Intergenerational Mobility in American History: Accounting for Race and Measurement Error[*]

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Abstract: A large body of evidence finds that relative mobility in the US has declined over the past 150 years. However, long-run mobility estimates are usually based on white samples and therefore do not account for the limited opportunities available for non-white families. Moreover, historical data measure the father's status with error, which biases estimates toward greater mobility. Using linked census data from 1850-1940, I show that accounting for race and measurement error can double estimates of intergenerational persistence. Updated estimates imply that there is greater equality of opportunity today than in the past, mostly because opportunity was never that equal.

Keywords: intergenerational mobility, measurement error, persistence

JEL Codes: J62, N31, N32

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I. Introduction

One of the reasons America tolerates inequality is the belief that opportunity is available for everyone, whether they grew up rich or poor. This deep-seated belief is rooted in history when millions migrated from the rigid class structures of Europe and entered a relatively free and open society (de Tocqueville 1835). However, a large body of research documents a decline in relative intergenerational mobility since the mid-19th century, suggesting that the United States has turned into a class-based society and lost its exceptional character (Ferrie 2005, Long and Ferrie 2013a, Feigenbaum 2018, Parman 2011, Song et al. 2020).[1]

In this paper, I challenge the long-standing idea that relative mobility was higher in America's past. Like Solon (1992), I argue that the high mobility estimates are biased by measurement error and unrepresentative samples. Correcting these issues can double estimates of the transmission of status from father to son. The corrections in historical data are so large that the results suggest there is greater relative mobility for cohorts born after World War II than for cohorts born before, in contrast to much of the literature. This paper updates the history of *relative* mobility, which measures whether having rich or poor parents matters for lifetime outcomes. However, due to data limitations, it does not focus on *absolute* mobility, or the growth of income across generations (Chetty et al. 2017).[2] Therefore, this paper relates more to the concept of "equality of opportunity," which appears to be greater today than in the distant past.

One reason why prior work overstates relative mobility is measurement error. When estimating mobility, the ideal measure is how strongly lifetime or permanent income transmits across generations. However, permanent income is not available in historical sources. Instead, studies use a single observation of the father's occupation as a proxy, which can be problematic if there are transitory shocks or errors in the data (Kambourov and Manovskii 2008, Mazumder and Acosta 2015). Regardless of where errors come from, they attenuate the father-son association and

[1] The evidence for the decline comes from comparing the same mobility measure over time, such as the Altham statistic for occupational mobility (Ferrie 2005, Long and Ferrie 2013a), the IGE or rank-rank slope for income mobility (Feigenbaum 2018, Parman 2011), or the rank-rank slope for an occupational human capital score (Song et al. 2020). However, there is debate over the trend in relative mobility over the 19th and 20th centuries (see Hout and Guest 2013). Chetty et al. (2017) show a downward trend in "absolute income mobility" from the mid to late 20th century, which differs from relative mobility.

[2] Estimating absolute mobility in historical data is difficult since income or wealth is not always available. Rather, it is more straightforward to rank occupations on a 0-100 scale (the method used in this paper) than to pinpoint the absolute level of income or wealth. I discuss this issue at further length in Appendix L.

falsely imply high mobility (Solon 1992, Clark 2014). A standard way to address this problem is to average multiple father observations to better proxy for his permanent status. However, this approach is seldom used due to the high cost of linking historical censuses (see Ward (2020) for an exception), a cost which has fallen to zero with the release of publicly available linked data (Abramitzky et al. 2020).[3]

In addition to measurement error, unrepresentative samples bias historical estimates because most studies include few, if any, African Americans. While this omission may seem odd, it is due to data limitations. Since research often starts to measure mobility in 1850 – before emancipation – most Black families are unobservable.[4] Other studies take advantage of income data from early 20th century Iowa, but Iowa was 99 percent white at the time.[5] Since most Black families are not in the historical data, studies also drop Black families in later decades (including well after emancipation) to make comparisons over time consistent. Therefore, the documented decline in relative mobility is actually a decline in *white* mobility. An undisputed pattern throughout history is that Black sons had limited opportunities to advance, which suggests that overall mobility was not that high (Collins and Wanamaker 2022). Indeed, de Tocqueville's classic depiction of a high-mobility America was limited to "Anglos"; in the same work, he decried the treatment of Black families: "Oppression has, at one stroke, deprived the descendants of the Africans of almost all the privileges of humanity" (de Tocqueville 1835, pg. 426).

Using linked samples of fathers and sons that cover the 1850 to 1940 censuses (Abramitzky et al. 2020), I show that estimates of intergenerational persistence can more than double after accounting for race and measurement error. First, I find that the father's occupation was highly unstable across censuses, which conflicts with the assumption that one observation accurately captures permanent status. This instability influences mobility estimates: for a sample of white families, going from one snapshot to averaging three father observations increases the father-son association of status by 23 to 29 percent. But averaging three father observations may still not

[3] Ward (2020) shows that measurement error is important for understanding why ethnic occupational gaps converged slowly for immigrant descendants during the Age of Mass Migration. This paper differs by estimating the importance of measurement error for the Black and white population, extending the data backward to cover 19th century cohorts and forward to cover modern-day cohorts, showing the importance of error across a variety of mobility measures, testing the classical measurement error assumption, and decomposing measurement error into data error versus transitory shocks.
[4] See Ferrie (2005), Long and Ferrie (2013), Olivetti and Paserman (2015), Song et al. (2020).
[5] See Feigenbaum (2018) and Parman (2011).

perfectly capture his permanent status. A simple assumption is that the data are subject to classical measurement error, which is surprisingly consistent with patterns in the data. Assuming classical measurement error, eliminating noise leads to "true" father-son associations that are 31 to 48 percent higher than when using one father observation. An alternative fix for measurement error is to instrument one father observation with a second, which leads to similar estimates (Altonji and Dunn 1991; Modalsli and Vosters 2019). Updated estimates suggest that up to 74 percent of the gaps across white fathers persisted to the next generation, which changes our understanding of generational inequality in the past.[6]

After establishing the importance of measurement error for white families, I then show that including Black families in the sample further increases the father-son association by 11 to 46 percent. For Black and white families, the father-son association ranges from 0.61 to 0.84 for 1840-1910 birth cohorts. I can directly observe intergenerational relationships for Black sons in post-emancipation data; for pre-emancipation data, I assume that Southern-born Black sons had enslaved fathers and impute the father's status to be the lowest rank in the distribution. The increase to the father-son association after including Black families is not because the Black father-son association was especially high; rather, it reflects that gaps across white and Black families were large and persistent (i.e., a between-race effect rather than a within-race effect). The results are consistent with Collins and Wanamaker's (2022) results on gaps in Black-white mobility throughout American history; however, my paper differs by focusing on the pooled Black and white association rather than the mobility gap across races. Therefore, my contribution is to quantify how much relative mobility estimates miss the mark when using a white-only sample to describe the country as a whole.

Given this revision to mobility estimates in pre-World War II data, I then compare historical estimates to modern-day estimates using the PSID. I find, in contrast to recent work by Song et al. (2020), that intergenerational persistence was higher for pre-World War II birth cohorts than for post-World War II cohorts (see Figure 1). The pooled Black and white father-son association falls from 0.84 for the 1840 birth cohort to 0.37 for the 1980 cohort; the white-family only association falls from 0.72 to 0.38. Relative mobility was lowest in the 19th century, in part

[6] Becker and Tomes (1986) note that a 0.40 parent-child elasticity implies that gaps across families mostly disappear by three generations ("from shirtsleeves to shirtsleeves"). If accounting for measurement error updates the estimate to 0.60, then it would take five generations.

reflecting the wide regional inequalities across the North and South, and racial inequalities across Black and white males. Overall, the evidence consistently shows that historical mobility was never that high. Instead, it appears that the long-standing idea that United States is a land of equal opportunity more aptly describes today than the distant past.

Figure 1 shows that revisions to mobility estimates are stronger for historical cohorts than for modern cohorts. One reason is that measurement error is greater in historical occupation data. This could occur if modern-day surveys classify occupations more accurately than census enumerators 100-plus years ago (Kambourov and Manovskii 2008). Indeed, I show suggestive evidence from the re-enumeration of the 1880 St. Louis census that occupational miscoding was substantial: only two-thirds of occupations agreed across enumerations, despite enumerations reflecting the same period. The other reason historical estimates are revised more than modern ones is because racial disparities were wider in the past. Thus, the average Black child had to jump further to reach the average white child's outcome.

There are a few limitations to my approach. First, since income is unavailable in historical censuses, I measure economic "status" on a 0-100 scale based on a percentile ranking of one's occupation, race and region of residence.[7] Allowing for racial and regional differences within occupation is important since occupation-only scores fail to catch key inequalities within occupation (Saavedra and Twinam 2020). However, variation within occupation, race and region is unobserved. A second limitation is that linked data contain false positives (Bailey et al. 2020), which I aim to reduce with conservative linking methods (Abramitzky et al. 2020). Finally, due to data limitations, I do not include females or non-Black racial minorities, and thus am missing other important groups in the population. However, I show that these groups are unlikely to change the trend in relative mobility, either because female mobility was similar to male mobility when using a name-based estimator (Olivetti and Paserman 2015), or because the non-white/non-Black population was too small to alter population estimates.

The results contribute to the literature on historical mobility, which has exploded in recent years due to the release of digitized censuses.[8] While many papers aim to uncover what causes

[7] Similar to Song et al. (2020), occupation, race and region cells are ranked by their level of human capital, using literacy for data between 1850 and 1930, and years of education for years 1940 and beyond.
[8] For examples of historical mobility research using linked data, see Abramitzky et al. (2019), Ager et al., (2019), Bailey et al. (2020), Collins and Wanamaker (2022), Connor and Storper (2020), Craig et al. (2019), Dupraz and

mobility to increase or decrease, which is the ultimate goal of the literature, my paper takes a step back and tries to correctly measure mobility in the first place. My mobility estimates are an outlier and suggest much higher intergenerational persistence in the past (see Table A1 for other mobility estimates in the literature).[9] The results suggest caution when using a subpopulation (e.g., whites) to describe overall mobility if there is substantial between-group inequality. The results further raise the possibility that measurement error varies across source, time and space; therefore, comparative mobility research may be biased. For example, I show that the measurement error is more severe in the past, perhaps because the census enumerators inconsistently recorded occupations. The difference in error is significant enough to revise the trend in mobility. Future comparative studies should try to account for variation in measurement error with multiple father observations.

The findings also contribute to the debate over the relationship between inequality and relative mobility. While it is well known that high-inequality countries have low mobility (i.e., the "Great Gatsby" curve, Corak 2013), there is debate over whether this relationship holds within the United States over time (Chetty et al. 2014b, Davis and Mazumder 2020, Jácome et al. 2021, Lee and Solon 2009). Available evidence on historical inequality, while sparse, shows that inequality was high in the 19th and early 20th centuries – equal to or even higher than inequality today (Goldin and Katz 2008, Lindert and Williamson 2016, Saez and Zucman 2020). I find that relative mobility was low during this high-inequality era, which is consistent with the "Great Gatsby" relationship. At the same time, for post-1960 birth cohorts, I do not find a fall in relative mobility during the recent rise in inequality. The results raise the possibility that the relationship between inequality and relative mobility has weakened over time, perhaps due to institutional changes over the last 100 years that have aimed to improve opportunity for children from poorer backgrounds.

Ferrara (2021), Feigenbaum (2015), Feigenbaum (2018), Ferrie (2005), Grusky (1986), Guest et al., (1989), Kosack and Ward (2020), Karbownik and Wray (2019), Long and Ferrie (2013), Long and Ferrie (2018), Modalsli (2017), Pérez (2017), Pérez (2019), Song et al. (2020), Tan (2018), Ward (2019) and Ward (2020).

[9] The level of my historical estimates do align some estimates from Clark (2014), but I also find that relative mobility improved over time, in contrast to Clark's argument of stable relative mobility.

II. Measuring intergenerational mobility

There are many ways to measure intergenerational mobility, but I focus on relative mobility, or whether the father's place in the economic distribution matters for the child's place. The most common relative mobility estimates come from regressing the son's outcome ($y_{i,s}$) on the father's outcome ($y_{i,f}$):

$$y_{i,s} = \beta_0 + \beta_1 y_{i,f} + \varepsilon_{i,s} \tag{1}$$

The coefficient of interest is β_1. A high-mobility economy has a β_1 near zero such that the parent's income or percentile rank of income is weakly associated with the child's outcome. A low-mobility economy has a β_1 near one. Sometimes β_1 is referred to as capturing "persistence" since a greater β_1 reflects a stronger transmission from parent to child. Note that all estimates in this paper are noncausal.

This paper argues that historical mobility estimates of β_1 are biased by measurement error and the use of white-only samples. To understand how these issues connect to β_1, consider the within-between decomposition of β_1 (Hertz 2008, Bailey et al. 2020):

$$\hat{\beta}_1 = \underbrace{\sum_{g=1}^{G} \theta^g \hat{\beta}_1^g}_{within\ group} + \underbrace{\theta^b \hat{\beta}_1^b}_{between\ group} \tag{2}$$

where θ^g is the share of variation in the father's status from within-group *g* (e.g., white or Black), and θ^b is the share of variation between group means.[10] Estimate $\hat{\beta}_1^g$ is the measure of group-specific relative mobility (e.g., $\hat{\beta}_1^{white}$ for the white population), and $\hat{\beta}_1^b$ is the persistence of group

[10] See (Greene 2002, Chapter 13). Let $S_{XX}^{within} = \sum_{g=1}^{G} \sum_{i=1}^{N_g} (x_{ig} - \overline{x_{\iota g}})^2 = \sum_{g=1}^{G} S_{XX}^g$ and $S_{XX}^b = \sum_{g=1}^{G} N_g (\overline{x_{\iota g}} - \overline{\overline{x_{\iota g}}})^2$, where one bar denote the group mean and a double bar denotes the overall mean. N_g is the number in the group. Also let $S_{XY}^{within} = \sum_{g=1}^{G} \sum_{i=1}^{N_g} (x_{ig} - \overline{x_{\iota g}})(y_{ig} - \overline{y_{\iota g}}) = \sum_{g=1}^{G} S_{XY}^g$ and $S_{XY}^{between} = \sum_{g=1}^{G} N_g (\overline{x_{\iota g}} - \overline{\overline{x_{\iota g}}})(\overline{y_{\iota g}} - \overline{\overline{y_{\iota g}}})$. The estimates for the within-group and between-group associations are $\hat{\beta}^g = \frac{S_{XY}^g}{S_{XX}^g}$ and $\hat{\beta}^b = \frac{S_{XY}^b}{S_{XX}^b}$. Therefore, $\hat{\beta}_1 = \frac{S_{XY}^{total}}{S_{XX}^{total}} = \frac{S_{XY}^{within} + S_{XY}^{between}}{S_{XX}^{total}}$
$= \sum_{g=1}^{G} (\frac{S_{XX}^g}{S_{XX}^{total}} \hat{\beta}^g) + \frac{S_{XX}^{between}}{S_{XX}^{total}} \hat{\beta}^b = \sum_{g=1}^{G} (\theta^g \hat{\beta}^g) + \theta^b \hat{\beta}^b$.

means across generations (e.g., persistence of the Black-white gap). While the focus of this paper will be grouping by race, Black and white, the decomposition can be used for any partition.[11]

As seen in this decomposition, the literature primarily estimates $\hat{\beta}_1^{white}$, which can differ from $\hat{\beta}_1$ if Black mobility is different from white mobility, or if there is a large (θ^b) and persistent ($\hat{\beta}_1^b$) Black-white gap. Moreover, measurement error attenuates the within-group associations $\hat{\beta}_1^g$ for both the Black and white population, such that current estimates of $\hat{\beta}_1^{white}$ do not capture the true level.

Measurement error attenuates within-group father-son associations

Measurement error biases mobility estimates because the ideal measure of the father's and son's outcome, permanent income, is rarely observed. Instead, many in the early literature used short-run proxies for permanent income, but short-run proxies are noisy and attenuate β_1 toward zero (Solon 1992, Mazumder 2005). Under the assumption of classical error where the parent's income varies from permanent income by random noise ($y_{i,f} = y_{i,f}^* + v_{i,f}$), then attenuation bias falls when averaging the father's income more times (T):

$$plim\ \widehat{\beta_{avg}} = \beta_1 \frac{var(y_{i,f}^*)}{var(y_{i,f}^*) + {var(v_{i,f})}/{T}} \tag{3}$$

Recognizing this problem, modern-day studies use long-run averages of ten or fifteen years; however, historical data rarely go beyond $T = 1$ due to the (formerly) high costs of linking censuses. Note that this model focuses on measurement error in the father's outcome and not the son's since classical error in the son's income does not bias estimates, though nonclassical error will.[12]

Since this model is motivated by transitory income shocks, measurement error may not be important for occupational-based measures of "permanent income" in historical data. However, others have shown that attenuation bias is important for occupational-based measures in both modern-day and historical data (Mazumder and Acosta 2015, Ward 2020). Instead of transitory

[11] Data limitations do not allow me to include other races, such as American Indians or Asians, in the data. Later in the paper, I will test to the importance of these missing groups for mobility estimates.
[12] See Haider and Solon (2006) and Nybom and Stuhler (2017) on non-classical measurement error based on the age of observation for the son. I will later show that the age of the son does not lead to substantial bias in historical census data.

shocks, measurement error could also come from data error. This type of error was found in the PSID and CPS due to inconsistent coding of occupations (Kambourov and Manovskii 2008, Moscarini and Thomsson 2007). I will later show suggestive evidence that inconsistent recording of occupations is an important reason for error in historical censuses.

In the context of the within-between decomposition, measurement error attenuates within-group estimate $\hat{\beta}_1^g$. The share of within-group variation may also be overstated due to the extra noise in the data. On the other hand, between-group persistence $\hat{\beta}_1^b$ should not be biased if error does not influence group means. Indeed, group averages are often used to explicitly address this issue of measurement error (Clark 2014).

The historical literature has mostly ignored between-race disparities

The second measurement issue is the literature's focus on white-only samples. Using white-only samples ignores both within-Black mobility and the between-group component that captures persistent racial disparities. Discounting between-race effects is nontrivial. First, between-race persistence $\hat{\beta}_1^b$ has been strong throughout American history. Chetty et al. (2014) estimate the Black-white income $\hat{\beta}_1^b$ was 0.99 between 1980 and 2010; a rate of 0.99 reflects that there was almost no convergence of the Black-white income gap during this period. Historical income estimates between 1870 and 1940 suggest that $\hat{\beta}_1^b$ was between 0.91 and 0.94 (Margo 2016). The between-race share of variation θ^b can also be large; for instance, Hertz (2008) estimates it at 0.20 when using the Panel Study of Income Dynamics (PSID). Since the Black-white income gap was larger in the past, the between-group share of variation θ^b was also likely higher, suggesting that ignoring between-race effects is more important for historical estimates than for modern ones.

The main exception to the literature's focus on white samples is Collins and Wanamaker (2022), who provide the first estimates of $\hat{\beta}_1^{Black}$ in the late 19th and early 20th centuries. This paper differs by focusing on how pooling Black families with white families influences the Black-and-white estimate $\hat{\beta}_1$, and also highlights the importance of the between-race component $\theta^b\hat{\beta}_1^b$.

III. Data

To test how relative mobility estimates change when accounting for race and measurement error, I need linked data that include Black families and have multiple father observations. I use links from the Census Linking Project, which were generated using an automated matching algorithm (Abramitzky et al. 2020). Links can be created across 1850-1940 United States Censuses since they are deanonymized 72 years after enumeration, making it possible to link individuals by first name, last name, year of birth and birthplace. Links to and from the 1890 Census are not included since the original manuscripts were destroyed in a fire. These links are attached to the full-count census from IPUMS to obtain information on parent-child relationships, occupation, region, age and place of birth (Ruggles et al. 2020).

For the main results, I use a conservatively linked sample that reduces the probability of false positives (Bailey et al. 2020). The conservative method uses matches based on *exact* first and last name string rather than matches created after standardizing names with an algorithm (e.g., NYSIIS). Further, I use matches where first name, last name, and birthplace combinations are unique within plus or minus 2 years, which reduces the number of false matches.

I aim to build an intergenerational dataset where the father is observed multiple times. To do this, I first create a "standard" historical dataset where the father and son are observed once. This is done by linking 0-14-year-old sons observed with their fathers forward to their adult outcome 20-, 30-, or 40 years later. I then limit the sons to be between 25 and 55 years old, to capture the prime part of the lifecycle.[13] The linking process is separately done by starting census (e.g., linking children in the 1850 Census to the 1880 Census); after creating these intergenerational linked datasets, the final dataset pools each intergenerational dataset between the 1850-1940 period.

Importantly, I include both Black and white sons in the linked data. However, I do not include non-Black minorities (i.e., Asians and American Indians) since the accuracy of the linking algorithm for these groups is unclear. Nevertheless, I will later explicitly show that including other

[13] While the wider age range may raise concern about nonclassical error in the son's outcome due to lifecycle bias (Haider and Solon 2006), I do not find substantially different estimates by the son's age (see Appendix Figure A1). This result is consistent with Feigenbaum (2015) and suggests that nonclassical error in the son's outcome is not as problematic for historical data.

groups do not matter much for the father-son estimates since they are a small share of the overall population (less than 0.1 percent). Hispanics are included in the estimates since they were mostly recorded as white.[14]

Most Black children cannot be linked forward from the 1850 or 1860 Censuses since they were enslaved. Yet excluding enslaved children discounts the most important institutional change in American history: emancipation. While emancipation caused significant absolute mobility, the impact on relative mobility is unclear since the Black population remained at the bottom of the distribution. To uncover the importance of this group, I append Southern-born Black adults from the 1870 and 1880 censuses to the linked data set. I assume that their fathers were enslaved and assign the slave "occupation" the lowest status level on the 0-100 scale, which I explain later.

The next step in the data creation process is to link the fathers to a second and sometimes third observation. These links are taken from censuses ten years later or earlier. I use father observations that are between 25 and 55 years old and have a reported occupation. The preferred results will be for a dataset with two father observations and one son observation (a "double-linked" dataset). After these restrictions, the double-linked sample contains nearly 2.5 million father-son pairs. While almost all estimates in this paper are from the double-linked sample, it will also be useful to show how measurement error affects results when averaging up to three father observations. For these results, I will use the subset of the double-linked dataset where a third father observation is observed (a "triple-linked" dataset). The "first" occupation used in the data is the one where the father is observed with his child. Sometimes children are linked to multiple later censuses. In these cases, I use the one closest to age 40 as the dependent outcome.

The benefits of these data come with the cost of having a select sample. While the sample is large, it is only 3-6 percent of the possible children to link, depending on the birth cohort. Linking rates are low because of transcription error and common names: for example, Abramitzky et al. (2021) link two transcriptions of the 1940 Census to each other and find a linking rate of about 50 percent, which suggests that there is an upper bound on match rates. The linking rate is even lower for Black sons (1 percent), perhaps because African Americans had fewer unique surnames, higher mortality rates, or greater enumerator error.

[14] The 1930 census separately categorized "Mexican" as a race, but I allow for Mexican-white or white-Mexican transitions in the data.

A low linking rate is only problematic if the sample is unrepresentative of the underlying population. To address selection into the sample, I reweight it to match the adult son population's characteristics on race, age, occupation category (white collar, farmer, semi-skilled, unskilled), region of residence, urban location, and whether one is an internal migrant (See Appendix B). These inverse propensity weights are created separately for each son's adult year of observation; for example, if the son's outcome is observed in 1870, then I weight his characteristics to the 1870 Census (Bailey et al. 2020).[15] The most important part of this process is that Black sons are given three times the weight of white sons, such that the Black share of the linked data reflects the population's share. Despite weighting, the sample may still be unrepresentative on unobservables, which could bias estimates toward greater or lower mobility. For example, if children from poorer backgrounds who remain poor in adulthood are less likely to be linked due to name misspellings, then I would overestimate relative mobility. This issue is presumably exacerbated when linking multiple times. However, linking multiple times does not strongly influence mobility estimates. If I estimate the same regression of the son's status on a single father's status in a single-linked, double-linked or triple-linked sample, I find a similar trend over time (Appendix Figure A2).

Measuring status

The modern-day economics literature estimates income mobility, but income is unavailable in historical censuses. Instead, studies often impute income or earnings, such as with the 1950 occupational income variable from IPUMS (Olivetti and Paserman 2015) or with wage information from the 1940 Census (Abramitzky et al. 2021, Collins and Wanamaker 2022). However, applying mid-20th century earnings estimates to mid-19th century data is controversial, especially for farmers (Long and Ferrie 2013, Xie and Killewald 2013). Song et al. (2020) address this problem with a status measure that is based on literacy/education by occupation, information that is available in each census back to 1850.[16] Using auxiliary data from 1850-2019, occupations in a given birth cohort are percentile ranked based on their average human capital level, which results in a 0-100 score that is merged into the linked sample.[17] Since occupations are ranked by

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[15] Since I weight with respect to the son's *adult* outcome, the sample is representative of children who survived to adulthood. Therefore, the sample is representative of sons who survived, for example, the Civil War or World War I.
[16] The occupations in the Song score are 70 "microclass" occupations, which are more consistent over time, rather than the 3-digit codes for the 1940 score.
[17] The auxiliary samples are full-count data between 1850 and 1940, and the samples available from IPUMS for post 1940 data.

birth cohort, the status of an occupation can change over time. For brevity, I refer to this measure as the "Song score" or "occupation-only status."

The Song score is valuable since it captures time-varying changes to relative status; however, it discounts key inequalities within occupation across race and region (Collins and Wanamaker 2022). Ignoring these disparities understates inequality, which in turn affects intergenerational mobility estimates (Saavedra and Twinam 2020). Since racial and regional inequality was high in American history, instead of using the Song score, I use what I term the "adjusted Song score." My adjustment is to percentile rank an occupation, race, and region's literacy rate/educational level. This change addresses, for example, that the literacy rate for farmers born in the 1850s varied from 96 percent for white farmers in the North, 85 percent for white farmers in the South, and 44 percent for Black farmers in the South. See Appendix C for more details on the Adjusted Song score. In addition to showing the trend for these human-capital-based measures, I will also estimate the trend in mobility based on imputed earnings from the 1940 census (similar to Collins and Wanamaker 2022).[18] While caution should be applied to interpreting these estimates in the 19th century, they are similar in magnitude to the preferred measures, the adjusted Song score.

The descriptive statistics of the main sample are presented in Table 1. To be consistent with Song et al. (2020), statistics are presented by decadal birth cohort rounded to the nearest year (e.g., the 1880 birth cohort covers 1875-1884 births). Importantly, after weighting, the Black share of the sample reflects the population.[19] Another important pattern in Table 1 is the decline of farming. The share of sons that were farmers fell from 39.1 percent for the 1840 cohort to 10.5 percent in 1910. Since agriculture was a key sector of the historical economy, I will later explore whether historical mobility trends are driven by the share of the population in farming.

Fathers are more likely to be farmers than sons, even when holding the time period fixed. For example, 29.2 percent of the 1870 birth cohort are farmers as adults in 1900 or 1910. However,

[18] I use the 1940 earnings score described in Appendix C of Kosack and Ward (2020), which follows Collins and Wanamaker (2022). This score is primarily based on the average wage income for wage workers by occupation, race and region. However, the 1940 census does not include self-employed income. Self-employed income is imputed using information from the 1960 Census, where the key assumption is that the ratio of total earnings across self-employed and wage workers by occupation is the same in 1940 as in 1960. Further, farm laborer and farmer income are increased to account for perquisites using information on perquisites from the USDA.

[19] The Black share drops over time because of an increase in the number of children of white immigrants.

almost half of the fathers of children born around 1900 and 1910 are farmers. One reason why fathers are more likely to be farmers than sons is that farmers had more children than other occupations.[20] Since sons are not required to have a child of their own to be in the sample, the son's generation is not similarly tilted towards farmers. The fact that farmers had more children is not necessarily problematic for estimates, but one should note that since each father-son link is a separate observation in the dataset, larger families contribute more to estimates than smaller families. Instead of giving equal weight to each son, one can instead adjust weights to reflect the inverse of family size. This method produces similar mobility estimates, so the main estimates do not adjust for family size (Appendix Figure A5).

Due to the destruction of the 1890 microdata in a fire, there is a change in sample characteristics between the 1880 and 1890 birth cohorts. For instance, the son's average age increases from 34 in the 1880 cohort to 44 in the 1890 cohort.[21] Given this difference in average age, one may be concerned that lifecycle effects will bias the trend in mobility (Haider and Solon 2006). However, lifecycle bias is not as important for historical occupational data compared to modern-day income data (Appendix Figure A1). While there is some uncertainty about the trend for these birth cohorts, there is suggestive evidence that missing a census does not strongly bias estimates. If one does a placebo "burning" of the 1860 or 1870 Censuses, then mobility estimates do not change strongly for the 1860 cohort (Appendix Figure A6).

Finally, parent-child associations may not be linear due to extremely high or low-income individuals (Chetty et al. 2014). Appendix Figure A7 shows that the father-son association of

[20] See Appendix Figure A3 Panel A for the trend in farmers for the overall population and for the father population. Another reason why fathers are more likely to be farmers is that farmers are more likely to be linked to a second father observation than other occupations. This bias is not fully addressed by weights because the weights are based on the son's *adult* observables and not the father's. One could alternatively weight the data based on the father's observables to address the bias, but this method causes the sons to be more farmer heavy than the underlying population (Online Appendix Figure A3, Panel B). Ideally, one would weight simultaneously to match the father's and son's adult distributions, but this information in not available in a representative cross section. Nevertheless, I also create weights based on the father's observables and show that results are similar when weighting for the son's adult observables (Figure A4).

[21] A given birth cohort could be observed with fathers in two different censuses. For example, the 1880 birth cohort (rounded from 1875-1884) could be observed in the 1880 Census as young children (0-5), or in the 1890 Census as old children (6-14). Missing the 1890 Census causes the 1880 cohort to only be young children, as observed in the 1880 Census. It also causes the 1890 cohort to only be older children, as observed in the 1900 Census. This causes the 1890 cohort to be older than average, and the 1880 cohort to be younger than average. Missing the 1890 Census also increases the average age of sons for the 1860 cohort since they are mostly linked 40 years later to 1900 rather than 30 years later to 1890.

scores is approximately linear in the past. Note that since the status measure is primarily based on occupation, extremely high or low incomes are unobserved.

IV. Measurement error attenuates estimates of intergenerational persistence

Measures of occupational instability for the father

In this section, I show that measurement error biases mobility estimates. For now, I focus on white families and thus estimate the within-group association for the white population (β_1^{white}). However, the following patterns hold qualitatively if one pools Black and white families.

Before showing the influence of measurement error on mobility estimates, the first pattern of note in the data is that the father's occupation was unstable across observations. Figure 2A plots the fraction of white fathers who held the same 3-digit occupation by the son's birth cohort.[22] Only 54-62 percent report the same occupation in the second census. One possibility is that false links in the historical data drive this result, but other high-quality data confirm the same pattern. The Civil War Veterans' Children Census data, which was linked by genealogists, suggest that only 44 percent of 25-55-year-olds held the same 3-digit occupation in 1910 and 1920.[23]

Instability in occupation does not imply instability in status, but status was also weakly correlated across censuses.[24] Figure 2B plots the association after regressing the father's first score on the second one. The associations are far from one (0.62 to 0.67), which suggests that one snapshot does not accurately capture the father's long-run status. These occupational switches could be real or falsely reported, but either would cause error when estimating relative mobility. Note that these father-to-father associations in Figure 2B will eventually be the first stage of an instrumental variables strategy (IV) that aims to eliminate measurement error, where the first father observation is instrumented with the second one. Since these father-to-father associations are less

[22] Age fixed effects are controlled for to account for lifecycle variation.

[23] The Veterans' Children's Census data was accessed at uadata.org (Costa et al. 2019). Author's calculation based on occ1950 codes for those with an occupational response in both 1910 and 1920.

[24] It is possible that income was stable while occupations changed across censuses. Income is unobserved in historical data, but one can test this pattern in the PSID. The rank-rank correlation of status (imputed by occupation/race/region) across PSID observations approximately ten years apart was 0.81. The same correlation for labor income is 0.63. For those who switched occupations, the rank-rank correlation of status was 0.60 and the correlation of labor income was 0.54. These results suggest that incomes changed when occupations changed.

than one, then Figure 2B gives the first indication that IV estimates of the father-son association will be higher than traditional OLS estimates.

Figure 2B shows that instability is greater for non-farmers than for farmers. While the share of fathers with agreeing occupations is 0.54-0.62 for the whole sample, dropping farmers causes the share to fall to 0.47-0.48. If one categorizes occupations into four broad groups (farmer, unskilled, semi-skilled and white collar), then farming was the most stable category (70-80 percent in the same category), while the unskilled category was the least stable (20-30 percent) (Appendix Figure A8). Semi-skilled and white-collar occupations were more consistently reported, with 40-55 percent of fathers belonging to the same category. However, it is unclear whether these differences across categories reflect true differences or whether non-farming occupations were more difficult to consistently categorize.

Time-averaged estimates of relative mobility

The standard way to address measurement error is to average multiple father observations to better proxy for his status. Since it will be informative to show how estimates change when averaging up to three father observations, all of the estimates in this section will be for a subset of the data where I can link fathers to a third observation ("triple-linked dataset").[25] However, these estimates will not be the primary trend estimates since I do not have enough observations for the 1880 or 1890 cohorts due to the missing 1890 microdata. Similarly, the 1840 cohort does not have enough observations since pre-1850 censuses were not enumerated individually.

Figure 3A shows the trend in the father-son association β_1^{white} when using the typical specification in the literature: regressing the son's outcome on *one* father observation. For the adjusted Song score, going from one observation to an average of two father observations increases the father-son association (or "persistence" estimate) by 14-19 percent. These results are exactly as expected given measurement error in the father's outcome. Indeed, Figure 3A is the historical version of Solon's (1992) result that measurement error in income biases income mobility estimates. If there were no error, then the father-son associations should not have changed.

If one goes further and averages three father observations, then persistence estimates are 23-29 percent higher than the one observation estimate. Since it is commonly thought that

[25] Appendix Figure A2 shows that mobility estimates for the triple-linked data are similar to the double-linked data.

transitory fluctuations in occupation are not strong, the further increase suggests that error is due to data quality issues, such as from reporting, enumeration, or digitization. Later I will argue that data error partially explains why measurement error exists.

If one instead uses the more traditional occupation-only measure of status (unadjusted Song score), then persistence estimates increase by a larger amount than the adjusted score (29-67 percent, Figure 3B). The greater increase suggests that error in occupation-only status is greater than for adjusted scores. This could be because additional information besides occupation help to pinpoint one's location in the economic distribution.

How one measures economic status also matters for the trend of mobility. The adjusted score, which allows for regional differences within occupation, finds that the father-son association fell and then increased between 1850 and 1910 birth cohorts (Figure 3A). This pattern contrasts with the others who find that persistence increased over time when using occupation-only status (Olivetti and Paserman 2015, Song et al. 2020). When I use occupation-only status, I also find increased persistence over time (Figure 3B).[26] The difference in mobility estimates across status measures partially reflects that regional differences in economic development were large in the past. Since those born in poorer regions (i.e., the South) tended to stay in poorer regions, persistence estimates can increase when accounting for within-occupation gaps across region (Mitchener and McLean 1999). For the rest of the paper, my preferred estimates are for the adjusted score. However, I will also show results for occupation-only status which is commonly used in the literature.

Estimating father-son mobility based on classical measurement error or instrumental variables

Averaging three father observations may still not perfectly capture his permanent status. Under the assumption of classical measurement error, it is possible to project the "true" father-son association after eliminating noise. Before doing this projection, I can test whether the assumption is reasonable by comparing the *actual* three-father association (i.e., an average of three father observations) to the *predicted* three-father association under classical measurement error. Based on how the association changes from one to two father observations, the predicted three-father

[26] I find a stronger increase in the father-son association across 1840-1910 birth cohorts than Song et al. (2020). The difference appears to be due to the linking method. See Appendix D for a detailed examination of the difference in estimates across studies.

association under classical measurement is surprisingly similar to the actual three-father association (see Figure 4A).[27] The difference between the predicted and actual associations is 1-3 percent, where the classical error assumption slightly understates the actual persistence. Therefore, while the classical measurement error assumption is simplistic, it is consistent with patterns in the data.

In addition, I can also test whether error in the *son's* status matters for estimates. For a subsample of the data, I observe multiple son occupations. For example, if a son was successfully linked forward from childhood 20 and 30 years later. If one averages the son's status across different occupations, then the father-son association changes by up to 4 percent (See Appendix Figure A9). This result suggests that nonclassical error is not as important for historical mobility estimates, in contrast to modern-day estimates (Nybom and Stuhler 2017).

Since the classical error assumption is consistent with patterns in the data, I can use it to eliminate error and predict the "true" father-son association. Based on this assumption, the predicted "true" father-son association is 31-49 percent higher than the typical estimate using one father observation.[28] These estimates imply that 52-72 percent of initial economic gaps across white families persisted to the next generation, which paints American history as highly immobile rather than highly mobile.

Rather than using the classical measurement error formula, one could instead use instrumental variables to estimate father-son association. This method instruments one father observation with another one under the assumption that the transitory components are not correlated across observations (Altonji and Dunn 1991, Modalsli and Vosters 2019). If one takes this approach and instruments the first father observation with the second father observation, then the estimated father-son associations are similar to the predictions after fixing classical

[27] Based on Equation (3), I estimate $\hat{\beta}_{three\ obs} = \left[\left(3\hat{\beta}_{one\ obs} \times \hat{\beta}_{two\ obs}\right) \big/ \left(4\hat{\beta}_{one\ obs} - \hat{\beta}_{two\ obs}\right) \right]$. See Appendix E for derivation.

[28] Based on Equation (3), I estimate $\hat{\beta}_{"true"} = \left[\left(\hat{\beta}_{one\ obs} \times \hat{\beta}_{two\ obs}\right) \big/ \left(2\hat{\beta}_{one\ obs} - \hat{\beta}_{two\ obs}\right) \right]$. See Appendix E for derivation.

measurement error (Figure 4B). Given the similarity of estimates across methods, I will use IV estimates as the main ones for the rest of the paper.[29]

One may be concerned that an IV estimate upward biases father-son associations due to nonclassical error when incorrectly linking individuals across censuses. To understand how false positives bias IV estimates, first recall that the IV estimate is a ratio of the reduced form and the first stage ($\beta_{IV} = \frac{\delta_{RF}}{\pi_{FS}}$). The reduced form is the son's outcome regressed on the father's outcome from the *second* observation, and the first stage is the father's first outcome regressed on the second outcome. If this first stage between father observations is attenuated due to incorrect links, then the IV estimate will be upward biased. However, upward bias from false positives in the first stage is counteracted by downward bias in the reduced form, also from false positives. Thus, the overall bias to the IV estimate depends on whether the reduced form or the first stage is attenuated by more (See Appendix G for a more formal discussion). Note that classical measurement error in the father's second observation attenuates both the first stage and reduced form by the exact same amount, which is why IV fixes classical measurement error.[30] However, it is unclear whether linking error biases the reduced form or first stage more. A natural expectation is that the reduced form is attenuated by more since it is based on two links (the son's link from childhood to adulthood, and the father's link to a second observation), while the first stage is based on only one link (father's link to a second observation). If false positives are more likely to occur for the reduced form, then the IV estimate will be downward biased.

It is possible to test for bias from linking error by comparing IV estimates across datasets with more incorrect links and fewer incorrect links. Fortunately, the Census Linking Project includes multiple linking methods, some of which are less precise than the preferred algorithm for the main results. When using IV and after weighting, estimates from less precise (or less conservative) algorithms are lower by 0-3 percent relative to more precise algorithms.[31] Thus,

[29] It is possible to test how IV performs when estimating the IGE for income mobility in the PSID. Appendix Figure A10 compares estimates of the IGE when using long-run averages of the father's labor income (up to 15 observations) to using an IV estimate (where the instrument is labor income roughly 10 years apart from the first observation). The time-averaged estimate is about 0.51 after averaging five years of labor income; the IV estimate is similar at 0.50.

[30] Both the first stage and reduced form use the second father's observation as the regressor. Thus, the reliability ratio will be the same in the first stage and reduced form.

[31] See Appendix Figure G1. If one instead uses the classical measurement error formula for algorithms that are less conservative, then the predicted "true" father-son association based on the measurement error formula and the IV method are similar (Figure G4).

linking error does not appear to upward bias IV estimates, but instead slightly biases it downward. However, the downward bias from incorrect links is smaller when using IV than when using OLS (see Appendix G), suggesting that the IV method partially but not completely addresses attenuation bias from false positives (Bailey et al. 2020).

Measurement error exists not only for the scores used in this paper, but for any score that infers status by occupation. For example, if one imputes earnings by occupation/region using data mostly from wage workers in the 1940 Census (similar to Collins and Wanamaker 2022), then persistence estimates increase by 48-59 percent after accounting for error (Appendix Figure A12). Similarly, if one uses the 1950 log occupational income from IPUMS, the father-son association increases by 49-85 percent. In Appendix E, I further show that measurement error is also important when estimating father-son correlations instead of associations.

V. Why is measurement error so strong? Evidence from 1880 St. Louis

One way to interpret the evidence so far is that while *intergenerational* mobility was low, *intragenerational* mobility was high due to occupational switches throughout the lifecycle. On the other hand, a weak association across father occupations could simply be due to data error, like that found in the PSID and CPS (Kambourov and Manovskii 2008, Moscarini and Thomsson 2007).

One way to check the validity of the census data is to compare results from two different sources that report the same occupation. While such data is uncommon in the past, I can exploit a unique event in 1880: the re-enumeration of St. Louis. Allegations of an undercount led to a recount five months after the initial June enumeration, with both using June 1st as a reference date (Goeken et al. 2017). Fortunately, the recount includes occupations, which is not true for other recounts (such as in 1870 New York and Philadelphia). Therefore, a comparison of the two occupation reports should indicate the extent of measurement error from occupational reports. However, this is not a foolproof test. One limitation is that some people moved away from St. Louis between June and November. In this case, a neighbor would report the missing information, which may lead to higher error than otherwise expected. To address this problem, I focus on measurement error for those less geographically mobile, which I take as those 30 years and older.

After linking the two enumerations together, focusing on 30- to 60-year-old males, and weighting the data to be representative, I find that occupations are highly inconsistent across enumerations (see Appendix G for linking details).[32] Table 2 shows that only 65 percent had an agreeing 3-digit occupation code. The one-digit codes are not much better: only 69 percent agreed. If one assigns the adjusted Song score to each report, then the correlation across enumerations is 0.79 – far less than one. The correlation of the unadjusted Song score is lower at 0.67. The weak correlations in the St. Louis data suggest that part of the low associations I estimated across father observations was due to inconsistent recording of occupations.[33]

One reason why disagreements occurred was that the reports were more detailed in one observation than in the other observation.[34] The most common disagreement was for "laborer, not elsewhere classified", where only 60 percent matched across enumerations. For these occupations, the second enumeration recorded a more specific occupation, such as teamster, porter or boatman (see Appendix Table G2). While laborer was the most common disagreement, this is because laborer was one of the most common occupations. The lowest *rate* of agreement was for the "foreman" occupation (17 percent); instead of foreman, many were recorded as "Operative and kindred worker, not elsewhere classified." The few farmers in the sample were sometimes miscoded: one was a "milkman" in the second enumeration, which placed him in the "Deliverymen" category; another was in the "pork business," which placed him in the "Managers, not elsewhere classified" category. While these results may suggest that one should not use detailed occupations (3-digit level) when measuring status, there was still an occupational mismatch for highly aggregated categories. For example, only 80 percent of the data were the same broad occupation category (white-collar, farmer, semi-skilled and semi-skilled) across enumerations.

To quantify the importance of coding error relative to transitory shocks, one can compare the status correlations observed in the same year (1880) to those observed across ten years (1870-

[32] The IPUMS variable occ1950 is not available in the Ancestry data. To create this code, I assign the most common occ1950 code for a given occupation string. I first look for the string in the 1880 full count data. If no code is found in 1880, then I search for the most common code per string in 1870, then 1860 and then 1900. I only keep those with a found occupational code.

[33] It is possible that a correlation less than one is due to false positives within the data, but I find the same estimates if I limit the sample to those with matching parental birthplace.

[34] Error could occur due to assigning the same occupation string to different codes, but an examination of the occupation strings (*occstr*) suggests there was almost no variation of occupational codes (*occ1950*) within string.

1880). For example, if 1880 St. Louis residents linked to the 1870 Census also had a 0.79 correlation across observations, then inconsistent recording would entirely explain why occupations disagreed across censuses. After linking 1880 St. Louis residents back to 1870, the correlation of status is 0.56. Based on this number, a back-of-the-envelope calculation suggests that one-third of the error in status is due to data error, while two-thirds is due to true occupational shifts.[35] These results suggest that both miscoding and transitory shocks are important in the past; however, it is unclear how evidence from 1880 St. Louis applies to more rural areas or other censuses.

Given that there is error in occupations, one may wonder whether other variables in the census contain error, which would similarly bias historical studies or causes issues with linking. Price et al. (2021) use genealogically linked data between the 1900, 1910, and 1920 Censuses and find that many other variables are stable across censuses. For instance, race agreed 99 percent of the time, birthplace 98 percent of the time, and birth year (within plus/minus 3 years) 97 percent of the time. Therefore, it appears that occupation was more likely to contain error, which is consistent with modern-day studies that document significant error in occupation codes (Moscarini and Thommson 2007, Kambourov and Monvskii 2008).

VI. Estimating mobility when accounting for racial persistence

So far, I have only estimated mobility for white males ($\hat{\beta}_1^{white}$), which is just one piece of the within-between decomposition $\hat{\beta}_1 = \theta^{white}\hat{\beta}_1^{white} + \theta^{Black}\hat{\beta}_1^{Black} + \theta^{b}\hat{\beta}_1^{b}$. In this section, I estimate mobility for the pooled set of Black and white males $(\hat{\beta}_1)$. Therefore, estimates change because within-Black persistence ($\hat{\beta}_1^{Black}$) may differ from white persistence; moreover, between-race persistence ($\hat{\beta}_1^{b}$) may be strong. At the end of this section, I will discuss the importance of other groups missing from the data, such as females and non-Black minorities.

Figure 5A shows that the Black and white father-son association is 11-46 percent higher than the white father-son association. Now the maximum estimate is 0.84, a 15 percent increase

[35] Let one's measured status at time *t* be $y_{i,t} = y_i^* + u_{i,t} + v_{i,t}$, where y_i^* is one's permanent status, $u_{i,t}$ is data error, and $v_{i,t}$ is error from a transitory shock. Under the assumption that the error components are independent of each other and permanent status, then the correlation of two status measures is $corr(y_{i,t}, y_{i,s}) = \frac{\sigma_{y^*}^2}{\sigma_{y^*}^2+\sigma_u^2+\sigma_v^2}$. Let $\sigma_{y^*}^2$ be standardized to one. Based on this, $(\sigma_u^2/(\sigma_u^2+\sigma_v^2)) = 0.327$ after letting $\frac{\sigma_{y^*}^2}{\sigma_{y^*}^2+\sigma_u^2} = 0.793$ and $\frac{\sigma_{y^*}^2}{\sigma_{y^*}^2+\sigma_u^2+\sigma_v^2} = 0.556$.

from the white association of 0.73 for the 1840 birth cohort. The largest increase is for the 1880 birth cohorts (from 0.42 to 0.62). The reason why the increase is bigger for the 1880 cohort is that the between-race effect (product of the between-race association and share of variation) remained high while the white association fell.[36] For instance, the between-race elasticity was 0.93 (see Appendix Figure A14), and the between-race share of variation was 0.34 (see Figure 5B). While adding Black families increases the father-son association, the trend is similar between 1840 and 1910 where mobility improved between the 1840 and 1880 cohorts, then decreased to less mobility between 1890 and 1910.

The increase in the father-son association is not because of an especially high Black association $\hat{\beta}_1^{Black}$. The decomposition reveals that $\hat{\beta}_1^{Black}$ contributes little to the pooled association since the within-Black share of overall variation (θ^{Black}) is often less than one percent (see Figure 5B). A low share of variation is surprising since Black sons are 10-15 percent of the weighted sample, but it reflects that Black fathers were concentrated in relatively few occupations. The low contribution of the within-Black association reduces concern that a poorly linked sample strongly biases results. For example, Appendix Figure A13 shows that estimates of $\hat{\beta}_1^{Black}$ can be quite high, which is mostly due to widening disparities between the South and non-South Black population. However, bias from linking does not appear to be strongly affect inference. When holding the specification fixed, Appendix Figure A16 shows that Black mobility estimates based on a double- or triple-linked sample are often statistically indistinct from estimates based on a single-linked sample. While estimates of Black mobility may be are biased due to linking error or an unrepresentative sample, the low share of within-Black variation suggests that even a perfectly linked dataset that is fully representative would not move population estimates much. An area for future research is to create Black-specific linking algorithms that improve on current estimates of within-Black mobility.

Instead of a within-race effect, a large between-race effect explains the increase in the father-son association when pooling Black families. A large between-race effect exists for two

[36] One concern is that the sharp 22 percent drop of the father-son association from 0.54 to 0.42 between 1870 and 1880 birth cohorts is due to a sample composition issue from missing the 1890 Census. While this may be the case, it appears that the between 1870 and 1880 is primarily due to regional convergence in outcomes. If one instead estimates the trend in mobility within region, then there was not a sharp fall for this cohort (Figure A11). Alternatively, there is only a 6 percent drop between 1870 and 1880 cohorts when using occupation-only status (Figure 8B), in contrast to the 22 percent drop when adjusting for regional differences within occupation.

reasons. First, the between-race share of variation was high, peaking at 0.53 for the 1840 cohort, when the bulk of Black fathers were enslaved, and eventually settling at 0.29 for the 1910 cohort as the Black-white gap fell. This between-race variation would not matter if Black-white gaps converged completely (i.e., $\hat{\beta}_1^b = 0$). However, the between-race association was large, between 0.87 and 1.00 (Appendix Figure A14). Since both the between-race share of variation θ^b and between-race persistence $\hat{\beta}_1^b$ were high, about 30 percent of the historical Black and white association is due to a between-race effect, with the rest coming from a within-race effect.

If one uses occupation-only measures of status, then pooling Black families with white families has a smaller influence on estimates (Appendix Figure A15). The reason is simple: since racial inequality is understated in occupation-only measures, the between-race share of variation θ^b is much smaller. After emancipation, the between-race variation for the 1870-1910 cohorts is 0.04-0.14, in comparison to 0.29-0.37 for the adjusted score.[37]

Comparison to other methods of estimating mobility

Altham Statistic. In Appendix I, I gauge the importance of measurement error and racial disparities for the Altham statistic (Altham and Ferrie 2007, Ferrie 2005). The advantage of the Altham statistic is that one does not have to impute earnings (Long and Ferrie 2013b). However, it is unclear what one's "true" occupational category is when multiple father occupations are reported. Despite this ambiguity, I show in Appendix I that the association between the son's occupation category and the father's category is strengthened after averaging multiple father observations. Therefore, measures of occupational mobility (without imputing earnings) are also influenced by measurement error.

Absolute Mobility. This paper focuses on relative mobility rather than absolute mobility, or whether the child has a weakly better outcome than the father (Chetty et al. 2017). Loosely, absolute mobility captures the "American Dream," while relative mobility captures "equality of opportunity." However, due to the lack of income data, measuring absolute mobility is difficult in

[37] The occupational-only score does capture a large amount of between-race variation for the 1840 and 1850 cohorts since most Black fathers held the slave "occupation" and were at the bottom of distribution. For these cohorts, the estimate suggests that there was rapid Black-white convergence after emancipation (Figure A14). This occurs since the occupation-only score places the average son of the enslaved at a high percentile (29th on average). In contrast, the adjusted score places Black sons at a lower part of the distribution (6th), which more accurately reflects racial gaps in postbellum America.

historical data. The preferred score in this paper abstracts from absolute gains since it is based on a percentile ranking. While I do not have income data, in Appendix L, I create an absolute mobility measure based on the son being in an occupation, race, and region cell with a higher human capital level than the father. While this measure is rough (e.g., it does not capture a decline in absolute mobility during the Great Depression or for recent decades as in Chetty et al. (2017)), measurement error appears to be less important than for relative mobility estimates. Further, a white-only sample misses *greater* absolute mobility for Black families, such that white-only samples understate absolute mobility. Ultimately, more research is needed to better measure absolute mobility in historical data. See Appendix L for more discussion.

Name-based Methods. The directly linked estimates in this paper contrast with estimates from data indirectly linked using first names or last names (Olivetti and Paserman 2015, Clark 2014). Name-based estimates are created by (1) taking a cross section of children in year t, (2) averaging parental status by the first name or last name of the child, (3) finding adults of the same first name or last name in a later cross section (e.g., $t+20$), and then (4) proxying parental status with the name-based average. Since averaging is involved, name-based estimators may reduce measurement error (Clark 2014); however, name-based estimates may differ from directly linked estimates for a variety of reasons. For instance, names may capture environmental effects if names are place specific. More mechanically, if one uses samples instead of full-count data to average the parent's outcome, then name-based estimates can be attenuated. For example, if children in the first sample (e.g., 1 percent sample of the 1900 Census) are different from the adults in the second sample (e.g., 1 percent sample of the 1920 Census), then parental status will be measured with error since the wrong parents are used (Stuhler and Santavirta 2020). Besides these issues, another potential reason why first name estimates from Olivetti and Paserman (2015) differ from my estimates is that they do not include Black families.

After updating name-based estimates to use full-count data and include Black families, I find that name-based estimates are similar in level and trend to directly linked estimates (see Figure 6A). All methods find that the father-son association fell between the 1840 and 1910 birth cohorts. The directly linked method estimates a fall from 0.84 for 1840 birth cohorts to 0.69 for 1910, which is surprisingly close to the trend for the surname estimates (0.82 to 0.69). The first-name estimates find a smaller fall from 0.89 to 0.82. The higher level and slower downward trend for the first-name method may be due to additional informational content of first names (Olivetti and Paserman 2015, Santavirta and Stuhler 2020). Nevertheless, this evidence is consistent with improving mobility over time. See Appendix J for more detail.

Accounting for groups missing from the data

Females. The key advantage of the first-name method is that it produces estimates of female mobility since first names, unlike last names, do not change between childhood and adulthood. One issue with measuring female mobility in historical data is that most females do not have a reported occupation.[38] Therefore, a standard method is to proxy the daughter's status with her husband's status (Olivetti and Paserman 2015, Craig et al. 2019). In Figure 6B, I show that adding females to the male sample does not substantially change name-based estimates. On average, estimates increase by 1 to 4 percent. Note that there is little to no between-group effect when adding females, unlike when adding Black families to a white sample, since the fathers of daughters and sons have almost the same average status. Overall, it appears that an ideal dataset that included female links would not find a substantially different trend of historical mobility.

Other racial minorities. One issue with the data is that other racial minorities (i.e., American Indians and Asians) are not included in the data since it is unclear how well they are linked across censuses. The within-between decomposition $\hat{\beta}_1 = \sum_{g=1}^{G} \theta^g \hat{\beta}_1^g + \theta^b \hat{\beta}_1^b$ can be used to check whether this exclusion matters for the overall father-son association. Since I do not have linked data for other racial minorities, one can turn to repeated cross-sections to fill out missing parts of the decomposition. For instance, between-race persistence $\hat{\beta}_1^b$ can be measured across censuses under the assumption that generations are 30 years apart (Borjas 1994, Card et al. 1999). The share of within-race variation (θ^g) and between-race variation (θ^b) can also be directly measured in the census. The missing piece of the decomposition (within-group mobility $\hat{\beta}_1^{Asian}$ or $\hat{\beta}_1^{American\ Indian}$) is unobserved in cross-sectional data, but one can gauge the sensitivity of overall mobility $\hat{\beta}_1$ to different levels of within-race mobility, such as Asian mobility being equal to white mobility. Based on this indirect method, the father-son association is expected to move by less

[38] The modern-day literature uses family income instead of father income (Chadwick and Solon 1999), but this is complicated by the fact that female work in historical data is underreported (Goldin 1990).

than 0.003 when pooling in other racial minorities (Appendix Figure A17, Panel A).[39] The reason why other racial minorities are relatively unimportant for overall mobility estimates is that the share of variation from these other groups was too small.

VII. Reevaluating the long-run trend in relative mobility

Data details for estimating the mobility trend.

So far, I have shown with 1850-1940 Census data that father-son associations increase after accounting for race and measurement error. This revision suggests that the long-run trend in relative mobility between the 19th century and today should be revised. However, it is unclear whether the issues of race and measurement error are similarly important for modern-day estimates. In this section, I extend the trend to the 1960-1980 birth cohorts (rounded to the nearest decade) and show that modern-day relative mobility is higher than historical mobility.

To push the data forward to 1980 birth cohorts, I use 1968-2019 data from the Panel Study of Income Dynamics (PSID). To mimic the linked historical data, I include white and Black fathers who have two occupation observations that are ten years apart. If there are no observations that are ten years apart, then I search for those that are nine years apart, and so forth until a minimum distance of three years. The father's and son's occupations are both observed at the 3-digit level, which matches the detail of the earlier census data.[40]

There is some concern that selective attrition in the PSID understates intergenerational persistence for later birth cohorts (Schoeni and Wiemers 2015). However, income mobility estimates from the PSID are similar to estimates from tax data (Mazumder 2016). Nevertheless, I create custom weights for the PSID using the inverse propensity method used for the historical linked data (see Appendix K for more detail). Other surveys used to estimate intergenerational mobility, such as the General Social Survey or NLSY, do not contain multiple father occupation

[39] This method can also be used to test the robustness of pooling Black sons with white sons in the data. Specifically, I can measure the share of variation from within-Black families and also the rate of between-race persistence using censuses 30 years apart. Appendix Figure A17, Panel A shows that this method produces similar results as the linked data: adding Black families to the white sample substantially increases persistence estimates. Figure A17, Panel B shows that various estimates of within-Black mobility also do not change the population estimate.

[40] I use the 3-digit occupation codes for the father from the Retrospective Occupation-Industry File. The Retrospective Occupation-Industry File recodes the 1-digit occupations in the original dataset to the 3-digit level after going back through the original interviewer files. Kambourov and Manovskii (2008) argue that this retrospective coding of occupations has less measurement error than the original data.

observations, but typically a recall of the father's occupation (Song et al. 2020). Since multiple occupation observations are key for addressing measurement error in the same way as the historical data, I only use the PSID. However, I show in Appendix Figure D3 that relative mobility estimates in the PSID are similar to other surveys like the General Social Survey and 1979 National Longitudinal Survey of Youth when one has access to a single father occupation.

The trend in relative mobility between 1840 and 1980

In contrast to the literature, I find that intergenerational persistence has *decreased* since the mid-19th century (Figure 7). The highest rate of persistence was for the 1840 birth cohort, when the father-son association is estimated at 0.84; the lowest is for the 1980 birth cohort, which is estimated at 0.37. The sharpest fall in persistence occurred between the 1910 and 1960 birth cohorts, when the father-son association fell by 36 percent from 0.69 to 0.44. Therefore, it appears that the "Great Compression" (Goldin and Margo 1992) of income inequality in the mid-20th century was associated with a sharp shift in relative mobility, which is consistent with Great Gatsby curve. Indeed, Jácome et al. (2021) use survey data over the 20th century and similarly find that relative mobility improved between 1910 and 1960, with the sharpest fall in the parent-child association between 1910 and 1940 birth cohorts. Jácome et al.'s results are especially useful because their data include Black and white women, and bolsters confidence in the result that there was a long-run improvement in relative mobility during this period.[41]

After the 1960 cohort, relative mobility estimates are steady, but also noisy. A steady rate of relative mobility in the modern period is consistent with *income* mobility estimates from Lee and Solon (2009) for 1952-1975 birth cohorts. Chetty et al. (2014b) also find a trendless rate of rank-rank income mobility for 1971-1993 birth cohorts when using tax data. However, a flat rate of mobility in the modern period is surprising since increasing inequality is cross-sectionally

[41] While the results in this paper agree with Jácome et al. (2021) on the trend of relative mobility, point estimates differ. There are many reasons why our point estimates could differ, such as the measure of economic status, coarseness of occupation categories, or measurement error in the underlying data source. One concern is that my estimates are different because they are based on an unrepresentative sample due to the linking process. However, recall that Appendix Figure A2 showed that linking multiple times does not substantially change estimates. Another reason why our estimates could differ is that I use multiple father observations (i.e., time-averaged or IV estimates) while Jácome et al. (2021) have a single recall of the parent's occupation. For instance, the IV estimate of the father-son association for the PSID is 28 percent higher than the OLS estimate using a single father observation. It could be that OLS estimates from other survey data, like Occupational Changes in a Generation, would similarly increase if one had multiple father observations.

associated with lower mobility (Corak 2013).[42] While the time-series relationship between inequality and mobility does not appear to be strong in the modern period, the long-run evidence is consistent with the "Great Gatsby" curve since income inequality and wealth inequality are estimated to have been higher in the 19th and early 20th century than in recent times (Goldin and Katz 2008, Lindert and Williamson 2016, Saez and Zucman 2020). One possibility is that the relationship between inequality and mobility has weakened over time due to institutional changes that improved outcomes for children from poorer backgrounds, such as access to better schools or health care.

Besides accounting for race and measurement error, a key difference between my estimates and earlier work is that I allow for status to vary within occupation by race and region. However, this adjustment does not drive the result that relative mobility was lower in the past. Figure 8A shows that if one uses occupation-only status, then intergenerational persistence fell from 0.50-0.63 in the pre-World War II data to 0.33-0.44 for post-World War II data.[43]

The long-run trend of improved mobility also holds when limiting the sample to white families, though the fall over time is less steep (Figure 7B). Rather than a 56 percent fall in the father-son association between the 1840 and 1980 birth cohorts, I find a 47 percent drop. However, relative mobility for white families is not always lower in the past than in the present. For the 1880 and 1890 birth cohorts, persistence estimates are similar (0.42-0.47) to estimates for modern-day cohorts (0.38-0.47). Nevertheless, the father-son association was highest for the 1840-1850 birth cohorts (0.72-0.74), the period when the literature had measured persistence to be the lowest.

Excluding Black males from the sample matters more for historical estimates than for modern-day estimates. For instance, while including Black males causes estimates to be 11-46 percent higher in the historical data, there is little to no movement for modern-day cohorts. One reason is that the between-race share in variation was higher in the past (i.e., Black-white inequality was higher), and thus between-race persistence is given greater weight in historical data. For

[42] It could be that earlier birth cohorts in the 1940 and 1950s experienced less inequality and had higher mobility than for those born in the later birth cohorts (Davis and Mazumder 2020).

[43] Another way I measure status differs from the broader economics literature since I rank occupations by their average level of human capital rather than income or earnings. Yet, I also find a fall in mobility when using imputed earnings by occupation, race and region (Figure A18). Recall that the reason I rely on human capital measures is that they are more reliable for the 19th century, in contrast to earnings-based imputations.

instance, the between-race share of variation (θ^b) falls from 0.52 in the 1840 cohort (when nearly all Black fathers were enslaved), to 0.30 in 1910 and 0.18 in 1980.[44]

In addition to race mattering more for historical data, measurement error also matters more, though there is nuance to this result. The importance of measurement error can be inferred by comparing the IV to the OLS estimates. For the preferred score, the difference between IV and OLS estimates is similar over time (~20-30 percent), which suggests that measurement error does not vary much over time.[45] However, measurement error is greater for the historical *occupation* variable than for the modern one. This result is clearly seen when comparing the IV to OLS estimates for the occupation-only measure of status for white families (Figure 8B). For this score, correcting for error increases estimates by 59-139 percent in the historical period, but only by 38-57 percent in the modern period. The reason why error is greater for occupation-only status than for status by occupation/race/region is simple: occupation is much more likely to mismatch across censuses than race or region. This result suggests that using additional information besides occupation to impute status reduces measurement error.

Structural change out of agriculture appears to be an important reason why *white* mobility has changed over time, but it does not explain the Black and white trend. If one drops farmer fathers, then the long-run decrease of the Black and white persistence estimates is similar to the main estimates (Figure 9A).[46] After dropping farmers, the high association for the earlier birth cohorts reflects Black-white differences; if one drops Black families, then the white non-farmer father-son association is 0.50 rather than 0.84. For white families, the non-farmer father-son association falls from 0.50 to 0.38 (Figure 9B). This finding is consistent with others who argue that the trend in mobility over time is primarily due to structural change (e.g., Guest et al. 1989, Blau and Duncan 1967, Xie and Killewald 2013, Song et al. 2020). However, this result applies only to white families; since racial inequality was high in the past, the overall father-son association was also high for the 1840 birth cohort. Of course, numerous other changes could

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[44] The between-race association is smaller for human-capital based status measures since Black-human capital gaps converged more rapidly than income gaps (Bayer and Charles 2018). However, the result that relative mobility is higher today than the past holds when using imputed earnings (see Figure A18).

[45] If one uses an IV estimate for labor income instead of the proxy for economic status in the PSID, then the ratio of the IV estimate (0.50) to OLS (0.38) is about 32 percent. The IV estimate is similar to using a 15-year average of log income (Figure A10).

[46] The estimates keep the formerly enslaved in the data since they were not farmers.

explain mobility besides structural change and racial inequality, such as changes to fertility, household formation, assortative mating, residential segregation, the education premium, internal migration, and institutions, to name a few. Ultimately, more research is needed to understand what drove mobility trends in the long run.

VIII. Conclusion

This paper's main message is that historical mobility was lower than previously estimated in linked data. To show why, I account for two measurement issues: unrepresentative samples and measurement error. First, I account for unrepresentative samples by adding Black families, who historical studies routinely drop. Second, I address measurement error by using multiple father observations to more accurately capture his permanent economic status. These issues are not new to the literature (e.g., Solon 1992, Duncan 1968, Hertz 2005), but due to various data limitations, they had not been fully addressed in historical linked studies that found high mobility. I show that historical father-son associations more than double after accounting for race and measurement error. Within-race associations also increase after accounting for measurement error.

As a general point, the results suggest that researchers should be aware that using occupation or occupational-based measures of earnings or status may bias estimates. The bias is more significant when using occupational-based measures as a right-hand-side variable. For example, if one is interested in the causal effect of a policy, such as compulsory schooling on adult outcomes, controlling for parental occupation will not fully capture parental status. Other mobility studies that directly measure occupational change, such as estimates of intragenerational mobility or multigenerational mobility, will also be biased.

When I estimate the trend in mobility over time using consistent methods, the results suggest the optimistic conclusion that relative mobility is far greater today than in the past. A decrease in persistence over time is consistent with others who estimate that institutional changes over the 20th century helped to improve outcomes for disadvantaged groups (e.g., Card and Krueger 1992, Hoynes et al. 2016, Reber 2010).[47] Of course, there are many factors other than

[47] Derenoncourt (2019) also shows evidence that institutional changes affected mobility rates in northern cities following the Great Migration. However, instead of institutional changes increasing equality of opportunity, they decreased it for African Americans.

institutional change that affect relative mobility trends. But before we can understand what causes mobility to change over time, we must first accurately measure mobility in the past.

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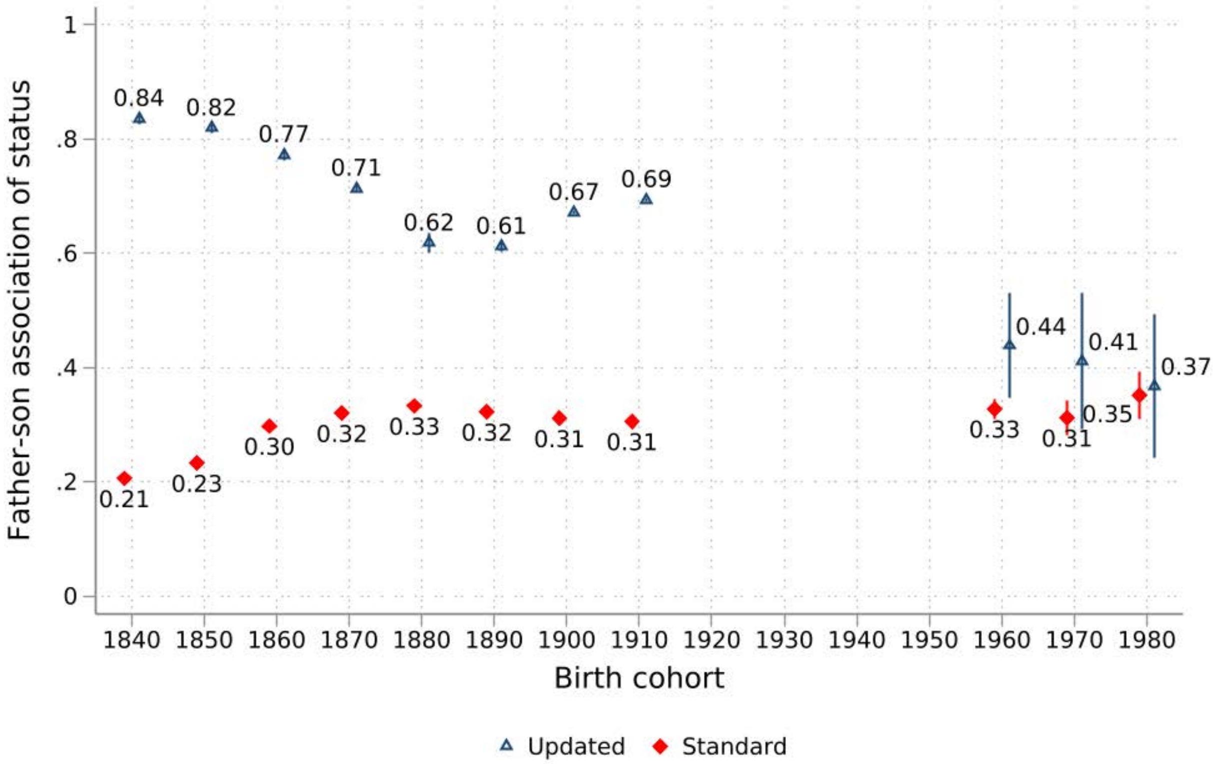
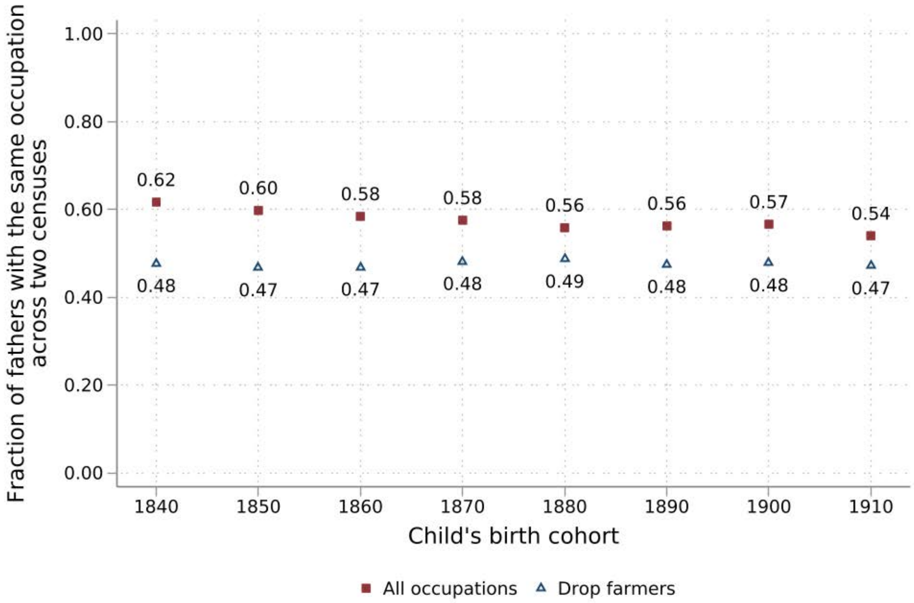
Figure 1. Updated estimates of intergenerational mobility

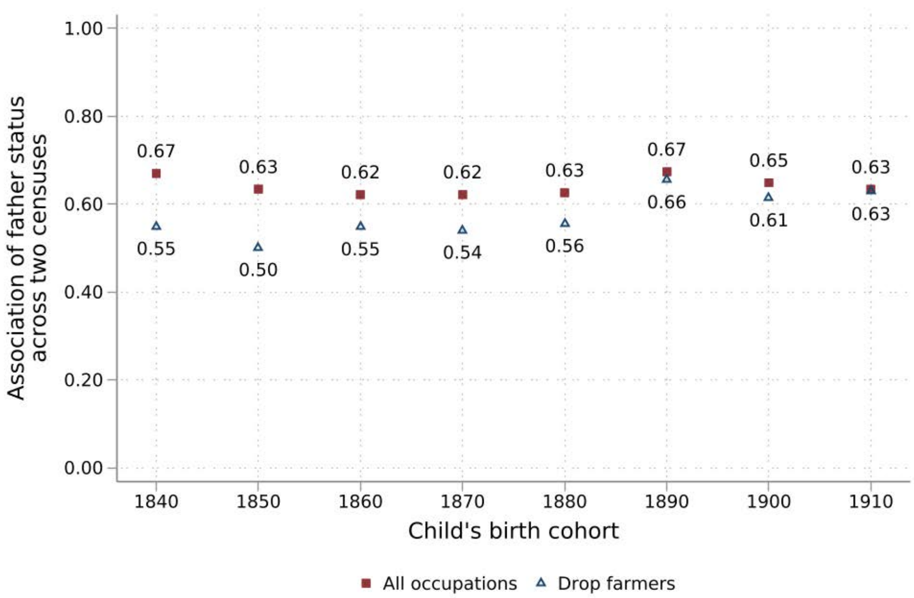


Notes: The plotted estimates are the slope coefficient of the son's status (on a 0-100 scale) on the father's status. Estimates are presented by the son's birth cohort, rounded to the nearest decade. The "standard" estimates are from Song et al.(2020) and reflect standard historical mobility estimates in the literature. For example, only white males are in the data, one occupation observation is assumed to capture permanent status, and within-occupation differences is status by race or region are ignored. The "updated" estimates make multiple changes to the standard estimates, which are described in Appendix D. The most important changes are: (1) Black families are pooled with white families, (2) measurement error is accounted for via instrumental variables, and (3) the status measure allows for within-occupational differences by race and region. Other differences in estimates include weighting and the linking method, but these differences are not as important for the long-run trend between 1840 and 1980 birth cohorts.

Figure 2. The father's occupation and status level are unstable across observations
Panel A. Holds the same 3-digit occupation in both censuses

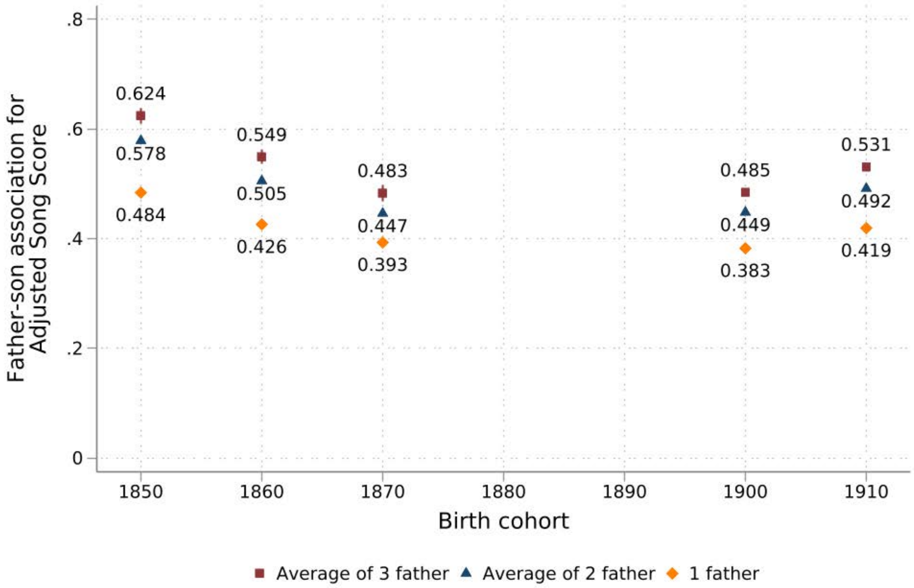


Panel B. Association of status between first and second father observation

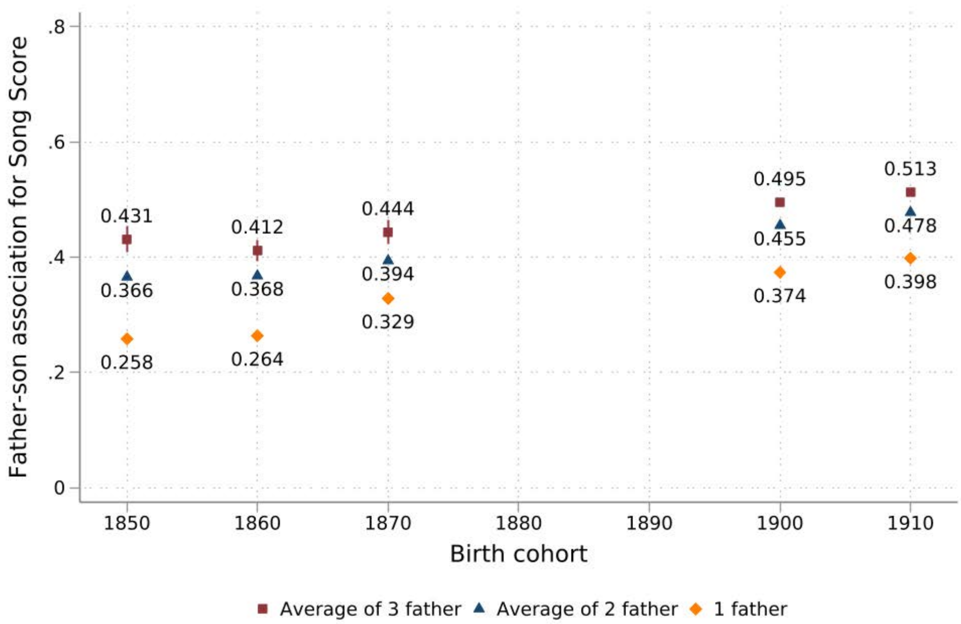


Notes: Data are a double-linked sample of white fathers and sons from the 1850-1940 United States Censuses. Panel A shows the fraction of the sample that has the same 3-digit occupation code across two censuses (*occ1950*). Lifecycle effects are controlled for with age fixed effects. Panel B shows the slope estimate from a regression of the father's status in the first census on the status in the second census. Status is a based on the percentile rank of the mean human capital level in an occupation, race and region cell (see Appendix C). Estimates labeled "drop farmers" are for the subset of the data where the father is not a farmer in the first or second occupation observation. Estimates are presented by the son's birth cohort, rounded to the nearest decade. Note that Panel B is the first stage of an instrumental variables regression to eliminate measurement error, where the first father status observation is instrumented with a second one. 95% confidence intervals are plotted after clustering by father's household.

Figure 3. The father-son association is attenuated by measurement error
Panel A. Status measure adjusts for within-occupation differences by region



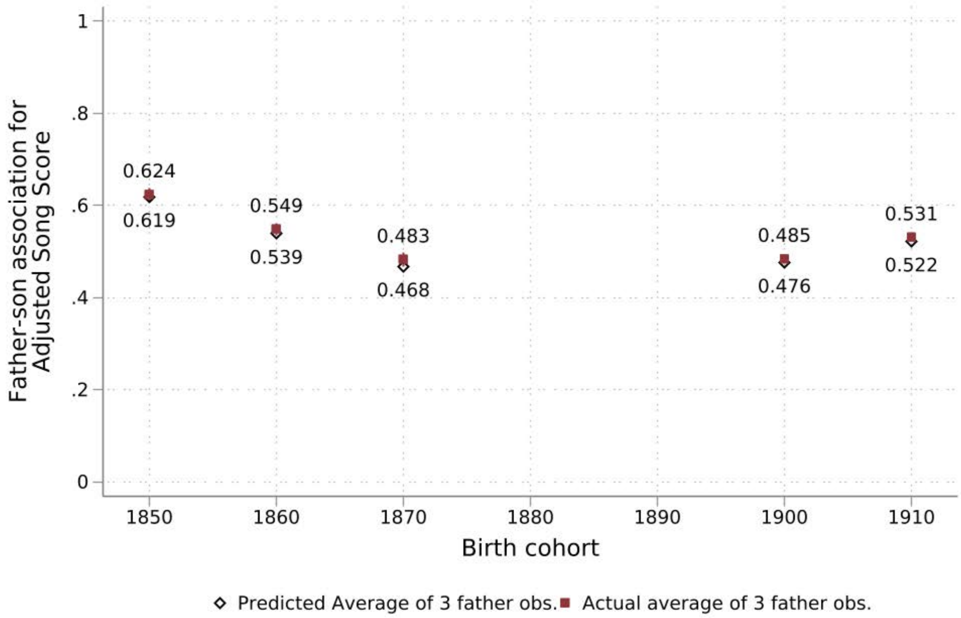
Panel B. Status measure ignores within-occupation differences by region



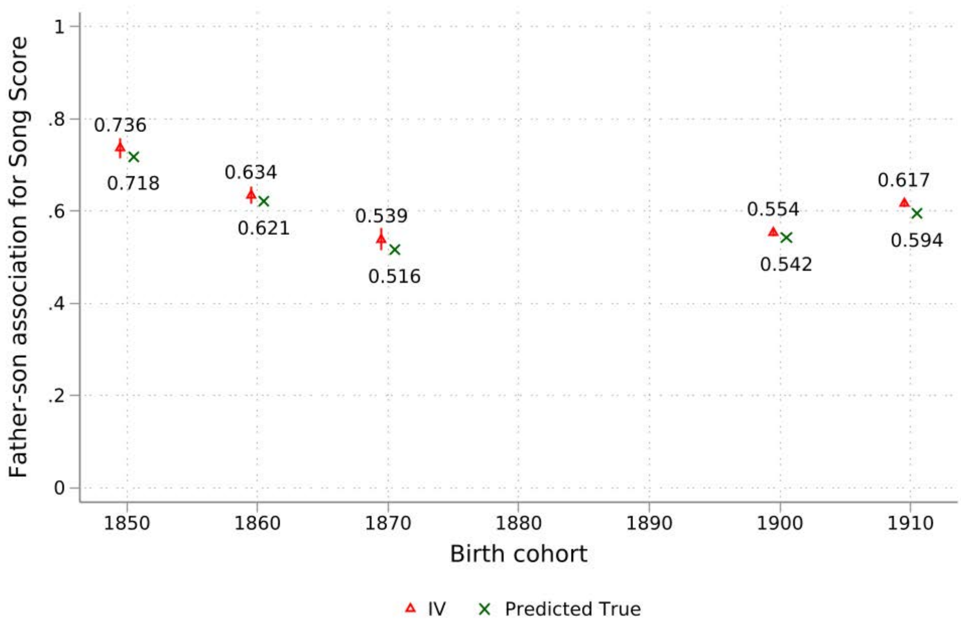
Notes: Data are a triple-linked sample of white fathers and sons from the 1850-1940 United States Censuses. The figure shows the estimate from regressing the son's status on the father's status. The estimates differ based on a single father observation ("1 father"), averaging two father observations ("Average of 2 father') or averaging three father observations ("Average of 3 father"). The same linked sample is used for all estimates. Estimates for the 1840, 1880 and 1890 cohorts are not included because fathers cannot be triple linked to the 1840 or 1890 censuses. Panel A uses a status measure that percentile ranks occupation, race, and region cells by their mean level of human capital ("Adjusted Song score"). Panel B uses a status measure that percentile ranks occupation cells by their mean level of human capital, which discounts within-occupation differences by race/region (Song et al. 2020). 95% confidence intervals are plotted after clustering by father's household.

Figure 4. Predicted mobility from classical measurement error is similar to actual mobility

Panel A. Predicted mobility from classical measurement error formula is similar to actual mobility

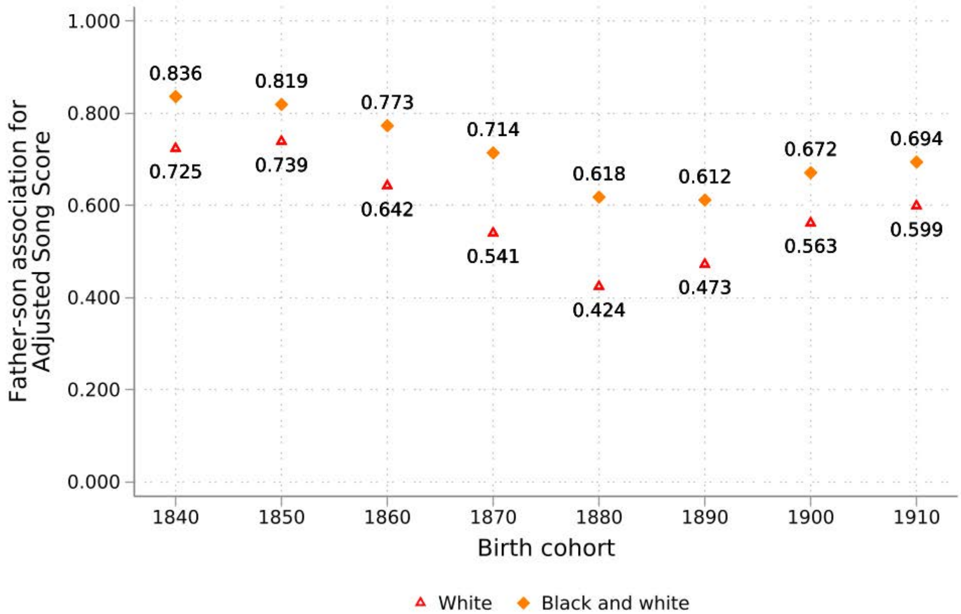


Panel B. Mobility estimates using instrumental variables or classical measurement error formula

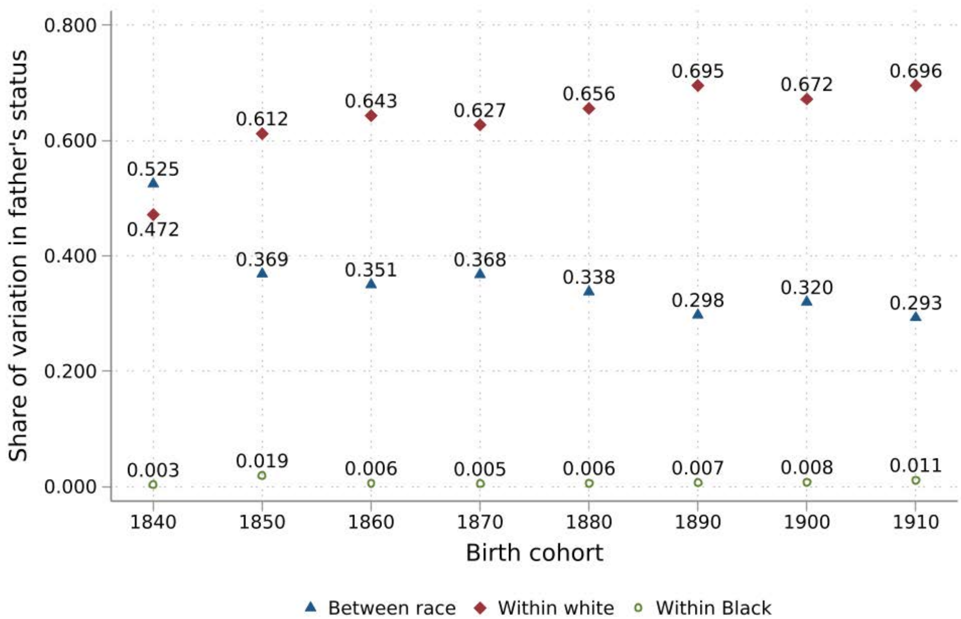


Notes: Data are a triple-linked sample of white fathers and sons from the 1850-1940 United States Censuses. For Panel A, the "predicted average" is the predicted slope coefficient for the regression of the son's status on an average of three father observations. The prediction is based on the classical measurement error formula. The "actual average" is the actual slope coefficient estimated in the data. For Panel B, the IV estimate instruments the first father observation with second. The "Predicted True" estimate is based off the classical measurement error formula after completely eliminating noise. 95% confidence intervals are plotted after clustering by father's household. Estimates for the 1840, 1880 and 1890 cohort are not included because fathers cannot be triple linked to the 1840 or 1890 censuses.

Figure 5. Father-son associations increase after including Black families
Panel A. Status measure adjusts for within-occupation differences by race and region



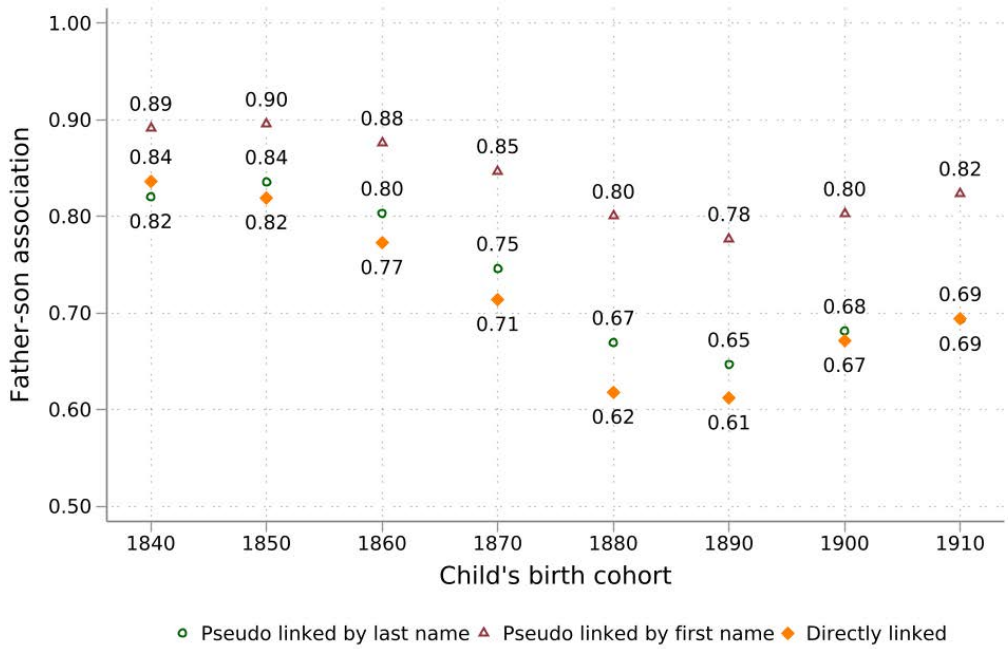
Panel B. Within-between decomposition of variation of father's status



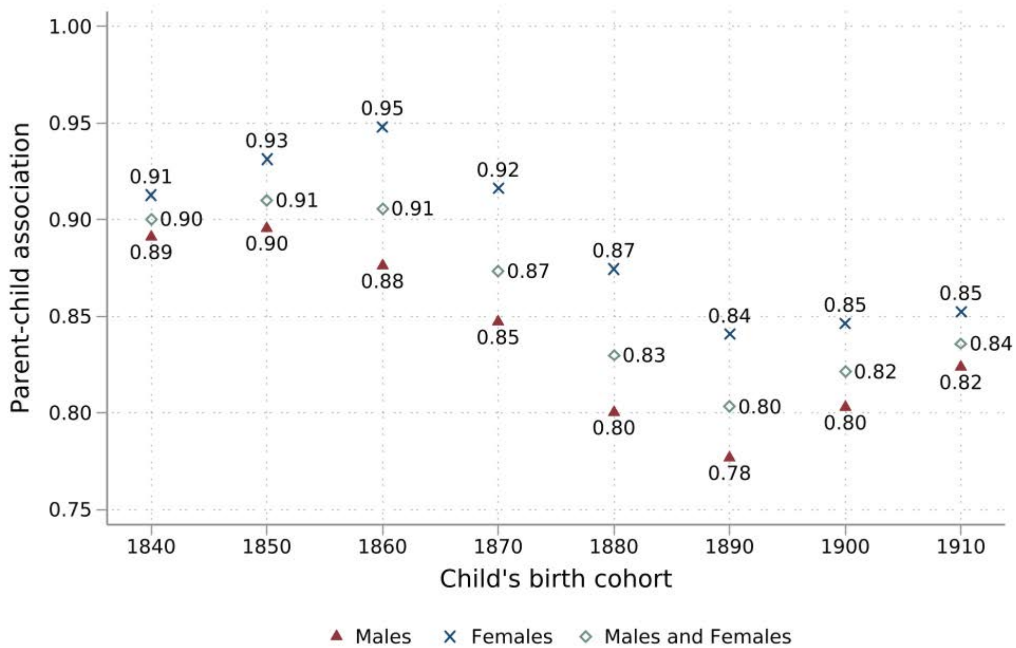
Notes: Data are a double linked sample of fathers and sons from the 1850-1940 United States Censuses. Panel A shows IV estimates of the father-son association based on a sample of white fathers and sons ("White"), or a pooled sample of Black and white fathers and sons ("Black and white"). The first father observation is instrumented with the second father observation. Panel B plots the within-race shares and between-race shares of variation in the linked data. For example, 53% of the total variation in the father's status for the 1840 birth cohort is between race. The classical measurement error formula is used to eliminate error when calculating shares of variation. Note that point estimates slightly differ from Figure 4 since Figure 4 is based on a triple-linked sample while this figure is based on a double-linked sample.

Figure 6. Name-based estimates are consistent with directly linked estimates

Panel A. Estimates based on first name, last name, or directly linked

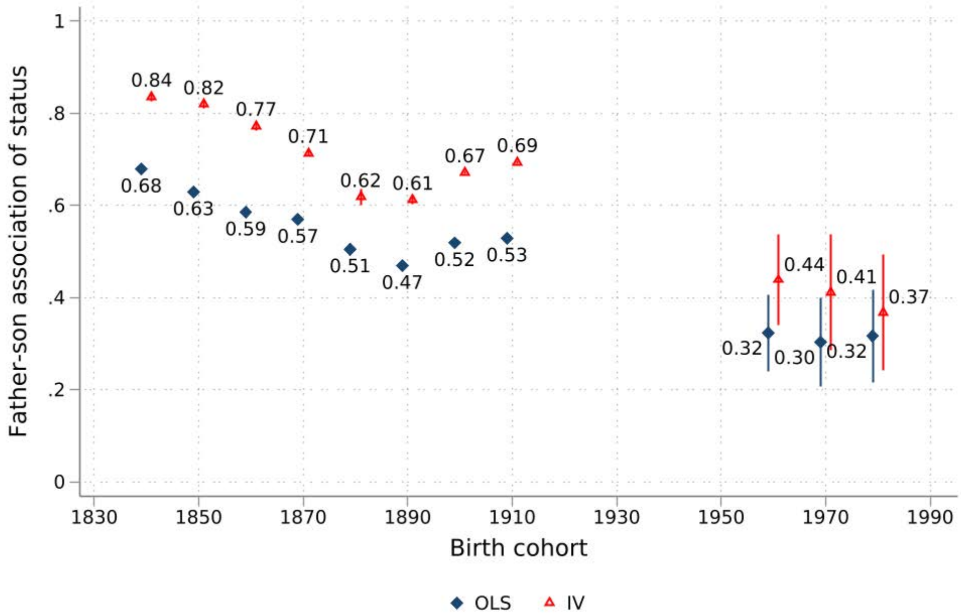


Panel B. Including females does not strongly alter the mobility trend estimated via first name

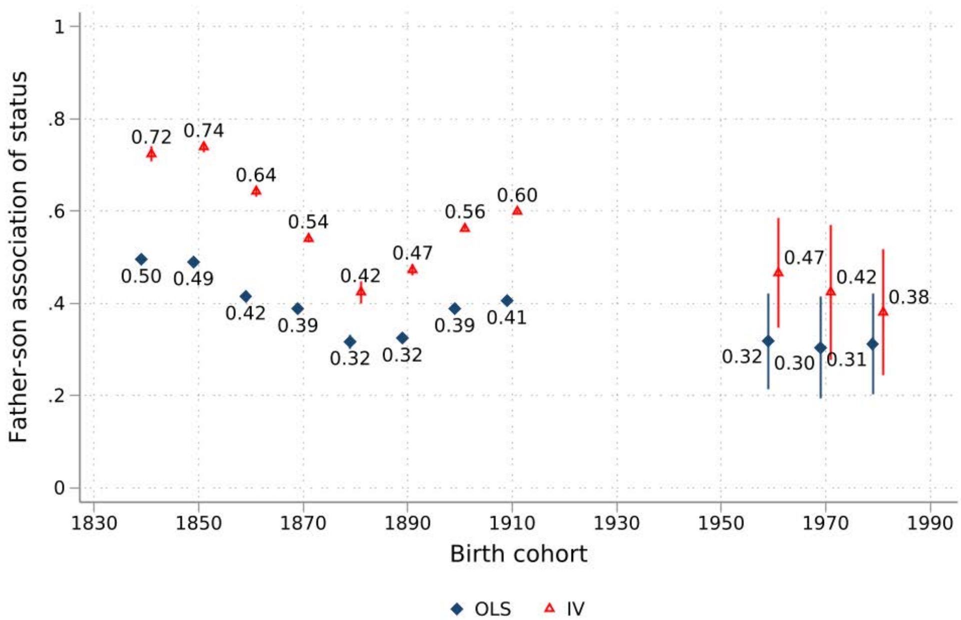


Notes: Data are from the 1850-1940 United States Censuses. "Pseudo-linked by last name" are estimates where the father's status is inferred by surname. "Pseudo linked by first name" are estimates where the father's status is inferred by the child's first name. "Directly linked" estimates are the main estimates with linked data. Panel A shows mobility for only males. Panel B estimates female mobility with the father/son-in-law association and the first-name method (Olivetti and Paserman 2015). See Appendix I for more detail on creating name-based estimates.

Figure 7. Long-run trend of relative mobility
Panel A. Black and white families

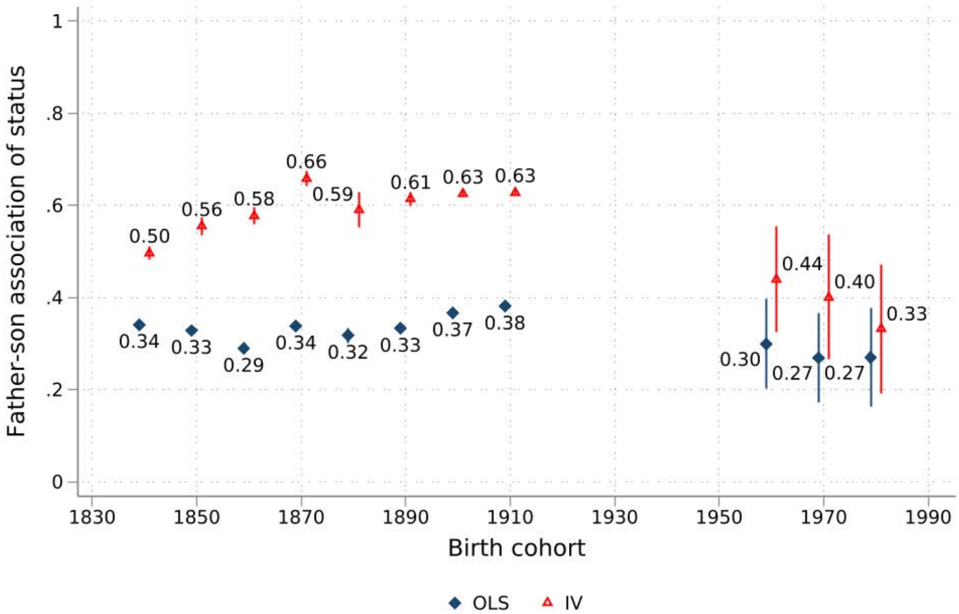


Panel B. White families

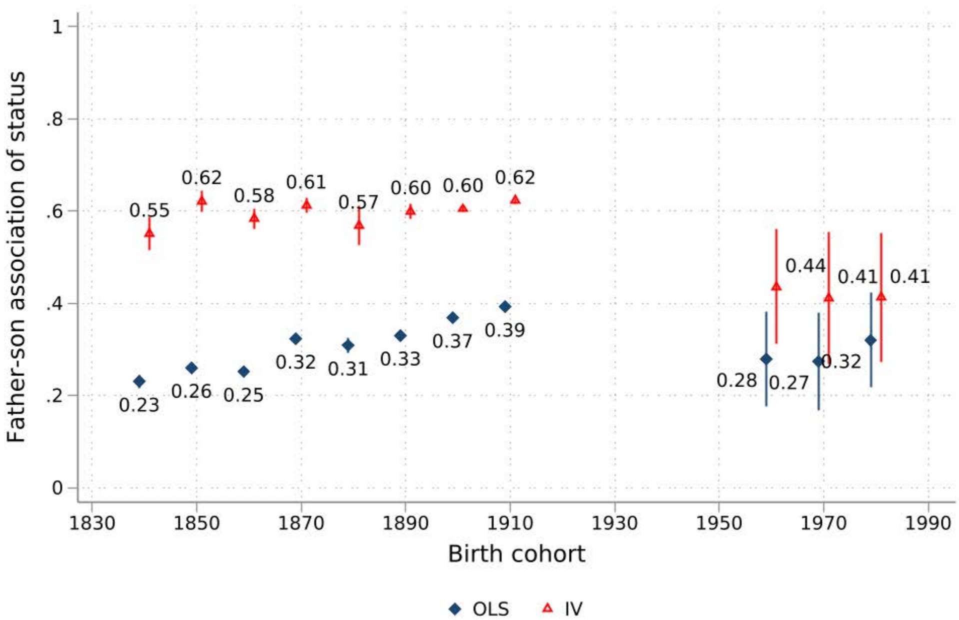


Notes: Data are from historical linked samples and the PSID. 95% confidence intervals plotted. OLS estimates are the slope coefficient from a regression of the son's status on a single father observation. IV estimates instrument one father observation with a second, which is aimed to eliminate measurement error. Panel A uses the entire sample of Black and white sons to estimate mobility. Panel B uses only the white samples. Status is measures on a 0-100 scale based on a percentile ranking of the mean literacy rate by occupation/race/region in 1850-1930 data, and education level in post 1940-data. Estimates are plotted by the son's birth cohort, rounded to the nearest decade (i.e., 1960 for those born between 1955-1964).

Figure 8. The trend when measuring status only by occupation
Panel A. Black and white families

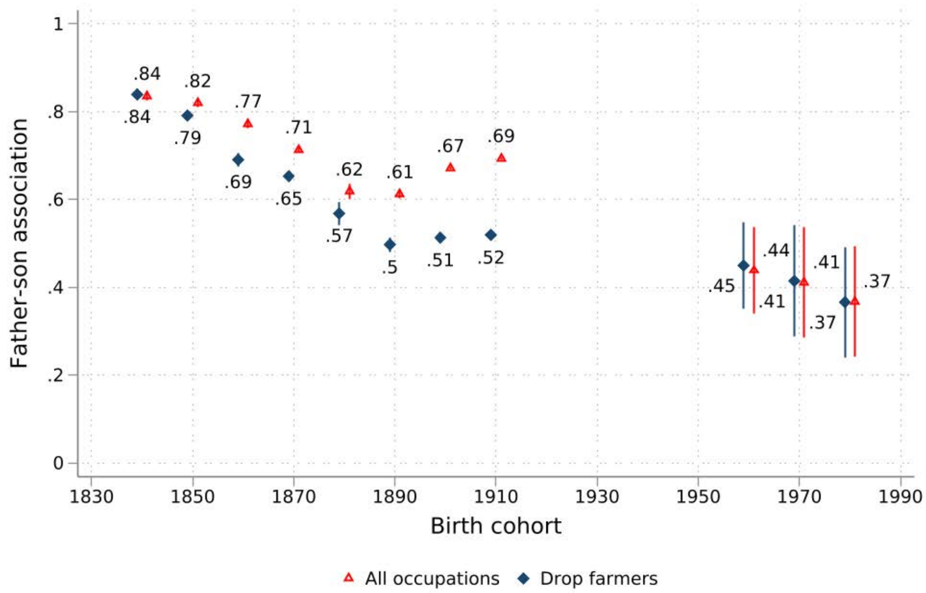


Panel B. White families

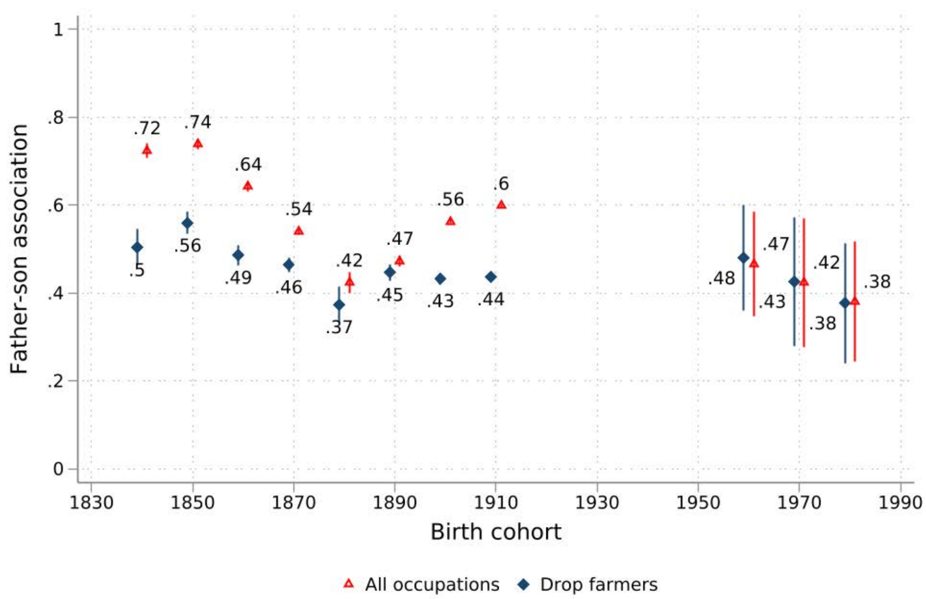


Notes: Data are from historical linked samples and the PSID. 95% confidence intervals plotted. OLS estimates are the slope coefficient from a regression of the son's status on a single father observation. IV estimates instrument one father observation with a second, which is aimed to eliminate measurement error. Panel A uses the entire sample of Black and white sons to estimate mobility. Panel B uses only white samples. Status is measures on a 0-100 scale based on a percentile ranking of the mean literacy rate by occupation/race/region in 1850-1930 data, and education level in post 1940-data. Estimates are plotted by the son's birth cohort, rounded to the nearest decade (i.e., 1960 for those born between 1955-1964).

Figure 9. The trend in mobility after dropping the sons of farmers
Panel A. Black and white families



Panel B. White families



Notes: Data are from historical linked samples and the PSID. 95% confidence intervals plotted. IV estimates are presented, where one father observation is instrumented with a second. Panel A uses the entire sample of Black and white sons to estimate mobility. Panel B uses only white samples. Estimates that "drop farmers" drop farmer (owner/tenant) fathers from the sample (*occ1950*=100). Status is measures on a 0-100 scale based on a percentile ranking of the mean literacy rate by occupation/race/region in 1850-1930 data, and education level in post 1940-data. Estimates are plotted by the son's birth cohort, rounded to the nearest decade (i.e., 1960 for those born between 1955-1964).

Table 1. Descriptive statistics for fathers and sons by son's birth cohort

	1840	1850	1860	1870	1880	1890	1900	1910
Characteristics of sons								
Black	15.1	11.2	11.2	12.4	12.9	9.1	10.3	10.0
	(35.8)	(31.6)	(31.5)	(32.9)	(33.5)	(28.7)	(30.3)	(30.0)
Age	38.5	36.4	42.7	36.6	34.3	43.9	38.0	29.9
	(4.8)	(9.9)	(6.8)	(7.1)	(6.1)	(8.2)	(5.4)	(3.1)
Adjusted Song score	49.4	48.6	51.2	50.6	51.0	55.1	54.5	52.2
	(30.0)	(29.0)	(28.9)	(29.4)	(29.1)	(28.3)	(28.3)	(28.1)
Unadjusted Song score	51.4	49.2	51.7	50.9	50.9	55.1	53.9	52.0
	(27.7)	(28.5)	(27.8)	(28.9)	(28.5)	(27.9)	(27.9)	(27.9)
White Collar	16.1	15.6	18.5	20.0	20.7	29.0	29.8	27.4
	(36.8)	(36.3)	(38.8)	(40.0)	(40.5)	(45.4)	(45.8)	(44.6)
Farmer	39.1	36.9	36.1	29.2	26.9	18.3	14.5	10.5
	(48.8)	(48.3)	(48.0)	(45.5)	(44.4)	(38.7)	(35.2)	(30.7)
Unskilled	31.0	33.8	30.0	34.6	35.7	32.6	37.4	47.3
	(46.2)	(47.3)	(45.8)	(47.6)	(47.9)	(46.9)	(48.4)	(49.9)
Skilled	13.8	13.7	15.4	16.2	16.6	20.1	18.2	14.8
	(34.5)	(34.3)	(36.1)	(36.8)	(37.2)	(40.1)	(38.6)	(35.5)
Characteristics of father								
Age	41.1	41.1	40.9	41.9	39.0	40.8	40.6	40.3
	(6.0)	(6.7)	(6.5)	(7.0)	(6.5)	(5.3)	(6.8)	(6.9)
Adjusted Song score	49.0	49.2	49.0	48.1	46.5	49.6	49.6	49.6
	(28.9)	(27.6)	(27.1)	(27.8)	(27.9)	(27.1)	(27.5)	(27.4)
Unadjusted Song Score	48.8	51.3	50.6	51.5	50.6	51.3	51.0	50.1
	(26.6)	(25.2)	(24.7)	(23.7)	(23.7)	(24.0)	(24.4)	(24.9)
White Collar	7.5	9.4	10.0	11.5	11.4	13.7	15.7	16.7
	(26.4)	(29.2)	(30.0)	(32.0)	(31.8)	(34.4)	(36.4)	(37.3)
Farmer	51.8	52.6	50.6	52.4	53.1	47.5	46.1	42.4
	(50.0)	(49.9)	(50.0)	(49.9)	(49.9)	(49.9)	(49.8)	(49.4)
Unskilled	12.9	15.6	23.8	23.1	23.2	22.9	22.7	24.2
	(33.5)	(36.3)	(42.6)	(42.1)	(42.2)	(42.0)	(41.9)	(42.8)
Skilled	13.0	14.3	14.2	13.0	12.3	15.9	15.4	16.7
	(33.7)	(35.0)	(34.9)	(33.6)	(32.9)	(36.6)	(36.1)	(37.3)
Observations	64,004	161,541	207,289	304,341	94,303	254,557	743,267	628,747

Notes: Data are a linked sample of fathers and sons from the 1850-1940 United States Censuses. A unit of observation is a father/son pair. Descriptive statistics are weighted. Each column are results by son's birth cohort, which is rounded to the nearest decade. The Adjusted Song score is based on a percentile ranking of human capital by occupation/race/region. The Song score is based on a percentile rank of human capital by occupation (see Appendix C). 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).

Table 2. Occupations are poorly correlated across 1880 St. Louis enumerations

Proportion with agreeing occupations in 1880 St. Louis:	
3-digit Occupation code (occ1950)	0.647
	(0.005)
First digit of 3-digit code	0.694
	(0.005)
Observations	9,319
Correlation of status measures in 1880 St. Louis:	
Adjusted Song Score	0.793
Unadjusted Song score	0.670
Observations	9,319
Correlation of status measures from 1870-1880 linked data (Residents of 1880 St. Louis)	
Adjusted Song score	0.556
Unadjusted Song score	0.401
Observations	3,492
Implied fraction of measurement error from inconsistent coding of occupation, assuming no transitory fluctuations between St. Louis enumerations	
Adjusted Song score	0.327
Unadjusted Song score	0.330

Notes: Data are linking the two enumerations of St. Louis in the 1880 census. The two enumeration should contain the same occupation since both refer to the June enumeration date. The 1870-1880 linked data are 1880 St. Louis residents linked to the 1870 census (Abramitzky et al. 2020). See Appendix G for further details.