward (2021) | Song et al. (2020) | Does it matter? | Comment |
|---|---|---|---|---|
| *Main differences:* | | | | |
| Accounts for error in father's status | Yes | No | Yes (Figures 3, 4) | Accounting for error in the father's observation, whether via averaging or IV, increases the father-son association. |
| Target population | Black and white males | White males | Yes (Figures 5, 6) | Including Black males increases the father-son association if you allow for within-occupational differences by race. |
| Status imputed by | Occ, race and region | Occupation | Yes (Figure 3A/3B) | Differentiating within-occupation can increase the father-son association due to regional and racial inequality within occupation. |
| *Historical data structure:* | | | | |
| Linking method | ABE, with exact strings and conservative links | Ferrie (1996), with common names and closest string distance | Yes, for some cohorts (Figure D1) | Mobility estimates can be higher or lower for the ABE method. |
| Number of links | Up to three | One | No (Figure A2) | Linking multiple times does not matter if one weights the data. However, if one does not weight the data, it can change estimates |
| Underlying Census data | 1850-1940 | 1850, 1880, 1910, 1940 | No | The main difference is the son's age in adulthood, but this does not matter much for mobility estimates (Figure A1) |
| *Regression estimate:* | | | | |
| Weights historical data | Yes | No | Yes (Figures B2-B5) | Weights can change mobility estimates, especially if Black families are in the sample. |
| Main mobility metric | Father-son association | Father-son correlation | Yes (Appendix F) | While the level is different, the trend is not. If correlations are used, one needs to pay attention to error in the son's outcome as well as the father's. |
| Lifecycle controls | Quartic in father's and son's age | None | No (Figure D2) | Controlling for lifecycle effects with a quartic in the father's and son's age does not strongly influence estimates. |

**Table D2.** Difference in mobility estimates for modern-day data

| Key research choices: | Ward (2021) | Song et al. (2020) | Does it matter? | Comment |
|---|---|---|---|---|
| Underlying linked data source | None | Linked Census/ACS/CPS | Maybe | Mobility is greater in modern linked data than in survey data, which may be due to greater error in the census/ACS/CPS. |
| Underlying survey data | PSID | PSID, GSS, OCG, WLS, NSFH, NLSY79, NLSM Older and Younger cohort, SIPP | No (Figure D3) | Point estimates are not statistically significant across sources, but pooling more data narrows standard errors. I do not use other data sources since two father observations are required. |

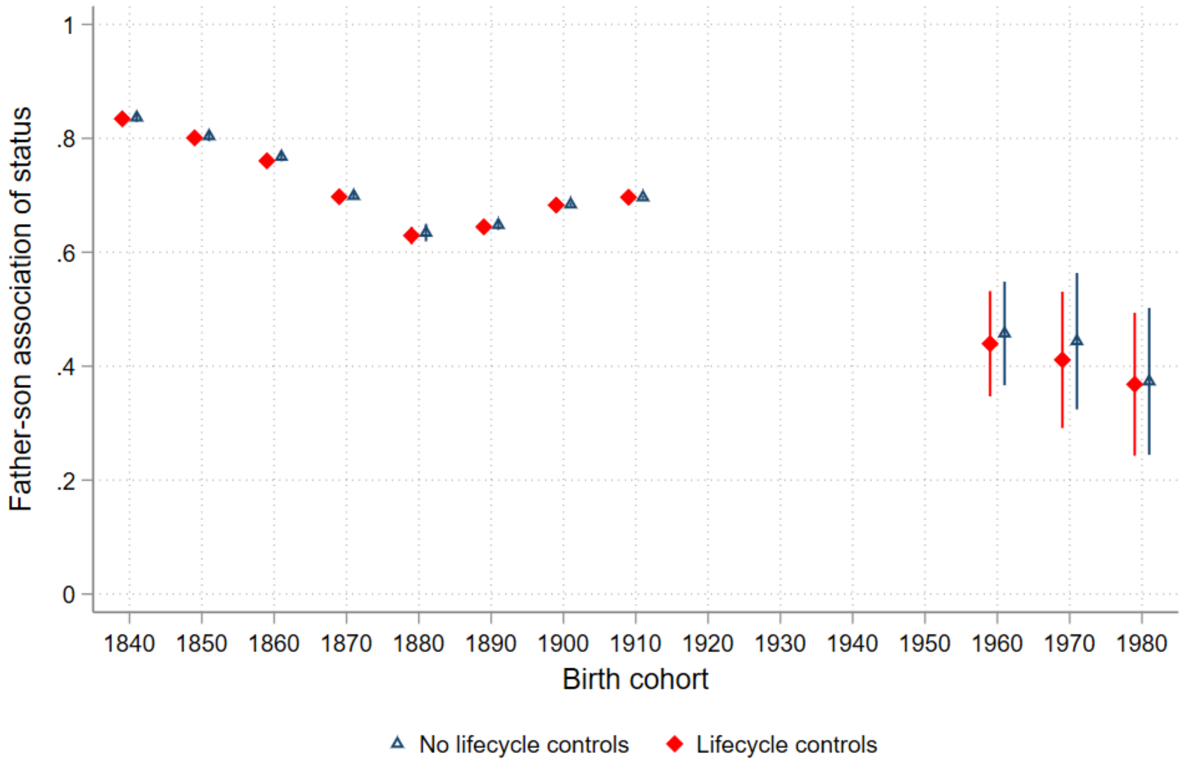
*GSS: General Social Survey; OCG: Occupational Changes in Generation; WLS: Wisconsin Longitudinal Survey; NSFH: National Survey of Families and Households; NLSY79: National Longitudinal Survey of Youth, 1979 cohort; NLSM Young/Old Cohort: National Longitudinal Survey of Older and Younger Men; SIPP: Survey of Income and Program Participation; Linked Census/ACS/CPS data: Linked data is between the census, American Community Survey and Current Population Survey (see Song et al. 2020).

**Figure D1.** The linking method matters for historical mobility estimates



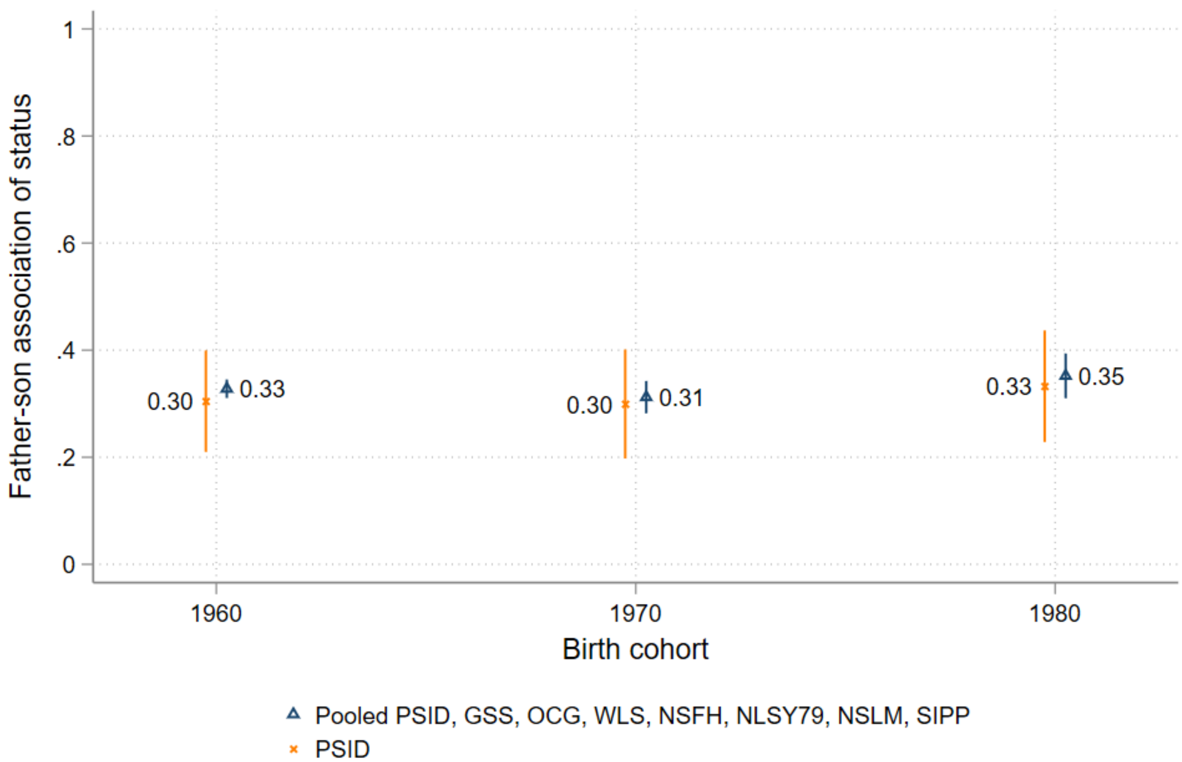

Notes: This figure shows mobility estimates for the same regression across linked methods. The regression is the son's Song score on the father's Song score, without weights or lifecycle controls. The dataset if only of white families, and the age of sons are restricted to be the same to match the Song et al. (2020) data.

**Figure D2.** Controlling for lifecycle effects does not strongly influence estimates




Notes: This figure shows how the father-son association varies when controlling for a quartic in the father's and son's age (lifecycle controls) relative to not controlling for age. The adjusted Song score is used as the status measure, and IV estimates are presented.

**Figure D3.** The use of the PSID does not lead to different mobility estimates.




Notes: The pooled PSID, General Social Survey (GSS), Occupational Changes in a Generation (OCG), Wisconsin Longitudinal Survey (WLS), National Survey of Families and Households (NSFH), National Longitudinal Survey of Youth (NLSY79), NLSM (National Longitudinal Survey of Older and Younger Men), SIPP (Survey of Income and Program Participation) are data taken from Song et al. (2020). The second set of estimates "PSID" are from the main text. Relative mobility estimates are shown when using an occupation-only status measure. This figure shows that the use of different data sources does not produce different mobility estimates.

**Appendix E.** Predicting outcomes with the classical measurement error formula

This section derives the predicted father-son associations based on one or two father observations. First, under the classical error assumption, let the probability limit of estimates $\beta_{T=1} = \beta_1 \frac{\sigma_{y*}^2}{\sigma_{y*}^2+\sigma_v^2}$ and $\beta_{T=2} = \beta_1 \frac{\sigma_{y*}^2}{\sigma_{y*}^2+\sigma_v^2/_2}$, where $\beta_1$ is the true father-son association. Since $\beta_1\sigma_{y*}^2$ are in both equations, one can set the following terms to be equal:

$$\beta_{T=2}\left(\sigma_{y*}^2 + {\sigma_v^2}/_2\right) = \beta_{T=1}\left(\sigma_{y*}^2 + \sigma_v^2\right) \tag{E1}$$

First, solve for $\sigma_v^2$:[8]

$$\sigma_v^2 = \sigma_{y*}^2\left(\frac{2(\beta_{T=2} - \beta_{T=1})}{(2\beta_{T=1} - \beta_{T=2})}\right) \tag{E2}$$

This formula for the variation in measurement error can be plugged back into the original formula when using two father observations $\beta_{T=2} = \beta_1 \frac{\sigma_{y*}^2}{\sigma_{y*}^2+\sigma_v^2/_2}$:

$$\beta_{T=2} = \beta_1 \frac{\sigma_{y*}^2}{\left[\sigma_{y*}^2 + \sigma_{y*}^2\left(\frac{2(\beta_{T=2} - \beta_{T=1})}{(2\beta_{T=1} - \beta_{T=2})}\right)/2\right]} \tag{E3}$$

Solving for the "true" father-son association as a function of the coefficients leads to the formula:

$$\beta_1 = \left[{(\beta_{T=1} \times \beta_{T=2})}\Big/{(2\beta_{T=1} - \beta_{T=2})}\right] \tag{E3}$$

This formula is the one used to predict the "true" father-son association in Figure 4B.

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[8] I derive the formulas in terms of estimated coefficients, but a simpler approach is to directly estimate variation in the father's status. When using a single observation variation is $var(y_{father}) = \sigma_{y*}^2 + \sigma_v^2$ and when using two father observations variation is $var(\overline{y_{father}}) = \sigma_{y*}^2 + \sigma_v^2/2$. Based on these variations, $2var(\overline{y_{father}}) - var(y_{father}) = \sigma_{y*}^2$. Thus, the reliability ratio can be calculated as $\frac{2var(\overline{y_{father}}) - var(y_{father})}{var(y_{father})}$.

Figure 4A also uses the classical measurement error is to predict the father-son association when using an average of three father observations $\beta_{T=3} = \beta_1 \frac{\sigma_{y*}^2}{\sigma_{y*}^2 + {\sigma_v^2}/{3}}$. To derive the formula used in Figure 4A, plug in equation E3 for $\beta_1$, and equation E2 for the error term $\sigma_v^2$. The predicted three-father observation estimate simplifies to:

$$\beta_{T=3} = \left[{(3\beta_{T=1} \times \beta_{T=2})}/{(4\beta_{T=1} - \beta_{T=2})}\right] \tag{E4}$$

**Appendix F**. Rank-rank correlations

The main estimates are based on a *score* from 0-100. Importantly, this score is created from auxiliary samples and not from the fathers and sons in the data. However, others in the literature (e.g., Chetty et al. 2014), percentile rank fathers and sons within the data. One advantage of percentile ranking within the data is that the rank-rank association is the same as the rank-rank correlation. On the other hand, a benefit of using a score is that the score more accurately places one's location in the national distribution. Another benefit is that percentile ranking can introduce nonclassical error into the son's outcome (Nybom and Stuhler 2017). In this appendix, I show that rank-rank correlations are also biased by measurement error and the racial composition of the dataset.

Figure F1 shows that averaging the father's outcome, before percentile ranking it, tends to increase the father-son association by 0 to 8 percent. This increase in the father-son association is less than presented in the main text (15-20 percent in Figure 3A). The reason why measurement error influences the association more than the correlation is that measurement error adds extra "bad" variation to the scores. However, "bad" variation is not added to percentile ranks since the percentile rank transformation fixes the variation.[9] Nevertheless, the father's true rank is better measured after averaging observations, which leads to a stronger rank-rank slope.

Of course, averaging two father observations does not eliminate measurement error. To eliminate error, I used the classical measurement error formula or IV. However, these methods do not work for correlations since error also exists in the son's outcome (Nybom and Stuhler 2017). It is possible to predict the "true" rank-rank slope despite non-classical measurement error from the rank transformation. Using a generalized errors-in-variables model, Nybom and Stuhler (2017) propose a method to correct the bias based on the association between two father observations.[10]

[9] Theoretically, the variation in percentile ranks for the [0,100] interval should be $\frac{1}{12}(100)^2$, or 833.3, no matter how many father's observations are averaged before ranking them. However, the variation of percentile ranks in my data is less than this theoretical value since many fathers have the same percentile rank due to having the same occupation, race and region.

[10] Following Nybom and Stuhler (2017), let $\lambda_f$ be the attenuation factor from mismeasuring the father's rank and $\lambda_s$ be the attenuation factor from mismeasuring the son's rank. That is, let $\tilde{y} = a + \lambda\tilde{y}^* + \tilde{w}$, where $\tilde{y}^*$ is the true percentile rank and $\tilde{y}$ is the observed rank (see Haider and Solon (2006) for a similar model). Due to the percentile rank transformation, $\lambda$ is less than or equal to one (Nybom and Stuhler 2017). Based on this formulation, $\rho_{observed} = \lambda_s\lambda_f\rho_{true}$ where $\rho_{observed}$ is the rank-rank correlation between father and son based on one father observation and one son observation. Nybom and Stuhler (2017) show that a regression of the father's percentile rank on another father

Based on this method, the predicted rank-rank slope is similar to the estimates presented in the main text (see Figure F2). Using an IV strategy where the father's first percentile rank is instrumented with the second one produces similar estimates since both methods to eliminate error are based on a similar process. Since the error-corrected estimates are much higher than the baseline OLS estimates, one should not necessarily prefer correlations to the father-son associations.

Finally, Figure F3 shows that using a white-only sample to measure correlations produces very different estimates than the pooled Black and white sample. This pattern is consistent with a large between-group effect existing due to strong and persistent racial disparities.

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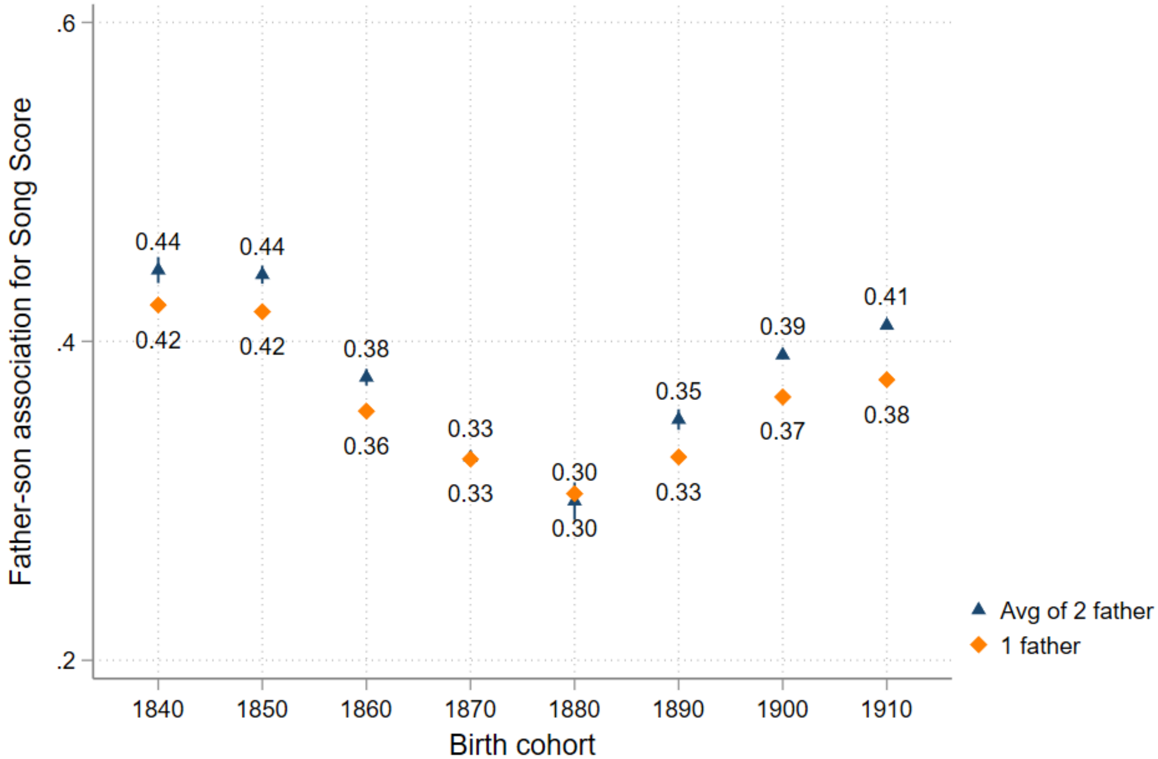
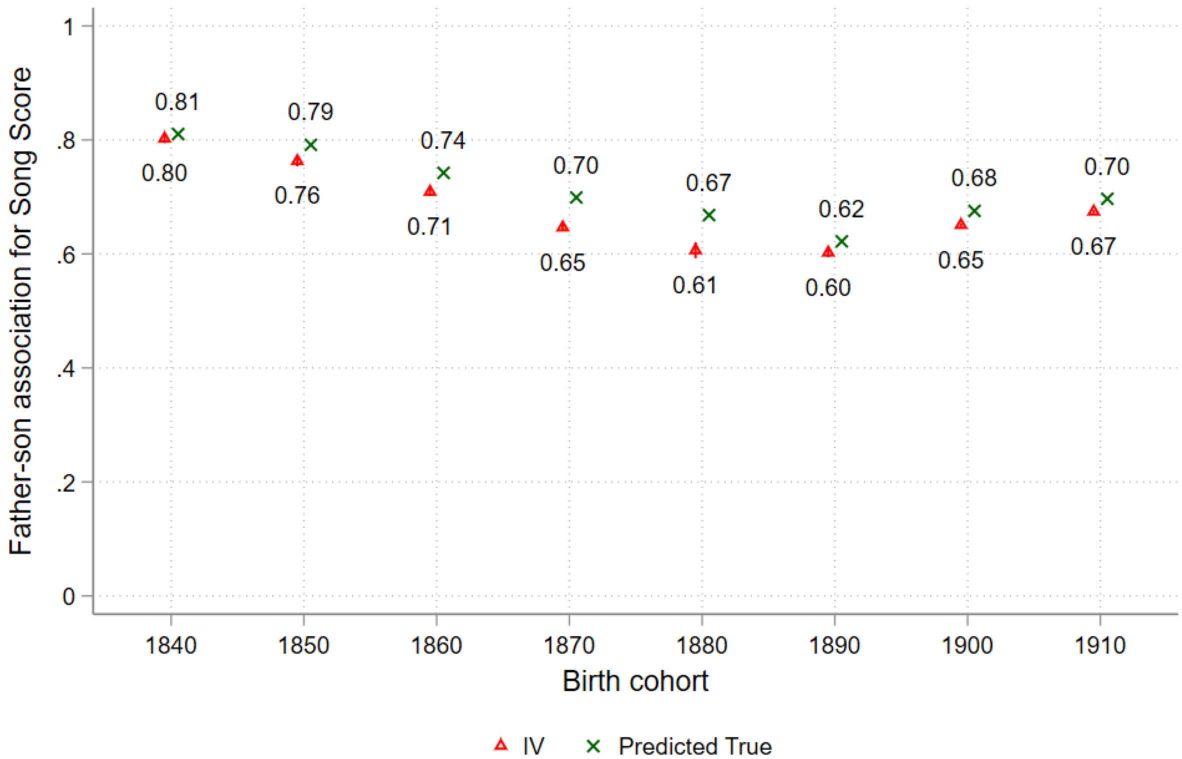
observation is equal to $\lambda_f^2$ if the error terms are uncorrelated. I use this regression to back out $\rho_{true}$, where I assume that $\lambda_f = \lambda_s$.

**Figure F1**. Averaging increases father-son associations when using correlations



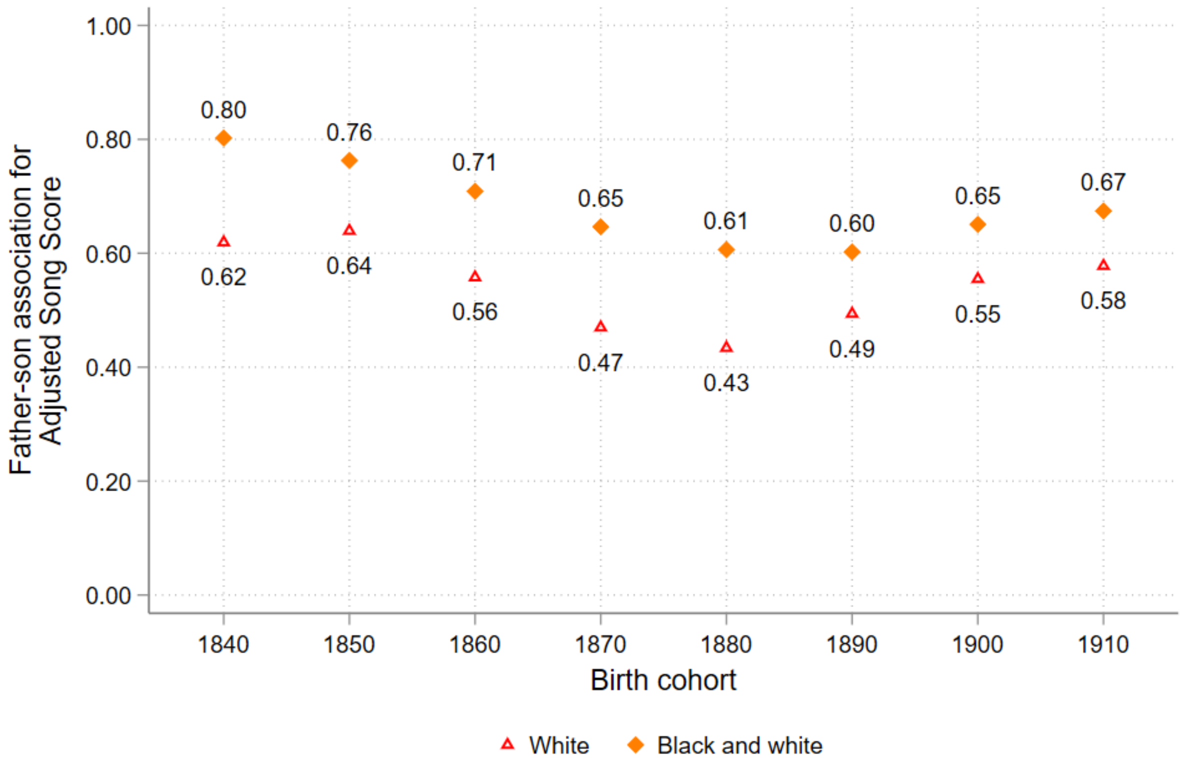

Notes: Data are a double-linked sample of fathers and sons from the 1850-1940 United States Censuses. The figure shows the estimate from regressing the son's percentile rank of status on the father's percentile rank of status. The estimates vary based on a single father observation or averaging two father observations before ranking. The same linked sample is used for all estimates.

**Figure F2.** Methods to reduce measurement error suggest that the father-son association is attenuated




Notes: Data are a double-linked sample of fathers and sons from the 1850-1940 United States Censuses. The IV estimate is when instrumenting one father's observation with a second. The "Predicted True" estimate follows Nybom and Stuhler (2017) for addressing nonclassical error in both the father and son's outcome.

**Figure F3.** White-only estimates of persistence differ from Black and white estimates




Notes: Data are a double-linked sample of fathers and sons from the 1850-1940 United States Censuses. IV estimates are presented. The figure shows that a white-only sample estimates a substantially smaller father-son correlation than a Black and white sample.

**Appendix G.** The two problems of classical measurement error and linking error

This appendix more formally considers the dual problems of classical measurement error in the father's status and linking error. Moreover, I discuss how instrumental variables help to solve both issues.

To understand the two problems of linking error and classical measurement error, consider the within-between decomposition applied to the groups of "correct" and "incorrect" links (Bailey et al. 2020):

$$plim\widehat{\beta_1} = \theta^{correct} RR_f \beta_1^{correct} + \theta^{incorrect} RR_f \beta_1^{incorrect} + \theta^b \beta_1^b \tag{G1}$$

where $\theta^{correct}$ is the share of overall variation within the correctly linked group, $\theta^{incorrect}$ is the share of variation within the incorrectly linked group, and $\theta^b$ is the share of variation between group means. The within-group father-son associations $\beta_1^{correct}$ and $\beta_1^{incorrect}$ are attenuated by the reliability ratio $RR_f = \frac{var(y_{i,f}^*)}{var\left(y_{i,f}^*\right) + {}^{var(v_{i,f})}/_{T}}$. Bailey et al. (2020) show that one way linking error affects the pooled father-son association $\widehat{\beta_1}$ is that the father-son association for incorrect links is less than for correct links ($\hat{\beta}_1^{incorrect} < \hat{\beta}_1^{correct}$). This bias is magnified if the rate of false positives increases since $\theta^{incorrect}$ increases.

Note that measurement error may also influence the share of variation that occurs within-group and across-group. If measurement error is mean zero, then the total amount of between-group variation should not change. However, measurement error does add to the amount of within-group variation by adding to the data. Therefore, error not only attenuates within-group estimates, but it can also cause the overall association $\widehat{\beta_1}$ to be weighted too much relative to within-group mobility.

To see how instrumental variables are related to linking error, first let $x_f = x_f^* + v_{1f}$ be the first observation of the father's permanent status and $z_f = x_f^* + v_{2f}$ be the second, where $v_{1i}$ and $v_{2i}$ are random noise with mean zero. The reduced-form and first-stage equations are:

$$y_s = \delta_0 + \delta_1 z_f + \varepsilon_s \tag{G2}$$

$$x_f = \pi_0 + \pi_1 z_f + \epsilon_f$$

Based on this formulation, the instrumental variables estimate for the father-son association can be rewritten as:

$$plim\hat{\beta}_1^{IV} = plim\frac{\widehat{\delta_1}}{\hat{\pi}_1} = \frac{\theta_Z^* RR_f \delta_1^* + \theta_Z^i RR_f \delta_1^i + \theta_Z^b\ \delta_1^b}{\theta_Z^* RR_f\ \pi_1^* + \theta_Z^i RR_f \pi_1^i + \theta_Z^b \pi_1^b} \tag{G3}$$

where $\theta_Z^*$ is the share of variation for true links for the second father's observation $z_f$ (Stephens and Unayama 2019).

For instrumental variables to address *both* measurement error in status and linking error, the reduced form ($\widehat{\delta_1}$) and the first stage ($\hat{\pi}_1$) must be attenuated by the same amount such that the biases cancel. This canceling out is why classical error in status is eliminated by IV since both the first-stage and reduced form are attenuated by the same reliability ratio $RR_f$.[11] However, it is unclear whether linking error will attenuate the first-stage and reduced form by the same amount. A natural expectation is that there will be less linking error in the first stage since it is based on a single link between the father's first and second observations. The reduced form, on the other hand, is based on two links: the first link between the father and his second observation, and then the second link for the child to his adult observation. If the reduced form is attenuated more, then the IV estimate will still understate the true parent-child association.

One way to test whether IV addresses the issue of linking error is to check how IV estimates compare across more and less conservative linking algorithms. First, Bailey et al. (2020) recommend that linking algorithms should refrain from using standardizing names via a cleaning algorithm (such as the New York State Identification and Intelligence System (NYSIIS)), and instead use exact strings. Therefore, one may expect more false positives for an NYSIIS-linked dataset versus and an exact-linked dataset. Second, Abramitzky et al. (forthcoming, Figure 2) show that requiring names and birthplace to be unique within a 5 year-of-birth band reduces false positives, with the tradeoff of number of links. In the Census Linking Project, the "standard" method does not make this restriction, while the "conservative" method does.

[11] When ignoring linking error ($\theta_Z^i = \theta_Z^b = 0$), both the reduced form and first stage are biased by the same reliability ratio and therefore the instrumental variable strategy uncovers the "true" father-son association (i.e., $\frac{RR_f \delta_1^*}{RR_f\ \pi_1^*} = \beta_1$).

Figure G1A shows that linking algorithms that have greater false positives also have lower father-son associations, when using OLS. The "NYSIIS, Standard" algorithm has the lowest point estimates of the four linking algorithms, and are approximately 0-6 percent lower than the "Exact, Conservative" algorithm. The difference between the more and less conservative linking algorithms is greater (5-21 percent) when using the occupation-only score (Figure G2A). The bias is greater because false links are more likely to disagree on occupation but not on the region of residence.

Figure G1B shows that the difference in mobility estimates across linking algorithms narrows when using instrumental variables. For the adjusted Song score, the IV estimates for the "NYSIIS, Standard" method are 0-3 percent less than the "Exact, Conservative" method. This is less than half of the difference when using OLS. For the unadjusted Song score, the difference in estimates across linking methods is also more than halved. Therefore, the results are consistent with IV reducing bias from linking error.

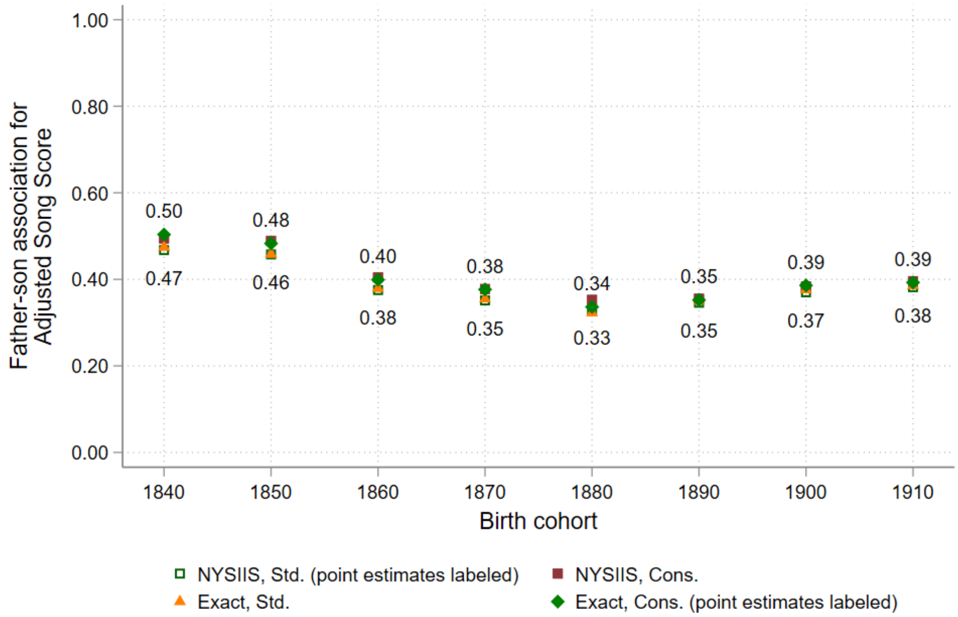
While the IV method reduces bias from linking error, it does not eliminate it. The less conservative methods still estimate less persistence than the more conservative methods. The differences in estimates are small: only 0-4 percent for the adjusted Song score. This result suggests that a more conservative linking method may still understate intergenerational persistence. Based on the IV formula, this would occur if linking error attenuates the first stage by more than the reduced form. Since attenuation bias is not fully eliminated, it appears that the reduced form may be more attenuated than the first stage, perhaps because the reduced form requires two correct links (son from childhood to adulthood and first father observation to the second one) rather than the first stage (first father observation to the second).

Linking errors may be more important for minority populations that are difficult to link. Figure G3 shows how OLS and IV estimates vary for the Black population. There is more movement of estimates by linking method due to smaller sample sizes, but most estimates are statistically indistinct from each other. IV estimates are narrower for cohorts born between 1890 and 1910, which may suggest that census quality was poorer in the 1800s.

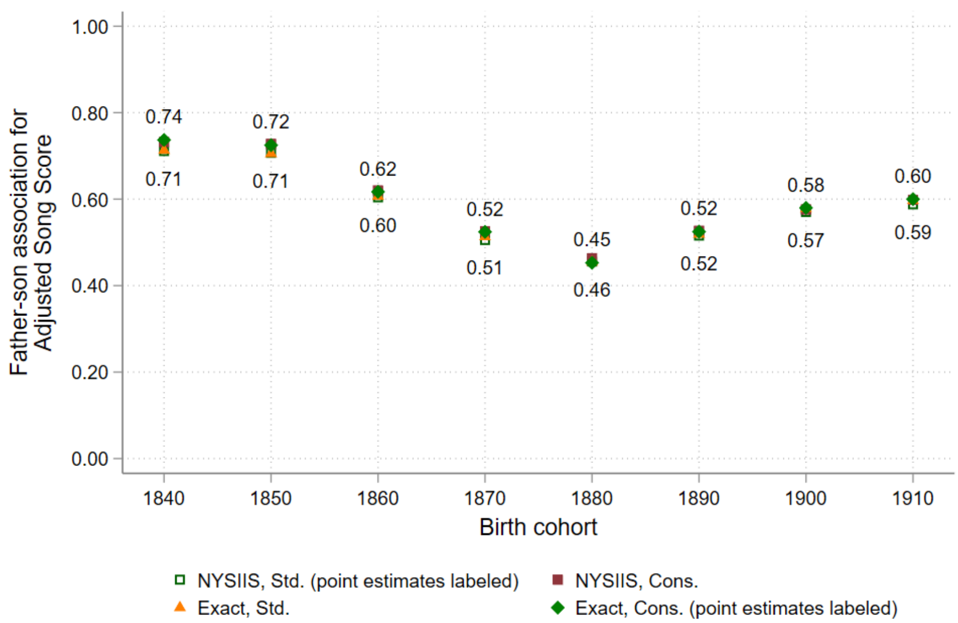
Finally, Figure G4 compares IV estimates to "true" estimates from the classical measurement error formula. The results in Panel A are similar to the results from the main text for the triple-linked sample (Figure 4B), but the estimates in Figure G4 are now for the double-linked sample. When using less precise linking methods, the classical measurement error formula's predicted "true" estimates are smaller than IV estimates by 5 to 10 percent. While these differences are small, it may be that attenuation bias from linking error in less precise algorithms is not fully addressed by the classical error formula.

**Figure G1.** Less conservative linking algorithms attenuates persistence estimates but IV partially addresses issue

Panel A. Less conservative linking methods lead to attenuated estimated of persistence (OLS)



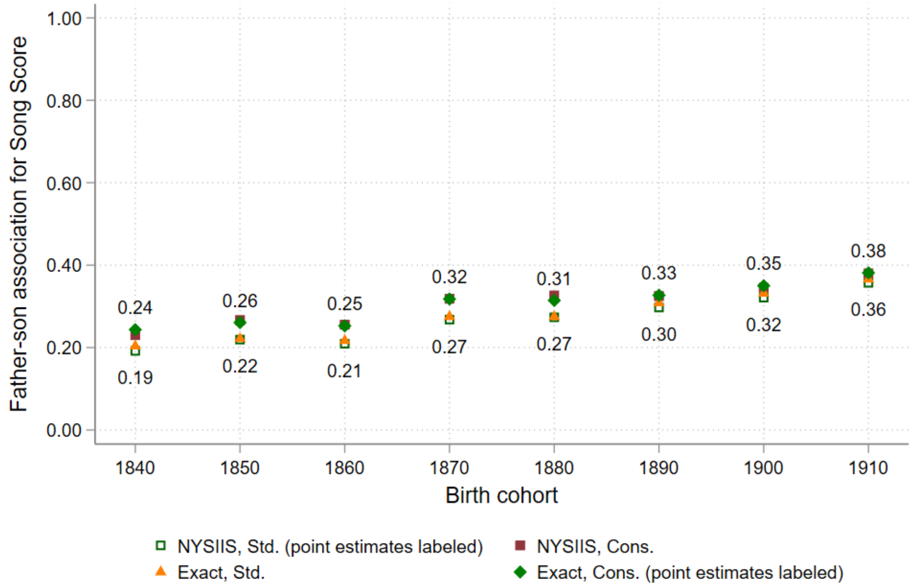

Panel B. IV estimates align point estimates between more and conservative linking algorithms.



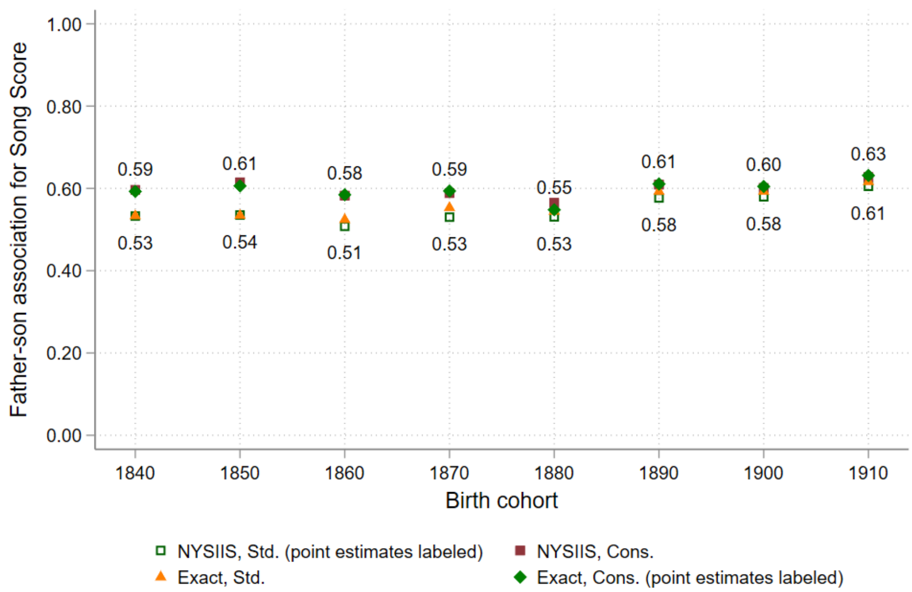

Notes: Estimates vary by the linking method used to create estimates. Weights are created separately for each linking method. Standard estimates ("Std.") do not drop first name, last name and year or birth combinations that appear within plus/minus two years. Conservative ("Cons.") estimates do. Panel shows OLS estimates, while Panel B shows IV estimates. Status is based on occupation, race and region.

**Figure G2.** Less conservative linking algorithms attenuates persistence estimates but IV partially addresses issue (occupation-only status)

Panel A. Less conservative linking methods lead to attenuated estimated of persistence (OLS)

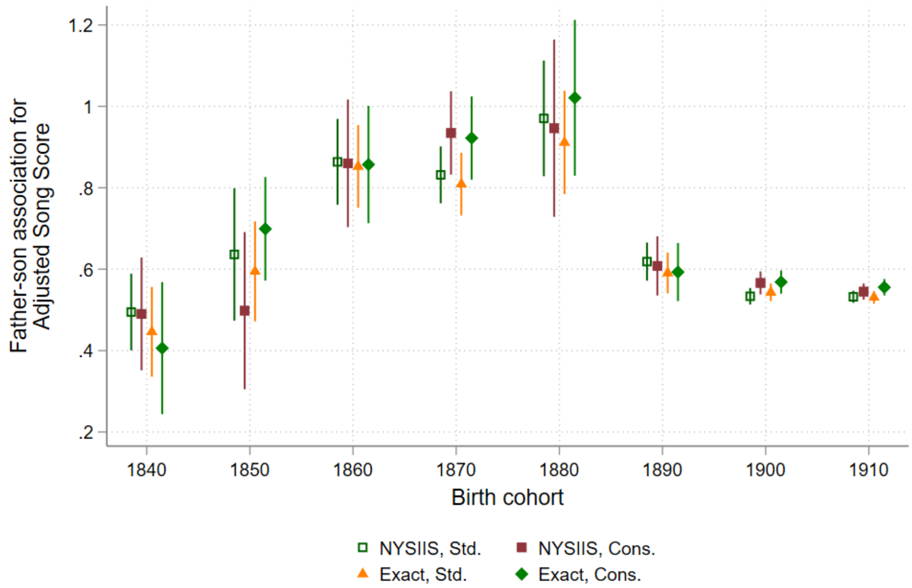



Panel B. IV estimates align point estimates between more and conservative linking algorithms.




Notes: Estimates vary by the linking method used to create estimates. Weights are created separately for each linking method. Standard estimates ("Std.") do not require individuals to be unique by first name, last name and birthplace within a five-year age band. Conservative ("Cons.") estimates do. Panel shows OLS estimates, while Panel B shows IV estimates. Status is based on occupation.

**Figure G3**. Different linking methods produce similar mobility estimates for Black families
Panel A. OLS estimates

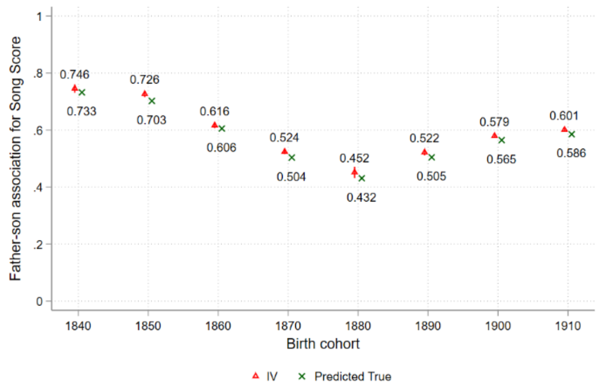



Panel B. IV estimates

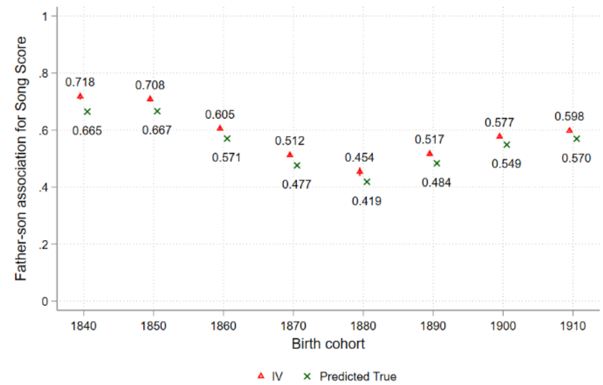

Notes: Estimates vary by the linking method used to create estimates. Weights are created separately for each linking method. Standard estimates ("Std.") do not require individuals to be unique by first name, last name and birthplace within a five-year age band. Conservative ("Cons.") estimates do. Panel shows OLS estimates, while Panel B shows IV estimates. Status is based on occupation.

**Figure G4.** Predicted father-son associations and IV estimates are similar when using different linking methods

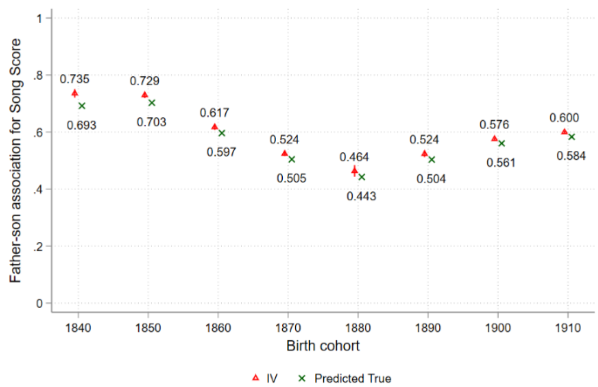
Panel A. Exact string and Conservative



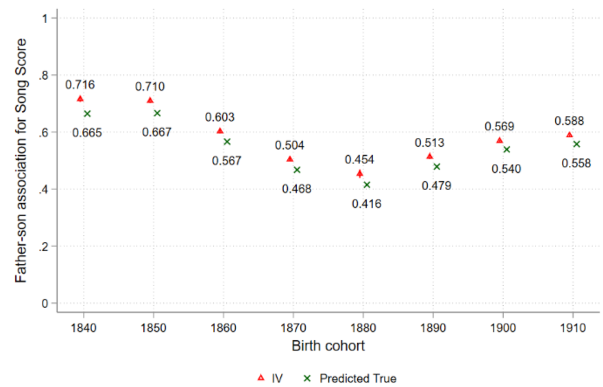

Panel B. Exact string and standard




Panel C. NYSIIS string and Conservative




Panel D. NYSIIS string and Standard




Notes: Each figure shows father-son associations under alternative linking methods. Exact strings link people based on exact first and last name strings, while NYSIIS links people after using a phonetic algorithm to adjust names. Conservative links require individuals to be unique by first name, last name and birthplace within a five-year age band, while standard links do not.

**Appendix H.** Linking the two enumerations of 1880 St. Louis

In this section, I provide further detail about linking the two St. Louis enumerations. The two St. Louis enumerations can be accessed using the raw 1880 Ancestry.com data provided to the NBER. The enumerations can be identified by the image file name: the first enumeration starts with leading digits 4242051 to 4242059, while the second enumeration goes from 4242099 to 4242109. After grouping the data into two enumerations, the first one contains 321,760 individuals while the second one contains 350,900 individuals.

To link the two enumerations, I need to first clean the birthplace, race, and age fields. For birthplace, I merge the St. Louis enumerations with the most common BPL code for each birthplace string in the full-count 1880 Census from IPUMS. For those not found in 1880, I then check the 1870 and 1860 full-count census. I do not link those without a matching string, which loses about 1.7 percent of the data. For race, I code "Black" and "mulatto" groups together. Finally, for age, I destring the data – note that this method misses the ages of young children less than one, whose ages were often written in "months." However, since I am interested in the outcomes of adults, I ignore this group.

I link the two enumerations using the conservative method from Abramitzky, Boustan, and Eriksson (2012). Specifically, I match on exact first name string, exact last name string, year of birth (+/- 2 years), birthplace, and race. I focus on linking only 30-60-year old males since this age range was less likely to move away from St. Louis between the June and November enumerations. For this age range, there are 54,430 in the first enumeration and 62,155 in the second enumeration. I can link 10,477 of them across censuses, which is a rate of about 17-19 percent. This is a surprisingly low rate since both enumerations were in the same city. However, note that matching on exact first and last name strings misses many links, as detailed by Goeken et al. (2017). Further, Abramitzky et al. (forthcoming) link two different transcriptions of the 1940 Census from Ancestry.com and Family Search and only find a match rate of 51 percent when using the same approach, which reflects problems arising from transcription error and common names.

The goal is to compare occupational responses across enumerations. To do this, I first need to assign each occupation string an OCC1950 code from IPUMS. I use the same method as assigning BPL codes, where I first look for the codes in the 1880 full-count census from IPUMS, and then search through the 1870 and 1860 censuses (in order). Of the 10,477 males links, I can only find matching occupational responses for 9,318 of them (excluding those with OCC1950 codes above 970). While it may be concerning that I lose people, unusual strings may be more ambiguous to code into a specific group, so it is unlikely that error is more for those with identifiable strings than for those without identifiable strings.

I then weight the data after pooling the linked double-enumeration sample with the 1880 full-count data St. Louis data from IPUMS. I predict the probability of linkage based on race, age, age squared, region of birth, whether one is an interstate mover, and occupation category. Note that this is a similar weighting scheme as the main text.

Table 2 in the main text shows in several ways that occupations are poorly correlated across enumerations. The 3-digit occupational codes only agree 65 percent of the time. The low rate may be because similar types of jobs are placed in the wrong 3-digit category, but even the 1-digit occupational codes agree only 69 percent of the time. Table H1 shows the occupational "transition" matrix between the first and second enumerations, although no transition should have taken place. About 85 percent of white-collar workers in the first enumeration were also white-collar workers in the second generation, which is the highest persistence rate across the two observations. Interestingly, only half of the farmers in the first enumeration are farmers in the second enumeration. It is unclear why a clear occupation like farming would be recorded differently across enumerations. Some farmers were recorded as "milkman" in the second enumeration, which is assigned the "Deliverymen and routemen" code, while another one was recorded as a "milk dealer," which is assigned the "Managers, officials, and proprietors (n.e.c.)" code. Other farmers were recorded as "gardeners" in the second census.

Table H2 shows the most common strings that are coded into different occupations. By far, the most common miscoding was a mismatch between laborer and teamster. It is possible that whoever was providing the information to the census enumerator was not specific enough to be classified as a "teamster." The Adjusted Song score gap across these two occupations was about 3 ranks, which was not large. Figure H1 plots the density of differences in ranks. The average difference was 0.3 ranks, indicating similar average levels of coding across enumerations. The standard deviation was 17.7 ranks, which confirms that miscoding could lead to wide differences across observations. Finally, Table H3 shows the agreement rate by occupation, for occupations with more than 30 observations. The most agreeing occupation was "Physician and surgeon" where over 90 percent of observations agreed. On the low end was “foreman,” where only 17 percent agreed.

**Table H1.** Occupational "Transition" Matrix

| 1st enum occ. in row | 2nd enumeration's occupation: White Collar | Semi-Skilled | Unskilled | Farmer | Row sum |
|---|---|---|---|---|---|
| Full population | | | | | |
| White Collar | 2,797 | 342 | 134 | 8 | 3,281 |
| | (85.2) | (10.4) | (4.1) | (0.2) | |
| Semi-skilled | 309 | 3,364 | 370 | 6 | 4,049 |
| | (7.6) | (83.1) | (9.1) | (0.1) | |
| Unskilled | 145 | 438 | 1,330 | 16 | 1,929 |
| | (7.5) | (22.7) | (68.9) | (0.8) | |
| Farmer | 8 | 7 | 13 | 31 | 59 |
| | (13.6) | (11.9) | (22.0) | (52.5) | |
| Column sum | 3,259 | 4,151 | 1,847 | 61 | 9,318 |
| White population | | | | | |
| White Collar | 2,777 | 341 | 129 | 8 | 3,255 |
| | (85.3) | (10.5) | (4.0) | (0.2) | |
| Semi-skilled | 307 | 3,294 | 301 | 5 | 3,907 |
| | (7.9) | (84.3) | (7.7) | (0.1) | |
| Unskilled | 134 | 385 | 1,095 | 13 | 1,627 |
| | (8.2) | (23.7) | (67.3) | (0.8) | |
| Farmer | 7 | 7 | 13 | 31 | 58 |
| | (12.1) | (12.1) | (22.4) | (53.4) | |
| Column sum | 3,225 | 4,027 | 1,538 | 57 | 8,847 |
| Black population | | | | | |
| White Collar | 20 | 1 | 5 | 0 | 26 |
| | (76.9) | (3.8) | (19.2) | 0.0 | |
| Semi-skilled | 2 | 70 | 69 | 1 | 142 |
| | (1.4) | (49.3) | (48.6) | (0.7) | |
| Unskilled | 11 | 53 | 235 | 3 | 302 |
| | (3.6) | (17.5) | (77.8) | (1.0) | |
| Farmer | 1 | 0 | 0 | 0 | 1 |
| | (100.0) | 0.0 | 0.0 | 0.0 | |
| Column sum | 34 | 124 | 309 | 4 | 471 |

Notes: Data are from the two 1880 St. Louis Enumerations.

**Table H2.** Most common occupational string disagreements

| First enumeration | Second Enumeration | Count |
|---|---|---|
| laborer | teamster | 31 |
| teamster | laborer | 24 |
| laborer | porter | 17 |
| carpenter | laborer | 11 |
| porter | laborer | 9 |
| carpenter | builder | 8 |
| laborer | carpenter | 8 |
| builder | carpenter | 7 |
| laborer | engineer | 7 |
| laborer | servant | 7 |
| carpenter | cabinet maker | 6 |
| carpet cleaner | laborer | 6 |

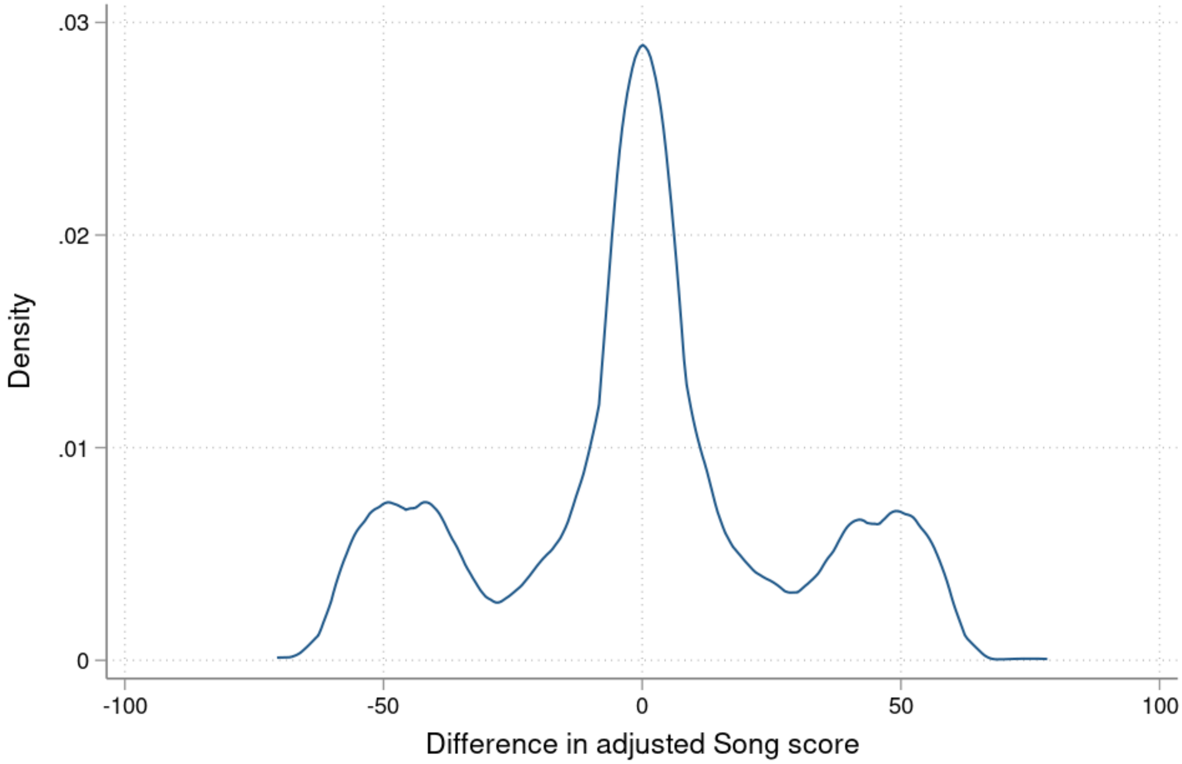
Notes: Data are from the two 1880 St. Louis Enumerations.

**Table H3.** Rate of agreement and disagreement by occupation codes

| Most agreeing occupation codes | | | Least agreeing occupation codes | | |
|---|---|---|---|---|---|
| Occupation category | Agreeing | N | Occupation category | Agreeing | N |
| Physicians and surgeons | 0.952 | 108 | Foremen (n.e.c.) | 0.180 | 43 |
| Barbers, beauticians, and manicurists | 0.906 | 64 | Sailors and deck hands | 0.244 | 44 |
| Lawyers and judges | 0.905 | 115 | Bartenders | 0.360 | 47 |
| Policemen and detectives | 0.880 | 84 | Private household workers (n.e.c.) | 0.438 | 39 |
| Tailors and tailoresses | 0.876 | 169 | Clerical and kindred workers (n.e.c.) | 0.460 | 172 |
| Teachers (n.e.c.) | 0.875 | 33 | Operative and kindred workers (n.e.c.) | 0.464 | 827 |
| Plasterers | 0.849 | 40 | Stationary firemen | 0.509 | 55 |
| Blacksmiths | 0.846 | 122 | Gardeners, except farm and groundskeepers | 0.520 | 44 |
| Meat cutters, except slaughter and packing house | 0.845 | 139 | Farmers (owners and tenants) | 0.531 | 58 |
| Bakers | 0.842 | 88 | Porters | 0.545 | 131 |
| Jewelers, watchmakers, goldsmiths, and silversmiths | 0.836 | 31 | Guards, watchmen, and doorkeepers | 0.561 | 69 |
| Compositors and typesetters | 0.834 | 84 | Buyers and shippers, farm products | 0.563 | 61 |
| Clergymen | 0.826 | 41 | Officers, pilots, pursers and engineers, ship | 0.570 | 75 |
| Plumbers and pipe fitters | 0.819 | 32 | Laborers (n.e.c.) | 0.589 | 1282 |
| Pharmacists | 0.818 | 41 | Salesmen and sales clerks (n.e.c.) | 0.592 | 441 |
| Craftsmen and kindred workers (n.e.c.) | 0.787 | 222 | Hucksters and peddlers | 0.596 | 56 |
| Painters, construction and maintenance | 0.786 | 165 | Bookkeepers | 0.613 | 140 |
| Shoemakers and repairers, except factory | 0.783 | 167 | Machinists | 0.642 | 108 |
| Musicians and music teachers | 0.780 | 51 | Cabinetmakers | 0.661 | 53 |
| Carpenters | 0.774 | 469 | Stationary engineers | 0.678 | 127 |
| Brickmasons, stonemasons, and tile setters | 0.772 | 150 | Truck and tractor drivers | 0.693 | 305 |
| Insurance agents and brokers | 0.757 | 34 | Stone cutters and stone carvers | 0.706 | 55 |
| Real estate agents and brokers | 0.735 | 42 | Molders, metal | 0.714 | 81 |
| Managers, officials, and proprietors (n.e.c.) | 0.731 | 1475 | Conductors, railroad | 0.718 | 33 |
| Cooks, except private household | 0.722 | 36 | Tinsmiths, coppersmiths, and sheet metal workers | 0.721 | 50 |

Notes: Data are from the two 1880 St. Louis Enumerations. Data are limited to occupations with more than 30 observations at first enumeration.

**Figure H1.** Difference in Adjusted Song score across individuals whose occupations disagree




Notes: Data are from the two 1880 St. Louis Enumerations.

**Appendix I.** The influence of measurement error and racial disparities on the Altham Statistic

An important methodological choice is to measure economic status with scores. This method allows me to place people on a univariate scale such that I can calculate a father-son association. Sorting people on a univariate scale is criticized by Long and Ferrie (2013b) since it is unclear how well imputations capture actual income further back in time. This criticism is more important when one goes as far back as Ferrie (2005) and Long and Ferrie (2013a) do – that is, back to the 1850 Census. However, more recent work by Song et al. (2020) suggests that scores can be used to place occupations on a scale using information on literacy.

Nevertheless, it is important to understand how alternative measures of mobility that do not rely on a univariate scale change when accounting for race and measurement error. One way to measure mobility is based on the row and column associations in an occupational transition matrix (i.e., the Altham statistic) (Altham and Ferrie 2007).[12] Given two $r \times s$ transition matrices $\boldsymbol{P}$ and $\boldsymbol{Q}$, with $p_{ij}$ and $q_{ij}$ as elements, the Altham statistic is:

$$d[\boldsymbol{P}, \boldsymbol{Q}] = \left[\sum_{i=1}^{r}\sum_{j=1}^{s}\sum_{l=1}^{r}\sum_{m=1}^{s}\left|\log\left(\frac{p_{ij}p_{lm}q_{im}q_{lj}}{p_{im}p_{lj}q_{ij}q_{lm}}\right)\right|^2\right]^{1/2}$$

Often researchers report the Altham statistic of a matrix $\boldsymbol{P}$ from an independent matrix $\boldsymbol{J}$ where each element is one, indicating perfect mobility. The standard method places fathers and sons into four occupational categories (white-collar, farmer, semi-skilled and unskilled).

In contrast to the Adjusted Song score, the Altham statistic is slightly increased when going from a white-only sample to a Black and white sample (see Figure I1). While the preferred scores increase by about a third when adding Black families, the Altham statistic increases by less than 5 percent. (Note that these estimates do not account for measurement error with multiple father observations.) The lack of movement when adding Black families is because the Altham statistic does not capture Black-white disparities within occupation category. Recall that when using the occupation-only score, which ignores racial and regional disparities within occupation, increased by at most 13 percent when adding Black families (Figure A15). Based on this result, it is

[12] For recent examples of studies that use the Altham statistic, see Cilliers and Fourie (2018), Ferrie (2005), Long and Ferrie (2013a), Long and Ferrie (2018), Modalsli (2017), Pérez (2017), and Pérez (2019).

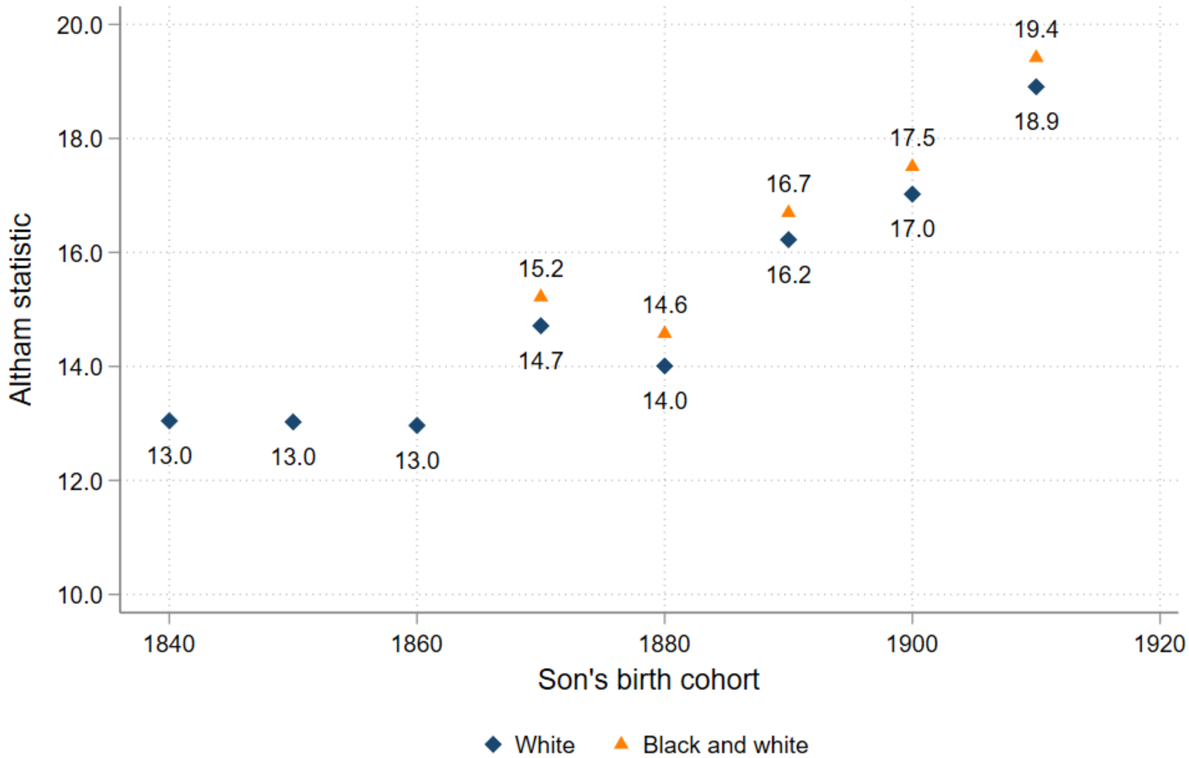
unsurprising that the Altham statistic does not change much when adding Black families. This result reiterates the point that to fully account for racial disparities, one must both include Black families and use a status measure that captures the historical Black-white income gap.

Accounting for measurement error in the Altham statistic is not straightforward. The problem arises because averaging the father's occupational category across censuses does not place him in one discrete category. For example, if a father is a farmer in one census and a white-collar worker in another census, which category is his "true" one? To address this problem, I restrict the sample to sons with fathers who are "truly" in a specific occupation group; that is, if he is observed in the same group in all three observations. Others could use this approach if they only had access to two father observations. At the same time, this method drops part of the sample where fathers switch categories.

This approach to addressing measurement error, where I restrict the sample to fathers observed in the same group three times, does strongly influence the Altham statistic (Figure I2). Based on this method, the Altham statistic increases by 37-55 percent from the baseline estimate, depending on the cohort. Therefore, it appears that there is less intergenerational occupational mobility for the subsample where the father's "true" occupation group can be more precisely pinpointed. At the same time, this method suffers from dropping a large share of the sample, making it unclear whether it is preferable to the first method. Overall, addressing both error and race increases the Altham statistic by 39-52 percent (Figure I3). Note that I cannot calculate Altham statistics for many of the cohorts, either because it is not possible to observe three father observations or because it is pre-emancipation.

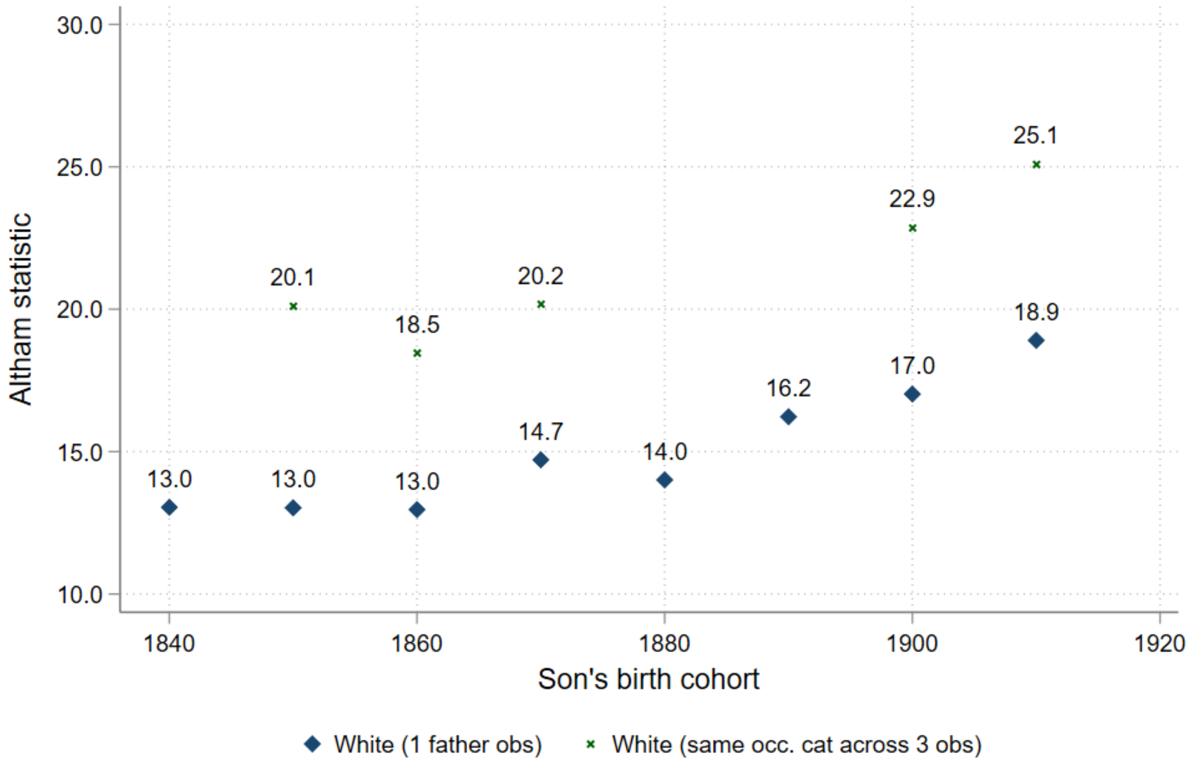
Rather than using the Altham statistic, a simple regression of the son's occupation group on the father's more clearly shows that measurement error matters for occupation categories. For this regression, the father's outcome is an average of zero-one variables across three censuses. Instrumenting the father observation with a second one can more than double associations. However, the IV estimates may overstate the relationship between father and son due to non-classical measurement error in categorical variables (Bingley and Martinello 2017, Dupraz and Ferrara 2018). At the same time, there is non-classical error in the son's occupational category, which may attenuate the IV estimate. Overall, the overall persistence of occupational category from father to son is unclear. Nevertheless, it appears that measurement error matters – even for broad occupation groups.

**Figure I1.** Pooling Black families with white families has a modest influence on the Altham Statistic



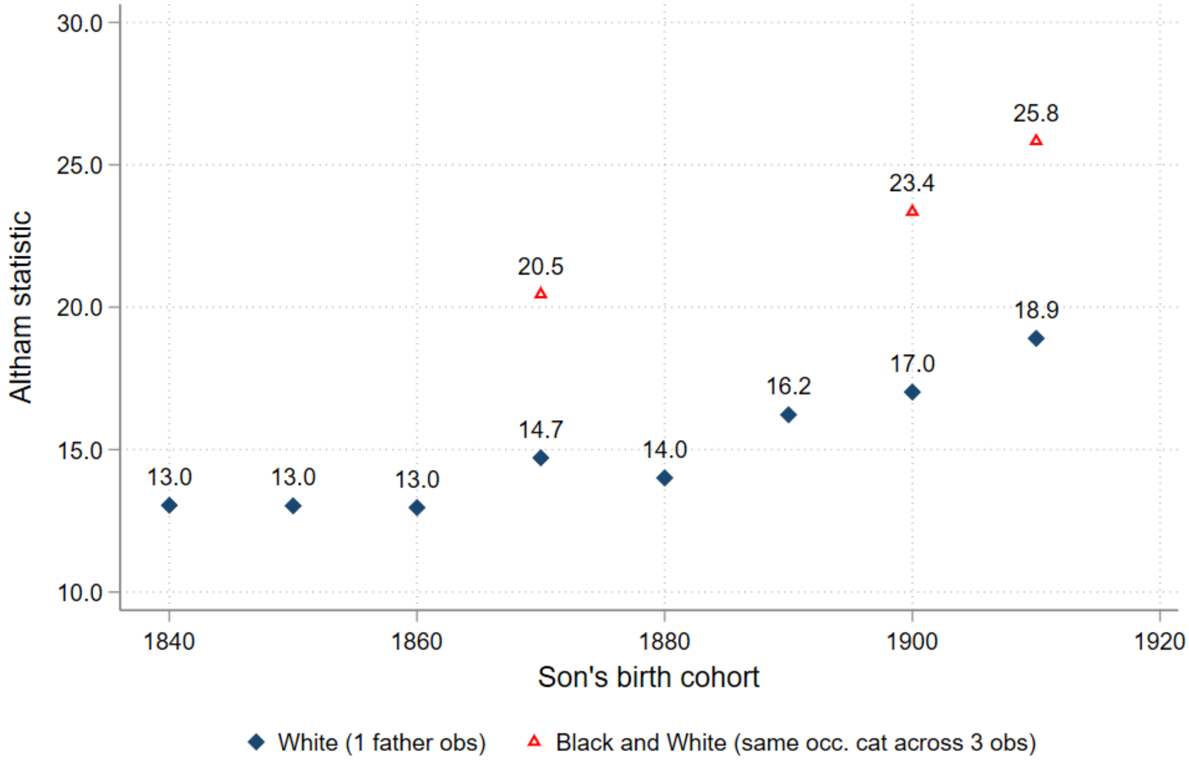

Notes: Data are a linked sample of sons and fathers from the 1850 to 1940 United States Censuses (Ruggles et al. 2020, Abramitzky et al. 2020). The reported Altham statistic is the distance from an independent matrix. All results are statistically significant.

**Figure I2**. Limiting the sample to those fathers with the same occupation increases the Altham statistic



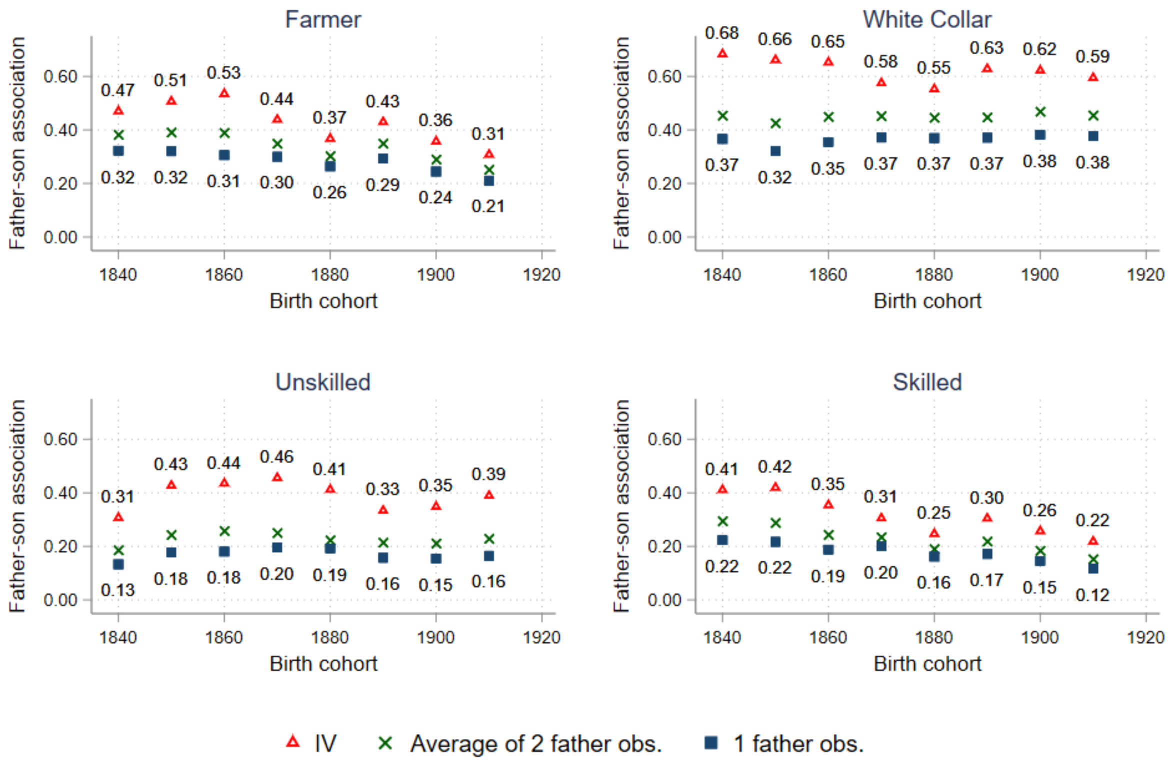

Notes: Data are a linked sample of sons and fathers from the 1850 to 1940 United States Censuses (Ruggles et al. 2020, Abramitzky et al. 2020). The reported Altham statistic is the distance from an independent matrix. All results are statistically significant.

**Figure I3.** Updated, but limited, estimates of the Altham statistic




Notes: Data are a linked sample of sons and fathers from the 1850 to 1940 United States Censuses (Ruggles et al. 2020, Abramitzky et al. 2020). The reported Altham statistic is the distance from an independent matrix. All results are statistically significant.

**Figure I4.** Intergenerational associations of belonging to an occupation category




Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. White-collar occupations are professional (*occ1950* codes: 0-99), managers (200-299), clerical (300-399), and sales (400-499). Farmers are owners and tenants, as well as farm managers. Unskilled are operatives (600-699), Service workers (700-799), farm laborers and laborers (800-970). Skilled are Craftsmen (500-599).

**Appendix J.** Name-based estimates of relative mobility

In this appendix, I discuss how I update intergenerational mobility estimates based on the first-name method developed by Olivetti and Paserman (2015, henceforth "OP"), and the last-name method, similar to Clark (2014). Since the main "directly linked" estimates use the same data source as OP (1850-1940 United States Censuses), the bulk of this appendix will discuss recreating name-based estimates in the context of OP's methodology. First, I will describe how I update OP's data and method to account for measurement error and racial composition. Second, I will present updated estimates of relative mobility. Ultimately, the name-based estimates support the conclusion that parent-child persistence was high in the mid-19$^{th}$ century and decreased between 1840 and 1910 birth cohorts. I do not create name-based estimates after 1910 because publicly available sources do not include names.[13]

OP develop an innovative methodology to estimate the trend in relative mobility for *both* males and females based on the informational content of first names. OP show that since first names carry informational content, then one could infer a parent's economic status based on his or her first name. The way this method is implemented is to take all fathers of children with a given name in a census *t-20*, average the status of all fathers of children with the same name, and then use this average to proxy for the father's status in later census *t* when the child is an adult. For example, if fathers of children named "Xavier" were high-status in 1900, and adults named Xavier were also high-status in 1920, then one can back out an estimate of relative mobility. A similar method could be applied to last names: if fathers of children with the surname "Ward" were high status in 1900, and adults with the last name Ward were also high status in 1920, then one can back out a separate estimate of relative mobility. See Stuhler and Santavirta (2020) for an in-depth examination of name-based estimators and how they relate to estimates based on directly linked data.

There are many reasons why name-based estimates differ from directly linked estimates. Names may have additional informational content that influence the child's outcome, such as

[13] Chetty et al. (2014, Appendix D) discuss name-based estimates of relative mobility when using tax data. When using the entire sample, they find that the surname rank-rank slope is 0.39, while the individual sample is 0.30. It is unclear whether the difference in estimates reflects additional informational content of surnames, or because averaging reduces error. Chetty et al. speculate that the high associations in Clark (2014) is because the use of rare surnames may capture ethnic or racial differences. I use the entire sample for this analysis, so this rare surname issue is not a concern.

capturing the effects of geography or ethnicity. Names may also have a direct effect on outcomes, perhaps due to discrimination. One nuanced reason for the difference between name-based and directly linked estimates is the amount of *overlap* between the children in the child sample, and the adults in the sample decades later (Stuhler and Santavirta 2020). The intuition is as follows: measurement error occurs if the children in year *t-20* are different from the adults observed in year *t*. Going back to the example of children named "Xavier" in year 1900, if the child Xaviers in the one-percent sample from 1900 are not be the same as the adult Xaviers in the one-percent sample from 1920, then the father's status will be imputed from different families and thus not be fully accurate. While OP (2015) use one-percent samples, I will use full-count data. Using the full-count data reduces bias from limited overlap since the child Xaviers in year 1900 should be the same Xaviers in year 1920 (ignoring death, outmigration or under enumeration).

The original estimates based on the first-name method from OP contrast with my estimates. While they find relative mobility was highest in the mid-19$^{th}$ century and mostly fell over time, I find that relative mobility was lowest in the mid-19$^{th}$ century and improved over time. However, I will show that the first-name method, the last-name method, and the directly linked method find similar estimates after updating OP's data and method in three ways:

1) I include Black families. OP drop Black families since most of them cannot be observed in census manuscripts before 1870. For those born before 1865, I assume that Black southern-born individuals had fathers with a "slave" occupation.
2) I use the full-count data. OP originally used the 1850-1940 one-percent samples from IPUMS since the full-count data was not yet available. I also use adults aged 25-55, which matches my directly linked sample. As mentioned previously, this helps with the issue of overlap between the adults in year *t* and children in year *t-20.*
3) I pseudo-link across censuses not only based on sex and first name, but also based on race and state of birth. For example, I match *white Texas-born* Xaviers with the average father status of *white Texas-born* Xaviers in the census 20 years prior. These additional variables allow for a more accurate imputation of the father's status (Craig et al. 2019).
4) I separately create pseudo-links based on the last name. This reflects Clark's (2014) method. I do this not only just for last name, but also based on last name/race/state of

birth (similar to step 3). I pseudo link with the exact last name, without any name standardization, to reflect the problems with cleaning names from Bailey et al. (2020).

To replicate OP's method, I use the restricted-access full-count data from IPUMS and available at the National Bureau of Economic Research. I will show how estimates vary when using one-percent samples or when using 100 percent sample. Whenever I use a one-percent sample, I take my own sample from the data.

First, to replicate the method in OP I use one-percent samples of white males and pseudo-link based only on name (and sex/race). To be clear, I plot the slope estimates of

$$y_{i,child} = \beta_0 + \beta_1 \bar{y}_{i,father} + \varepsilon_i$$

where a child's status $y_{i,child}$ is regressed on the average status of fathers of children of the same first name or last name ($\bar{y}_{i,father}$). This average is taken from a prior cross-sectional census. Based on this method, the first-name method estimates a similar trend as in the original OP paper: persistence was low in the mid-19$^{th}$ century, increased over time, and then eventually reverted (green dashed line in Figure J1). However, this finding contrasts with the directly linked estimates that find an *increase* in relative mobility throughout the 19$^{th}$ century (orange line in Figure J1). On the other hand, the last-name method finds a relatively flat trend of mobility that is at a low level of 0.20, which does not match the estimates of Clark (2014) around 0.75.

A key reason why the trend differs across methods is due to regional disparities in status. Regional disparities within occupation are lost when pseudo-linking *only* on name. (I do pseudo-link by race for these estimates, so racial disparities within occupation are not an issue.) Going back to the example of Texas-born Xaviers, if one pseudo links only on first name, then fathers from different regions will be averaged into the father's score. If names are not region specific, then key information on inequality is lost. A simple way to rectify this issue is to additionally pseudo-link on the birthplace of the child, since the birthplace of the child is stable across censuses. See Craig et al. (2019) for a similar discussion.

Figure J1B shows that if one additionally pseudo-links on birthplace, the level and trend of mobility line up surprisingly well between the directly linked and first-name methods. The first-name method now finds that relative mobility was lowest in the mid-19$^{th}$ century and improved over time. In addition, the level of mobility is similar, even though I am still using the one-percent sample to create name-based estimates. This consistency is surprising because the name-based estimator should be attenuated when using the one-percent samples (Stuhler and Santavirta 2020). However, Stuhler and Santavirta (2020) also show that for their data, first-name estimates are higher than directly-linked estimates, perhaps due to additional information content. The surname-based estimates find a similar trend to first-name estimates, but once again, at a lower level.

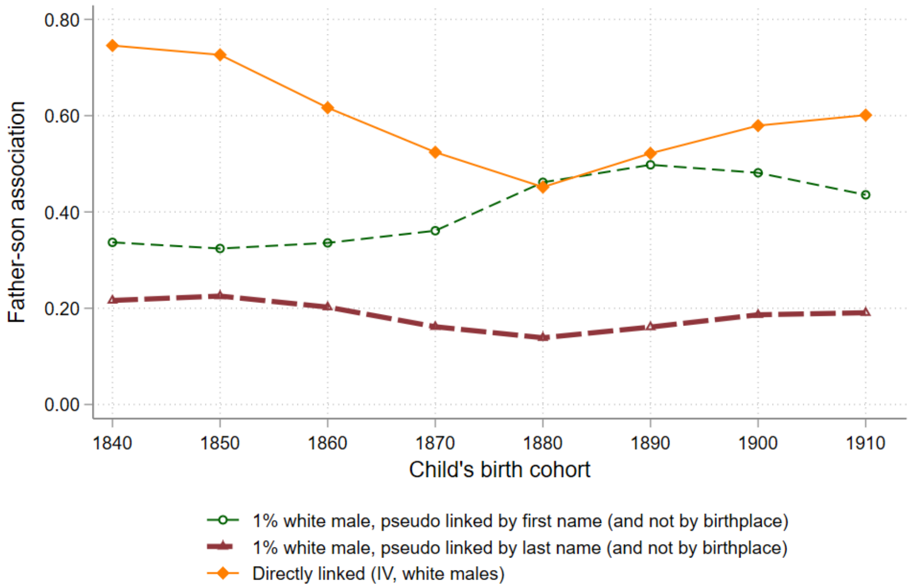
Now I turn from the one-percent samples to the full-count data to address issues of limited overlap. I still limit the data to white families and will discuss racial disparities later. Figure J2A plots first-name estimates and shows that using full-count data increases the level of the estimates by about 0.20. This pattern is consistent with limited overlap attenuating estimates. While going from the one-percent to the full-count data changes the *level*, it does not change the *trend*. This result suggests that OP's original method of using one-percent samples instead of full-count data does not bias the trend – which was their main argument in the first place. Therefore, the difference in trends between our results has more to do with the measure of status and additionally linking by birthplace. Figure I2B plots surname-based estimates, and shows that the level and trend of mobility are strikingly similar to the directly linked estimates.

The estimates so far are only for white males. Figure J3 shows that after pooling Black families in the sample, the *trend* in mobility is consistent across all three estimates. Specifically, intergenerational persistence was highest for the mid-19$^{th}$ century cohorts, decreased until 1890 birth cohorts, and then reverted up for later birth cohorts. However, the *levels* of the father-son association differ across the first name and directly linked estimates, which may reflect additional informational content of first names. The level of surname estimates is similar to that of the directly linked estimates.

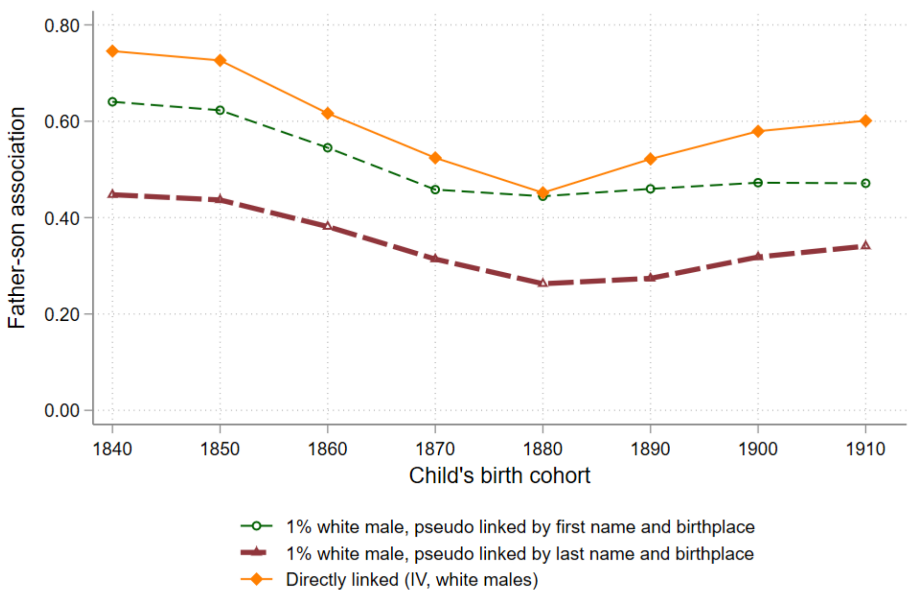
A key benefit of the first-name method is uncovering how female mobility (as proxied with persistence between father and son-in-law) differed from male mobility. Figure 6 from the main text shows that the first-name method consistently finds *higher* intergenerational persistence for females than for males. This finding contrasts with OP, who find mixed evidence. However, the difference between male and female mobility is not large, where female persistence is at most 9 percent higher. Therefore, the pooled (male and female) estimates of relative mobility are only five percent higher than male relative mobility.

**Figure J1.** Pseudo-linking by birthplace matters for name-based estimates

Panel A. Pseudo linking by name, sex and race



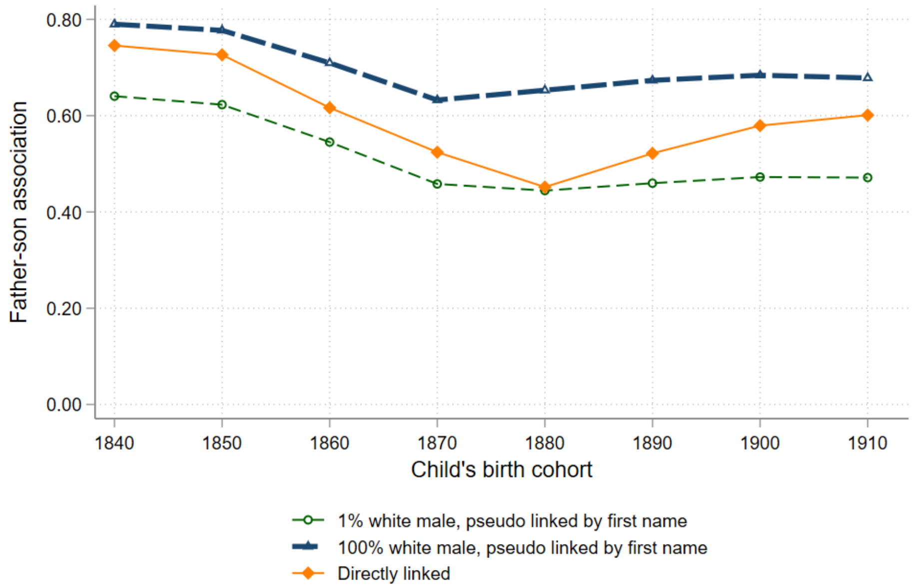

Panel B. Pseudo linking by name, sex, race and birthplace



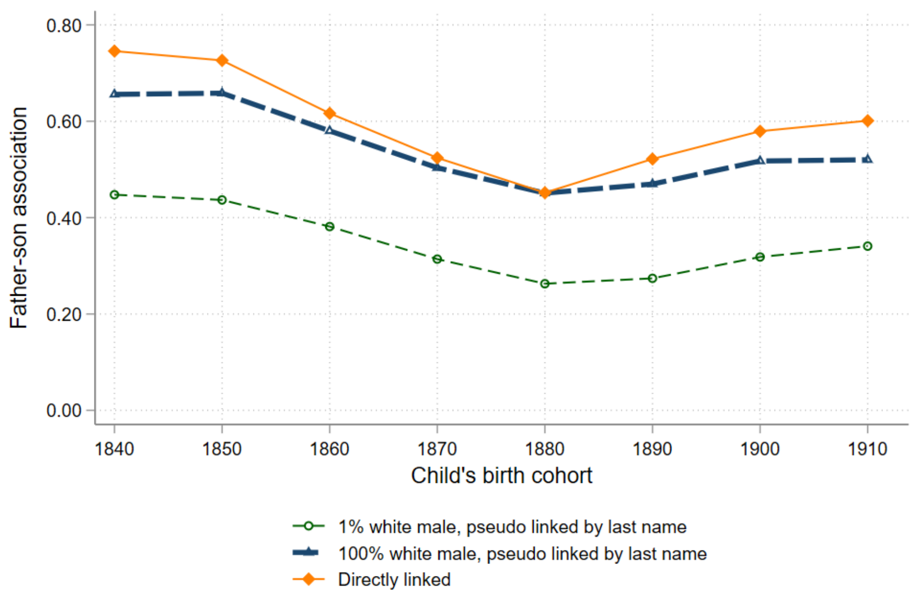

Notes: Data are from the United States 1850-1940 Censuses. Psuedo-linking implements the method described in Olivetti and Paserman (2015). Psuedo links are also made by race and sex. Directly linked data are the IV estimates from the main text. The data are only of white families.

**Figure J2.** Using full-count data instead of samples increases name-based estimates

Panel A. Pseudo linking by first name, race, sex, and birthplace

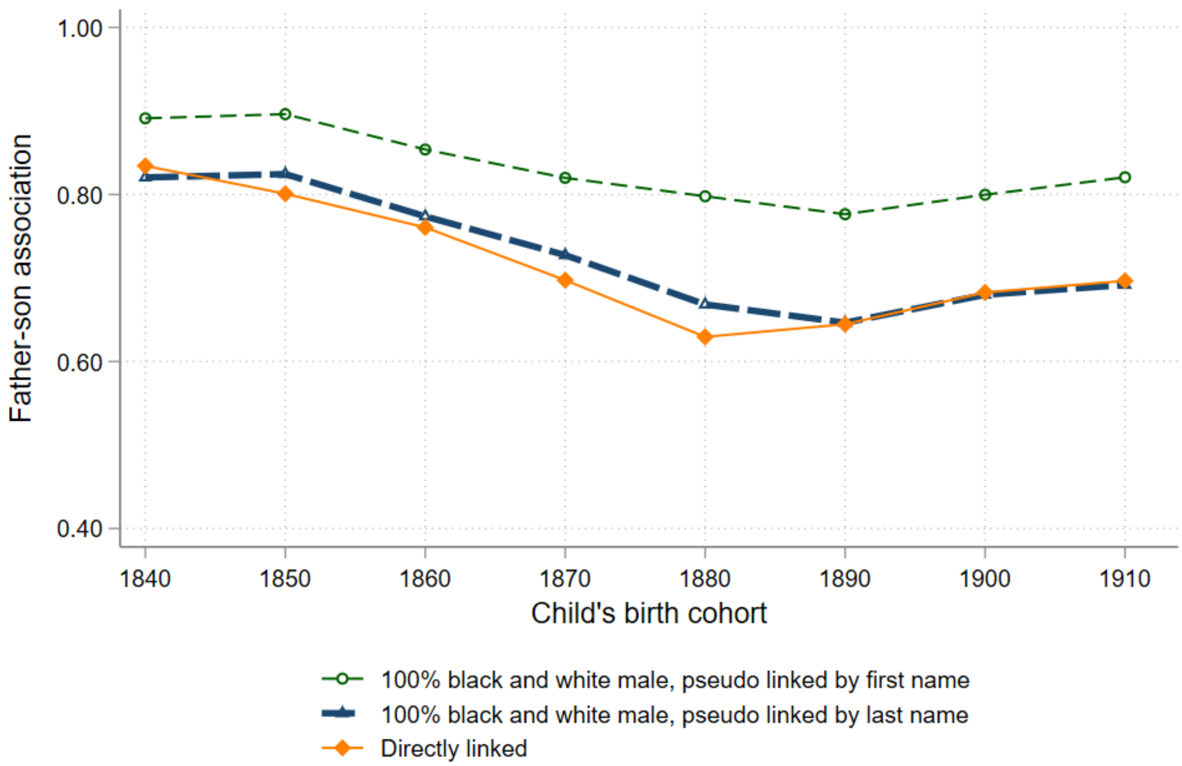



Panel B. Pseudo linking by last name, race, sex, and birthplace




Notes: Data are from the United States 1850-1940 Censuses. Psuedo-linking implements the method described in Olivetti and Paserman (2015). Psuedo links are also made by race and sex. Directly linked data are the IV estimates from the main text. The data are only of white families.

**Figure J3.** The trend in relative mobility for Black and white sons across linking methods




Notes: Data are from the United States 1850-1940 Censuses. Psuedo-linking implements the method described in Olivetti and Paserman (2015). Psuedo links are also made by race and sex. Directly linked data are the IV estimates from the main text.

**Appendix K.** Details on the PSID sample

To extend the mobility trend to birth cohorts after World War II, I use the Panel Study of Income Dynamics, which has data between 1968 and 2019. I keep 25 to 65-year old Black and white males who are observed with an occupation. If there are multiple occupation observations, then the preferred one is the occupation that is recorded closest to age 40. I then link fathers to sons using the Family Identification Mapping System. Fathers need to have a second occupation observation that is between 3 and 10 years away from a first occupation observation. The "second" occupation is the one observed furthest away (e.g., if a father occupation is observed in 1968, 1971 and 1978, I keep the 1978 one as the second occupation).[14] Keeping the more distant occupations mimics the census data, which observations occupations at 10-year gaps. Due to this structure, the average age of sons in the dataset is 37.0, and the average age of fathers for the preferred occupation is 38.2. The total number of observations is 776 in the 1960 cohort (1955-1964), and 478 in the 1970 cohort (1965-1974) and 635 for the 1980 cohort (1975-1984).[15]

To assign the unadjusted and adjusted Song scores to the PSID, I need to create crosswalks to the 1950 occupation codes from the 1970 and 2000 codes. I first create a crosswalk between the 1970 and 1950 occupation codes using the most common 1950 code for each 1970 occupation in the 1970 IPUMS 1% random sample. I create another crosswalk between 1950 and 2000 codes using the 5 percent sample of the 2000 census. After attaching the 1950 codes to the PSID, I use the crosswalk between the 1950 codes and the 70 "micro-class" occupations from Song et al. (2020). With this in place, it is straightforward to match the unadjusted and adjusted Song score to each person in the PSID.

The PSID may not be representative of the population for later birth cohorts, such that the PSID estimates are too lower, though using weights mitigates this issue (Schoeni and Wiemers 2015). To keep the weighting method consistent over time, I use an inverse propensity weighting method after pooling the "linked" sample with a dataset that reflects the population. For the

[14] I prefer the occupation 10 years later, and then check for an occupation 10 years earlier. Then I check for an occupation 9 years later, and so forth.

[15] I use the weights provided by the PSID for the son's year of observation, but also upweight the Black population. This is because the Black proportion in the PSID data is less than in the population, perhaps because attrition is higher. The practical effect of upweighting Black sons is that I estimate a higher persistence rate for the 1950 and 1960 cohorts; since I find that the 1950 and 1960 persistence rates are the lowest across the trend, upweighting the Black population does not drive the results.

historical data, I pooled the linked samples with the full-count censuses and estimated which observables predicted being in the linked sample. For the modern-day data, I pool the PSID sons with the 1980-2019 CPS and estimate which observables predict being in the PSID. I keep the models consistent over time, I predict inclusion in the PSID based on:

- Age (ten-year bins), interacted with 5-year dummy variables in case selection varies over time
- Black indicator, interacted with 5-year dummy variables
- Region of residence, interacted with 5-year dummy variables
- Holding a white-collar, farmer, unskilled or skilled occupation, all interacted with 5-year dummy variables
- Five-year dummy variables

While I focus on the Song score measures for the long-run trend in mobility, I also estimate the long-run trend using imputed earnings. For censuses between 1850 and 1940, I use the same 1940 earnings score measure as presented in the main text. For the PSID data, I assign fathers and sons an income score from the nearest decadal census (i.e., average total income within occupation/race/region cells). Those in the PSID who either did not report an occupation or were living outside of the United States are dropped from the sample.

**Appendix L.** Absolute mobility estimates

The primary estimates in this paper are for *relative* mobility, or the association between the child's place in the economic distribution compared to the father's place in the distribution. While relative mobility is important for understanding the transmission of status across generations, another measure of interest is *absolute* mobility, or whether the child ends up with a higher income or better outcome than the father. Note that this paper uses measures of "status" since income is unavailable, and status is relative by nature – not everyone within a society can be "high status" or "low status." Instead of absolute mobility based on status, the economics literature measures absolute income mobility. Indeed, Chetty et al. (2017) connect absolute mobility to the concept of the "American Dream" and measure it by the fraction of children who have weakly higher incomes than their parents. More explicitly, while relative mobility is measured by the magnitude of $\beta_1$ from the regression $y_{i,s} = \beta_0 + \beta_1 y_{i,f} + \varepsilon_{i,s}$, absolute mobility is measured with an indicator variable for weakly improving on parental income $Absolute_{is} = 1[y_{i,s} \geq y_{i,f}]$.

Note that the trend in relative mobility need not be similar to the trend in absolute mobility. Conceptually, income growth could be very rapid across generations (high absolute mobility), but one's relative position in the distribution may remain constant across generations (low relative mobility). Chetty et al. (2017) estimate a downward trend in absolute income mobility for birth cohorts since 1940 but assume that relative rank mobility has been constant for the same birth cohorts. Chetty et al. (2017) argue that slowing income growth and widening income inequality could explain the difference in trends. Chetty et al. (2014b) use the analogy of climbing an income ladder: children's chance of climbing the ladder remains the same, but the gaps between rungs have widened.

Measuring absolute mobility in historical data is less straightforward than measuring relative mobility. Absolute mobility is a function of income growth, inequality in the parent's and child's generation, and the rank-rank transition matrix (i.e., copula). This paper focuses on the last component by measuring the father-son slope. However, it is difficult to measure income growth or changes in inequality in pre-1940 data. The basic limitation is that pre-1940 data lack income. One way to circumvent the income problem is to use occupational income or earnings, but these measures do not capture growth over time since they are mostly from the 1940 or 1950 censuses. Moreover, occupational income does not capture changing income inequality since they are based on one census year. Therefore, two of the key pieces that create an absolute mobility estimate (income growth and inequality) are not well measured. Indeed, studies often rank these occupational earnings scores in part due to uncertainty applying them to earlier years (e.g., Collins and Wanamaker 2021, Abramitzky et al. 2021), but ranking loses information on growth and inequality. Instead of earnings scores, one could instead use literacy to measure absolute mobility ($1[literate_{i,s} \geq literate_{i,f}]$). However, literacy is bounded by one and thus the fraction of children who weakly improve on their parents approaches one as literacy becomes universal. A finer measure of human capital may be more useful, but there is none for 1850-1930 data.

Despite these limitations, it is still useful to understand how measures of absolute mobility may be influenced by race and measurement error. To be consistent with the human-capital-based measure in the main text, I measure absolute mobility as whether the child is in an occupation/race/region cell with a higher average literacy rate than his father's cell. Therefore, the measure captures the skill level of people working in an occupation, separately estimated by race and region. For example, suppose that both the father and son are white Southern farmers. Further, suppose that 60% of white Southern farmers are literate for the father's generation and 70% are for the son's generation. Given this information, I would measure the son as absolutely mobile (70% ≥ 60%). Since the census data does not include literacy in the 1940 Census and beyond, I use average years of education for the PSID data. I never compare a son's education level to a father's literacy rate.

The only other estimates of absolute mobility in 19th and 20th century data that I am aware of come from Song et al. (2020).[16] Song et al. (2020) measure absolute mobility by whether a son is in a higher ranked (but not percentile ranked) occupation than his father. For example, suppose the father was a farmer, the average literacy rate was 60 percent, and this percent placed farmers as the 20th most literate occupation (where 1st is best). Suppose the son was also a farmer and 70 percent of farmers were literate for the son's birth cohort. However, because other non-farmer occupations had greater increases in literacy over time, farmers dropped to the 30th most literate occupation. Song et al. (2020) would measure this son as *not* absolutely mobile since the rank of the occupation did not improve; however, I would measure the son as absolutely mobile since there

[16] Others have measured upward rank mobility (e.g. Collins and Wanamaker 2021), or whether the son has a higher percentile rank than the father. However, percentile ranking misses growth across generations.

was growth over time. Since ranking loses information on growth, I prefer to use the absolute literacy level.

The main point of this paper is to understand how the issues of measurement error and race influence mobility estimates. While measurement error attenuates estimates of the father-son association, error's effect on absolute mobility estimates are less clear. To see why, let $Absolute_i = 1[y_{i,s} \geq y_{i,f}]$ be an indicator variable that is equal to one if the son weakly improves on his father's permanent income. Let $\pi_{01}$ be the probability that a true absolute move upward is misclassified $P[Upward = 0|Upward^* = 1]$ and let $\pi_{10}$ be the probability that a true absolute movement downward is misclassified $P[Upward = 1|Upward^* = 0]$. Therefore, the level of absolute mobility depends on the relative rates of misclassification $\pi_{10}$ and $\pi_{01}$. I can explicitly show how averaging occupations may change measures of absolute mobility. However, I will be unable to eliminate error (unlike the estimates of relative mobility) since measurement error in binary variables is, by definition, nonclassical.

The bias from racial disparities is more straightforward: the rate of absolute mobility $Absolute_i$ for the population is a weighted average of $Absolute_i$ for each race. If Black children are more likely to improve on their father's level of status, then $Absolute_i$ would be downward biased for the whole population.

*Estimates of the trend in absolute mobility*

Figure L1 shows the trend in absolute mobility for white males between 1840 and 1980 birth cohorts. For the interpretation of these coefficients, 44 percent of the 1840 cohort ended up in an occupation/region with a weakly higher average literacy rate than their father's. For the 1960 cohort, 77 percent of children ended up in an occupation/region with a higher mean years of education than their father's. The trend in absolute mobility suggests that absolute mobility started low for children in the 1840 and 1850 birth cohorts, where less than two-thirds of white children ended up weakly higher than their fathers. This increased to a maximum of 85 percent for the 1900 birth cohort, which was also a cohort that was entering school during the start of the high school movement. Given the increase in human capital levels over time, it may not be surprising that a large majority of this cohort improved on their father.

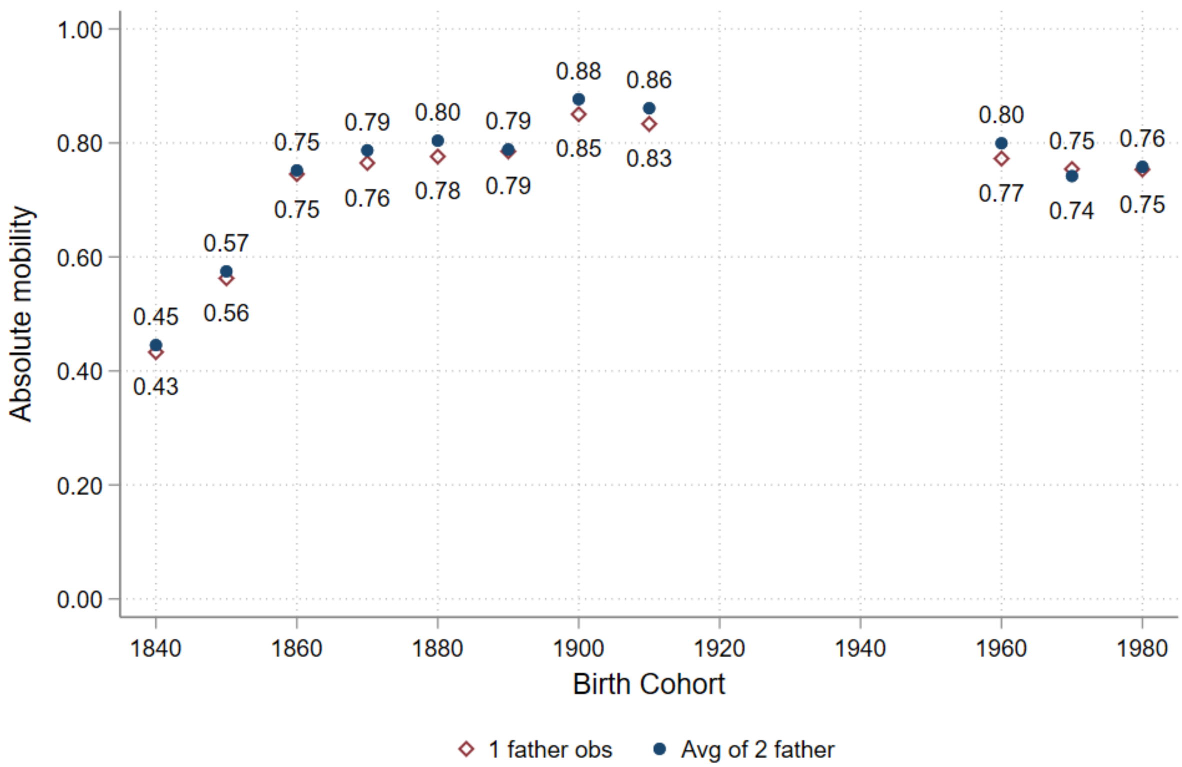
Figure L1 shows that measurement error for the father does not matter much for absolute mobility estimates. Absolute mobility rates increase slightly after averaging two observations, but the increase is small. As mentioned before, it is difficult to fully address error due to the nonclassical form.

Figure L2A plots absolute mobility by race. There are substantial differences in absolute mobility by race. The data suggest that 88-99 percent of Black sons improved on their father's outcome in the historical data. The 99 percent is due to emancipation causing children to have better "occupations." In contrast, only 45 percent of whites were absolutely mobile for the 1840 cohort. The post-1950 cohorts find that absolute mobility is higher for the Black population than for the white population, perhaps because Black parents were starting off with lower education levels. This is a novel finding, though it would be fruitful to check it if holds for income data. Since Black families tended to have higher levels of absolute mobility in the past, the white-only estimate understates absolute mobility for Black and white population (Figure L2B).

Including Black families *increases* absolute mobility estimates but *decreases* relative mobility estimates. While this may seem like a contradiction, it is not. Black sons improved from a very low base; however, conditional on the father's status, they did not improve as much as white sons (Collins and Wanamaker 2022). Therefore, the Black son's location in the distribution did not change much for the decades following the Civil War.

Ultimately the results in this section suggest that measurement error does not have a strong influence on absolute mobility estimates, and that the focus on white samples does have an impact. While I have shown these results on a limited measure of absolute mobility, future research could try to better capture changes to income growth and inequality in the historical data.

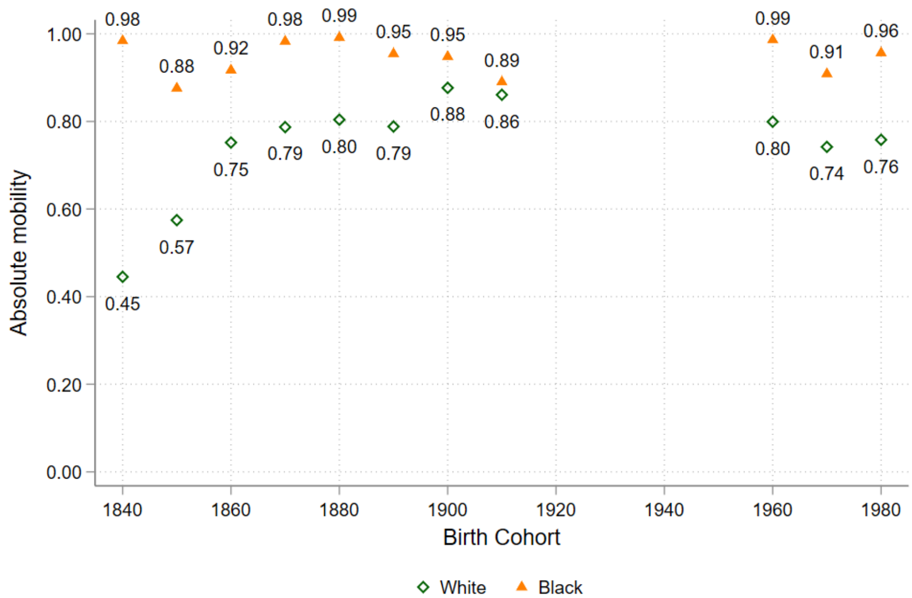
**Figure L1**. Absolute mobility for the white population between 1840 and 1980



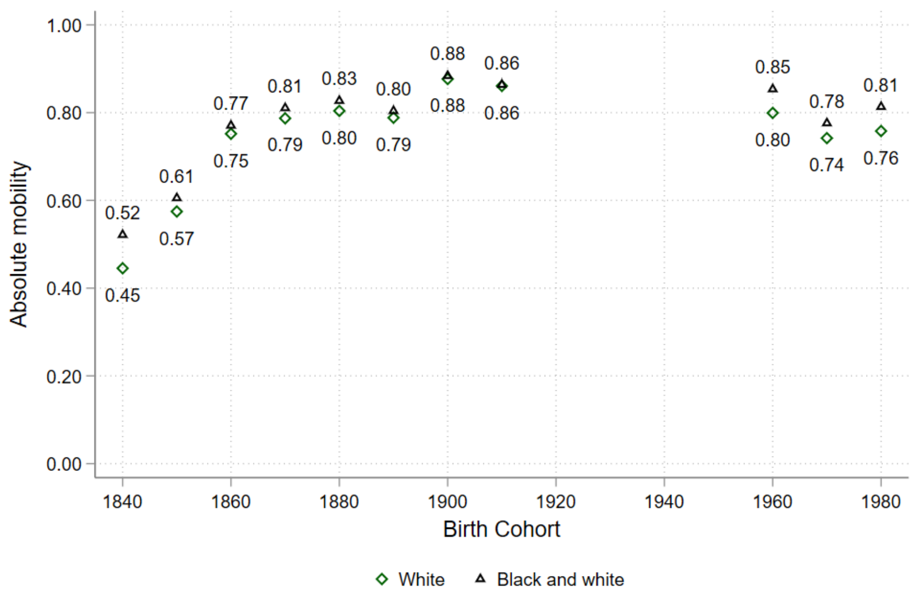

Notes: Data are a linked sample of sons and fathers from the 1850 to 1940 United States Censuses (Ruggles et al. 2020, Abramitzky et al. 2020). The figure plots the fraction of children who are in an occupation/race/region cell that has a higher human capital level than their father's cell. Human capital is literacy for pre World War II cohorts and years of education for post World War II cohorts.

**Figure L2.** Absolute Mobility by race

Panel A. White v. Black absolute mobility




Panel B. White v. Pooled




Notes: Data are from the United States 1850-1940 Censuses and the PSID. The figure plots the fraction of children who are in an occupation/race/region cell that has a higher human capital level than their father's cell. Human capital is literacy for pre World War II cohorts and years of education for post World War II cohorts.

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