Online Appendix

Mobility for All: Representative Intergenerational Mobility Estimates over the 20th Century

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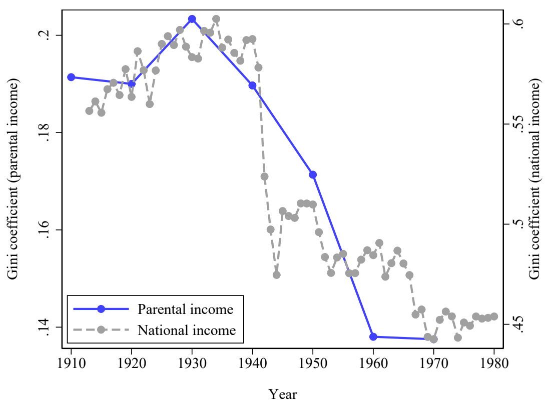
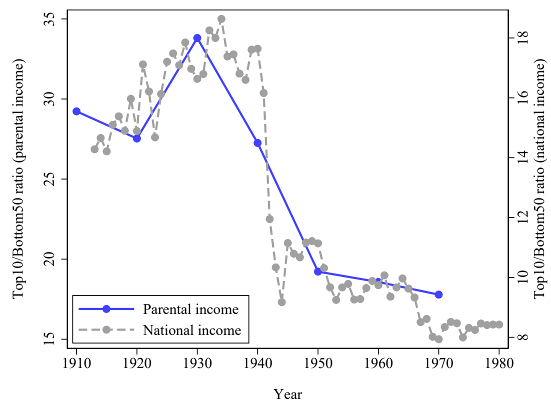
A Additional figures and tables referenced in the text

Figure A.1: Measures of inequality and Black-white income gap of predicted parental income, by birth cohort

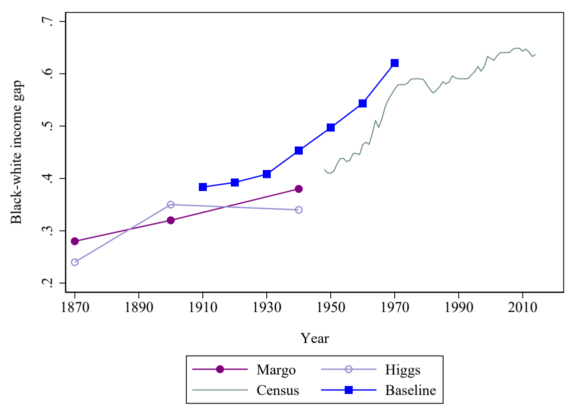
(a) Gini coefficient



(b) Top 10 to Bottom 50 ratio



(c) Black-white ratio of (predicted) parental income

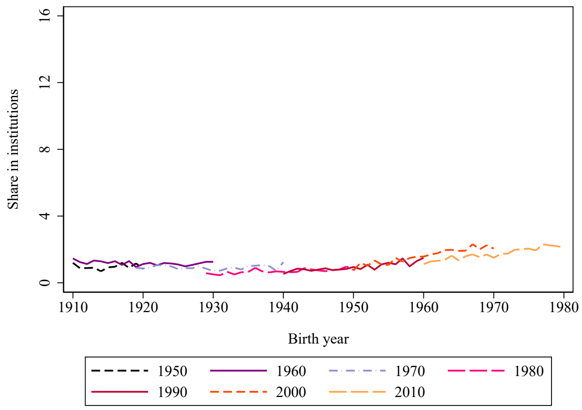


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E. Data on national income comes from the World Inequality Database. Data on the Black-white income gap comes from Margo (2016).

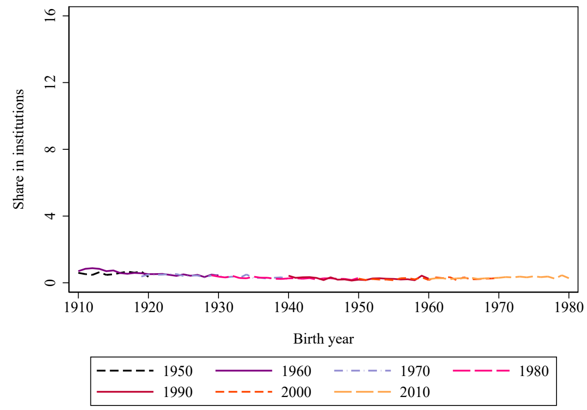
Notes: The first panel plots the Gini coefficient of predicted parental income for each birth cohort in the baseline sample as well as the Gini coefficient of national income. The second panel plots the ratio of total income in the top 10% of the income distribution relative to the total income in the bottom 50%, using predicted parental income for each cohort as well as national income. The third panel plots the ratio of the average parental income of Black respondents to that of white respondents in each birth cohort as well as Black-white income gaps from other data sources. In the first two panels, to predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). To be consistent with other data sources, in the third panel we use father's personal income (conditional on occupation, race, and region) to predict parental income.

Figure A.2: Share of Census respondents in institutions, by birth year and Census year

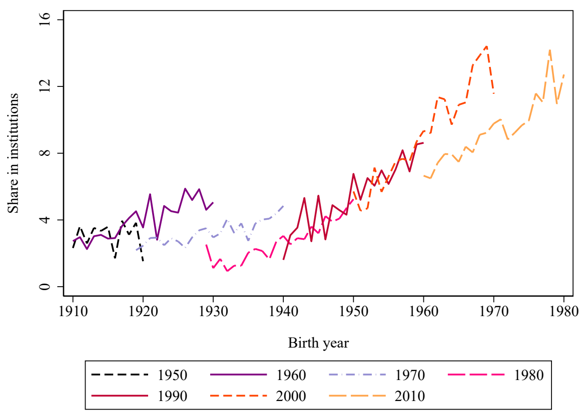
(a) White men



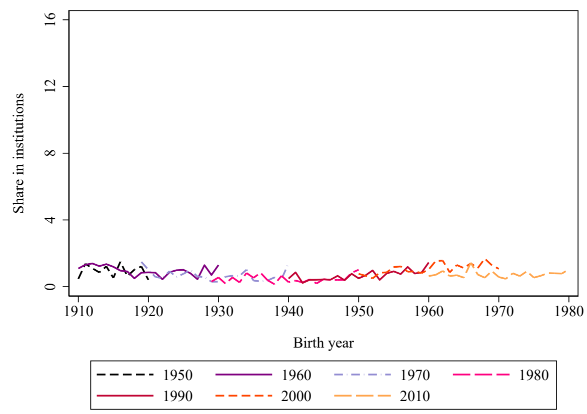
(b) White women



(c) Black men



(d) Black women

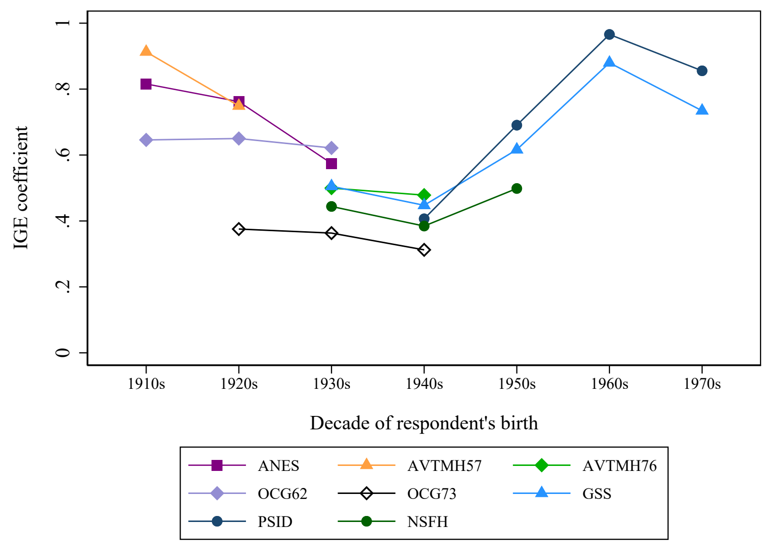


Sources: 1950–2000 1% Census samples and 2010 American Community Survey (Ruggles *et al.*, 2021).

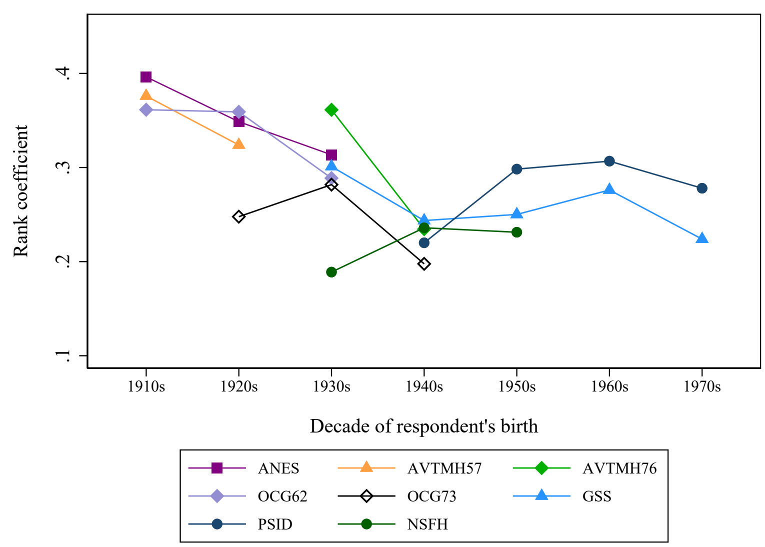
Notes: This figure plots the share of individuals born in a specific year that are living in institutions (measured using group quarter status), separately by Census year. The sample is restricted to white and Black U.S.-born Census respondents.

Figure A.3: Mobility measures by birth decade and by survey

(a) Intergenerational elasticity



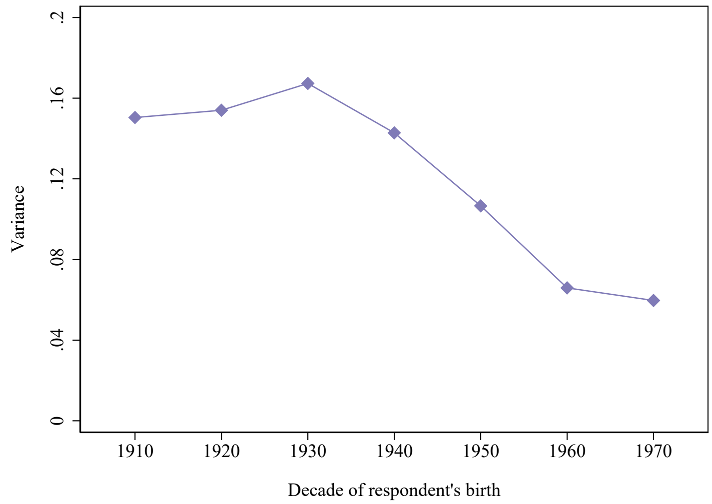
(b) Rank-rank coefficient



Sources: This figure uses 8 of the 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure plots the IGE and rank-rank coefficient estimated on each survey separately. We exclude surveys whose respondents are only of one race as well as surveys that are not representative of the 30–50 age group. We also exclude cohorts within a survey if there were fewer than 200 respondents born in that decade. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). In both panels, we use the baseline population-adjusted weights and in the bottom panel, we maintain the same ranking for respondents and their parents as in the baseline approach.

Figure A.4: Variance of parental income by birth cohort

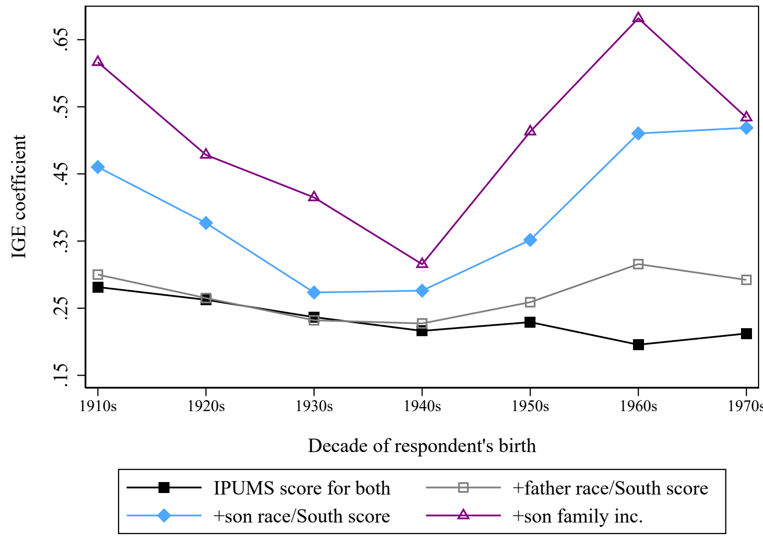


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

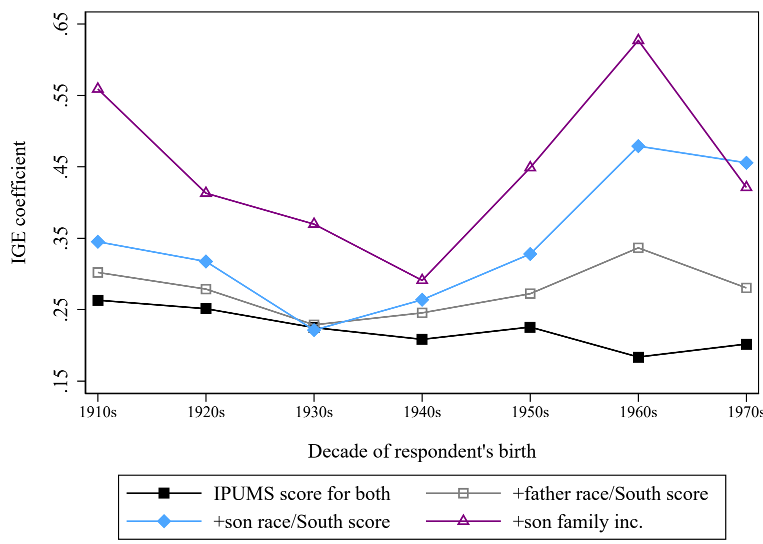
Notes: This figure plots the variance of predicted parental income for the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure A.5: IGE measure for men by birth cohort, using various ways of measuring parental and adult children's incomes

(a) All men



(b) White men

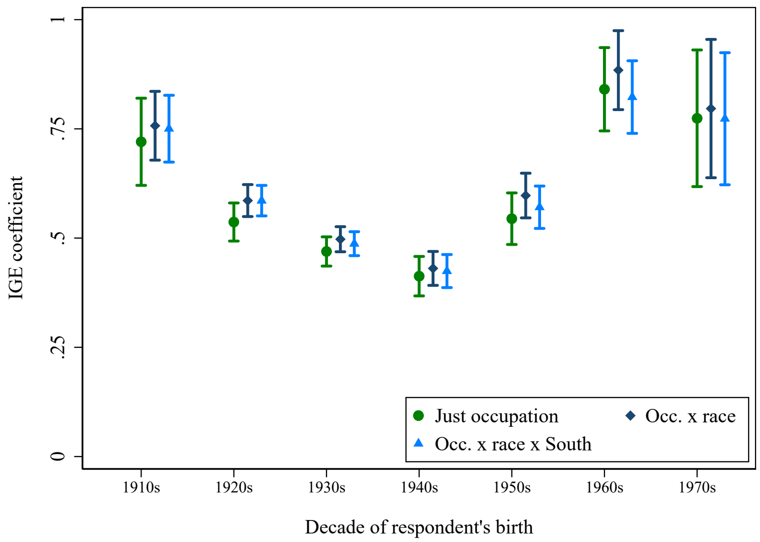


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

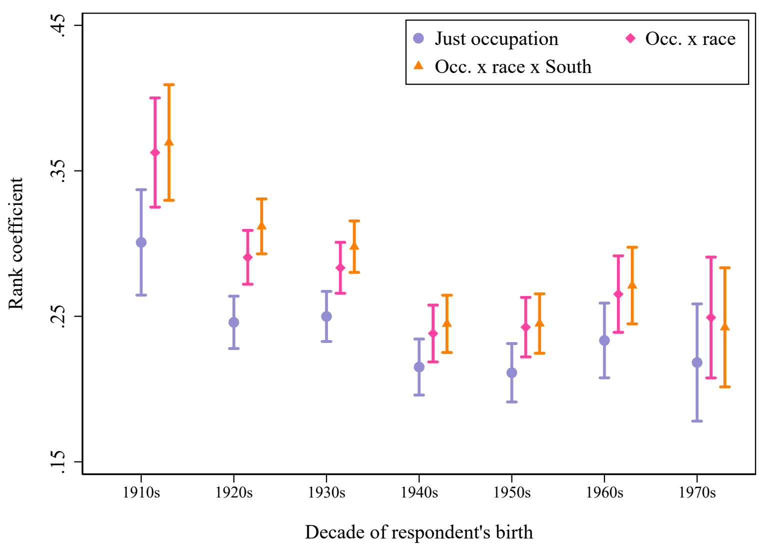
Notes: This figure plots the IGE coefficient as we alter how we measure respondent and parental income. Both panels only consider male respondents who reported their occupations as well as their fathers' occupations. The first series uses the IPUMS *occscore* variable to measure income in both generations. The second series replaces predicted parental income with the baseline family income prediction at the *occupation* × *race* × *South* level. The third series replaces the *occscore*-based income prediction for sons with an income prediction that varies at the *occupation* × *race* × *South* level using the two Censuses closest in time to when the respondent was 40 years old (i.e., using weighted averages of predicted income that are constructed using the 1940–2000 Censuses as well as the 2010 and 2019 American Community Survey from Ruggles *et al.* (2021)). The fourth series replaces the son's income prediction with the son's reported family income.

Figure A.6: Mobility measures by birth decade, adding detail to parental family income prediction

(a) Intergenerational elasticity



(b) Rank-rank coefficient

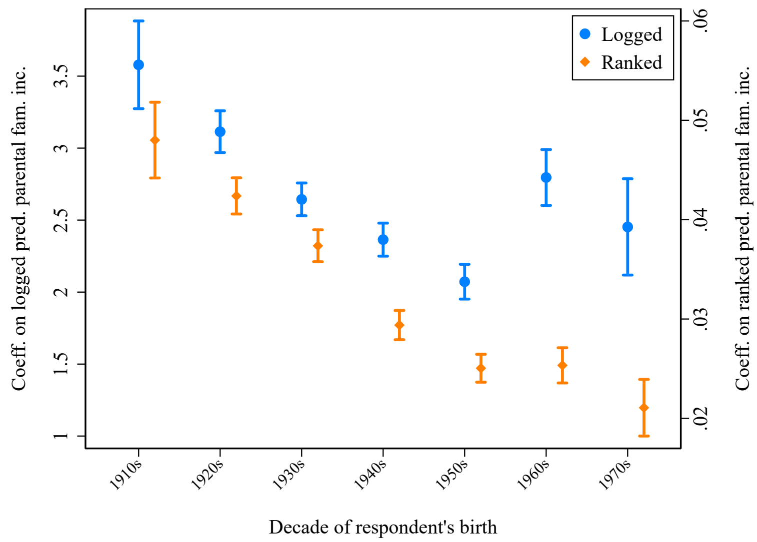


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

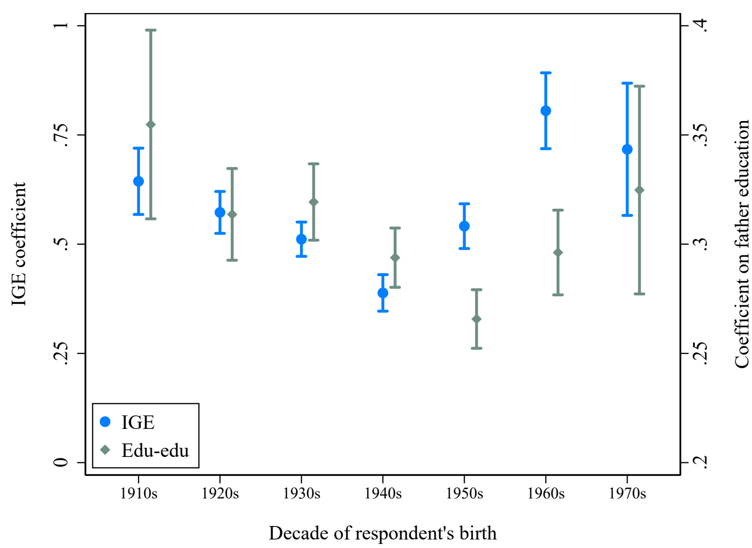
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. The first series uses predicted parental income that only varies by a father's occupation. The second series allows income to vary by father's occupation and race. The third series allows income to vary by father's occupation, race, and Southern residence. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in this sample so that they have representative *race* $\times$ *sex* shares.

Figure A.7: Weakening intergenerational relationship using educational attainment

(a) Respondent education & (predicted) parental income



(b) Respondent & parental education

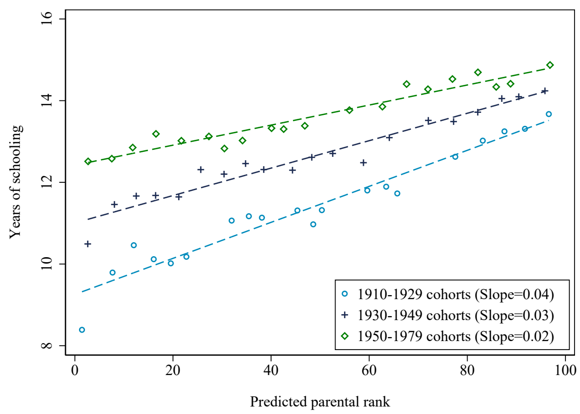


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

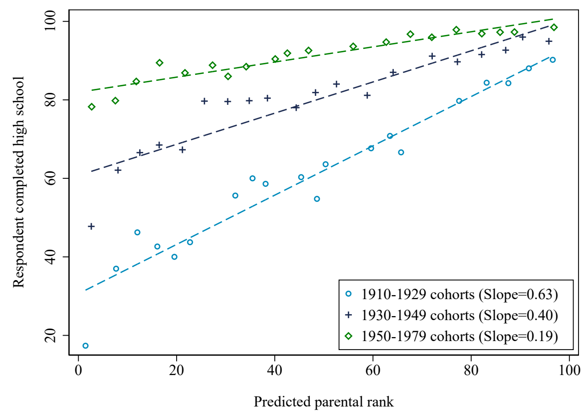
Notes: In the top panel, we use a respondent's years of schooling as the dependent variable and regress it on logged or ranked predicted parental income—similar to equations (2) and (3)—using the baseline sample of respondents ages 30–50. In the bottom panel, we restrict the sample to the 12 surveys that include information on father's education. The first series plots the IGE for this subsample and the second series plots the estimates from a regression of respondent years of schooling on parental years of schooling. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure A.8: Bin-scatter depictions of the weakening relationship between respondent education and parental rank

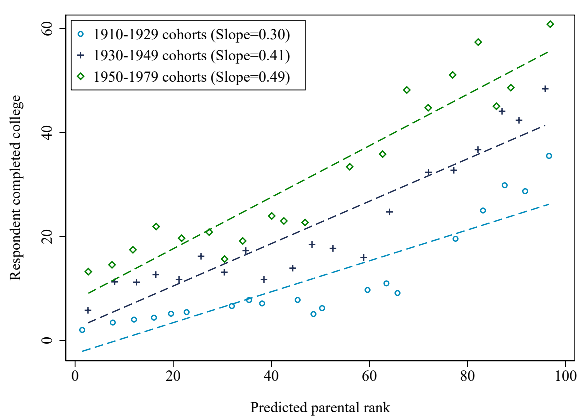
(a) Years of schooling



(b) High school completion



(c) College completion

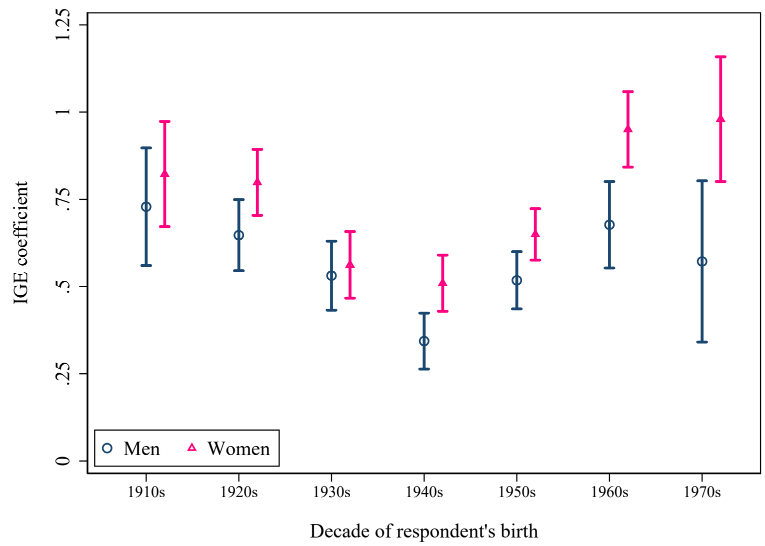


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Further details on the construction of education variables are available in Appendix E.

Figure A.9: Mobility measures by birth decade, by sex (restricted to common surveys)

(a) Intergenerational elasticity



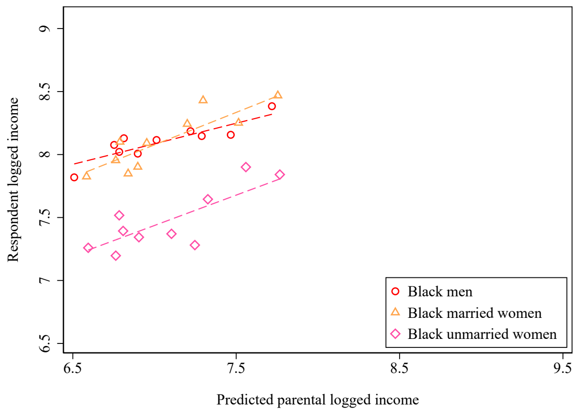
(b) Rank-rank coefficient

Sources: This figure combines the 7 surveys that include both male and female respondents.

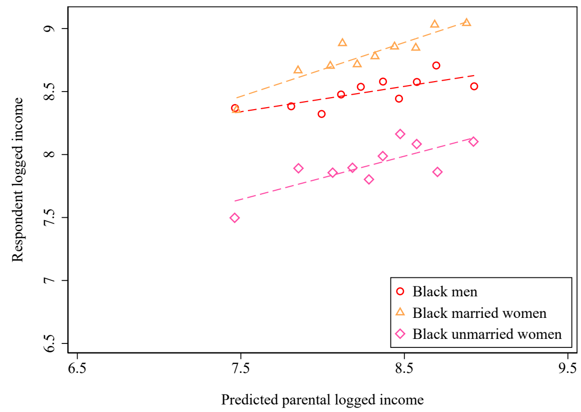
Notes: This figure is identical to Figure 4 except that in this figure, we only use surveys that include both men and women. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further weight each birth cohort in this sub-sample so that they have representative *race* × *sex* shares.

Figure A.10: Mobility by marital status for Black women and men, 1910s–1920s versus 1940s–1950s

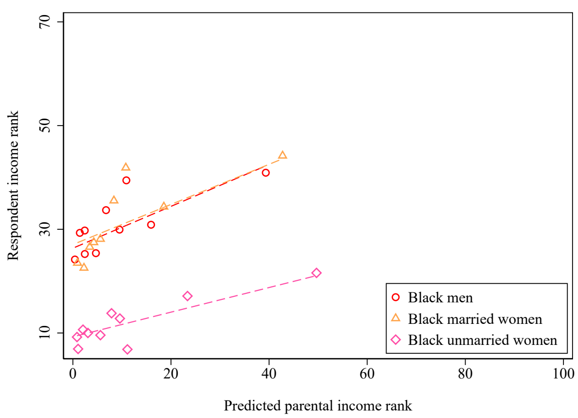
(a) IGE: 1910s–1920s



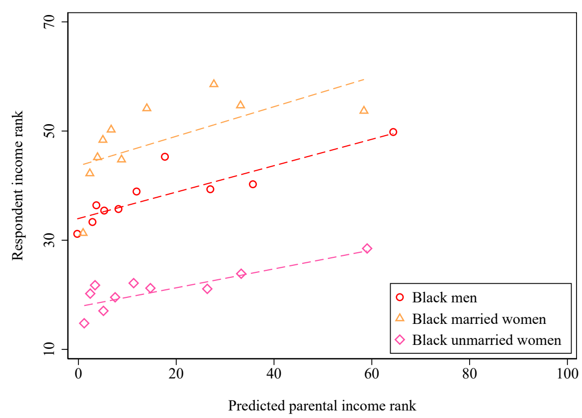
(b) IGE: 1940s–1950s



(c) Rank-rank correlation: 1910s–1920s



(d) Rank-rank correlation: 1940s–1950s

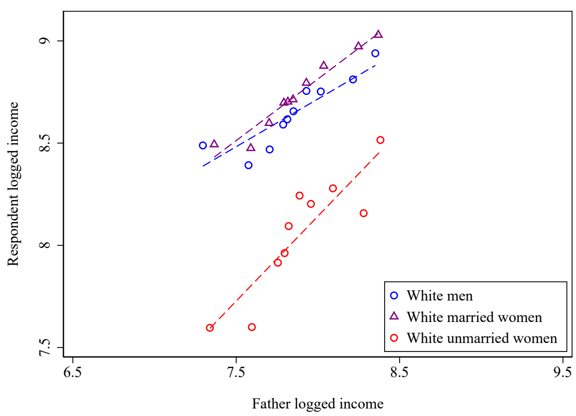


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

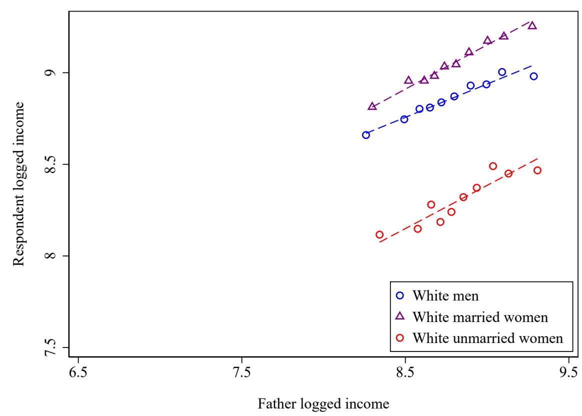
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure A.11: Mobility by marital status for white women and men, 1910s–1920s versus 1940s–1950s

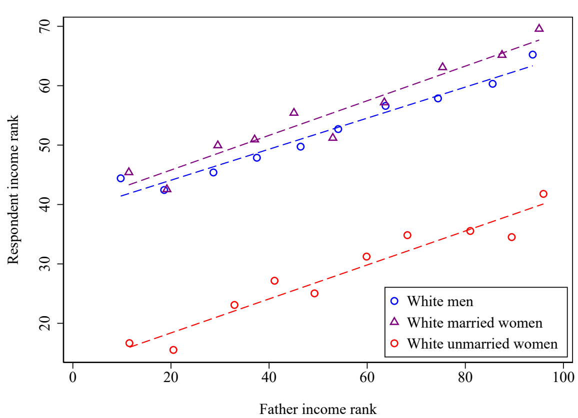
(a) IGE: 1910s–1920s



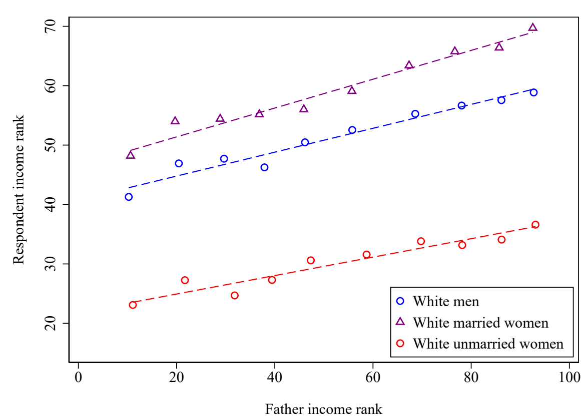
(b) IGE: 1940s–1950s



(c) Rank-rank correlation: 1910s–1920s



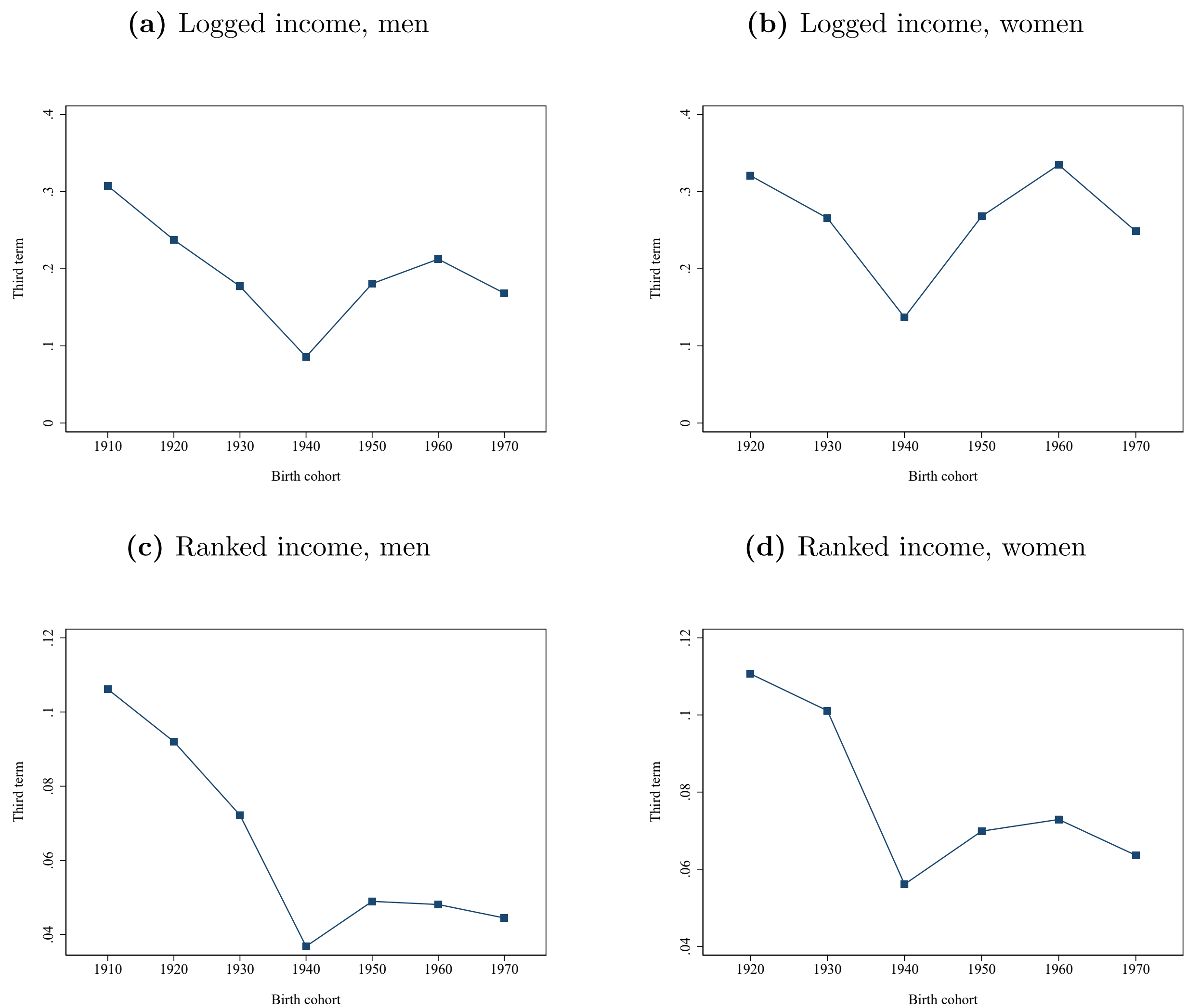
(d) Rank-rank correlation: 1940s–1950s



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure A.12: Black-white intergenerational convergence across cohorts using Census data (third term of decomposition)



Sources: 1920–2000 1% Census samples as well as the 2010 and 2019 American Community Survey (Ruggles *et al.*, 2021).

Notes: This figure plots the third term from equation (6) for both logged and ranked income using Census data. To identify the fathers of children born in a given cohort, we consider the two Censuses in which that cohort would be aged 0–18 (e.g., for the 1920s cohort, we look at fathers in the 1930 and 1940 Censuses). We then restrict the sample to men ages 30–50 with a child present in the household who was born in that cohort. To identify individuals born in a given cohort, we consider the two Censuses in which that cohort would be aged 30–50 and restrict the sample to U.S.-born individuals. Fathers are assigned predicted income using their occupation, race, and Southern location and using the closest Census to that year (i.e., fathers in the 1920–40 Censuses are assigned a 1940-based income prediction with 1936 fixes, and fathers from later Censuses are assigned an income prediction constructed using later Censuses). Adult children are assigned their total family income. Women in the 1910s cohort are excluded because their total family income cannot be accurately measured in the 1950 Census (because of the sample-line restriction). In both generations, we restrict the sample to individuals whose race is classified as white or Black. Fathers are re-weighted so that the subgroup population share is the same for both generations. In panels (c) and (d), adult children are ranked relative to other adult children of the same age and fathers are ranked relative to fathers of the same age.

Table A.1: Summary statistics, comparing survey respondents to Census respondents

	1910–1929		1930–1949		1950–1969	
	Census	Survey	Census	Survey	Census	Survey
Panel A: White Men						
Share of Men	0.90	0.91	0.90	0.90	0.87	0.86
Age	39.51	43.30	38.69	37.07	40.59	38.13
High school graduate	0.51	0.60	0.81	0.80	0.92	0.91
College graduate	0.12	0.15	0.27	0.27	0.30	0.32
Southern born/grew up	0.30	0.28	0.31	0.31	0.28	0.27
Resides in the South	0.28	0.27	0.33	0.31	0.34	0.31
Married	0.87	0.90	0.81	0.84	0.68	0.66
Widowed	0.01	0.01	0.00	0.01	0.00	0.01
Family income, 1950$	6,124	6,762	7,712	8,150	8,519	8,338
Respondent rank	52.57	51.64	53.20	52.62	52.20	53.46
Observations	195,091	12,281	214,612	11,956	297,783	6,737
Panel B: Black Men						
Share of Men	0.10	0.09	0.10	0.10	0.13	0.14
Age	39.41	44.57	38.54	37.65	40.13	37.98
High school graduate	0.21	0.28	0.62	0.60	0.85	0.82
College graduate	0.03	0.04	0.10	0.12	0.13	0.15
Southern born/grew up	0.86	0.84	0.77	0.73	0.60	0.62
Resides in the South	0.54	0.54	0.51	0.57	0.57	0.61
Married	0.75	0.82	0.63	0.69	0.50	0.53
Widowed	0.02	0.02	0.01	0.02	0.01	0.01
Family income, 1950$	3,817	4,257	5,738	6,086	6,318	6,132
Respondent rank	27.59	31.53	39.19	37.95	38.72	40.20
Observations	21,002	1,212	24,293	1,392	38,206	1,110
Panel C: White Women						
Share of Women	0.89	0.79	0.88	0.80	0.86	0.80
Age	39.50	40.97	38.74	38.56	40.64	38.34
High school graduate	0.55	0.66	0.81	0.82	0.94	0.93
College graduate	0.07	0.09	0.17	0.19	0.30	0.31
Southern born/grew up	0.30	0.31	0.31	0.31	0.28	0.27
Resides in the South	0.28	0.30	0.32	0.32	0.34	0.31
Married	0.86	0.86	0.79	0.77	0.70	0.65
Widowed	0.03	0.03	0.02	0.02	0.01	0.01
Family income, 1950$	6,033	6,865	7,527	7,738	8,469	8,064
Respondent rank	51.06	51.64	51.45	50.72	51.75	51.64
Observations	201,503	3,977	217,061	8,529	302,610	7,816
Panel D: Black Women						
Share of Women	0.11	0.21	0.12	0.20	0.14	0.20
Age	39.27	40.88	38.70	37.83	40.18	38.00
High school graduate	0.25	0.32	0.63	0.59	0.88	0.83
College graduate	0.04	0.05	0.09	0.11	0.17	0.15
Southern born/grew up	0.86	0.84	0.77	0.73	0.61	0.66
Resides in the South	0.55	0.60	0.51	0.57	0.58	0.64
Married	0.66	0.64	0.50	0.52	0.40	0.37
Widowed	0.08	0.09	0.06	0.06	0.03	0.03
Family income, 1950$	3,560	3,598	4,962	4,823	5,706	4,997
Respondent rank	23.72	23.81	32.87	29.25	34.65	32.68
Observations	24,081	1,065	29,808	2,158	45,166	1,899

Notes: Survey shares are based on the baseline sample of respondents ages 30–50 and are unweighted. We use the 1% samples of the 1960, 1980, and 2000 Censuses from Ruggles *et al.* (2021) and keep Census respondents born in the same years as survey respondents.

Table A.2: Summary Statistics in Panel Study of Income Dynamics

	1968 Men	1968 Fathers	With Child in Survey	Father's Income		
				1 year	5 years	10 years
Age	40.06	39.98	39.75	39.61	38.39	36.26
Black	0.09	0.08	0.07	0.07	0.06	0.06
HS educated	0.56	0.58	0.61	0.62	0.62	0.65
College edu.	0.16	0.17	0.19	0.19	0.20	0.20
Family income	10,986	11,109	11,363	11,399	11,346	11,264
Observations	1,765	1,472	1,077	959	802	558

Notes: This table uses the Panel Study of Income Dynamics dataset from 1968 through 2015. The first column considers all men ages 30–50 in the 1968 wave of the PSID. Column 2 restricts that sample to household heads with children present in the family unit, away from home, or in an institution. Column 3 further restricts the sample to those who were identified by the PSID as the biological or adoptive fathers of other survey respondents using the Family Identification Mapping System (FIMS). Columns 4–6 then restrict the sample to fathers with 1, 5, and 10 years of available income between the ages of 30–50, respectively, and whose children had at least one year of available income between ages 30–50.

Table A.3: IGE and rank coefficient, by birth cohort

(a) Intergenerational elasticity

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
IGE coefficient	0.750	0.586	0.487	0.424	0.571	0.823	0.773
	[0.039]	[0.018]	[0.014]	[0.019]	[0.025]	[0.042]	[0.077]
Lower & Upper Bound	(0.67, 0.83)	(0.55, 0.62)	(0.46, 0.51)	(0.39, 0.46)	(0.52, 0.62)	(0.74, 0.91)	(0.62, 0.92)
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

(b) Rank-rank coefficient

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Rank coefficient	0.369	0.312	0.298	0.245	0.245	0.271	0.242
	[0.020]	[0.010]	[0.009]	[0.010]	[0.010]	[0.013]	[0.021]
Lower & Upper Bound	(0.33, 0.41)	(0.29, 0.33)	(0.28, 0.32)	(0.23, 0.26)	(0.22, 0.27)	(0.24, 0.30)	(0.20, 0.28)
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

Notes: The IGE and rank-rank estimates—calculated using equations (2) and (3), respectively—are based on the baseline sample of respondents ages 30–50. "Lower & Upper Bound" refers to the 95% confidence interval of the corresponding estimate. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.4: Quantifying the decline between the 1910s and 1940s birth cohorts

	IGE		Rank-rank	
	(1) 1910–1940 difference	(2) Linear difference	(3) 1910–1940 difference	(4) Linear difference
Difference	-0.3259	-0.0067	-0.1247	-0.0039
	[0.0435]	[0.0012]	[0.0226]	[0.0006]
Observations	16,796	42,570	16,796	42,570

Notes: This table quantifies the decline in the IGE and rank-rank correlation between the 1910s and 1940s birth cohorts using the baseline sample of respondents ages 30–50. "1910–1940 difference" considers the difference between respondents born in the 1910s birth cohorts and those born in the 1940s cohorts, using specifications like equations (2) and (3), but allowing the slope and intercept to differ by cohort. The reported coefficient and standard error correspond to the interaction term, which measures the difference in the slope between the two cohorts. "Linear difference" considers all respondents born in the 1910s–1940s cohorts and models the decline in the slope linearly. Specifically, we run specifications in which we interact predicted parental income (or rank) with a variable that measures the number of years between a respondent's birth and 1911 (including birth-year fixed effects). To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.5: Predictive power (R-squared) of occupation, race, region, and education on 1940–1990 Census measures of logged household income

	Occupation	Race	Occ. × race	Race × South	Occ. × race × South	Occ. × race × South × edu.
1940 Census	0.27	0.05	0.28	0.09	0.31	0.34
1940 Census, 1936 fix	0.24	0.05	0.25	0.08	0.28	–
1960 Census	0.23	0.07	0.27	0.11	0.29	0.33
1970 Census	0.20	0.03	0.21	0.06	0.23	0.28
1980 Census	0.15	0.01	0.15	0.02	0.17	0.21
1990 Census	0.19	0.01	0.20	0.02	0.21	0.26

Notes: This table regresses predicted logged family income of fathers ages 30–50 on various characteristics and reports the R-squared from these regressions. Each row uses a different Census sample. The first and second columns regress income on (coarsened) occupation fixed effects and race fixed effects, respectively. The third column allows the occupation fixed effects to vary by the race of the father. The fourth column regresses income on race fixed effects that vary by Southern location. The fifth column regresses income on occupation × race × South fixed effects and the sixth column uses occupation × race × South × education fixed effects. The first row uses the 1940 Census for all occupations. The second row uses the 1940 Census in conjunction with farmers and self-employed workers from the 1936 Expenditure Survey (for whom educational level is not available).

Table A.6: IGE estimates by cohort and subgroup

	1910s	1920s	1930s	1940s	1950s	1960s	1970s
By sex:							
Men	0.65	0.49	0.44	0.32	0.52	0.68	0.57
	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.06)	(0.12)
Women	0.85	0.68	0.54	0.54	0.62	0.95	0.98
	(0.07)	(0.03)	(0.02)	(0.03)	(0.03)	(0.06)	(0.09)
By race:							
White	0.69	0.48	0.40	0.35	0.46	0.71	0.61
	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
Black	0.33	0.42	0.41	0.26	0.28	0.47	0.84
	(0.13)	(0.06)	(0.05)	(0.05)	(0.07)	(0.13)	(0.30)
By subgroup:							
White men	0.60	0.42	0.40	0.30	0.45	0.63	0.45
	(0.04)	(0.03)	(0.02)	(0.03)	(0.05)	(0.07)	(0.13)
+ white women	0.69	0.48	0.40	0.35	0.46	0.71	0.61
	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
+ Black men	0.68	0.51	0.42	0.35	0.50	0.71	0.66
	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
+ Black women	0.75	0.59	0.49	0.42	0.57	0.82	0.77
	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.08)

Notes: The IGE estimates are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.7: Rank-rank correlations by cohort and subgroup

	1910s	1920s	1930s	1940s	1950s	1960s	1970s
By sex:							
Men	0.36	0.29	0.28	0.22	0.24	0.24	0.21
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
Women	0.38	0.33	0.32	0.27	0.25	0.30	0.27
	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)
By race:							
White	0.31	0.25	0.25	0.21	0.20	0.23	0.20
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Black	0.13	0.41	0.42	0.27	0.19	0.18	0.27
	(0.10)	(0.06)	(0.07)	(0.05)	(0.04)	(0.05)	(0.07)
By subgroup:							
White men	0.30	0.25	0.24	0.20	0.20	0.23	0.17
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)
+ white women	0.31	0.25	0.25	0.21	0.20	0.23	0.20
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
+ Black men	0.34	0.28	0.27	0.22	0.22	0.24	0.22
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
+ Black women	0.37	0.31	0.30	0.24	0.25	0.27	0.24
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)

Notes: The rank-rank correlations are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.8: Differences in the 1910–1940 decline when looking at subgroups versus representative samples

	IGE	Rank-rank
Difference between 1910s and 1940s cohorts:		
White men	-0.295	-0.097
	(0.055)	(0.025)
All whites	-0.342	-0.100
	(0.058)	(0.026)
All whites & Black men	-0.336	-0.118
	(0.043)	(0.023)
Representative sample	-0.326	-0.125
	(0.043)	(0.023)
P-value (white men vs. rep. sample)	0.523	0.211
P-value (all whites vs. rep. sample)	0.703	0.036
P-value (all whites & Black men vs. rep. sample)	0.732	0.479
Linear decline using 1910s–1940s cohorts:		
White men	-0.0036	-0.0027
	(0.0018)	(0.0007)
All whites	-0.0053	-0.0028
	(0.0016)	(0.0007)
All whites & Black men	-0.0067	-0.0036
	(0.0012)	(0.0006)
Representative sample	-0.0067	-0.0039
	(0.0012)	(0.0006)
P-value (white men vs. rep. sample)	0.026	0.054
P-value (all whites vs. rep. sample)	0.240	0.00086
P-value (all whites & Black men vs. rep. sample)	0.942	0.182

Notes: The top panel considers the difference between respondents born in the 1910s birth cohorts and those born in the 1940s cohorts, using specifications like equations (2) and (3), but allowing the slope and intercept to differ by cohort. The reported coefficient and robust standard error correspond to the interaction term, which measures the difference in the slope between the two cohorts. In the second panel, we consider all respondents born in the 1910s–1940s cohorts and model the decline in the slope linearly. Specifically, we run specifications in which we interact predicted parental income (or rank) with a variable that measures the number of years between a respondent's birth and 1911 (including birth-year fixed effects). In both panels, the p-values correspond to a test of whether the two coefficients (using white men vs. representative samples, using all whites vs. representative samples, and using whites + Black men vs. representative samples) are equal using seemingly unrelated regressions.

B Robustness of 1910–1940 mobility increase

In this Appendix, we present alternative estimates of intergenerational mobility over the 20th century to consider the robustness of the full-population decline in persistence we document between the 1910s and 1940s cohorts. For more details on the construction of alternative income measures, we refer the reader to Appendix E.

B.1 Recall-adjusted estimates of mobility

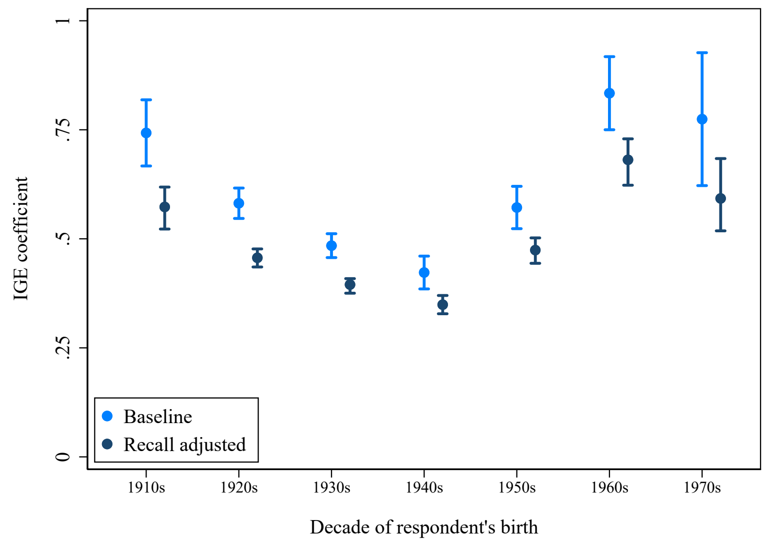
In this subsection, we perform an exercise to consider whether our main result might be driven by recall deteriorating over time (i.e., the decline in persistence between the 1910s and 1940s cohorts stemming from measurement error, rather than true changes in mobility). Specifically, one concern might be that the distribution of fathers' occupations is changing during this time period (e.g., away from agricultural occupations) in such a way that survey respondents might have a more difficult time recalling their fathers' occupations. To consider this possibility, we turn to the sample of households heads in the PSID for whom we have both retrospective answers about fathers' occupations as well as their father's self-reported answers in earlier survey waves (described in detail in Section C.3).

We begin by using this sample to calculate a matrix that denotes the likelihood that a respondent who reports occupation i for their father has their father report occupation j. As an example, among respondents who said their fathers were accountants, 75% of the corresponding fathers reported being accountants, while 15% and 6% of fathers said they were businessmen and clerical workers, respectively. This matrix thus allows us to get a sense of which occupations are easier to recall and importantly, the types of mistakes that are commonly made for each occupation.

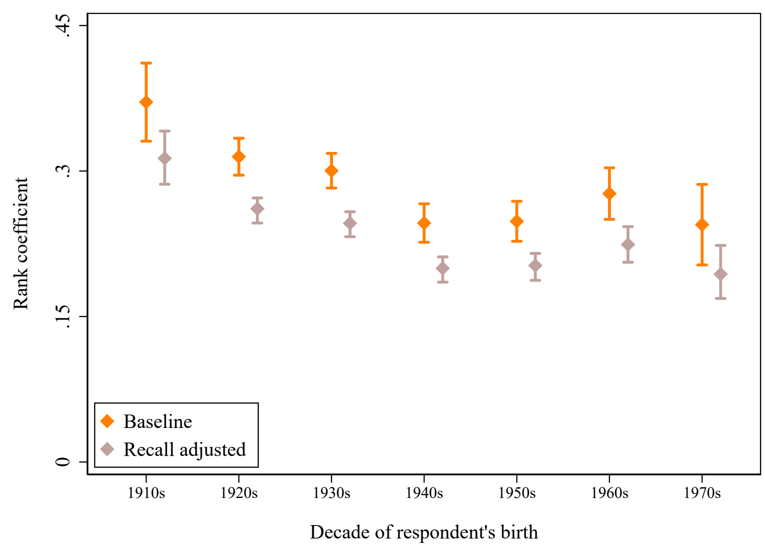
We then return to the baseline sample of respondents from our surveys and use this matrix to alter the occupation of a random share of fathers. Using the accountant example above, we allow a random 75% of respondents who said their fathers were accountants to keep their answer, but we change the fathers' occupations for the remaining 25%. Importantly, the changes we make reflect the distribution of mistakes in the PSID (i.e., 15% and 6% would be changed to businessmen and clerical workers, respectively). We then predict parental income using these recall-adjusted occupations as well as race and Southern location, and we re-calculate the estimates of mobility. We repeat this exercise 200 times, so that we change the occupation of a different share of fathers and allow for different types of mistakes in recall. Appendix Figure B.1 plots the baseline estimates of mobility alongside the estimates from this simulation. The recall-adjusted estimates are generally attenuated, but the main finding of an increase in mobility in the first half of the 20th century is unchanged.

Figure B.1: Mobility measures by birth decade, robustness to recall bias

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we allow for the possibility of mistakes in recall. Specifically, we use the matrix of mistakes from the PSID to change the occupation of a random share of fathers, and we then re-estimate the IGE and rank-rank correlation. We repeat this process 200 times. The point estimate in the second series plots the average value obtained, while the upper and lower bound of the confidence intervals plot the values corresponding to the 2.5$^{\text{th}}$ and 97.5$^{\text{th}}$ percentiles of the distribution of estimates. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

B.2 Unobserved within-cell variance

By construction, our baseline approach assigns each respondent a childhood income based on the mean family income in a *father occupation* × *race* × *South* cell from the appropriate Census or other auxiliary dataset, and it thus ignores within-cell variation. As noted, to the extent that some within-cell variation in a single Census year is merely transitory, excluding within-cell variation will better approximate permanent average childhood income. But to the extent that within-cell variation reflects systematic income differences missed by *father occupation* × *race* × *South*, our measure of predicted childhood income will bias us away—in an *a priori* unclear direction—from the persistence measure of interest.

We begin to address this concern by re-estimating predicted childhood income on subsets of our data that include more information on childhood background. As noted earlier, for more than half of our surveys, respondents were asked about their fathers' education. We can thus re-calculate measures of predicted childhood income using father' occupation, race, Southern location, *and* father's education for this subsample. *A priori*, father's education is one of the most likely factors to create systematic deviation from our *father occupation* × *race* × *South*-based mean family income. Indeed, adding information about father's education to our standard approach significantly increases predictive power (e.g., in 1960, the R-squared rises from 0.29 to 0.33; see Appendix Table A.5).

The first panel of Appendix Figure B.2 compares the IGE with the original predictions at the *father occupation* × *race* × *South* level—using the baseline sample as well as the sub-sample of respondents who are asked fathers' education—to the IGE using these augmented measures in this restricted sub-sample. Of the 15 surveys in our baseline sample, 12 include information about a father's educational attainment, representing nearly 80 percent of the baseline sample. The three series are very comparable in both levels and trends: in particular, they show the marked decline between the 1910s and 1940s birth cohorts. The second panel shows that the decline in the rank-rank measure is also unchanged by augmenting the income prediction with father's education. Thus, when we significantly improve our childhood income measures with an important predictor, the trends in mobility remain unchanged, providing some reassurance that systematic, unobserved within *father occupation* × *race* × *South* cell variation in income is not driving our results.

In Figure B.3, we again focus on the sub-sample of respondents that provide information about their father's educational attainment, showing how the IGE and rank-rank estimates change as we add more detail to the parental family income prediction. (This figure is analogous to Appendix Figure A.6, but it uses a sub-sample of respondents and specifically considers the importance of education as a predictor.) The first series only allows the measures to vary by occupation, but the second, third, and fourth series successively incorporate detail on the race, region, and educational attainment of the father. We see that the decline in the IGE and rank-rank estimates between the 1910s and 1940s cohorts is remarkably unchanged despite adding important information to the income prediction.[43] Table B.1 summarizes the results from this exercise, quantifying the decline between the 1910s and 1940s birth cohorts using these alternative ways of approximating parental income.

Similar to the previous exercise, for nine of our fifteen surveys, we have information on the *Census region* of birth or childhood. For respondents in this subsample, we can thus predict childhood income at the *father occupation* × *race* × *Census region* level, instead of collapsing region to South versus other. Appendix Figure B.4 shows how the baseline estimates vary as we transition to this subsample and to measures of predicted childhood income that vary at the regional level. The decline in persistence and the overall trends in mobility remain unchanged.

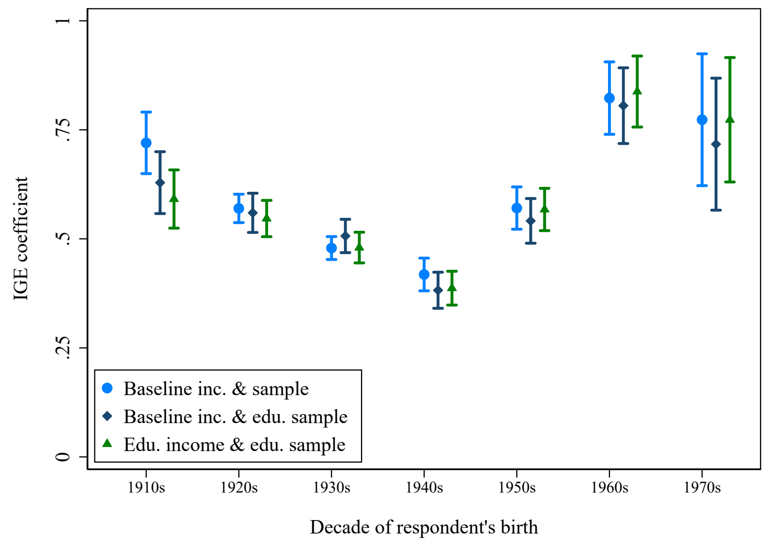
While we have shown robustness of our main result to using a richer set of predictors whenever our data allow, we now take a different approach to assessing the extent of potential bias due to unobserved within-cell variance. Essentially, we ask, even if we assume that *all* within-cell variance reflects true, permanent differences in childhood income, can we still detect a decline in intergenerational persistence between the 1910s and 1940s cohorts?

For each *father occupation* × *race* × *South* cell, we observe the actual family income values of all observations in that cell (i.e., from the appropriate Census or 1936 Expenditure Survey). We begin with a multiple-imputation estimation (see, e.g., Little and Rubin, 2019; Rubin, 1987), which uses the observed distribution of data in the Census to calculate various plausible values for the respondents' childhood income (conditional on the father's occupation, race, and Southern location). Note that for these exercises, we use the 1936 Survey and 1940 Census to impute parental income for respondents born in the 1910s–1930s birth cohorts (unlike our baseline approach). The first series of Appendix Figure B.5 shows the baseline IGE results and the second series shows the multiple-imputation-based results. While the multiple-imputation-based results are unsurprisingly attenuated (shown formally in Cortes-Orihuela *et al.* (2022)) and thus make it harder to detect changes over time, we can nonetheless see a decline in persistence between the 1910s and 1940s cohorts. Similarly, when we simply draw directly and non-parametrically from the empirical distribution of all observed family income values in a cell (the third series), we find similar results.

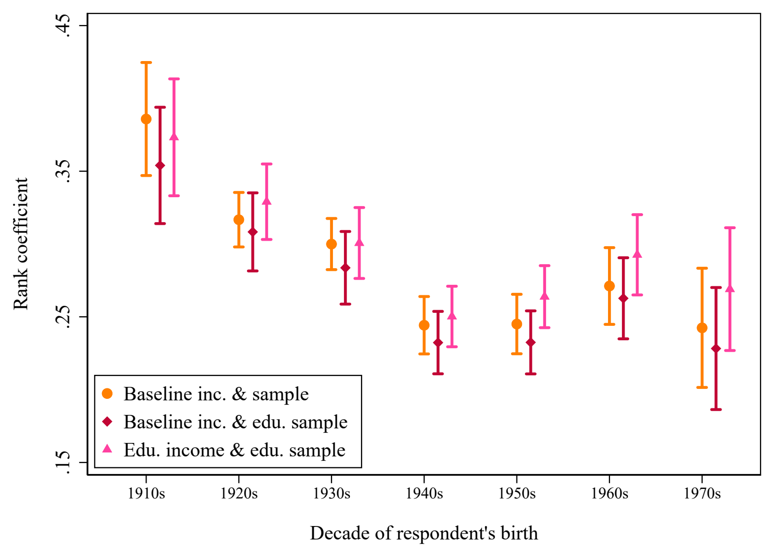
[43] An increase in the level of the rank-rank coefficient is unsurprising in this setting given that incorporating additional information into the income prediction likely increases the covariance between the rank of children and the rank of their fathers, while leaving the variance of the father's ranks fixed (by construction, given that ranks range from 0 to 100).

Figure B.2: Mobility by birth decade, adjusting predicted parental income for education

(a) Intergenerational elasticity



(b) Rank-rank coefficient

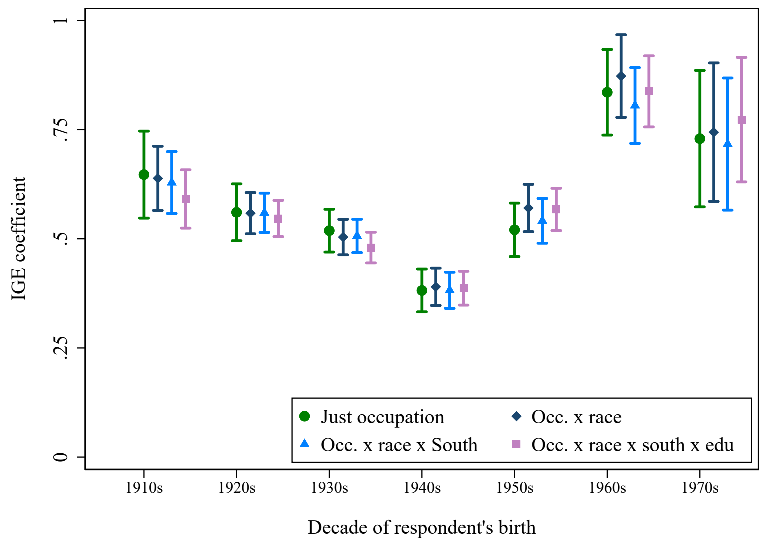


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

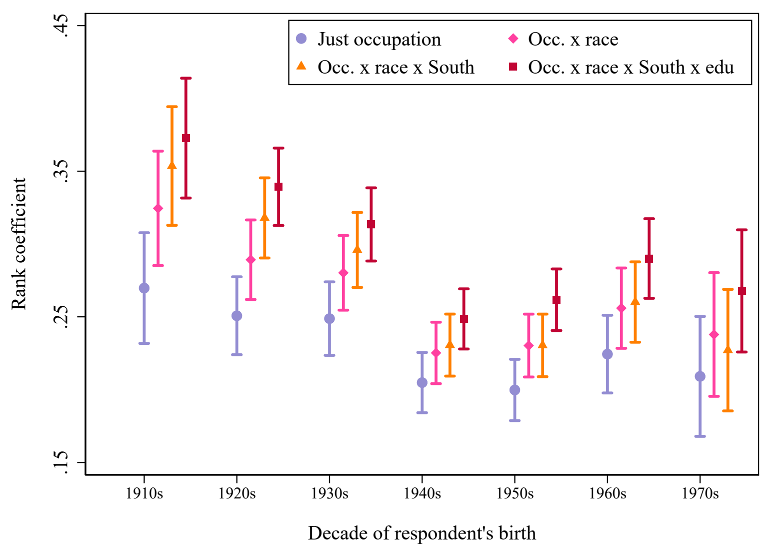
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we continue to use the baseline income prediction, but restrict the sample to respondents ages 30–50 who provided information on their fathers' education (available in 12 of the 15 surveys). In the third series, we use this smaller sub-sample in conjunction with income predictions that vary by a father's educational attainment. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* $\times$ *sex* shares.

Figure B.3: Mobility measures by birth decade, adding detail to parental income prediction

(a) Intergenerational elasticity



(b) Rank-rank coefficient

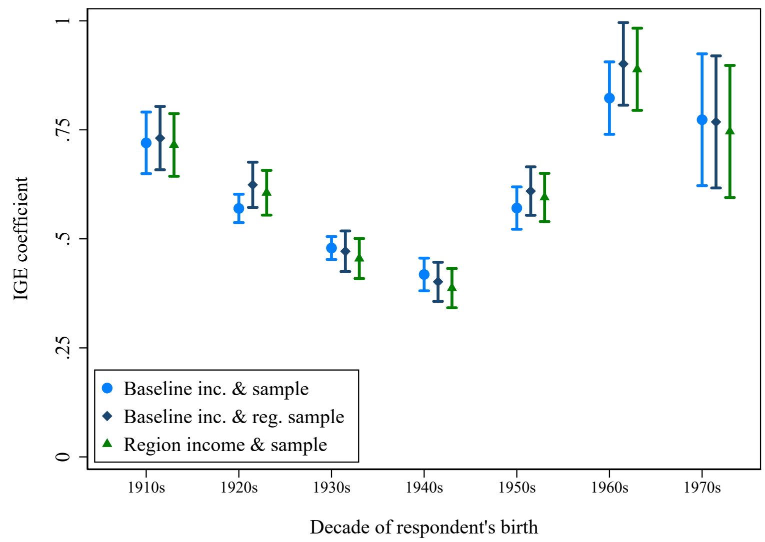


Sources: This figure combines 12 surveys in which respondents provide information on a father's educational attainment. Further detail is available in Appendix E.

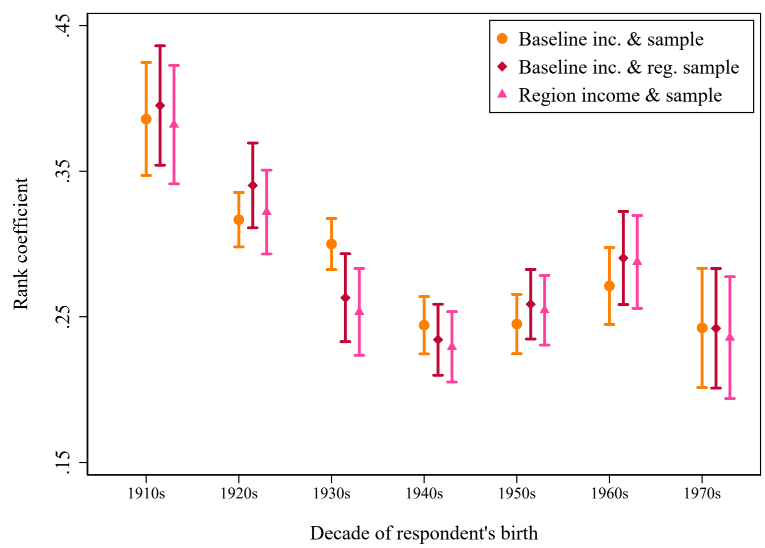
Notes: The IGE and rank-rank are based on the sample of respondents ages 30–50 who provided information about their fathers' educational attainment. The first series the parental income prediction only varies by a father's occupation. The second series allows predicted income to vary by father's occupation and race. The third series allows predicted income to vary by father's occupation, race, and Southern residence. The fourth series allows predicted income to vary by father's occupation, race, Southern residence, and father's educational level. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in this sample so that they have representative *race* × *sex* shares.

Figure B.4: Mobility measures by birth decade, robustness to regional differences in parental income

(a) Intergenerational elasticity



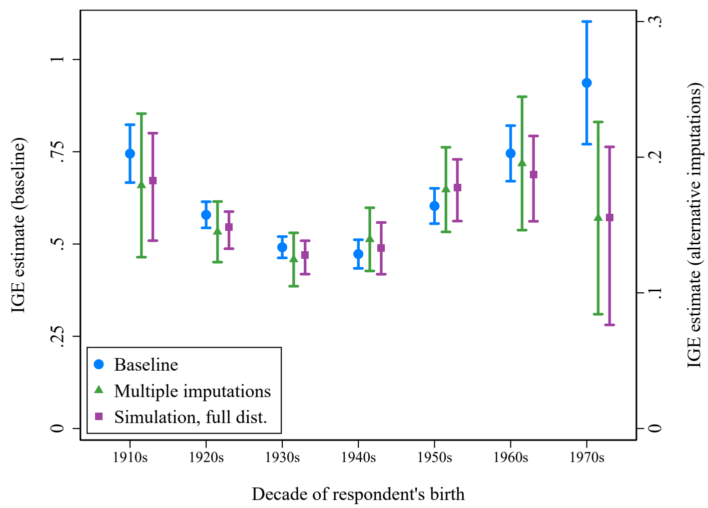
(b) Rank-rank coefficient



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we continue to use the baseline income prediction, but restrict the sample to respondents ages 30–50 who provided more detailed information on their fathers' region (available in 9 of the 15 surveys). In the third series, we use this smaller sub-sample in conjunction with income predictions that vary by a father's region (using the four Census regions). To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.5: Intergenerational elasticity by cohort, comparing baseline results with alternative imputation approaches



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series uses our baseline income prediction based on the average household incomes at the *occupation* × *race* × *South* level. The second and third series then allow for more variability in the income prediction. The second series imputes parental logged income based on occupation, race, and Southern residence using a multiple imputation approach with 100 imputations (see, e.g., Little and Rubin, 2019; Rubin, 1987; Schafer, 1997). In the third series, the parental generation is randomly assigned an income value within their *occupation* × *race* × *South* cell before estimating the IGE. This process is repeated 100 times, and the upper and lower bound of the confidence intervals plot the values corresponding to the 2.5$^{\text{th}}$ and 97.5$^{\text{th}}$ percentiles of the distribution of estimates. In all series, we predict parental income using the nearest Census to the respondent's childhood. Specifically, for the 1910–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively.

Table B.1: Quantifying the 1910–1940 decline in the IGE and rank coefficient as we add information about fathers to their family income prediction

(a) Intergenerational elasticity

	(1) Occupation	(2) Occ. x race	(3) Occ. x race x South	(4) Occ. x race x South x edu
Parental income	0.6111***	0.6210***	0.6227***	0.5887***
	[0.0438]	[0.0326]	[0.0311]	[0.0286]
Inc. x (Year-1910)	-0.0035**	-0.0045***	-0.0047***	-0.0038***
	[0.0017]	[0.0014]	[0.0013]	[0.0012]
Observations	31,108	31,108	31,108	31,108

(b) Rank-rank coefficient

	(1) Occupation	(2) Occ. x race	(3) Occ. x race x South	(4) Occ. x race x South x edu
Parental rank	0.2870***	0.3407***	0.3818***	0.3999***
	[0.0169]	[0.0174]	[0.0177]	[0.0172]
Rank x (Year-1910)	-0.0022***	-0.0032***	-0.0043***	-0.0042***
	[0.0007]	[0.0007]	[0.0007]	[0.0007]
Observations	31,108	31,108	31,108	31,108

Notes: All estimates are based on the sample of respondents ages 30–50 who provided information about their fathers' educational attainment and were born before 1950. Each column varies the information used to predict parental income. In all specifications, we interact father family income (or rank) with a variable that measures the number of years between a respondent's birth year and 1911. All specifications include birth-year fixed effects. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

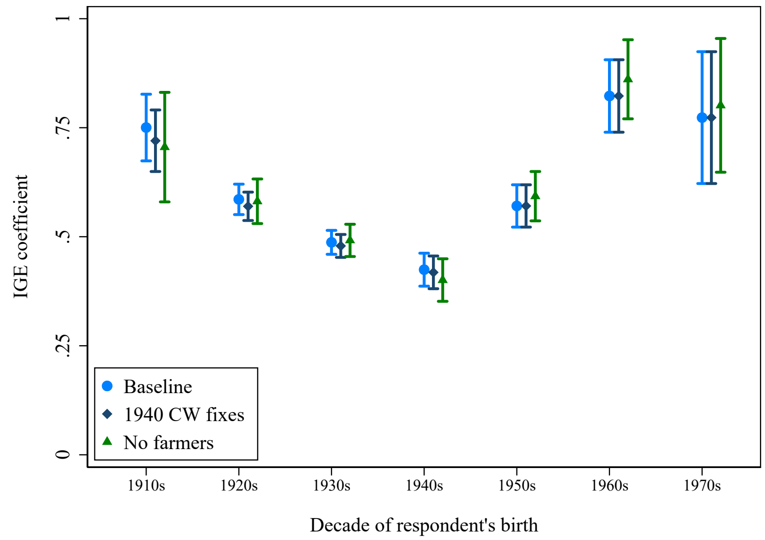
B.3 Farmer income

Our baseline measure of parental income acknowledges the difficulty in estimating farmer (and self-employed) income in the first half of the 20th century using conventional survey or Census data. We therefore use the 1900 Census of Agriculture (for farmers) as well as the 1936 Expenditure Survey (for farmers and self-employed) given the limitations of the 1940 Census for these groups.

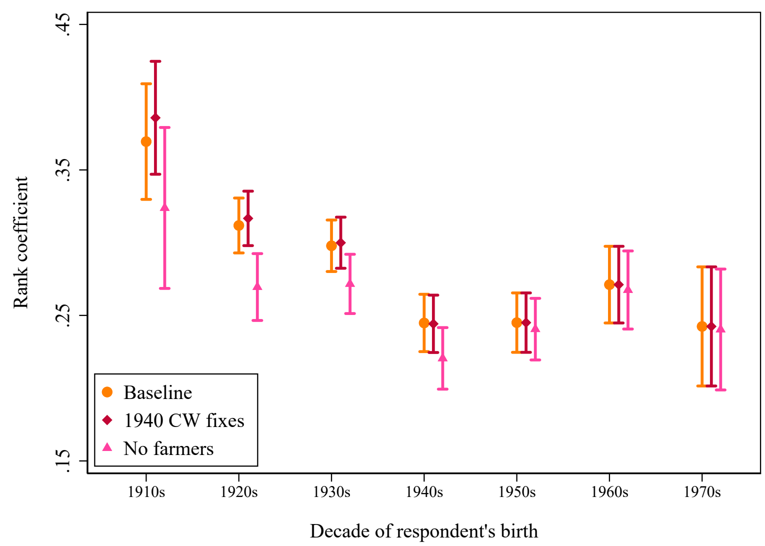
In this section we perform additional checks related to the prediction of income for farmer fathers. First, we follow Collins and Wanamaker (2022) and impute the income of farmers in 1940 using the income of farm laborers in 1940 as well as the ratio of farmer-to-farm-laborer income in the 1960 Census. We adjust the income of self-employed businessmen in 1940 using a similar approach. Appendix Figure B.6 shows that our main result of a marked decline in persistence remains unchanged when we follow this differing methodology. Second, also shown in Appendix Figure B.6, we simply drop farmers to ensure that our mobility patterns are not being entirely driven by this sizable population for which it is hard to estimate childhood income. Again, the conclusion that mobility increased substantially between the 1910s and 1940s birth cohorts is unchanged.

Figure B.6: Mobility by birth decade, incorporating various adjustments for farmers

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

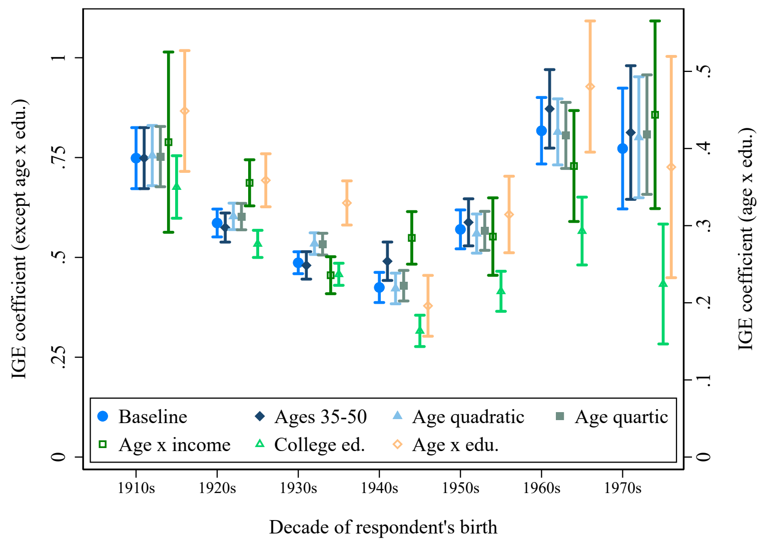
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline approach for predicting parental income (see Section 3.2 for more details). The second series uses the same methodology as Collins and Wanamaker (2022) to estimate the parental income of farmer and self-employed fathers in the 1910s–1940s birth cohorts. In both the first and second series, the IGE and rank-rank estimates are based on the baseline sample of respondents aged 30–50. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. The third series excludes all respondents whose fathers work in agricultural occupations; the remaining respondents are re-ranked in this sub-sample, and weights are constructed so that each birth cohort in this sub-sample also has representative *race* × *sex* shares.

B.4 Life-cycle bias

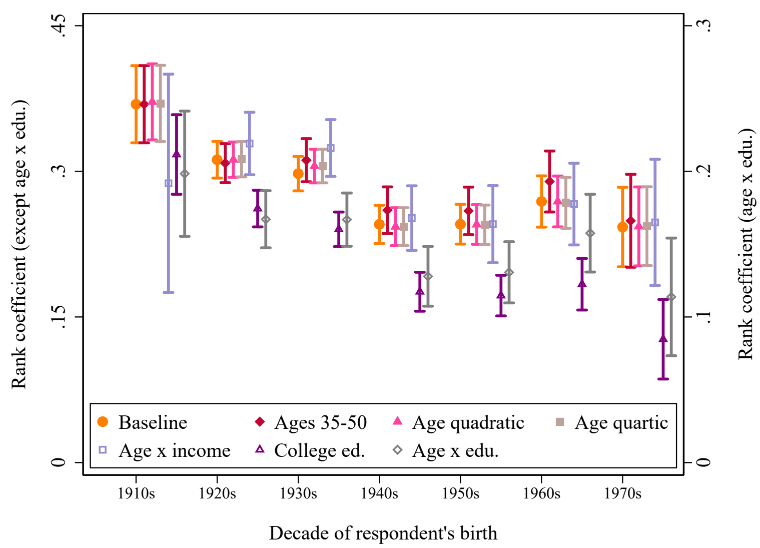
Various papers in this literature have noted that using current income to proxy for the adult children's lifetime earnings may bias estimates of mobility (see, e.g., Haider and Solon, 2006; Lee and Solon, 2009; Nybom and Stuhler, 2016). Recall we already restrict the sample to be observed at ages 30–50 to limit life-cycle effects. However, Appendix Figure B.7 shows the robustness of the main result to alternative specifications and sample restrictions that attempt to further minimize this life-cycle bias. We begin by restricting the sample to older respondents (i.e., those aged 35–50) whose total family income may be better approximations of their lifetime earnings. We also consider how the results change when we include quadratic age controls. We also follow Lee and Solon (2009) and include quartic age controls (relative to age 40) as well as interaction terms of the quartic terms with parental income. Finally, acknowledging the possibility of heterogeneous age-earnings profiles, we follow Nybom and Stuhler (2016) and include controls for college education, and we allow income to grow differentially with age depending on a respondent's education. Although none of these exercises can definitively eliminate life-cycle bias (i.e., cohorts likely have different life-cycle trajectories, and thus likely suffer from different degrees of bias), it is reassuring that the decline in persistence between the 1910s–1940s cohorts remains marked in all of these specifications.

Figure B.7: Mobility measures by birth decade, robustness to life-cycle bias

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series reproduces the baseline estimates in Figure 1. The second series restricts the sample to respondents ages 35–50. The third series includes a quadratic polynomial in age. The fourth series uses quartic polynomial in adult children's age minus 40. The fifth series uses the quartic polynomial in age as well as interactions of the adult child's age quartic with parental income. The coefficients displayed from this specification represent the age-40 measure of mobility as each cohort passes through that age. The sixth series returns to the quadratic specification, but controls for the respondent's college education. The seventh series (secondary y-axis) uses a quadratic polynomial in age and allows these coefficients to vary by a respondent's level of education (using six categories of educational attainment, as discussed in Appendix E).

B.5 Other robustness checks

Table 2 shows that the information needed to calculate predicted childhood income is not available for all respondents. This situation arises almost always because the respondent does not report father's occupation (presumably because she doesn't remember, chooses not to report it, or grew up without her father). In Appendix Figure B.8 we incorporate the roughly 2,000 respondents whose fathers were present but not working (e.g., retired, unemployed). The next series in this figure instead incorporates the roughly 4,750 respondents who provided information about their mother's occupation, assigning them measures of predicted childhood income based on mothers who were household heads in the 1940–1990 Censuses. Not surprisingly, the estimates hardly change for the 1910s–1940s cohorts, but from the 1950s onward we find more precise and slightly smaller persistence estimates (so the increase in the IGE from the 1950s to 1970s is smaller).[44] We also show robustness to a particularly extreme assumption about respondents who provided no information about either parent: that their household had zero income, or in other words, that their family had the lowest possible rank for predicted childhood income. Under all these various assumptions, the overall shape of the IGE and rank-rank estimates are unchanged over our sample period.

Next, one concern is that the decline in mobility might stem from pooling various different surveys across multiple years in our baseline sample. Appendix Figure B.9 shows that the results are quite stable after incorporating survey-by-year fixed effects (i.e., fixed effects for each of our fifteen surveys and a separate control for each survey-year for the surveys that span multiple years). Table 2 also shows that the share of survey respondents with income values that are top coded varies by cohort. Appendix Figure B.10 thus drops the three surveys that have the highest share of top coding in the adult children's generation and confirms that the 1910–1940 result is robust to their exclusion.

Appendix Figure B.11 shows the robustness of the main result to alternative weighting schemes: namely, using the provided survey weights without any additional adjustments for population shares and using no weights at all. We also consider robustness to re-weighting survey weights so that each birth cohort has representative *race* × *sex* × *education* × *age* shares where education refers to having a high school education and age refers to five-year age bins. In all of these checks, we continue to find an increase in mobility between the 1910s and 1940s cohorts.

One concern with this long-term view of mobility might be that mortality rates were high for early cohorts (for men and Black men in particular; see, e.g., Preston *et al.*, 2003), so that selection into the sample—i.e., remaining alive at ages 30–50—might be changing over time. If individuals who grow up in poorer households are those with higher mortality rates, then if anything, this decline in mortality would likely

[44] The second panel of Appendix Figure B.8 shows that the rank-rank correlation barely changes with this expanded sample, confirming the important role of the variance of parental income in inflating the IGE in our baseline sample.

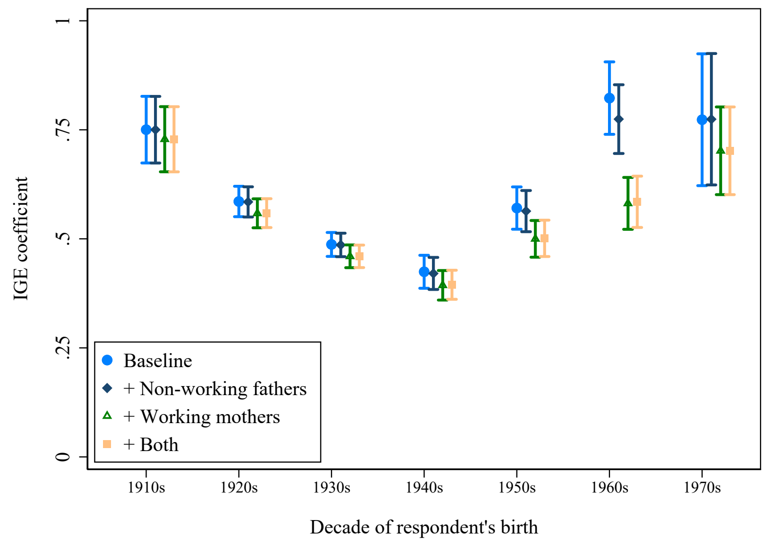
bias us against finding a decline in intergenerational persistence between the 1910s and 1940s birth cohorts (in the earliest cohorts, individuals born poor who are still alive at prime age would likely be positive selected, which would bias intergenerational persistence downward). Nevertheless, we still take seriously this consideration and Appendix Figure B.12 compares our baseline results to those that focus on individuals ages 30–45 and 30–40, both of which are less affected by differential mortality rates. The rise in mobility is unchanged in these sub-samples. Nonetheless, selection into fertility and mortality remains a concern for all intergenerational mobility estimates using historical data, including our recall-based method.

Another notable demographic change that took place in the 20th century was the change in household size for the mid-century cohorts (i.e., the Baby Boom). Appendix Figure B.13 adjusts the family income of adult children by self-reported household size and adjusts predicted parental income by household size in that *father occupation* × *race* × *South* cell when the respondent is ten years old. We see that this adjustment does not affect the rise in mobility between the 1910s and 1940s birth cohorts.

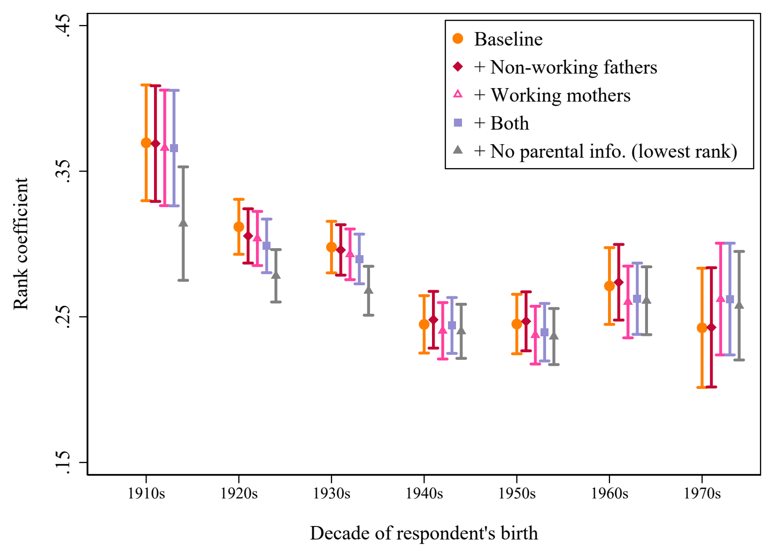
In Figure B.14 we present results that use alternative data sources for or alternative ways of predicting parental income. Recall that our baseline measure of predicted parental family income relies on measures of household income from various Censuses and other sources in the 20th century. We alter this preferred measure in a number of ways. First, instead of using measures of *household* income, we simply use individual-level wage information for fathers ages 30–50 to estimate parental income (second series). This measure more closely mirrors the construction of predicted income in related studies. Next, we use data from the closest Census to the survey respondents' childhood to approximate parental income (i.e., the 1940 Census with 1936 adjustments for the 1910s–1930s cohorts and the 1950–1980 Censuses for the 1940–1970 cohorts, respectively). Third, we return to our baseline measure and allow fathers with more children to receive greater weight when calculating average incomes at the *occupation* × *race* × *South* cell (i.e., weighting fathers by the number of children younger than 18 present in the household). Finally, we compare our results to simply using the 1950 IPUMS *occscore* variable (which, recall, pooled all adults and computed the nation-wide median income for each occupation). In all of these exercises, the decline in the IGE remains salient and in all but the last series, the *u*-shape of the IGE remains stark. The levels of the rank-rank measures all look quite comparable to each other, with the exception being the series that uses the 1950 IPUMS *occscore* variable, thus highlighting the value of incorporating information about race and region when predicting parental income.

Figure B.8: Mobility measures by birth decade, incorporating respondents with missing predicted parental income

(a) Intergenerational elasticity



(b) Rank-rank coefficient

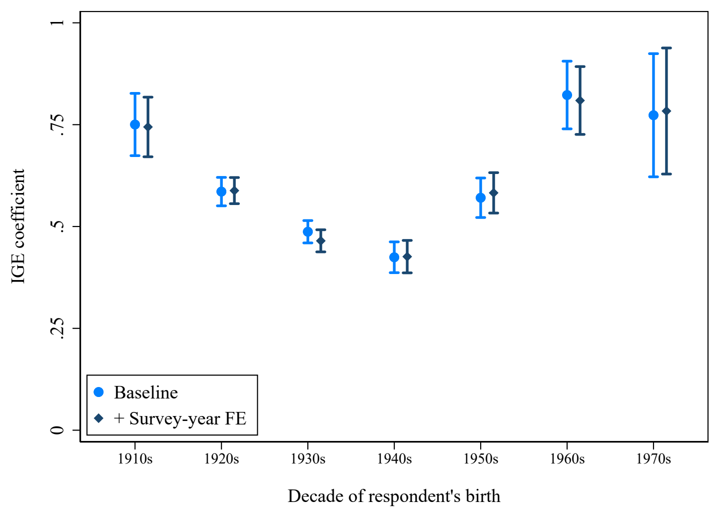


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

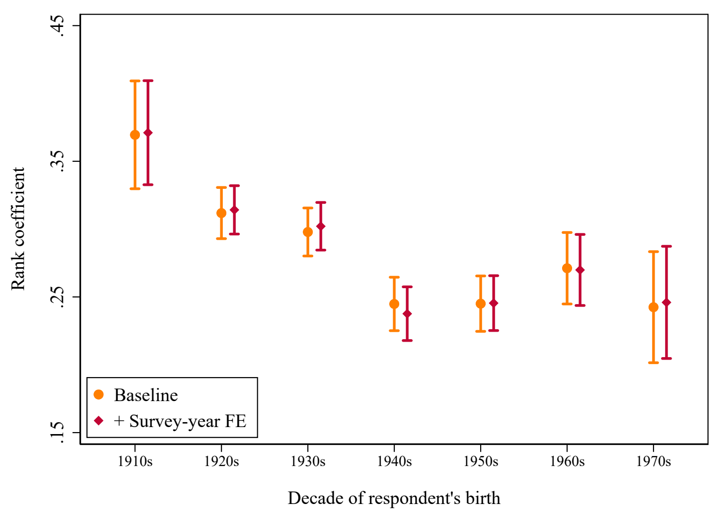
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction (see Section 3.2 for more details). In the second series, we include respondents from the 15 surveys whose father was present but not working (e.g., unemployed, retired). In the third series, we instead include respondents from the 15 surveys who provided information about their mother's occupation. The fourth series includes respondents who provided information about their non-working father *or* about their mother's occupation (and if both pieces of information were provided, we predict family income based on the mother's occupation). More detail on the income prediction for non-working fathers and working mothers are in Appendix E. In the final series of the bottom panel, we assign all U.S.-born respondents ages 30–50 in our 15 surveys that still have missing predicted parental income the lowest possible rank (i.e., assuming that their household had zero income in childhood).

Figure B.9: Mobility by birth decade, robustness to including survey × year fixed effects

(a) Intergenerational elasticity



(b) Rank-rank coefficient

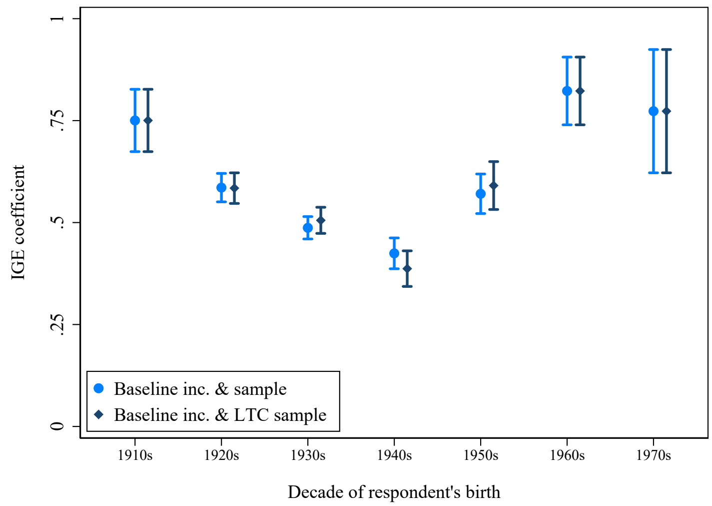


Sources: This figure combines 12 different surveys, which are described in Section 2 and in further detail in Appendix E.

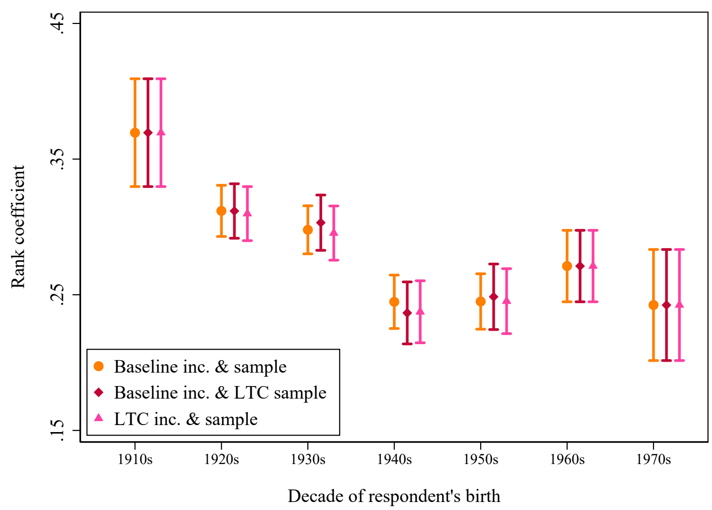
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and measures of predicted parental family income. In the second series, we include survey-by-year fixed effects in the specification. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.10: Mobility by birth decade, robustness to excluding surveys with high share of top coding of respondent income

(a) Intergenerational elasticity



(b) Rank-rank coefficient

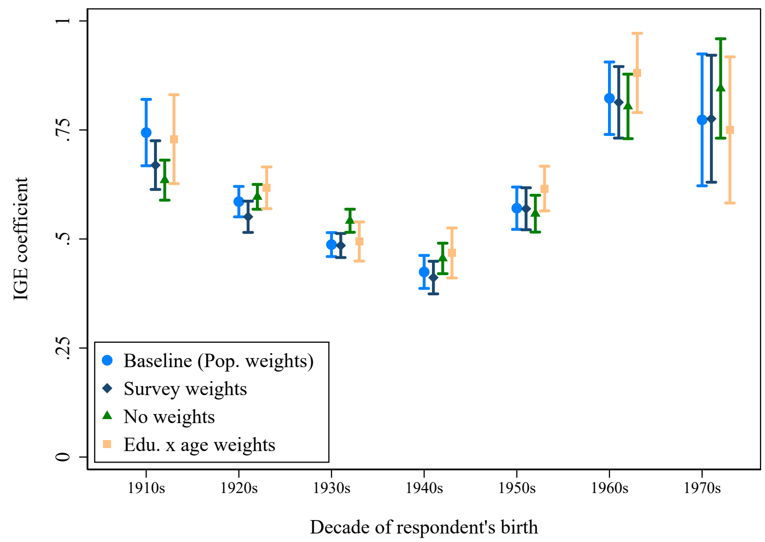


Sources: This figure combines 12 different surveys, which are described in Section 2 and in further detail in Appendix E.

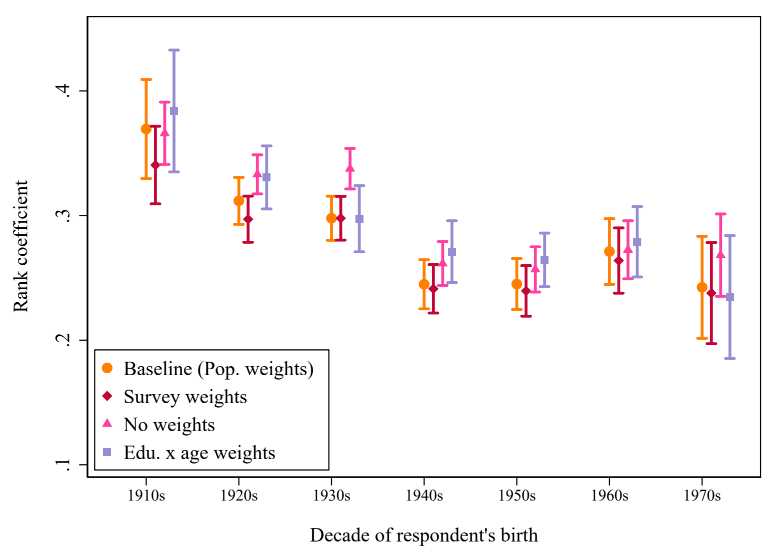
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and measures of predicted parental family income. In the second series, we continue to use the baseline income measures, but restrict the sample to the twelve surveys with the lowest shares of top coding of respondents' income (i.e., excluding respondents who are not in the National Fertility Study, the National Survey of Families and Households, and the National Longitudinal Survey of Young Women). In the third series of panel (b), we use this smaller sub-sample and as well as measures of ranked parental income that are only based on this sub-sample. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.11: Mobility by birth decade, robustness to weights

(a) Intergenerational elasticity



(b) Rank-rank coefficient

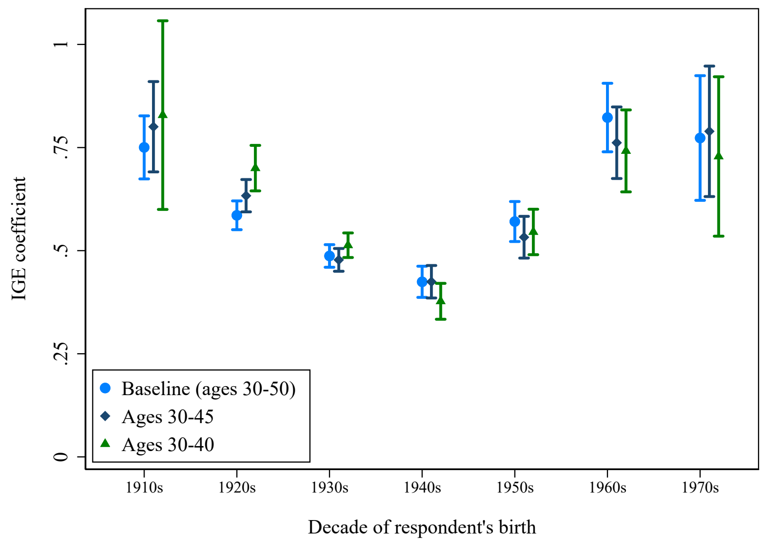


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50. The first series in both panels reproduces the main IGE and rank-rank estimates using the baseline population-adjusted weights. In other words, in the first series, we re-weight survey weights so that each birth cohort has representative *race* $\times$ *sex* shares. The second series uses the provided survey weights (or a weight of one when no survey weight is available). The estimates from the third series are unweighted. The fourth series uses alternative population-adjusted weights in which the *race* $\times$ *sex* $\times$ *education* $\times$ *age* shares vary over time. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details).

Figure B.12: Mobility by birth decade, robustness to age group

(a) Intergenerational elasticity



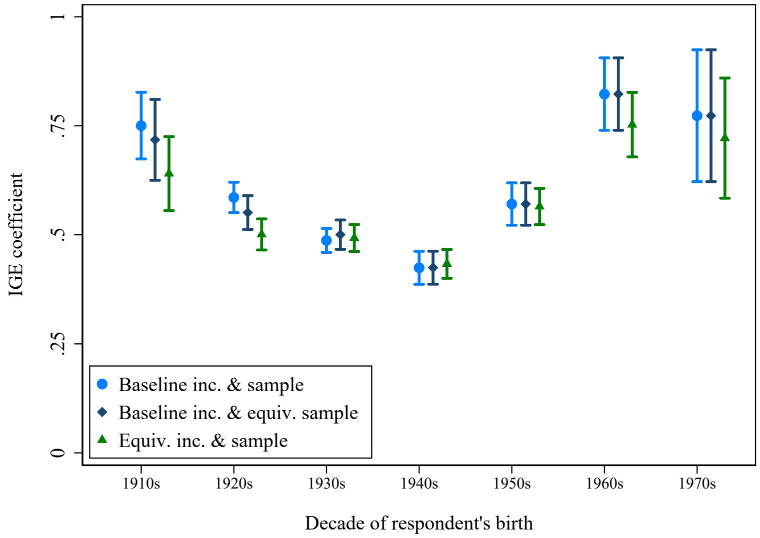
(b) Rank-rank coefficient

Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

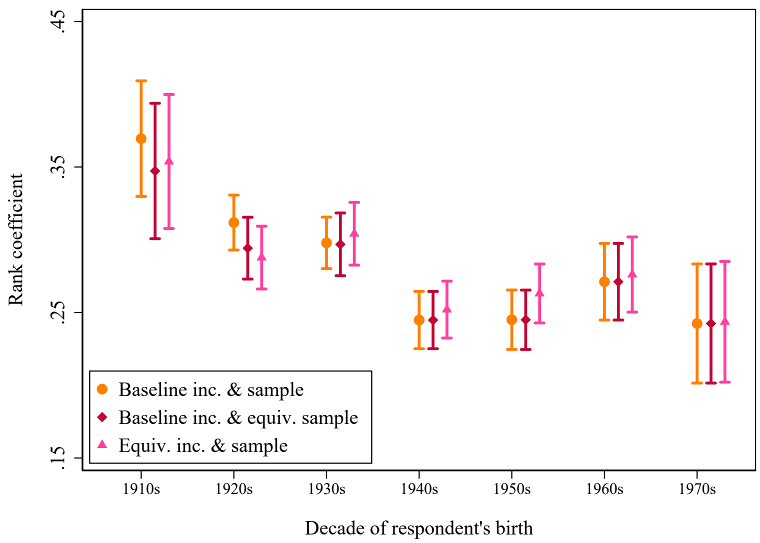
Notes: The estimates are based on the baseline sample of respondents aged 30–50. The first series in both panels reproduces the main IGE and rank-rank estimates using the baseline population-adjusted weights. In the second series, we restrict the sample to respondents aged 30–45. The third series further restricts the sample to those aged 30–40. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details).

Figure B.13: Mobility by birth decade, robustness to family size

(a) Intergenerational elasticity



(b) Rank-rank coefficient

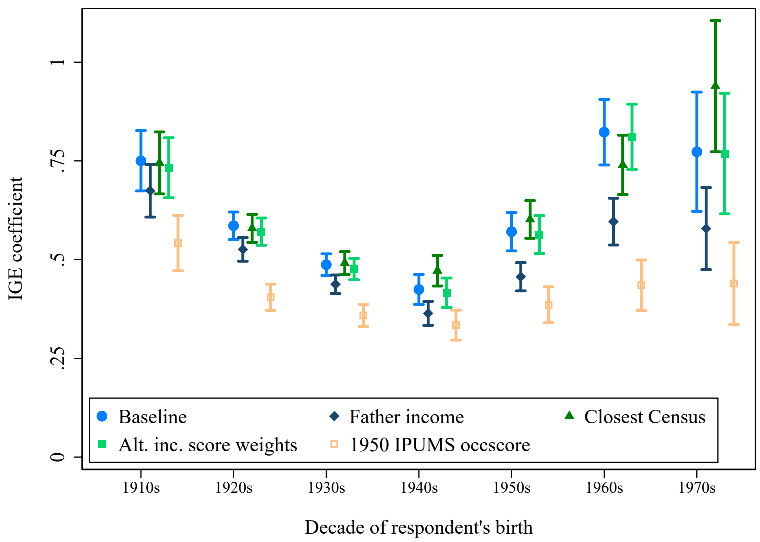


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E. We utilize data from Ruggles *et al.* (2021) to construct income predictions and measures of household size.

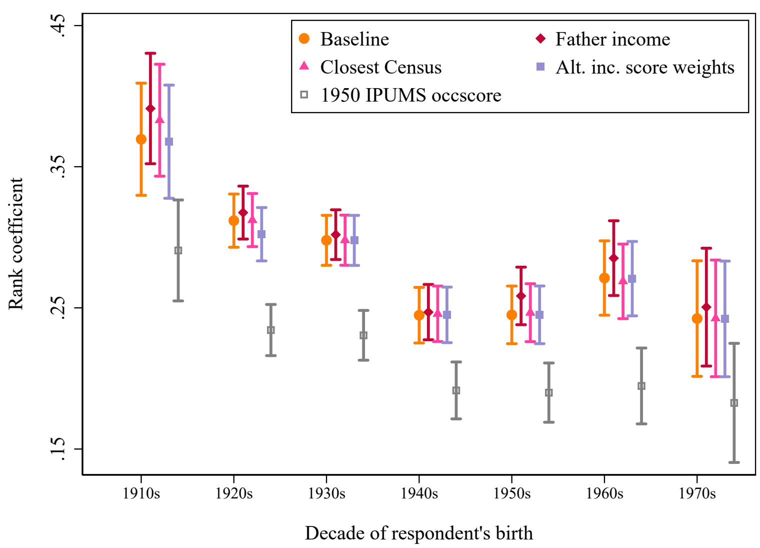
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction (see Section 3.2 for more details). In the second series, we restrict the sample to respondents that provided information about their household size (84% of the baseline sample). In the third series, we use this sub-sample and adjust the income prediction to account for differences in household size. For the respondent's generation, we divide own family income by the square root of a respondent's household size at the time of the interview. For the parental generation, we divide the baseline income prediction by the square root of the median household size. Specifically, we use the 1920–1990 Censuses to construct the median household size when the respondent is 10 years old (taking the weighted average of the median household size in that *occupation* × *race* × *South* cell and allowing the weights to reflect the year in which the respondent is 10).

Figure B.14: Mobility by birth decade, incorporating various adjustments to predicted income

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: All estimates in this figure are based on the baseline sample of respondents ages 30–50. The first series reproduces the main IGE and rank-rank estimates using our baseline income prediction (see Section 3.2 for more details). "Father income" refers to using individual (as opposed to household) income for fathers. "Closest Census" refers to using the closest Census relative to the survey respondent's childhood to calculate predicted income. In particular, we use the 1940 Census with the 1936 Expenditure Survey for the 1910s–1930s cohorts, the 1960 Census for the 1940s–1950s cohorts, the 1970 Census for the 1960s cohort, and the 1980 Census for the 1970s cohort. "Alt. inc. score weights" refers to using an income prediction in which fathers are weighted by the number of children in the household in the calculation of average family income. "1950 IPUMS occscore" refers to using the *occscore* variable from IPUMS. For more detail on the construction of these income predictions, see Appendix E.

C Assessing recall bias

Our estimates of mobility rely on survey respondents' recollection of their fathers' occupations. In this section, we consider the extent to which recall bias might be present in our estimates. We begin by comparing the fathers' occupations provided by male versus female respondents in our surveys. We then compare the fathers' occupations in our surveys to those of fathers in the decennial Censuses at the time that the respondents were growing up. We conclude by looking at the PSID—a survey that includes both retrospective questions as well as self-reported information about fathers' occupations when the respondent was growing up—to gauge the extent to which adult children's retrospective answers match fathers' self-reported occupations.

C.1 Comparing male and female survey respondents

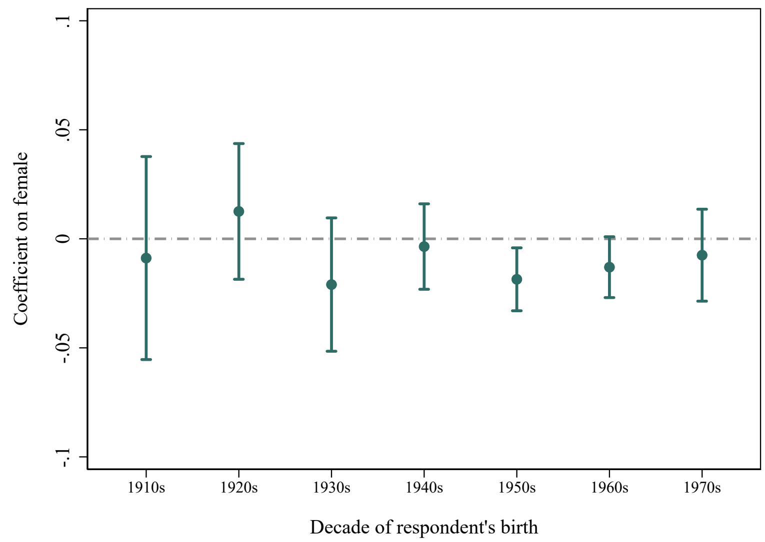
We begin by comparing the fathers' occupations reported by male and female respondents in our surveys. Roughly speaking, brothers and sisters grow up in the same families in the US, so adult men and women should report similar occupations for their fathers. Strictly speaking, small differences could arise between the predicted parental family income of men and that of women. If parents have sex-based stopping rules when making fertility decisions, then boys and girls might wind up growing up in systematically different families (as in Asher *et al.* (2018), using data from India). However, evidence for sex-based fertility patterns in the US is much weaker. Second, even if boys and girls grow up in identical families in terms of parental income, small differences might arise because men have higher mortality rates than women and thus selection into surviving into prime age could differ by gender (especially in our oldest cohorts, men are less likely to live until age 50).

These small potential discrepancies notwithstanding, we would be suspicious of any parental income estimate that gives significantly different estimates for male and female respondents. We thus regress the log as well as the rank of estimated parental income on a female dummy, separately for each of our birth decades, and report the results in Appendix Figure C.1. The coefficient on the female dummy is always close to zero and has no consistent sign. We repeat this analysis separately for white and Black respondents and report the results in Appendix Figure C.2. Again, we find no notable patterns or significant differences beyond what might be expected by chance. Importantly, these figures do not indicate that recall deteriorated between the 1910s and 1940s cohorts in a way that would drive our main result. Appendix Table C.1 shows the top five occupations reported by male and female respondents in each birth cohort. In all birth cohorts, at least four—if not all five—of the top occupations coincide between male and female respondents and in roughly the same order.

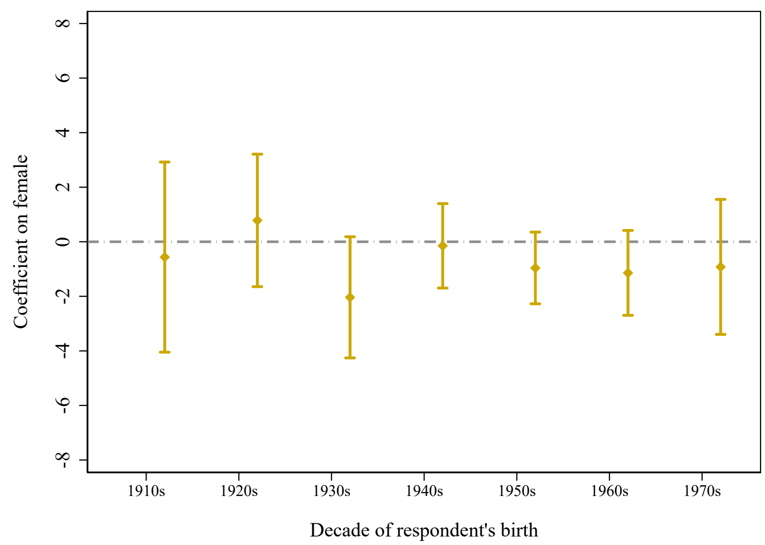
Finally, we can more directly compare survey respondents who are siblings, which occurs in four of our surveys and corresponds to around 7% of the baseline sample. As seen in Appendix Figure C.3, the predicted parental incomes implied by siblings' answers are highly correlated, providing another piece of evidence that individuals' recall of their fathers' occupation provides relatively accurate information about their upbringing.

Figure C.1: Differences in income prediction, by respondent sex and birth cohort

(a) Logged parental income



(b) Ranked parental income

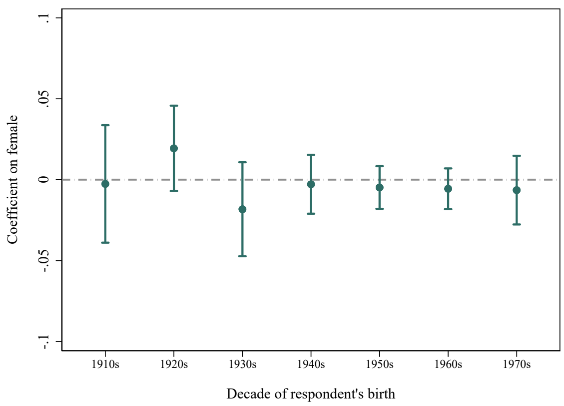


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

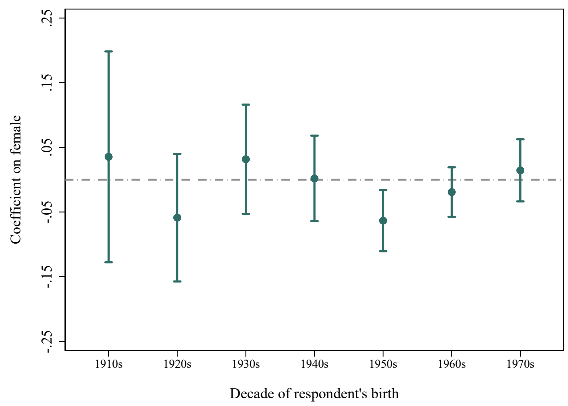
Notes: This figure uses our baseline sample ages 30–50 to regress logged and ranked predicted parental income on an indicator variable for whether a respondent is female. Survey-year fixed effects are included in both panels. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure C.2: Differences in income prediction, by respondent sex, race, and birth cohort

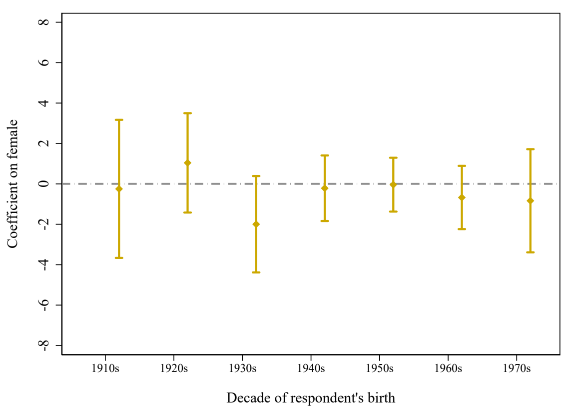
(a) Logged parental income, white respondents



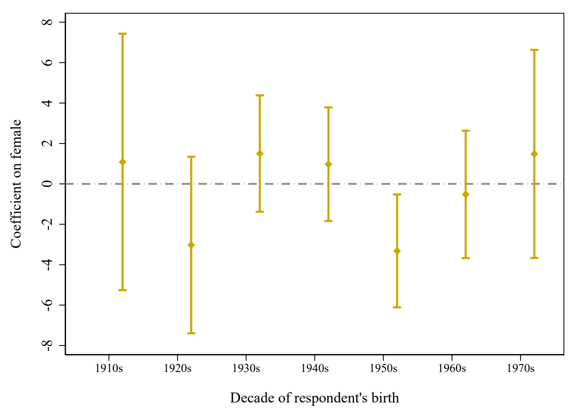
(b) Logged parental income, Black respondents



(c) Ranked parental income, white respondents



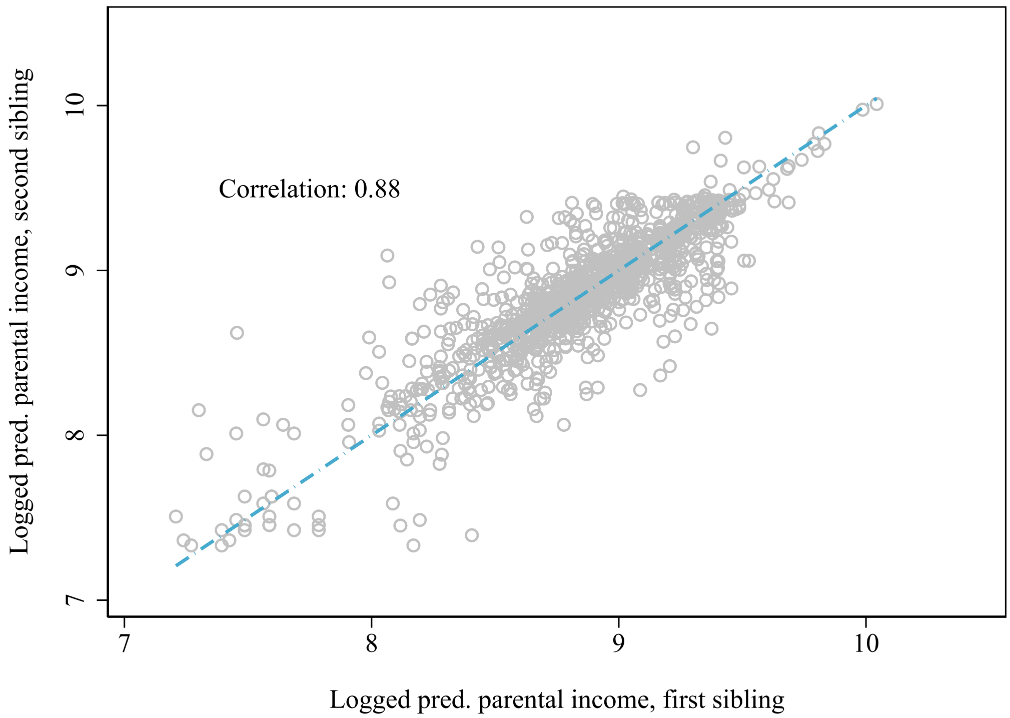
(d) Ranked parental income, Black respondents



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure uses our baseline sample of ages 30–50 to regress logged and ranked predicted parental income on an indicator variable for whether a respondent is female, separately by respondent race. Survey-year fixed effects are included in all panels. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure C.3: Scatterplot of correlation in parental income prediction among siblings



Sources: This figure combines data from four different surveys which can include respondents who are siblings (the PSID, the NLS of Youth, and the NLS Young Men and Young Women surveys).

Notes: This figure restricts the baseline sample of respondents ages 30–50 to individuals with one sibling in the baseline sample (4% of the baseline sample). The figure plots the parental income prediction of one sibling against that of the other sibling based on their responses about their father's occupation. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). The 45-degree line is shown.

Table C.1: Top five occupations reported by male and female respondents, by birth cohort

Birth Cohort	Male Respondents	Share of male sample	Female Respondents	Share of female sample
1910s	1. Farm operator	0.35	1. Farm operator	0.36
	2. Craftsman (skilled)	0.15	2. Craftsman (skilled)	0.16
	3. Craftsman (semi-skilled)	0.14	3. Craftsman (semi-skilled)	0.12
	4. Unskilled laborer (non-farm)	0.07	4. Unskilled laborer (non-farm)	0.08
	5. Businessman (self-employed)	0.05	5. Businessman (self-employed)	0.08
1920s	1. Farm operator	0.24	1. Farm operator	0.28
	2. Craftsman (skilled)	0.18	2. Craftsman (semi-skilled)	0.18
	3. Craftsman (semi-skilled)	0.17	3. Craftsman (skilled)	0.13
	4. Unskilled laborer (non-farm)	0.07	4. Businessman (not self-employed)	0.08
	5. Businessman (self-employed)	0.06	5. Unskilled laborer (non-farm)	0.08
1930s	1. Farm operator	0.19	1. Farm operator	0.21
	2. Craftsman (skilled)	0.19	2. Craftsman (semi-skilled)	0.19
	3. Craftsman (semi-skilled)	0.17	3. Craftsman (skilled)	0.16
	4. Unskilled laborer (non-farm)	0.07	4. Businessman (not self-employed)	0.10
	5. Businessman (self-employed)	0.06	5. Unskilled laborer (non-farm)	0.08
1940s	1. Craftsman (skilled)	0.20	1. Craftsman (semi-skilled)	0.18
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (skilled)	0.18
	3. Farm operator	0.12	3. Businessman (not self-employed)	0.11
	4. Businessman (not self-employed)	0.11	4. Farm operator	0.11
	5. Unskilled laborer (non-farm)	0.06	5. Unskilled laborer (non-farm)	0.07
1950s	1. Craftsman (skilled)	0.20	1. Craftsman (semi-skilled)	0.18
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (skilled)	0.18
	3. Businessman (not self-employed)	0.13	3. Businessman (not self-employed)	0.11
	4. Farm operator	0.07	4. Unskilled laborer (non-farm)	0.07
	5. Unskilled laborer (non-farm)	0.06	5. Farm operator	0.07
1960s	1. Craftsman (skilled)	0.20	1. Craftsman (skilled)	0.20
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (semi-skilled)	0.18
	3. Businessman (not self-employed)	0.14	3. Businessman (not self-employed)	0.12
	4. Unskilled laborer (non-farm)	0.05	4. Unskilled laborer (non-farm)	0.06
	5. Protective service officer	0.05	5. Protective service officer	0.05
1970s	1. Craftsman (skilled)	0.18	1. Craftsman (semi-skilled)	0.19
	2. Craftsman (semi-skilled)	0.15	2. Craftsman (skilled)	0.18
	3. Businessman (not self-employed)	0.13	3. Businessman (not self-employed)	0.13
	4. Protective service officer	0.09	4. Unskilled laborer (non-farm)	0.07
	5. Unskilled laborer (non-farm)	0.07	5. Protective service officer	0.07

Notes: Estimates are based on the baseline sample of respondents ages 30–50. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

C.2 Comparing survey respondents' fathers to Census fathers

Next, we compare the occupations of fathers in the surveys to those of actual fathers in the Census in the years when the respondents were children. Ward (2023) warns that Census-takers made errors in recording the occupation variable, but we would still be worried if our respondents' recollection of their fathers' occupations differed dramatically from fathers' occupations in the Census during the years in which the respondents were growing up. In all of the exercises in this section, we consider both the earlier and later corresponding Censuses, when respondents were ages 0–10 and 11–20, respectively.

We begin by comparing the predicted family income of fathers in the surveys with the predicted family income of fathers in the Census. Appendix Figure C.4 regresses the estimates of logged parental income on a dummy for whether the father's income measure came from the surveys.[45] We note that the predicted family income of fathers in the surveys is slightly lower than that of fathers in the Census, but the point estimates are small. More importantly, there does not seem to be any pattern in how the estimates are changing, suggesting that recall bias is not improving or deteriorating across cohorts. This lack of a consistent pattern, especially in the first half of cohorts, suggests that the rise in mobility that we find is not driven by respondents' remembering their fathers' occupations differently across cohorts (or in other words, it does not seem to be the case that the rise in mobility is driven by measurement error changing monotonically over time).

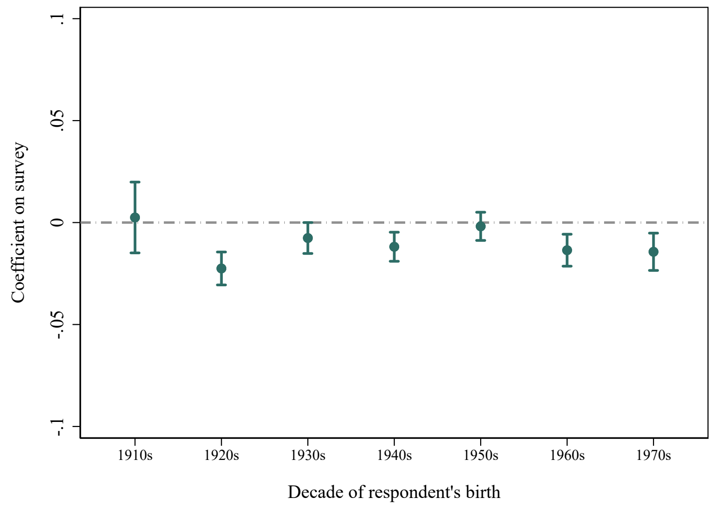
Appendix Tables C.2 and C.3 compare the mix of coarsened occupations that our respondents report their fathers as having to that of fathers in the Census. In both of these tables, we find that the share of fathers with each occupation are comparable to the corresponding shares in the Census.[46]

[45] We do not include an analogous exercise using ranked father's income. When calculating ranks for fathers in our main analysis, we rank a survey respondent's father relative to all fathers with children born in the same birth cohort. Because we are comparing these men to fathers in the decennial Census (most of whom have multiple children), it is not obvious which child's year of birth should be used in the ranking. Similarly, because we do not know the exact age of survey respondents' fathers, we cannot rank survey and Census fathers using their age.

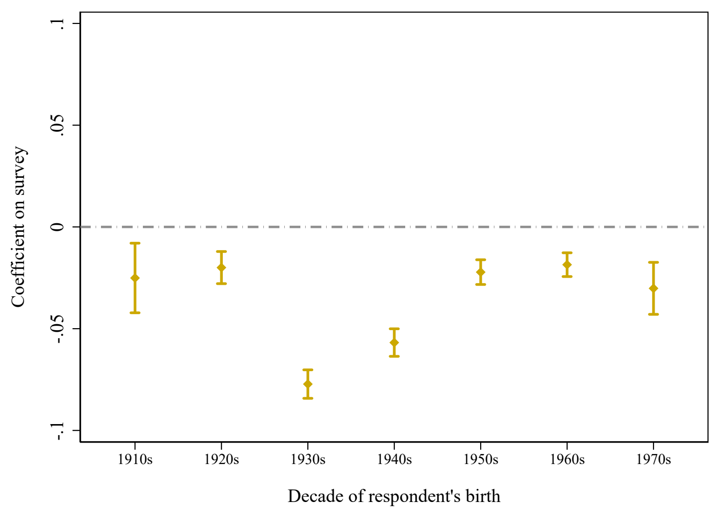
[46] The share of survey fathers who are farmers exceeds the analogous Census share in Appendix Table C.3. However, the decline of agriculture as a dominant occupation was occurring during this time period, so we would expect the Census shares to be lower than the survey shares when considering the later Census.

Figure C.4: Differences in logged family income prediction between Census fathers and survey respondents' fathers

(a) Using earlier Census



(b) Using later Census



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure uses our baseline sample of respondents ages 30–50 and Census fathers ages 30–50. In the top panel, the survey respondents' fathers are compared to the fathers in the Census when the respondents would have been between the ages of 1 and 10 (e.g., the fathers of survey respondents born in 1910–1919 are compared to 1920 Census fathers). In the bottom panel, the survey respondents' fathers are compared to fathers in the Census when the respondents would have been between the ages of 11 and 20. Survey respondents in the top panel are assigned a parental income prediction from the decade closest in time to when they fell in the 1–10 and 11–20 age range, respectively (if there is no nearest source, the respondent is assigned a weighted average of the two data sources closest to the desired age range). To predict parental income for Census fathers, we assign fathers a measure of predicted income using the nearest available data source (with the exception of fathers in the 1920, 1930, and 1950 Censuses who receive a weighted average of the two nearest data sources). All income predictions are conditional on father's occupation, race, and region (South vs. elsewhere).

Table C.2: Occupations of Survey Respondents' Fathers and Census Fathers (Using Earlier Census), by Birth Cohort

	1910–1919		1920–1929		1930–1939		1940–1949		1950–1959		1960–1969	
	Census (1920)	Survey	Census (1930)	Survey	Census (1940)	Survey	Census (1950)	Survey	Census (1960)	Survey	Census (1970)	Survey
Coarsened Occupations												
Accountants and auditors	0.34	0.55	0.52	0.48	0.73	0.58	0.88	0.63	0.99	1.08	1.12	0.95
Clergymen	0.46	0.62	0.41	0.72	0.46	0.62	0.40	0.60	0.44	0.78	0.49	0.57
Public-school teachers	0.34	0.62	0.48	0.46	1.05	0.52	0.85	0.89	1.14	1.37	2.03	2.11
Dentists	0.19	0.31	0.24	0.17	0.11	0.23	0.21	0.13	0.21	0.26	0.25	0.19
Physicians and surgeons	0.51	0.69	0.40	0.27	0.23	0.43	0.59	0.47	0.62	0.87	0.67	0.55
Engineers	0.53	1.10	0.72	0.90	0.84	1.09	1.56	2.14	2.58	3.36	3.48	3.86
Lawyers and judges	0.44	0.31	0.45	0.43	0.31	0.49	0.51	0.48	0.51	0.67	0.67	0.72
Social and welfare workers	0.04	0.02	0.03	0.03	0.06	0.08	0.08	0.08	0.12	0.13	0.18	0.20
Nurses (trained or student)	0.00	0.01	0.01	0.05	0.02	0.04	0.01	0.08	0.03	0.06	0.16	0.05
Other professional and technical	0.58	0.41	0.74	0.67	1.11	0.97	1.61	1.79	2.43	3.35	4.63	4.04
Semi-professional	0.69	0.85	0.88	0.66	0.91	1.18	1.49	1.76	2.35	2.24	3.01	2.47
Businessmen (self-employed)	6.44	6.43	6.35	4.29	1.09	3.85	6.52	3.19	4.29	2.82	3.28	2.97
Businessmen (not self-employed)	3.90	4.73	5.24	7.41	6.66	8.08	6.18	11.41	8.09	12.89	9.61	13.46
Bookkeeper	0.48	0.19	0.38	0.34	0.52	0.33	0.30	0.26	0.25	0.19	0.45	0.15
Stenographers	0.08	0.28	0.14	0.18	0.14	0.09	0.16	0.15	0.12	0.11	0.19	0.05
Other clerical workers	3.17	1.65	3.41	2.92	5.44	2.98	4.83	3.70	5.28	3.52	5.03	3.22
Sales: higher-status	0.96	1.33	1.41	1.09	1.00	1.01	1.11	1.27	1.52	1.74	2.05	2.18
Sales: inside sales	2.93	1.90	4.33	2.19	7.95	2.69	4.85	3.55	5.09	3.72	4.96	3.93
Sales: lower-status	0.17	0.39	0.19	0.18	0.08	0.20	0.05	0.06	0.05	0.06	0.06	0.06
Foremen	1.96	1.78	2.14	2.24	2.46	3.15	2.62	3.31	3.30	3.76	4.00	3.74
Craftsmen (skilled)	17.10	15.81	17.17	16.36	18.15	18.15	18.16	19.81	19.03	19.38	18.83	19.79
Craftsmen (semi-skilled)	13.46	13.41	15.07	17.80	21.91	18.36	20.41	18.06	20.46	16.83	18.74	16.96
Protective service officers	0.96	1.18	1.32	2.09	1.87	2.17	2.35	3.53	3.72	4.52	4.20	4.86
Private household workers	0.10	0.03	0.09	0.63	0.27	0.34	0.04	0.25	0.03	0.08	0.02	—
Other service workers	1.84	1.83	2.44	2.69	3.28	2.85	2.54	2.75	2.41	2.84	3.19	2.65
Farm laborers	3.20	1.84	3.37	2.81	4.36	3.32	2.13	2.68	1.37	1.66	1.04	1.20
Unskilled non-farm laborers	10.71	7.55	10.95	7.43	14.91	7.20	6.27	6.27	5.47	5.69	4.60	5.48
Farm operators	27.22	34.18	20.18	24.50	3.63	18.99	10.73	10.70	5.01	6.04	2.61	3.61

Notes: For survey estimates, we use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Census shares are weighted using provided weights. Census samples include all Black and white fathers ages 30–50. The fathers in the Census are compared to survey respondents' fathers when the respondents would have been between the ages of 1 and 10 at the time of the Census (e.g., survey respondents born in 1910–1919 are compared to 1920 Census respondents.)

Table C.3: Occupations of Survey Respondents' Fathers and Census Fathers (Using Later Census), by Birth Cohort

	1910–1919		1920–1929		1930–1939		1940–1949		1950–1959		1960–1969	
	Census (1930)	Survey	Census (1940)	Survey	Census (1950)	Survey	Census (1960)	Survey	Census (1970)	Survey	Census (1980)	Survey
Coarsened Occupations												
Accountants and auditors	0.52	0.55	0.73	0.48	0.88	0.58	0.99	0.63	1.12	1.08	1.05	0.95
Clergymen	0.41	0.62	0.46	0.72	0.40	0.62	0.44	0.60	0.49	0.78	0.57	0.57
Public-school teachers	0.48	0.62	1.05	0.46	0.85	0.52	1.14	0.89	2.03	1.37	2.83	2.11
Dentists	0.24	0.31	0.11	0.17	0.21	0.23	0.21	0.13	0.25	0.26	0.28	0.19
Physicians and surgeons	0.40	0.69	0.23	0.27	0.59	0.43	0.62	0.47	0.67	0.87	0.79	0.55
Engineers	0.72	1.10	0.84	0.90	1.56	1.09	2.58	2.14	3.48	3.36	2.96	3.86
Lawyers and judges	0.45	0.31	0.31	0.43	0.51	0.49	0.51	0.48	0.67	0.67	0.92	0.72
Social and welfare workers	0.03	0.02	0.06	0.03	0.08	0.08	0.12	0.08	0.18	0.13	0.29	0.20
Nurses (trained or student)	0.01	0.01	0.02	0.05	0.01	0.04	0.03	0.08	0.16	0.06	0.24	0.05
Other professional and technical	0.74	0.41	1.11	0.67	1.61	0.97	2.43	1.79	4.63	3.35	4.39	4.04
Semi-professional	0.88	0.85	0.91	0.66	1.49	1.18	2.35	1.76	3.01	2.24	3.55	2.47
Businessmen (self-employed)	6.35	6.43	1.09	4.29	6.52	3.85	4.29	3.19	3.28	2.82	3.73	2.97
Businessmen (not self-employed)	5.24	4.73	6.66	7.41	6.18	8.08	8.09	11.41	9.61	12.89	12.31	13.46
Bookkeeper	0.38	0.19	0.52	0.34	0.30	0.33	0.25	0.26	0.45	0.19	0.20	0.15
Stenographers	0.14	0.28	0.14	0.18	0.16	0.09	0.12	0.15	0.19	0.11	0.08	0.05
Other clerical workers	3.41	1.65	5.44	2.92	4.83	2.98	5.28	3.70	5.03	3.52	5.08	3.22
Sales: higher-status	1.41	1.33	1.00	1.09	1.11	1.01	1.52	1.27	2.05	1.74	2.01	2.18
Sales: inside sales	4.33	1.90	7.95	2.19	4.85	2.69	5.09	3.55	4.96	3.72	4.06	3.93
Sales: lower-status	0.19	0.39	0.08	0.18	0.05	0.20	0.05	0.06	0.06	0.06	0.10	0.06
Foremen	2.14	1.78	2.46	2.24	2.62	3.15	3.30	3.31	4.00	3.76	4.55	3.74
Craftsmen (skilled)	17.17	15.81	18.15	16.36	18.16	18.15	19.03	19.81	18.83	19.38	17.13	19.79
Craftsmen (semi-skilled)	15.07	13.41	21.91	17.80	20.41	18.36	20.46	18.06	18.74	16.83	16.97	16.96
Protective service officers	1.32	1.18	1.87	2.09	2.35	2.17	3.72	3.53	4.20	4.52	4.46	4.86
Private household workers	0.09	0.03	0.27	0.63	0.04	0.34	0.03	0.25	0.02	0.08	0.01	—
Other service workers	2.44	1.83	3.28	2.69	2.54	2.85	2.41	2.75	3.19	2.84	3.08	2.65
Farm laborers	3.37	1.84	4.36	2.81	2.13	3.32	1.37	2.68	1.04	1.66	0.79	1.20
Unskilled non-farm laborers	10.95	7.55	14.91	7.43	6.27	7.20	5.47	6.27	4.60	5.69	4.44	5.48
Farm operators	20.18	34.18	3.63	24.50	10.73	18.99	5.01	10.70	2.61	6.04	1.97	3.61

Notes: For survey estimates, we use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Census shares are weighted using provided weights. Census samples include all Black and white fathers ages 30–50. The fathers in the Census are compared to survey respondents' fathers when the respondents would have been between the ages of 11 and 20 at the time of the Census (e.g., survey respondents born in 1910–1919 are compared to 1930 Census respondents.)

C.3 Assessing recall bias using the PSID

In this section, we utilize the unique nature of the PSID to consider the extent to which retrospective questions convey accurate information about a father's income level. In particular, the PSID has both retrospective questions about a respondent's father's occupation as well as self-reported father's occupations and income when the respondent was growing up (i.e., in earlier waves of the survey). We focus on the 1997–2015 waves of the PSID because 1997 is the first year in which the retrospective questions are asked with sufficient detail (i.e., 3-digit occupation codes), so that they can be mapped to our coarsened occupations.

The way that we verify the retrospective answers is by looking at individuals who were household heads at some point between 1997–2015 and who were thus asked about their father's occupation while they were growing up. Then, using the Family Identification Mapping System (FIMS) provided by the PSID, we can find these individuals' fathers in earlier waves of the survey and see the fathers' self-reported (coarsened) occupations between the ages of 25–50 (i.e., when the respondents were growing up). We can then see whether the retrospective answers in 1997–2015 matched any of the self-reported occupations in earlier survey years. Note that we often see multiple observations of father's self-reported occupation, as household heads were asked about their current occupation during each survey wave.

We find that for 81% of adult children, their retrospective answers coincided with one of the self-reported occupations of their fathers during their childhood.[47] We can also then see what the most common mistakes were in identifying occupation (in other words, conditional on a respondent mis-reporting his/her father's occupation, what did the adult child typically report versus what did the father typically report). The four most common mistakes—which account for roughly 20% of all mistakes—are the respondents reporting that their fathers were skilled craftsmen, semi-skilled craftsmen, or unskilled non-farm laborers, when instead the father reported one of the other occupations on this same list.[48]

Even if one-in-five respondents are mis-reporting their fathers' occupations, it might still be the case that the retrospective answers convey accurate information about a father's income level. Appendix Figure C.5 plots the predicted income of fathers using the retrospective answers against the predicted income of fathers using self-reported occupations when they were around 40 years old. Both panels of this figure confirm that respondents' retrospective answers are highly correlated with fathers' self-reported answers, and thus convey similar information about the respondents' income level during their upbringing. Importantly, it also does not appear to be the case that respondents with poorer or richer fathers tend to differentially provide inaccurate

[47] There are some instances (roughly 10% of respondents) in which the adult children's retrospective answers change across waves (for example, as a result of re-interviews due to changing family composition), so we consider all of the retrospective answers provided.

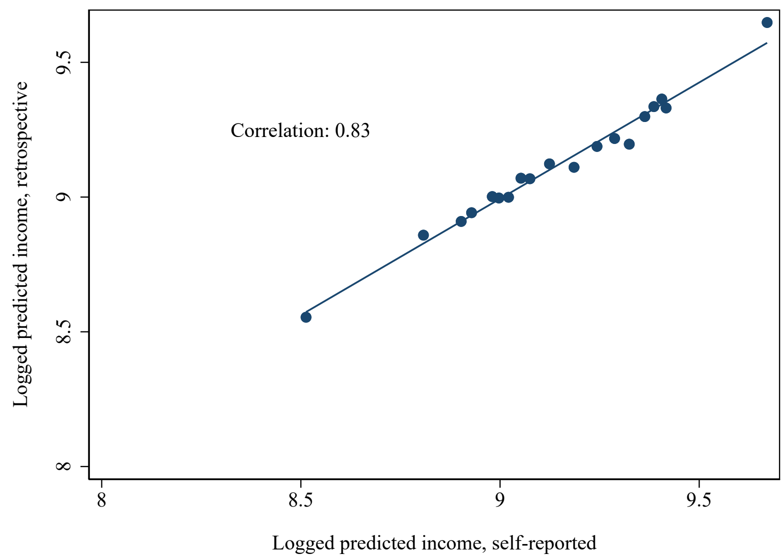
[48] To find the most common mistakes, we first select the 20% of respondents who were unable to accurately report any of their fathers' occupations. We then compare the modal retrospective answer in the data to the modal self-reported occupation of fathers between the ages of 30–50.

information. Appendix Table C.4 regresses the five-year average of a father's self-reported family income on alternative ways of measuring that father's income level. The coefficient of 1 in column 2, which uses the retrospective answer provided by the adult child, highlights that the retrospective answers seem to be reliable measures of a father's permanent income.

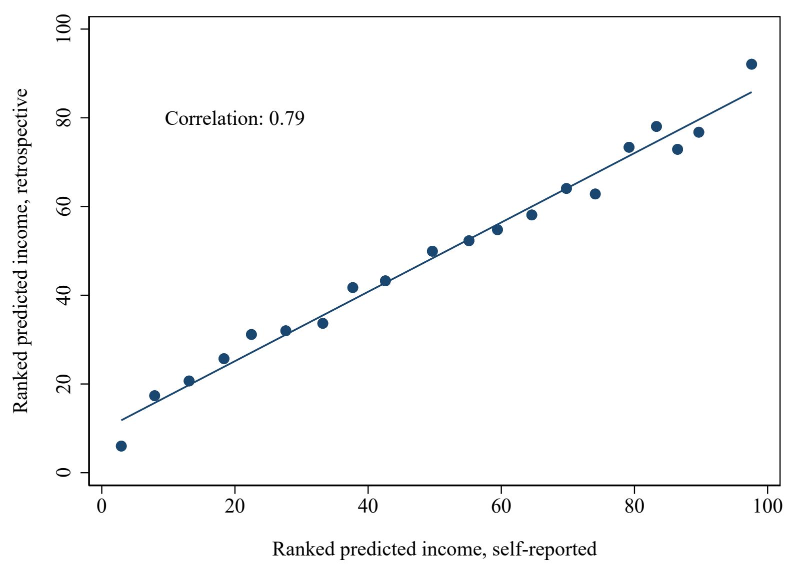
Moreover, most of the estimates in the historical intergenerational mobility literature come from linked Census data (20 or 30 years apart) and use one year of a father's occupation to predict their income. To get a better sense of how estimates that use our retrospective approach differ from those that use the typical approach in the literature, Appendix Table C.5 shows the IGE and rank-rank coefficient from using the adult children's retrospective answers in 1997 (column 1) versus using one year of father's self-reported occupation roughly thirty years earlier (column 2). The mobility estimates are similar using the two approaches. Ward (2023) notes the measurement error that can arise from using one year of father's occupation, so the last column of this table also reports mobility estimates using three years of self-reported father's occupations (and averaging their corresponding predicted income) around 1970.

Figure C.5: Comparison of father's family income prediction using adult children's retrospective answers and father's self-reported occupations

(a) Logged predicted income



(b) Ranked predicted income



Sources: This figure uses the 1968–2015 Panel Study of Income Dynamics.

Notes: These figures are bin-scatter depictions of the predicted family income of fathers using the retrospective answers (y-axis) of adult children versus using fathers' self-reported answers in earlier survey waves (x-axis). The y-axis assigns fathers an income prediction using the modal retrospective occupation reported by respondents. The x-axis assigns fathers an income prediction using the five self-reported occupations closest to age 40. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census.

Table C.4: Relationship between 5-year average of father's self-reported family income and various other ways to measure father's family income, 1997 PSID

	Predicted Income			Actual Income
	(1) Self-reported, in 1970	(2) Retrospective	(3) Self-reported, 1 year, age 40	(4) Self-reported, 1 year, age 40
Logged income	1.045	1.016	1.011	0.572
	[0.113]	[0.118]	[0.119]	[0.161]
Observations	898	898	898	898
R-squared	0.35	0.33	0.33	0.65

Notes: This table regresses the 5-year average of father's self-reported logged family income on four alternative ways to measure father's family income level (denoted in the column headers). The sample used is the fathers of household heads ages 30–50 who provided a retrospective answer in 1997. We include fathers who can be located in an earlier wave of the survey and who had at least five years of available income and occupation information between the ages of 30–50. The dependent variable is the average of five years of father's logged family income closest to age 40. Column 1 uses the income prediction associated with father's self-reported occupation around 1970 as the independent variable. Column 2 uses the income prediction corresponding to the retrospective answer provided by the household head about their father. Column 3 uses the income prediction corresponding to the father's self-reported occupation closest to age 40. Column 4 uses the family income of the father in one year closest to age 40. To predict income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census. All estimates are weighted using 1997 cross-sectional weights.

Table C.5: IGE and rank coefficient using various ways of measuring parental income, 1997 PSID

(a) Logged income

	(1) Retrospective	(2) Self-reported, 1 year	(3) Self-reported, 3 years
IGE	0.586 [0.106]	0.638 [0.100]	0.650 [0.101]
Observations	1,061	1,061	1,061

(b) Ranked income

	(1) Retrospective	(2) Self-reported, 1 year	(3) Self-reported, 3 years
Rank-rank correlation	0.224 [0.039]	0.230 [0.039]	0.223 [0.039]
Observations	1,061	1,061	1,061

Notes: This table reports estimates of the IGE and rank-rank coefficients from specifications that use respondents' retrospective answers about their fathers' occupations (column 1) and fathers' self-reported answers about their occupations in earlier survey waves (columns 2 and 3). The sample used is household heads ages 30–50 who provided a retrospective answer in 1997 and whose father can be located in an earlier wave of the survey. Column 1 uses the provided retrospective answers. Column 2 uses the self-reported occupation of fathers ages 30–50 roughly thirty years earlier (around 1970). The last column uses three years of self-reported occupations (between 1968–1972) and takes an average of the three corresponding income predictions. To predict income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census. All estimates are weighted using 1997 cross-sectional weights.

D Two-Sample Estimates

In this Appendix, we discuss the econometrics of our methodology. We begin by showing formally the sources of differences between OLS estimates (which are infeasible in the historical period, as we do not observe parental income) and the two-sample two-stage least squares (TS2SLS) estimates. The first is a bias term due to prediction error and the second is a bias term due to an exclusion-restriction violation. We then provide a variety of evidence that these bias terms are unlikely to vary over time in such a manner as to produce our main result—the decline in persistence from the 1910s to 1940s—as a mere artifact. Next, we show that our imputation-based OLS strategy is equivalent to TS2SLS estimates when the intergenerational mobility parameter is estimated using a specification in levels. Finally, we show that we can recover approximations to the IGE and rank-rank measures by combining these levels estimates with sample moments, and that these approximations are very similar to our main results.

D.1 Sources of bias from two-sample imputation approach

In this section, we discuss the potential biases that may arise from the two-sample imputation approach that we and other historical mobility papers tend to follow. We then discuss robustness tests with these biases in mind.

D.1.1 Deriving bias terms from the two-sample approach

Following Solon (1992), consider the standard OLS estimation of intergenerational persistence:

$$y_i = \alpha + \beta^{\text{OLS}} y_i^p + e_i, \tag{7}$$

where i denotes the individual child; y_i is her adult income; and y_i^p is the *permanent* income of the parents of child i. The coefficient β captures the covariance between the two income variables and does not typically take on a causal interpretation. As correlation and not causality is the goal, $\text{E}[y_i^p e_i] = 0$ by definition.[49]

The main challenge in our context—common in the historical U.S. mobility literature—is that we do not observe parental permanent income, y_i^p, for our children i. Instead, we have information on parental attributes Z_i that the children i report their parents to have had. We thus rely on a "first-stage" estimation of the relationship between parental income and attributes Z from an auxiliary dataset of parents j. Given the two samples involved in this procedure, this approach is a two-sample two-stage least squares approach (see, e.g., Angrist and Krueger, 1992; Inoue and Solon, 2010).

[49] While all of our main specifications omit covariates, note that we can include covariates that are unavailable in the first-stage in the second-stage *so long* as they are uncorrelated with the instruments and the error term in the first-stage; i.e., if a covariate would only improve precision in the first stage, it can be included in the second stage alone.

One population. We begin by considering the simplest scenario with only *one* population. Here, a researcher can do an analogous estimation, using half of the sample (henceforth group j) to estimate the relationship between parental income and attributes Z and then construct income values for the other half of the sample (henceforth group i) using the estimated first-stage parameters (i.e., a split-sample instrumental variables approach; Angrist and Krueger, 1995).

In other words, we can estimate income for parent j as a function of attributes Z_j:

$$y_j^p = Z_j\Gamma + V_j, \tag{8}$$

where $\mathrm{E}[V_j] = 0$, $\mathrm{Var}(V_j) < \infty$, and $\mathrm{Cov}(V_j, Z_j) = 0$. With an estimated $\hat{\Gamma}$ we can now return to children i and write the predicted income of child i's parental income as $\tilde{y}_i^p = Z_i\hat{\Gamma}$.

To clarify the resulting biases from this two-step approach, we can write the linear projection of y_i^p on $\tilde{y}_i^p$ as:

$$y_i^p = \theta\tilde{y}_i^p + w_i. \tag{9}$$

In this one-population scenario, the coefficient θ equals one.[50]

Finally, we can write the coefficient from this two-step procedure β^{TS2SLS} as a function of β^{OLS} as follows:

$$\beta^{\text{TS2SLS}} = \theta\beta^{\text{OLS}} + \underbrace{\frac{\mathrm{Cov}(e_i, \tilde{y}_i^p)}{\mathrm{Var}(\tilde{y}_i^p)}}_{\omega} \tag{10}$$

In this one-population scenario, $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$. The ω term arises from violations of the exclusion restriction, whereby attributes Z_i are correlated with unobserved determinants of y_{ic} that do not enter through y_i^p.[51]

To provide some intuition for the ω bias in our setting, consider the variable "born in Mississippi," which is largely unobserved to us, as we do not always have information on place of birth. Assume that growing up in Mississippi predicts lower income during childhood even conditional on race, father's occupation, and South (our set of instruments). Assume further that growing up in Mississippi also predicts lower adult income even conditional on parental income. Under this scenario, $\mathrm{Cov}(e_i, \tilde{y}_i^p) > 0$ and thus $\omega > 0$. While in principle it is possible that this covariance is negative, the ease of coming up with examples such as our Mississippi example suggests that the ω bias is likely positive (Zimmerman, 1992).

[50] Bloise *et al.* (2021) also studies the two biases that result from two-stage techniques. In their derivation, they write the linear projection of $\tilde{y}_i^p$ on y_i^p as $\tilde{y}_i^p = \gamma y_i^p + v_i$ so that their version of equation (10) is a function of γ and v_i.

[51] Note that $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$ also applies to any two-stage least squares coefficient, with one sample or two, and is not specific to the split-sample IV case.

Two populations. Now consider a different scenario, in which we have *two* populations: we use population j to estimate the relationship between parental income and attributes Z and then construct income values for population i. Here, we also have $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$ as long as the parents of children i and the parents j are drawn from the same underlying population and the relationship between attributes Z and parental income are equivalent in populations i and j.

Nevertheless, if the two populations have different data-generating processes, then the functional form of the relationship between parental income and attributes Z will be mis-specified in the first stage. As an example, if respondents imperfectly recall their parental attributes Z, then the regression of y_i^p on $\tilde{y}_i^p$ would not necessarily have a coefficient equal to one. In short, if for *any* reason, the first-stage structural equation is different in the two populations, then β^{TS2SLS} could be biased not just via failures of the exclusion restriction (ω), but also through the projection of the dependent variable onto its prediction not having a coefficient $\theta = 1$ (Zhao *et al.*, 2019).

Finite samples. Finally, we now consider additional bias that may enter the estimation from using finite samples, as we do in this paper.

Consider again the simplest case where we know the data-generating process is the same in the two samples (e.g., a split-sample instrumental variables approach). Here, even though the individuals i and j belong to the same population and have the same data-generating process for income, there is sampling error in these finite samples and $\hat{\theta}$ does not need to mechanically equal one when two samples are involved.[52] Even in this simplified case, sampling error means that β^{TS2SLS} could be biased from two sources of error, θ and ω.

In sum, the utilization of two finite samples in this paper means that we must consider both θ and ω biases. The primary concern with using β^{TS2SLS} to recover time-varying patterns in intergenerational mobility is that the θ and ω terms could vary over birth cohorts in a manner that causes β^{TS2SLS} to decline between the 1910s and 1940s birth cohorts whereas the true β^{OLS} trends differently. In the next subsection, we present a variety of evidence that this concern is unlikely to hold.

D.1.2 Is the 1910–1940 decline in persistence a mere artifact of changing bias terms?

The decline of the IGE and rank-rank correlation between the 1910s and 1940s birth cohorts is our main novel result, and there are several factors that we think will push against this finding being driven by changes in θ and ω across birth cohorts.

[52] Angrist and Krueger (1995) write of the same θ coefficient from SSIV estimation that it is the "matrix of coefficients from a regression of [true values] on [predicted values]" and "represents a kind of attenuation bias arising from the use of reduced-form coefficients from a separate sample. Corollary 1.1 explicitly provides a formula for $\mathrm{E}[\widehat{\theta}]$ in the SSIV case. They note that this property of SSIV "contrasts sharply with the tendency of conventional IV estimates to be biased toward OLS."

Consistent relationship between first- and second-stage datasets. As noted above, a key challenge that could arise in our setting is differences in the underlying populations of the two samples. We provide evidence that the families in the surveys are drawn from roughly the same underlying population as the families in the Census and that these patterns do not seem to be changing over time (Figure C.4, Tables C.2 and C.3). Related, we show that our measures of parental income track known trends in inequality as well as the Black-white income gap over the 20th century (Figure A.1), providing reassurance that the predictions convey useful information about the distribution of parental income.

Another challenge present in our approach is imperfect recall of parental attributes. Appendix C extensively considers the accuracy of recall by comparing answers across respondent sex (Figures C.1 and C.2, Table C.1); between siblings (Figure C.3); as well as by comparing answers between parents and children in the PSID, in which we see both self-reported parental answers and retrospective children's answers (Figure C.5, Tables C.4 and C.5). In particular, the exercises comparing answers across respondent sex over time also indicate that the accuracy of recall is not changing over time in a way that would drive the 1910s–1940s persistence decline. We summarize these results in Section 3.2 of the paper.

Robustness to changing sets of IVs. Our preferred set of instrumental variables is the *father occupation* × *race* × *South* triplets. They allow us the maximum predictive power for parental income among variables that are available in all of our surveys. But we can show robustness of our main result to adding more covariates (which necessitates using a subsample of the data) or to reducing covariates.

Why is this robustness test important? Each of these sets of IVs will have different prediction error from the first-stage estimation (the θ term) and a different exclusion-restriction violation (the ω term). We have no *ex ante* intuition on the relative sizes of these bias terms over time as the set of instruments changes. As $\beta^{\text{TS2SLS}} = \theta\beta^{\text{OLS}} + \omega$, then continuing to find a decline in β^{TS2SLS} as θ and ω are allowed to vary suggests that the decline is driven by a true decline in β^{OLS} from the 1910s to 1940s cohorts.

We have already shown in the paper that when we add *more* predictors in the first stage we continue to find a robust decline in β^{TS2SLS}. To recap, we use father's education as an additional instrument in Figures B.2 and B.3, and we use more detailed childhood regions (instead of merely a *South* dummy) in Figure B.4. In both cases, the results are very similar to our baseline figures and show a significant, monotonic decline in intergenerational persistence over the 1910s to 1940s birth cohorts.

We can show similar results when we *reduce* the set of IVs. Figure D.1 shows that we can recover the decline in the estimated IGE from 1910s to 1940s using any subset of our *father occupation* × *race* × *South* triplets. In almost all cases, not only do the results recover the 1910s–1940s decline, but the monotonicity of the decline is also replicated. Moreover, once occupation is included as a predictor of parental income, all of the estimates are very close to each other. In the spirit of an over-identification test, the fact that we continue to recover the decline given that each of these alternative estimates has different θ and ω values over time suggests again that β^{OLS} is likely driving the decline in our estimated β^{TS2SLS}. In particular the quantitative similarity of the estimates when occupation is included as an instrument, but not without, is consistent with the intuition that using race and region cells on their own are likely to fail the exclusion restriction, but interacting them with occupation yields valid instruments.

Comparison to results using direct measures of parental income. Panel data sources such as the NLS are too modern to allow us to examine persistence in the 1910s–1930s cohorts, but we can compare our two-sample results with OLS results from the NLS for the late 1940s and 1950s cohorts, like those in Davis and Mazumder (2022).

The NLS surveys interview both parents and children, thereby providing measures of observed parental income and predicted parental income (based on the recollection of their children). We can thus use these surveys to directly compare β^{TS2SLS} and β^{OLS} (and thus to directly examine the net effects of the θ and ω terms).

We show results from this exercise in Appendix Figure D.2 utilizing our usual sample of respondents for whom we have all necessary information to construct our predicted childhood income. In panel (a), the first series shows the estimated IGE when we directly use the average of observed parental household income. The second series shows results using our usual two-step imputation method. The second series always sits above the first, though they are not statistically distinguishable.[53]

But the important point we take from this figure is that the two series move together *in changes*. Our claim in the paper is not that our two-step estimates of mobility are the same *in levels* as ones that we could hypothetically estimate if we observed actual parental income. We instead make the claim that the decline in our two-step estimates indicates a decline in actual persistence. That these two estimates using data from the 1940s and 1950s largely move together in changes is reassuring.

We can repeat this exercise for the late 1940s and 1950s cohorts with the PSID, shown in Appendix Figure D.3, and again find that the mobility measures estimated using predicted vs. actual income are not statistically different from each other and mirror each other in changes.

While not a direct test of the full 1910s–1940s decline, it is heartening to see that our method provides very similar results to those using direct measures of parental income for the late 1940s and 1950s birth cohorts, the earliest cohorts for which we can perform this exercise in US data.

Varying prediction error. A potential concern is that the decline in β^{TS2SLS} could be driven by a decline in the prediction-error term θ while β^{OLS} and ω terms remain unchanged (or are even increasing).

[53] It is possible that using only a few years of observed parental income contains more measurement error than our two-step process. If parental income is very noisy from year to year, then our triplet may better capture its permanent component than an average based on only a few years.

Our first argument against this idea is that the quality of the data sources is improving over time. This improvement would cause the θ term to *increase* over the 1910s to 1940s birth cohorts, rather than decline, thus pushing *against* our basic result that persistence declined.

Our second argument is based on the multiple imputation results in Figure B.5. Recall that this exercise maps each IV triplet to a randomly chosen income value in the corresponding cell (and repeats this procedure 100 times), using microdata from the Census and the 1936 Expenditure Survey. This exercise attenuates the estimated IGE in every year. Cortes-Orihuela *et al.* (2022) conduct a similar exercise in administrative Chilean data and show that this produces a lower bound on the IGE. In our data, we also see a substantially attenuated IGE in the multiple imputations exercise, but we still see qualitatively similar results on the *trend* of the decline, suggesting that changing amounts of prediction error are not driving our results. While these estimates put a lower bound on the trend in θ, we note that they are still vulnerable to omitted variables bias from changing ω given that the choice of instruments is being held constant. But the robustness of the qualitative pattern to assuming that every deviation of father's income from the cell-average is prediction error is reassuring.

Mobility measures that do not depend on IV estimation. As noted, the key challenge for historical income mobility estimation is the absence of parental income and the need to model it as the first step in a two-step procedure. We thus show two measures of mobility that do *not* depend on IV estimation to assuage concerns that our main results are merely artifacts of changing bias terms across the 1910s to 1940s cohorts.

First, as we show in Section 6, part of the overall IGE and rank-rank measures come strictly from *between-group differences* and thus are free from the biases introduced from two-step estimation. We show in Figure A.12 that the between-group terms of both the IGE and the rank-rank follow the same pattern across cohorts as our baseline figures, despite our baseline estimates being susceptible to biases.[54] We thus conclude that these biases are not changing appreciably across time, as mobility measures stripped of this bias follow the same pattern as the baseline results across cohorts (that is, roughly a u-shape for the IGE and an L-shape for the rank-rank).

Another mobility measure we can estimate across our cohorts that does not depend on first-stage prediction is *educational* mobility, because in many of our surveys respondents are asked their fathers' level of education. We show these results in Figure A.7, which depicts a similar decline between the 1910 and 1940 period as our main figures (in this case, the decline continues through the 1950s cohorts), but does not rely on any characteristics Z_i to predict parental income.

[54] While the third term in the decomposition uses our predicted parental income variable, only aggregate, not individual, measures enter into the expression. Note that we show in Figure A.1 that our predicted Black-white gaps for parental income follow the aggregate time series in Margo (2016).

D.2 Imputation-TS2SLS Equivalence in Levels Specification

In this section, we further illustrate the similarity between our primary imputation-based OLS estimator and the TS2SLS estimator. We re-write the standard OLS estimation from equation (7) using levels of income and predicted parental income as the right-hand-side variable for a given birth cohort c:

$$Y_{ic} = \alpha + \beta_c^{\text{levels}} \tilde{Y}_{ic}^p + e_i, \tag{11}$$

In a levels specification, our imputation approach—using the mean household income for each combination of father's occupation, region, and race—is numerically identical to the TS2SLS estimates where the instruments are fully interacted. If P_Z is the projection matrix onto the vector of race-region-occupation cells Z, then the imputations are given by $\tilde{Y}^p = P_Z Y^p$. For illustrative purposes and to reduce notation, assume both samples have the same size.[55] Then we also have $\beta^{\text{TSIV}} = (P_Z Y^p)^{-1} P_Z Y_c$ as the TSIV estimator[56], which is numerically identical to the OLS estimate using imputed data $\beta^{\text{OLS}} = \frac{\text{Cov}(\tilde{Y}^p, Y_c)}{\text{Var}(\tilde{Y}^p)} = ((P_Z Y^p)'(P_Z Y^p))^{-1}(P_Z Y^p)' Y_c = (P_Z Y^p)^{-1} P_Z Y_c = \beta^{\text{TSIV}}$ by the usual idempotency and self-adjointness of P_Z.

While in theory, the standard errors could be larger due to error in the first-stage regression, in practice the standards errors between the two estimates are quite close, owing to the large sample sizes in the Census being used for the first stage. We further adjust the TS2SLS for heteroskedascity following Pacini and Windmeijer (2016).

Panel (a) of Figure D.4 shows the numerical equivalence between TS2SLS and OLS for $\hat{\beta}^{\text{levels}}$ in our data.[57] We present a table of coefficients from this specification in Appendix Table D.1.[58]

A natural concern with the levels specification is that it misses non-linearities in the underlying structural relationship, which would be implied, for example, by credit constraints (Loury, 1981). Consistent with the literature, we have a concave relationship in the levels-on-levels regression, and this appears stable over time. Table D.2 shows the non-linearity with a quadratic specification, following Løken *et al.* (2012). The quadratic term (the square of predicted parental income) is generally significant in every year and a similar order of magnitude over the 20th century, and the resulting effect at the 25$^{\text{th}}$ percentile also shows a u-shape over time.

The numerical equivalence between OLS and TS2SLS will not hold for the log-log

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[55] We also suppress the i subscripts for notational simplicity.

[56] Inoue and Solon (2010) show that this is dominated in efficiency terms by the TS2SLS estimator that adjusts for finite-sample issues in the empirical covariance matrix, but for illustration and because both of our samples are large, we focus on the TSIV estimator in the text, but conduct all estimates with the TS2SLS estimator.

[57] For completeness, Appendix Figures D.5 and D.6 also present the results by gender and by subgroup using equation (11).

[58] Note that the estimates in this table will not be identical to those plotted in Figure D.4 because of slightly different methodologies. The figure implements two-stage least squares using the nearest source of microdata relative to the respondent's childhood (thus only considering 1920s–1970s cohorts). In contrast, the table uses levels of parental income based on the baseline (interpolated) measure of parental income.

specification estimated in the main text because we impute the log of average income (as we only have mean incomes for the early cohorts in our sample) and the usual differences between $\log(\mathrm{E}[x])$ and $\mathrm{E}[\log(x)]$. Nonetheless, the difference is quantitatively small, and the basic "u-shape" pattern in the IGE under the TS2SLS approach is shown in panel (b) of Appendix Figure D.4.

While the TS2SLS and imputation approaches agree exactly in levels and approximately in IGE space, the primary limitation is that we have no microdata for the 1910s cohort of farmer fathers, owing to the lack of any agricultural microdata from this early cohort. Given the importance of farmers in this period, and the importance of this cohort in showing the trend of increasing mobility in the early part of the 20th century, we present the imputation-based estimates in the main text.

D.3 Connecting level-, log-, and rank-based estimates

The levels-on-levels specification is not completely unfamiliar to the mobility literature, being used in Dahl and Lochner (2012) and Løken *et al.* (2012). It is, however, not regularly used in estimates of U.S. mobility, which have traditionally focused on the log-log specification as in Becker and Tomes (1979) or, more recently, the rank-rank specification as in Chetty *et al.* (2014a). But these latter two measures can be approximated using β^{levels}. For example, the intergenerational elasticity (at the population mean) can be approximated by $\beta^{\text{IGE}} = \frac{\mathrm{E}[Y_i^p]}{\mathrm{E}[Y_{ic}]}\beta^{\text{levels}}$ using the levels-based specification in equation (11) and first-order Taylor approximations of $\log(Y_i^p)$ and $\log(Y_{ic})$ around their means (i.e., $\log(x) - \log(\mathrm{E}[x]) \approx \frac{x}{\mathrm{E}[x]} - 1$ for both generations). As shown in Figure D.7, the estimated IGEs using this approximation are generally lower than those from the main text, but they retain the visible and stark u-shape over the 20th century.

To approximate the rank-rank, note that if the income distribution is normal or lognormal then the rank-rank correlation is exactly equal to $\beta^{\text{RR}} = \frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2})$, where β^{IGC} is the intergenerational correlation, which can also be obtained from β^{levels} by multiplying it with the ratio of the standard deviations (i.e., $\beta^{\text{IGC}} = \sqrt{\frac{\mathrm{Var}[Y_i^p]}{\mathrm{Var}[Y_{ic}]}}\beta^{\text{levels}}$). The lognormal assumption is likely close to true in the large populations we are sampling from, even if it may not hold for the within-county income distributions of interest to the recent literature. Figure D.7 shows the approximated $\hat{\beta}^{\text{RR}} \approx \frac{6}{\pi}\arcsin\left(\frac{1}{2}\sqrt{\frac{\mathrm{Var}[\hat{Y}_i^p]}{\mathrm{Var}[\hat{Y}_{ic}]}}\hat{\beta}^{\text{levels}}\right)$ approximation (standard errors calculated using the delta-method) and that, in practice, the levels-based approximation of the IGC and rank-rank estimates using our baseline approach are very similar both quantitatively and qualitatively.[59]

To summarize, translating these population relationships to estimates, the TS2SLS and imputation-based OLS yield point estimates of $\hat{\beta}^{\text{levels}}$ that are quantitatively iden-

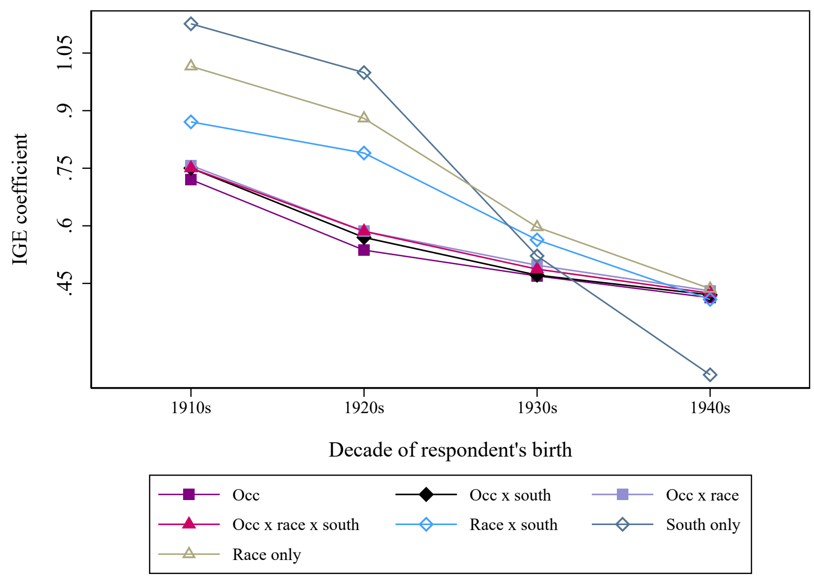
[59] A coarser approximation is to simply set $\beta^{\text{RR}} \approx \beta^{\text{IGC}}$, since note that the first two non-zero terms of the Taylor expansion of arcsin gives $\frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2}) \approx \frac{3}{\pi}\beta^{\text{IGC}} + \frac{1}{4\pi}(\beta^{\text{IGC}})^3$. Thus the approximation $\beta^{\text{RR}} \approx \beta^{\text{IGC}}$ is likely to be a good approximation given the coarseness of the historical data we are working with. As the IGC can be calculated using a simple linear regression rather than non-linear transformations it is a good measure for capturing "pure" mobility, independent of inequality, in data-constrained historical contexts.

tical. Further, armed only with TS2SLS estimates of $\hat{\beta}^{\text{levels}}$, we can recover approximations to the primary measures used in our paper (and in the literature), without using any non-linear transformations of predicted parental income. These approximated estimates exhibit qualitatively very similar patterns to their analogues in the main text. We conclude that none of our results depend on the use of imputed incomes in OLS versus TS2SLS regressions.

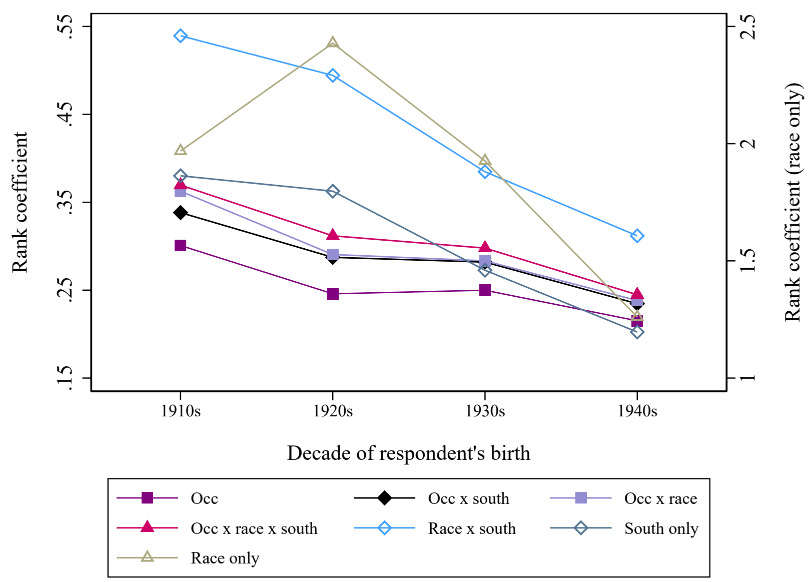
Finally, we note that one key difference between the logs-based and levels-based estimates is that the 1940s cohort is the most mobile when considering the IGE, whereas the 1950s cohort appears more mobile when considering levels of income, even as the rank-rank correlation stays constant. This difference can be explained by the standardizations of income discussed above. In short, holding the intergenerational correlation fixed (which appears to be approximately the case starting in the 1940s; see Figure 1), the IGE is equal to the IGC multiplied by the standard deviation of logged children's income over logged parent's income $\left(\text{i.e., } \frac{\hat{\sigma}_{y_{ic}}}{\hat{\sigma}_{y_i^p}}\right)$. The 1950s cohort had relatively lower parental inequality and higher adult children inequality, thereby making the IGE rise (the ratio of standard deviations increased from 1.92 to 2.36 between 1940 and 1950). Next, the levels coefficient is approximately equal to the IGE multiplied by the mean of children's income over parent's income using levels of income. Given immense growth of parental income in this time period, this ratio of $\frac{\mathrm{E}[\hat{Y}_{ic}]}{\mathrm{E}[\hat{Y}_i^p]}$ fell from 1.51 to 1.03, so that the levels coefficient declined between 1940 and 1950 (despite the increase in the IGE). In sum, the rapid rate of growth in parental income during a period of relatively lower inequality makes it so that the IGE rises, while the levels coefficient continues to decline (see Appendix Figure D.8).

Figure D.1: 1910–1940 IGE and rank-rank correlation varying sets of instruments

(a) IGE



(b) Rank-rank

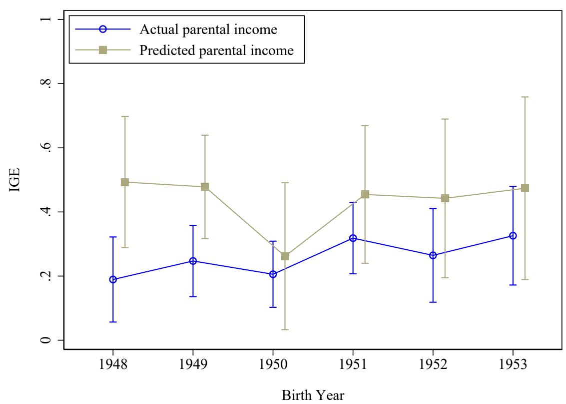


Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

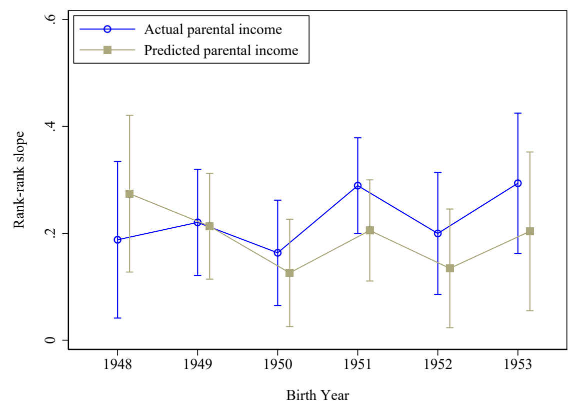
Notes: The IGE and rank-rank estimates are based on the baseline sample of respondents ages 30–50 using equations (2) and (3) for the 1910s–1940s cohorts. Each series uses different characteristics to predict parental income. To predict parental income, we use family income from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares. In panel (b), the estimates that only use race as a predictor of parental income are plotted on the secondary y-axis.

Figure D.2: IGE and rank-rank correlation using actual vs. predicted income in NLS sample

(a) IGE



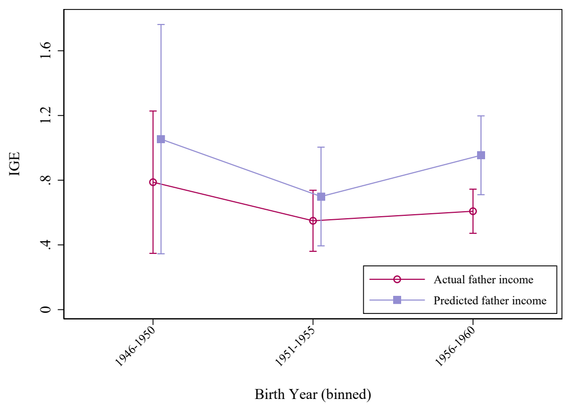
(b) Rank-rank



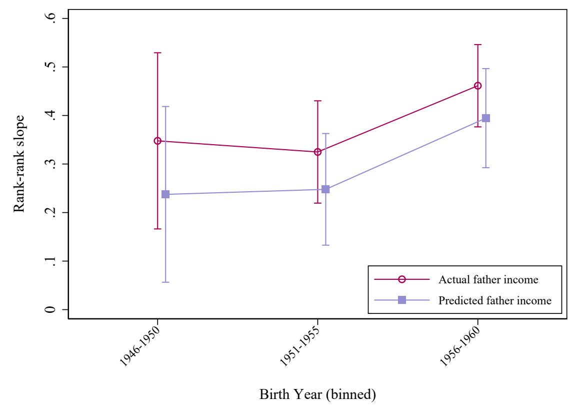
Notes: These figures plot the intergenerational elasticity and rank-rank correlation based on the sample from Davis and Mazumder (2022) using the National Longitudinal Surveys. The figures restrict the sample to individuals who reported a father's occupation retrospectively in the first wave of the survey. Actual income refers to the measure from Davis and Mazumder (2022) (i.e., calculated as an average of all total family income reports in the first three survey waves where income data was reported). Predicted parental income refers to utilizing our baseline income predictions (which vary by *occupation* × *race* × *South*). In both panels, we follow Davis and Mazumder (2022) and use the weights corresponding to the adult children in the first round of the survey.

Figure D.3: IGE and rank-rank correlation using actual vs. predicted income in PSID sample

(a) IGE



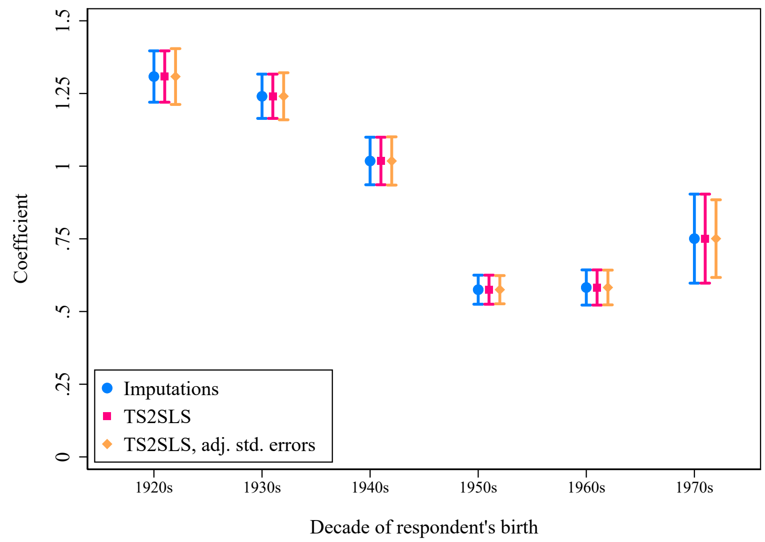
(b) Rank-rank



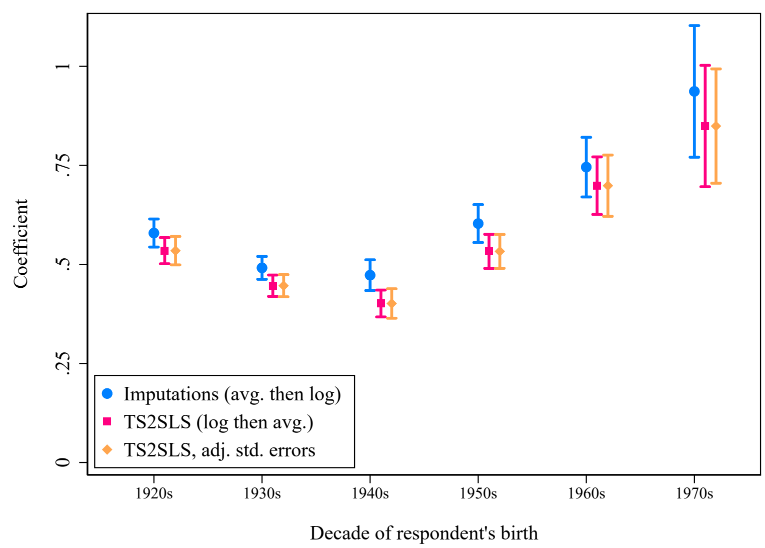
Notes: These figures plot the intergenerational elasticity and rank-rank correlation using the Panel Study of Income Dynamics. The first panel plots the IGE using actual and predicted parental income. The second panel plots the analogous rank-rank correlations. We create 5-year birth cohort bins, and respondents are ranked relative to other respondents in the same bin. Actual income is calculated as an average of total family income reports in the first three survey waves around when the adult child respondent turns 40. Actual parental income is calculated in the same manner. Predicted parental income refers to utilizing our baseline income predictions (which vary by *occupation* × *race* × *South*). In all panels, we use the 1997 cross-sectional, individual weights for adult children. The figures restrict the sample to adult child respondents with available actual income (3 years), working father actual income (3 years), and predicted working father income (i.e., individuals who reported a father's occupation retrospectively). All income measures are in 2015 dollars.

Figure D.4: Intergenerational mobility by birth decade using imputed average incomes and two-sample two-stage least squares

(a) Levels of income



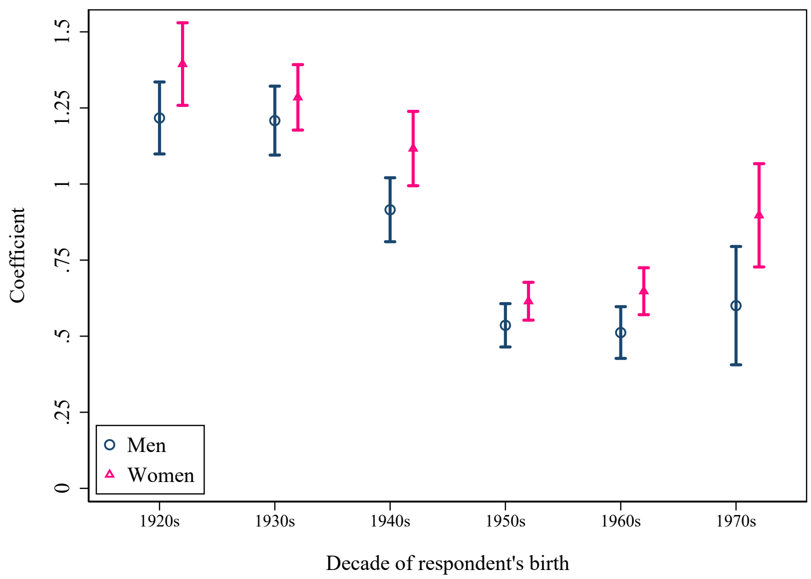
(b) Logged income



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50. The top panel considers levels of income and the bottom panel logged household income. The first series in both panels uses the baseline approach for predicting parental family income (i.e., calculating average household incomes at the *occupation* × *race* × *South* level and in the bottom panel, subsequently applying the log transformation). The second series in the top panel uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. The third series adjusts robust standard errors using Pacini and Windmeijer (2016). Panel (b) uses an identical approach for the last two series, except that it uses the Census to predict logged household income. To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920s–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively.

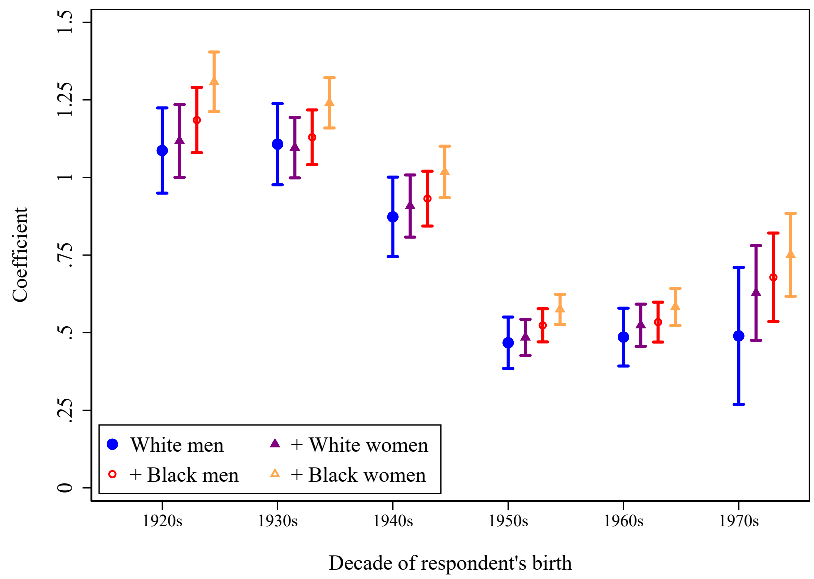
Figure D.5: Intergenerational mobility using levels of family income, by sex



Sources: This figure combines 15 surveys, described in Section 2 and in further detail in Appendix E.

Notes: This figure uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. All estimates report robust standard errors using Pacini and Windmeijer (2016). To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

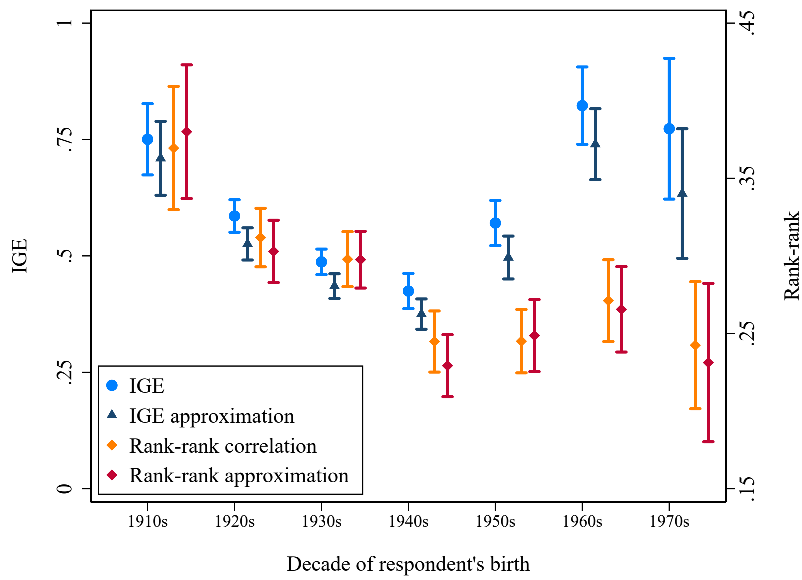
Figure D.6: Intergenerational mobility using levels of family income and including under-represented groups



Sources: This figure combines 15 surveys, described in Section 2 and in further detail in Appendix E.

Notes: This figure uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. All estimates report robust standard errors using Pacini and Windmeijer (2016). To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Figure D.7: Mobility by birth cohort, approximating IGE and rank-rank correlation with levels specification

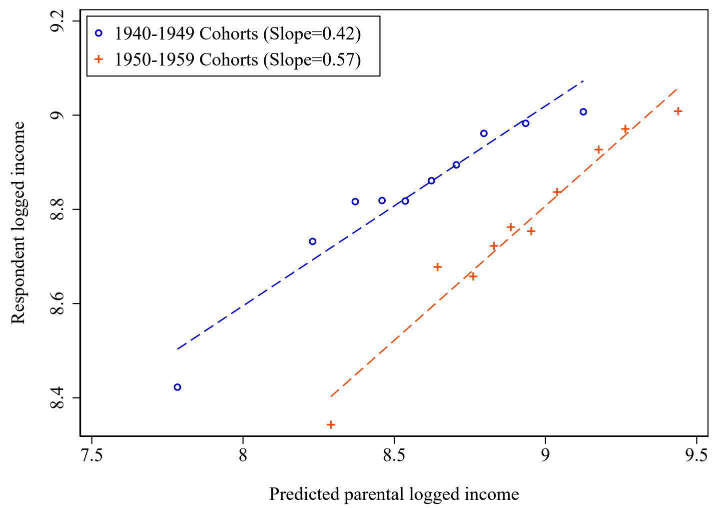


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

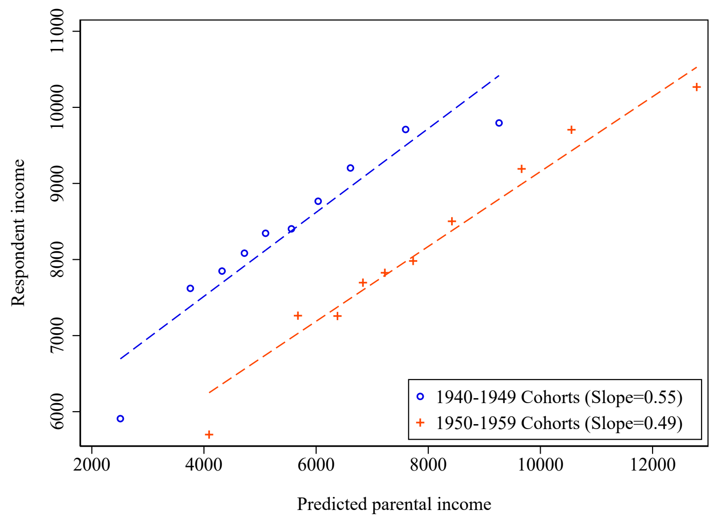
Notes: All estimates are based on the baseline sample of respondents ages 30–50. The first and third series reproduce the baseline IGE and rank-rank estimates from Figure 1. The second and fourth series come from linear specifications using levels of income for both generations (reported in Appendix Table D.1). The second series multiplies the levels-based estimate with the ratio of average parental income to average adult children's income. The fourth series transforms the levels-based estimate using $\beta^{\text{RR}} = \frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2})$, where $\beta^{\text{IGC}} = \sqrt{\frac{\text{Var}[Y_i^p]}{\text{Var}[Y_{ic}]}}\widehat{\beta^{\text{levels}}}$. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure D.8: Bin-scatter depictions of the change in intergenerational persistence between 1940s and 1950s cohorts

(a) Intergenerational elasticities



(b) Levels-on-levels estimates



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50 in the 1940s and 1950s cohorts. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Table D.1: Intergenerational mobility using levels of family income, by birth cohort

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Level coefficient	1.669	1.389	0.964	0.551	0.492	0.640	0.570
	[0.095]	[0.047]	[0.030]	[0.024]	[0.023]	[0.034]	[0.064]
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2), but using income levels for both generations. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table D.2: Intergenerational mobility using levels of family income and quadratic specification, by birth cohort

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Level coefficient, linear	2.071	2.311	1.484	1.075	0.932	1.184	1.524
	[0.348]	[0.174]	[0.111]	[0.084]	[0.082]	[0.146]	[0.218]
Level coeff., quadratic (×100)	-0.008	-0.017	-0.007	-0.004	-0.002	-0.003	-0.004
	[0.008]	[0.003]	[0.002]	[0.001]	[0.000]	[0.001]	[0.001]
Derivative at 25th percentile	1.780	1.617	1.113	0.709	0.633	0.783	0.885
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2), but using income levels for both generations and allowing for a quadratic term in parental income. For ease of exposition, the quadratic term is divided by 100 in the specification. "Derivative at 25th percentile" refers to the marginal effect evaluated at the income level that corresponds to the 25^{th} percentile of the predicted parental income distribution for the corresponding cohort. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table D.3: Intergenerational elasticity using quadratic specification, by birth cohort

	(1) 1910s	(2) 1920s	(3) 1930s	(4) 1940s	(5) 1950s	(6) 1960s	(7) 1970s
Linear coefficient	-0.167	0.672	0.393	1.887	2.649	2.266	8.859
	[0.930]	[0.427]	[0.336]	[0.484]	[0.699]	[1.658]	[5.104]
Quadratic coefficient	0.061	-0.006	0.006	-0.087	-0.118	-0.080	-0.440
	[0.061]	[0.028]	[0.021]	[0.029]	[0.040]	[0.091]	[0.276]
Coeff., linear only	0.750	0.586	0.487	0.424	0.571	0.823	0.773
Slope at mean	0.770	0.583	0.489	0.395	0.551	0.816	0.803
	(0.69, 0.85)	(0.54, 0.62)	(0.46, 0.52)	(0.35, 0.44)	(0.50, 0.60)	(0.73, 0.90)	(0.63, 0.98)
Slope, 10th perc.	0.705	0.589	0.483	0.471	0.636	0.862	1.035
	(0.58, 0.83)	(0.54, 0.64)	(0.44, 0.52)	(0.42, 0.52)	(0.57, 0.70)	(0.74, 0.99)	(0.63, 1.44)
Slope, 90th perc.	0.834	0.578	0.495	0.316	0.460	0.767	0.537
	(0.66, 1.01)	(0.50, 0.66)	(0.44, 0.55)	(0.24, 0.39)	(0.37, 0.55)	(0.62, 0.91)	(0.26, 0.82)
R-sq., linear only	0.163	0.117	0.101	0.049	0.058	0.067	0.054
R-sq., quadratic	0.164	0.117	0.101	0.050	0.059	0.067	0.057
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2) but including a quadratic term in logged parental income. "Coeff., linear only" refers to the intergenerational elasticity from the linear specification. "Slope at mean" refers to the marginal effect evaluated at the mean of the predicted parental income distribution for the corresponding cohort. "Slope, 10th perc." and "Slope, 90th perc." refer to the marginal effect evaluated at the 10th and 90th percentile of the predicted parental income distribution, respectively, for each cohort. The 95 percent confidence intervals of these slopes are all reported in parentheses underneath their corresponding estimate. "R-sq." refer to the R^2 from the linear and quadratic specifications.

Table D.4: Rank-rank correlation using quadratic specification, by birth cohort

	(1) 1910s	(2) 1920s	(3) 1930s	(4) 1940s	(5) 1950s	(6) 1960s	(7) 1970s
Linear coefficient	0.499	0.512	0.362	0.420	0.397	0.385	0.364
	[0.074]	[0.037]	[0.035]	[0.040]	[0.041]	[0.055]	[0.084]
Quadratic coefficient	-0.132	-0.203	-0.065	-0.178	-0.155	-0.116	-0.125
	[0.075]	[0.037]	[0.034]	[0.040]	[0.042]	[0.054]	[0.084]
Coeff., linear only	0.369	0.312	0.298	0.245	0.245	0.271	0.242
Slope at mean	0.376	0.320	0.300	0.250	0.248	0.273	0.244
	(0.34, 0.42)	(0.30, 0.34)	(0.28, 0.32)	(0.23, 0.27)	(0.23, 0.27)	(0.25, 0.30)	(0.20, 0.28)
Slope, 10th perc.	0.472	0.472	0.349	0.384	0.366	0.364	0.342
	(0.35, 0.59)	(0.41, 0.53)	(0.29, 0.40)	(0.32, 0.45)	(0.30, 0.43)	(0.28, 0.45)	(0.21, 0.48)
Slope, 90th perc.	0.263	0.150	0.245	0.107	0.125	0.185	0.147
	(0.14, 0.39)	(0.09, 0.21)	(0.19, 0.30)	(0.04, 0.17)	(0.06, 0.19)	(0.10, 0.27)	(0.01, 0.28)
R-sq., linear only	0.139	0.100	0.091	0.060	0.060	0.073	0.060
R-sq., quadratic	0.140	0.103	0.091	0.062	0.061	0.073	0.060
Observations	5,207	13,328	12,446	11,589	10,951	6,611	3,119

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2) but including a quadratic term in logged parental income. For ease of exposition, the quadratic term is divided by 100 in the specification. "Coeff., linear only" refers to the intergenerational elasticity from the linear specification. "Slope at mean" refers to the marginal effect evaluated at the mean of the predicted parental income distribution for the corresponding cohort. "Slope, 10th perc." and "Slope, 90th perc." refer to the marginal effect evaluated at the 10th and 90th percentile of the predicted parental income distribution, respectively, for each cohort. The 95 percent confidence intervals of these slopes are all reported in parentheses underneath their corresponding estimate. "R-sq." refer to the R^2 from the linear and quadratic specifications.

E Additional detail on data sources

E.1 Harmonizing surveys

We typically include a survey in the analysis if it meets two main conditions: First, it must ask survey respondents about their family income. And second, it must ask respondents about their fathers' occupation while they were growing up, and the available occupation codes must be able to be mapped to our coarsened occupations (discussed below). The surveys that meet these conditions usually also include other useful information, including demographic characteristics of the respondent as well as those of the father and mother.

In the end, we have fifteen harmonized surveys:

- American National Election Studies (ANES),1956–1970
- Americans View Their Mental Health (AVTMH), 1957 & 1976
- General Social Survey (GSS), 1972-2018
- National Fertility Survey (NFS), 1970
- NLS Mature Women (NLSMW), 1967
- NLS Older Men (NLSOM), 1966
- NLS of Youth, 2002 [60]
- NLS Young Men (NLSYM), 1981 [60]
- NLS Young Women (NLSYW), 1988 [60]
- National Survey of Black Americans (NSBA), 1979–1980
- National Survey of Families and Households (NSFH), 1987–1988
- Occupational Changes in a Generation (OCG), 1962 & 1973
- Panel Study of Income Dynamics (PSID), 1997 & 2017 [61]

The cohorts present in each survey and a description of each sample are displayed in Table E.1.

To minimize life-cycle bias, we restrict the sample to U.S.-born respondents aged 30–50. We also include respondents in this age range for whom we do not know where

[60] Note that the National Longitudinal Surveys can also be used as repeated cross-sections. For these three surveys, we select the cross-section to use by first observing the median age in the earliest cross-section of the survey. We then calculate the year in which the median age of respondents would be around 40. If the survey was not conducted in this year, we take the nearest survey year. Nevertheless, in these three NLS surveys (similarly to in the NLSMW and NLSOM) we typically use the first wave to collect demographic information about the respondent (e.g., sex, race, birth year, birthplace) as well as retrospective information about the parents' occupations and educational attainment. The one exception is that in the NLSYW survey, we collect information about the mother's occupation when the question was re-asked in 1978.

[61] Note that the PSID can be used as repeated cross-sections. We rely on the 1997 wave because this was the first year in which retrospective questions about parents' occupations were asked with sufficient detail and in which cross-sectional individual weights were available. We also then use the 2017 survey to bring in a new cohort of individuals ages 30–50. We intentionally exclude any 1997 respondents who appear again in 2017. Retrospective questions were only asked to household heads and their wives; whenever we have two respondents within a family with all of the available information for our analysis, we select a member at random.

they were born.[62] Finally, we restrict the sample to individuals whose race is recorded as white or Black.[63]

Once we identify and clean these surveys, we pool them together for the analysis. An individual is in our baseline sample if he/she has an available family income, recorded race, region of birthplace/childhood (South vs. non-South), and father's occupation. Together, these four components allow us to measure the respondent's income level and predict parental family income. Table E.2 shows how the sample size changes as we sequentially impose each sample restriction. The baseline cohorts and samples in each survey are summarized in Figure E.1 as well as Tables E.3 and E.4.

E.2 Respondent family income

In all of our harmonized surveys, respondents are asked about their family income in that year. Some surveys provide the information in categories, while others provide exact numerical values. To be consistent in our coding, we rely on the bin structure of the surveys and assign respondents the midpoint of that category. The exception to this step is that for individuals who make the least (i.e., whose income falls in the bottom bin, including those with zero income), we assign them 0.75 $\times$ the upper boundry of the category. For respondents who make the most (i.e., whose income falls in the top bin), we assign them 1.25 $\times$ the lower boundry of the category.

For surveys that report exact income values, we replicate the bin structure for assigning respondents a family income value. In particular, we first find a survey that took place around the same time period and use that survey's bin structure as a template. We then assign individuals the midpoint of their corresponding bin.[64] Ultimately, we want to observe a roughly equal proportion of respondents in each bin. When the outlined procedure does not yield this result, we consider alternative bin structures (namely, the bin structure in other surveys) until we find a template that results in a relatively equal distribution.

Finally, for consistency, we ensure that each survey has roughly 10–12 bins for respondent family income. For surveys that have significantly more bins, we combine bins and assign respondents the midpoint of the new category (while simultaneously ensuring that each bin has roughly the same share of respondents). Table E.5 summarizes that share of the baseline sample that is top and bottom coded in each survey as well as the number of income bins utilized.

[62] We exclude foreign-born respondents because we cannot know with certainty whether they grew up in or outside of the United States. Because we predict parental family income using U.S.-based data and because the average income for the same occupation can differ across countries, we refrain from predicting parental income for the fathers of these respondents and thus exclude these father-children pairs.

[63] Respondents who are classified as Hispanic in surveys are re-classified as white unless there is additional information available on race. Respondents of other races, who comprise a tiny share of survey samples, are excluded from the analysis.

[64] For instance, because NSFH interviews took place in 1987 and 1988, we use the 1988 bins from the GSS as a template for the bin structure of family income for NSFH respondents.

E.3 Predicting parental family income

E.3.1 Coarsened occupations

We obtain father occupation from the respondent, who typically reports his/her father's occupation when the respondent was growing up or around 14–16 years old. In many of the surveys, we are also able to obtain analogous information for the mother's occupation.

Across all surveys, we harmonize occupations into 28 categories, corresponding to the main occupations in the American National Election Survey. The ANES occupations we use are:

- Accountants and auditors
- Clergymen
- Teachers
- Dentists
- Physicians and surgeons
- Engineers
- Lawyers and judges
- Social and welfare workers
- Nurses
- Other professional and technical occupations
- Semi-professional occupations
- Self-employed businessmen, managers, and officials
- Businessmen, managers, and officials
- Bookkeepers
- Stenographers, typists, and secretaries
- Other clerical workers
- Higher-status sales workers in "outside" sales
- Inside sales workers (e.g., salesmen, clerks)
- Lower-status sales workers in "outside" sales (e.g., peddlers, newsboys)
- Foremen
- Skilled craftsmen and kindred workers
- Semi-skilled operatives and kindred workers
- Protective service workers
- Private household workers
- Other service workers
- Farm laborers
- Non-farm laborers
- Farm operators

E.3.2 Constructing baseline measures of parental income

In order to approximate parental family income, we use various datasets from throughout the 20th century. In particular, we rely on data from the 1901 Cost of Living Survey and the 1900 Census of Agriculture (which we refer to as our "1900-based" income predictions), the full-count 1940 Census and the 1936 Expenditure Survey (henceforth our

"1940-based" income predictions), and the 1960–1990 Censuses (Ruggles *et al.* 2021). We provide more details on the 1936 Expenditure Survey in Section E.3.4.

Because we want our baseline measures of parental income to approximate the income of the fathers' generation, we restrict these datasets whenever possible to individuals who resemble the survey respondents' fathers (Ruggles *et al.*, 2021). In particular, we restrict the samples to men who are between the ages of 30–50, whose race was recorded as either white or Black, and who had a child younger than 18 present in the household. We then build and use crosswalks that map the Census occupations into our 28 coarsened occupations.

Next, we calculate the mean income in each occupation for individuals with certain characteristics. In our baseline specification, we calculate the mean income by *occupation* $\times$ *race* $\times$ *South*.[65] Our preferred measure of income is mean household income, which sums the income of all family members within a household. To ensure that these measures are comparable throughout the analysis, all predicted income is reported in 1950 dollars.

The two exceptions to this straightforward approach are our 1900- and 1940-based income predictions. First, to construct 1900-based predicted income, we use information on average earnings by occupation from the 1901 Cost of Living Survey (Preston and Haines 1991) and collapse this information to our coarsened occupations. We then use the income of fathers ages 30–50 in the 1940 full-count Census to adjust these income values by race and Southern residence. For fathers who are farmers, we calculate predicted income using the 1900 Census of Agriculture. In particular, we use information on farm output and expenses from Merriam (1902) and follow the approach in Goldenweiser (1916) and Abramitzky *et al.* (2012) to calculate farmers' income by race and Southern residence. We additionally adjust these values by the share of farmers in that race and region that were owners (assuming that non-owners earn 50% of the estimated farm income).

Second, the 1940 income variable (i.e., wage and salary income) excludes income from self-employment and farming. We thus use an alternative data source from this time period — the 1936 Expenditure Survey — to calculate the average family income of farmers and of self-employed fathers separately by race and Southern residence.

After we construct these six versions of predicted income, we assign fathers an estimate using the data sources that are closest in time to when the respondent was 10 years old. In particular, we assign cohorts born between 1910 and 1930 a weighted average of the 1900- and 1940-based logged income predictions, with the weights reflecting the number of years between when the respondent was 10 years old and 1940. For cohorts born between 1930 and 1950, we assign a weighted average of the 1940-

[65] For any *occupation* $\times$ *race* $\times$ *South* cells with no available Black fathers, we impute the average income for that cell using the average racial income gap in that same region and the income of white fathers in that same occupation. In particular, we first calculate the white-Black income gap within an occupation and region, and then average these gaps across the occupations in that region, allowing occupations with more Black fathers to get greater weight in the calculation. Finally, we use the income of white fathers in the desired occupation and region in conjunction with the estimated average racial income gap in that region to impute the income of the missing cell.

and 1960-based logged income predictions, with the weights once again reflecting the number of years between when the respondent was 10 years old and 1960. For cohorts born between 1950 and 1960, we assign a weighted average of the 1960- and 1970-based logged income predictions, with the weights again reflecting the number of years between when the respondent was 10 years old and 1970. We continue this process for all respondents born in the 1960s–1970s cohorts using the income predictions constructed with the 1970–1990 Censuses.

A couple of times throughout the paper, we calculate and use predicted income for survey respondents, rather than using their total family income at the time of the survey. Similar to the method utilized to predict parental income, we use Census data as well as the 1936 Expenditure Survey to approximate respondents' income by their occupation, race, and Southern residence (as observed at the time of the survey). We assign respondents a predicted income value using the data sources that are closest in time to when the respondent is 40 years old. In practice, cohorts born between 1910 and 1920 are assigned a weighted average of the 1940- and 1960-based income predictions. All subsequent birth cohorts are similarly assigned predicted income values that are weighted averages constructed using the 1960–2000 Censuses as well as the 2010 and 2019 American Community Surveys.

E.3.3 Alternative ways to predict parental income

Our baseline 1940-based income predictions use the 1936 Expenditure Survey to estimate the average family income of farmer and self-employed fathers. However, we also consider in the robustness checks a different approach for predicting farmer and self-employed income for 1940. Following the approach in Collins and Wanamaker (2022), we use fathers ages 30–50 in the 1960 Census to calculate the ratio of farmer income to farm laborer income. We then use farm laborers' income in 1940 as well as these ratios to impute the 1940 income of farmers.[66] Second, we adjust the income of self-employed non-farm workers using a similar approach: we consider fathers ages 30–50 in the 1960 Census and compute ratios of mean earnings for self-employed workers relative to wage-and-salary workers. We then impute the earnings of self-employed non-farm workers in 1940 using these ratios. Throughout the analysis, we use the same level of granularity to compute ratios as we do when predicting parental income. Our preferred ratios therefore vary at the *race* × *South* level.[67] We then use this measure of farmer and self-employed income in the 1940-based income predictions, and blend these measures with earlier and later data sources similarly to in the baseline approach.

Throughout the robustness checks, we sometimes estimate parental income using fewer predictors of income than our baseline income predictions; sometimes the income predictions only use information about a father's occupation and other times they

[66] Throughout these calculations, we also follow Collins and Wanamaker (2022) and adjust farmer and farm laborer income measures upward to reflect the value of in-kind income.

[67] If there were fewer than 20 individuals in the 1960 Census cell (e.g., *occupation* × *race* × *South* × *education*), we rely on the mean income of individuals in the broader group (*occupation* × *race* × *South*) to construct ratios.

incorporate information on both occupation and race. For a sub-sample of respondents, we also predict parental family income using more predictors relative to our baseline predictions (namely, using information about the father's educational level as well as more-detailed information about the region of residence). For education variations, we use five levels of education—less than 8th grade, 8th grade, some high school, completed high school, and at least some college—to calculate income at the *occupation* $\times$ *race* $\times$ *South* $\times$*education* level.[68] For *occupation* $\times$ *race* $\times$ *region* variations, the four Census regions are used: Northeast, Midwest, South, and West. For both of these more granular versions of predicted income, we use the Collins and Wanamaker (2022) adjustment for farmers and self-employed fathers given the small sample size of the 1936 Expenditure Survey. Finally, we also calculate the mean of the 1950 *occscore* variable—which reflects the median total income of all persons with that particular three-digit occupation in that Census—for the 28 coarsened occupations (i.e., with no additional variation at the race or region level).

Next, we predict parental family income for working mothers and for non-working fathers. For individuals who provided information about their mothers' occupations, we predict income in an analogous way to our baseline approach, but utilizing the income of mothers who were household heads and ages 30–50 in the 1940–1990 Censuses.[69] Moreover, certain survey respondents had a missing father occupation not because the respondent did not know what the occupation was, but because the respondent reported that their father was not working (e.g., unemployed, retired). We assign a predicted income value to these non-working fathers using information on the average incomes of non-working fathers in the 1901 Cost of Living Survey and the 1940–1990 Censuses.

Throughout the robustness checks, we also consider other variations of the baseline income prediction. One variation only considers the income of fathers rather than household income. In another variation, we allow fathers with more children to get greater weight when calculating the average income in an *occupation* $\times$ *race* $\times$ *South* cell (i.e., weighting each father by the number of children younger than 18 present in the household). Last, we construct a version that uses the closest source of microdata relative to the survey respondents' childhood. In practice, this means excluding the 1900-based income measures and refraining from blending the predictions. Instead, respondents born in the 1910–1930s cohorts are assigned 1940-based income predictions (including the 1936 adjustments); those born in the 1940s are assigned 1950-based income predictions (incorporating the 1950 Census in this instance given that there is no blending); and those born in the 1950s, 1960s, and 1970s cohorts are assigned 1960-,

[68] Similar to the baseline income predictions, we use the full-count 1940 Census to adjust 1900-based income predictions by race, Southern residence, and education. Specifically, we calculate the ratio of income within an *occupation* $\times$ *race* $\times$ *South* $\times$*education* cell relative to the income in the analogous *occupation* $\times$ *race* $\times$ *South* cell. We then average these ratios across occupations (weighting by population) and use those averages as constant factors for scaling income in an occupation for a particular education level.

[69] Given that our 1900-based data sources do not include information about mothers, survey respondents born before 1930 with working mothers are assigned 1940-based income predictions with adjustments following Collins and Wanamaker (2022).

1970- and 1980-based predicted income, respectively.

A final variation adjusts our baseline income predictions for household size in both generations. We are able to retrieve respondent household size in thirteen of our fifteen surveys. This information is provided in one of two ways: 1) a pre-constructed survey variable for household size, or 2) a variable that lists the number of adults living in the respondent's household and another variable that lists the number of children aged 0–17 living in the household. If information on household size is given in the latter manner, we combine the two variables to obtain household size, on the condition that both variables are available for the respondent. If not, household size is recorded as missing. To adjust predicted parental income for household size, we use the 1910–1990 Censuses to calculate the median family size in that *occupation* $\times$ *race* $\times$ *South* cell when the respondent is 10 years old and take weighted averages of the median sizes in the two nearest Censuses.

E.3.4 1936 Expenditure Survey

The 1935–36 Study of Consumer Purchases, or "1936 Expenditure Survey", is one survey in the Bureau of Labor Statistics' (BLS) Cost of Living series, a recurring effort to estimate the cost of living for a "typical" American family (United States Department of Labor *et al.*, 2009). The survey consists of two data sources, an urban expenditure study conducted by the BLS and a concurrent study of small cities, villages, and farms by the Bureau of Home Economics (BHS). The purpose of this novel combination of urban and rural data on income, expenditures, and demographics was to "learn how families of different incomes, occupations, and family types apportion[ed] their expenditures among specific goods and services, in different parts of the country" (United States Department of Labor and Bureau of Labor Statistics, 1941). Income-related survey questions asked respondents to list all income received by each employed member of the household, as well as income from other sources such as gifts, interests and dividends, and pensions. Expenditure-related questions covered a wide range and include, but were not limited to, utility costs, medical care, educational expenses, automobile expenses, personal care costs, clothing expenses, furnishing expenses, and quantity of food items consumed in the week prior to the interview. Standard demographic characteristics such as relationship to the household head, age, sex, and occupation were recorded.

Primary sampling units were not chosen at random; rather, respondents were selected from 257 cities, villages, and rural counties within six geographic regions. Several subsequent rounds of random sampling within these geographic areas resulted in a sample of roughly 6,000 native-born families providing information on both income and expenditures. All families resided in the U.S. at the time of the survey.

In the urban component of the survey, the sample was further limited to families in which both husband and wife were present. The sample was almost exclusively white, with data from Black native-born families obtained only in New York, Columbus, and the Southeast. Families receiving welfare at any point during the year and families with an income level below the "customary" level (United States Department of Labor and Bureau of Labor Statistics, 1941) of non-recipient families were also excluded from the urban sample. Other restrictions related to recent housing changes and long-term guests or boarders were applied.

Similar to the urban study, husband and wife, both native-born, had to be present in the household for inclusion in the rural component of the survey. White-only families were interviewed in all regions other than the Southeast, where a separate study of Black families occurred. Families could not have moved during the survey year and had to have operated their farms, whether owned or rented. An exception was made in the Southeast, where sharecropper families were included. Selected families could not have received welfare during the 12-month period of the survey. As observed in a 1941 report from the United States Department of Agriculture (USDA), these eligibility requirements resulted in the omission of mostly less-advantaged groups from the rural study (i.e., foreign-born families, non-two parent families, large families, Black families, welfare recipients, and farm laborers) (Stiebeling *et al.*, 1941).

Like all of our other data sources, occupations in the 1936 Expenditure survey are mapped to our coarsened occupations. Table E.7 compares characteristics of 1936 respondents with those of fathers in the 1930 and 1940 Census, focusing on the five occupation $\times$ race $\times$ South groups that comprise most farmer and self-employed fathers in the early cohorts of our surveys.

As is clear from the above description, the 1936 data will not be strictly representative. However, some of its biases match the target sample (fathers with children) for our prediction exercise. As is clear from Appendix Table E.7, the 1936 sample is almost all married (as is the "target sample" from the 1940 Census that we use to calculate childhood income for our earliest cohorts). In general the Census families seem to have a somewhat greater number of children living with them than do our 1936 families.

Given its exclusion of those on welfare, we might expect the 1936 data to be somewhat positively selected.[70] However, we do not see any evidence of systematic positive selection with respect to education (which is asked in the 1936 rural, but not urban, sample), the most important socio-economic status marker we can compare. For white, non-Southern farmers, 83 (76) percent of our 1936 (1940 Census) sample finished 8th grade and 15 (13) percent finished high school. The educational attainment for white Southern farmers in the 1936 Expenditure and 1940 Census are nearly identical: 43 percent of the 1936 sample finished 8th grade, compared to 42 in the Census, and in both datasets seven percent finished high school. Black Southern farmers report very low levels of education in both datasets: two (seven) percent finished eighth grade in 1936 (1940) and two (one) percent finished high school.

[70] Conversations with Bob Margo suggest that the 1936 data might be missing both the top and the bottom parts of the distribution. As we are using the 1936 data mostly to estimate cell *means*, this concern is hopefully second-order, but still worth keeping in mind.

E.3.5 Merging predicted parental income to survey respondents' parents

As previously mentioned, we harmonize fathers' occupations (and mothers' occupation whenever available) into 28 coarsened categories. To do so, we construct crosswalks between the 1950 Census occupations and our coarsened occupations, as well as analogous crosswalks for the 1960, 1970, 1980, 2000, and 2010 Census occupations. If the occupations in a survey did not match the Census list of occupations, we created survey-specific crosswalks between the available occupation codes and our coarsened occupations.

Once we finish coarsening occupations, we merge our predicted parental income measures by father occupation, race, and whether the *respondent* grew up in the South. While our surveys provide father occupation, they do not report information on his race. We thus proxy father race with respondent race. Moreover, our surveys do not report the state or region in which the respondent's father worked when the respondent was growing up. We can, however, observe the region in which the respondent was born or grew up. We therefore use respondent residence in childhood/adolescence to proxy for father residence. Whenever we have information on both birthplace and childhood region, we use the latter to proxy for father residence.

Finally, whenever a father's occupation is unavailable but the occupation of the mother is provided, we merge in the corresponding measures of predicted income for mothers, again by mother's occupation, race, and whether the respondent grew up in the South.

E.4 Educational attainment

Our constructed measures of educational attainment always reflect years of schooling *completed*. In some surveys, respondent and father education are binned (i.e., "less than grade school," "grade school," "less than high school," etc.), while in other surveys they are categorical (i.e., 0-20+ years of schooling). To harmonize across surveys, we create two education variables.

The first binned variable assigns consecutive, ascending values as follows:

(0) no education (0 years)
(1) less than grade school (1-7 years)
(2) grade school (8 years)
(3) less than high school (9-11 years)
(4) high school (12 years)
(5) some college (13-15 years)
(6) college+ (16+ years)

In contrast, the second binned variable assigns *years of schooling* in the following manner:

(0) no education
(6) less than grade school
(8) grade school

(10) less than high school
(12) high school
(14) some college
(16) college+

We create these two variables for the respondent and for the respondent's father. Whenever available, we make similar variables for the respondents' mothers. Finally, we create indicator variables denoting high school and college completion for the respondent, for the father, and for the mother if possible.

E.5 Weights

We begin by taking the provided weight in each survey and dividing it by its mean so that the weight has an average of 1 within a survey. For surveys that consist of repeated cross-sections (i.e., the ANES and GSS), we re-center the weight in each survey year. If a survey does not have a weight, we create a weight with all values set to one. We then combine these re-centered weights into one variable, and this weight acts as our "survey weight" measure.

The main weight we use in the analysis builds on this centered weight, but adjusts it further for population characteristics. In particular, because some of our surveys are not representative by race or sex, certain cohorts in the pooled dataset will not be nationally representative. We therefore adjust the centered weights so that the share of white men, white women, Black men, and Black women in each cohort (i.e., decade) coincides with the corresponding share in the Census when these respondents were roughly 40 years old (e.g., for the 1920 cohort, we use the 1960 Census to calculate these shares). Table E.6 shows the relative weight of each survey in each birth cohort using this baseline, population-adjusted weight.

Throughout the analysis, we sometimes restrict the sample to certain respondents (e.g., individuals whose fathers are not farmers, individuals with available information on father's education). For these secondary samples, we also adjust the centered weight so that the share of white men, white women, Black men, and Black women in each cohort of that sub-sample correspond to the analogous share in the Census.

The final weight that we construct goes a step further and adjusts the survey weights not only using race and sex shares, but also the share of individuals who have graduated high school in that cohort as well as the shares of the population present in five-year age bins between ages 30–50. Similar to our baseline weight, we adjust the survey weights to match the corresponding $race \times sex \times education \times age$ shares in the Census when these respondents were roughly 40 years old.

E.6 Ranking respondents and their fathers

We rank respondents and their fathers based on family income and predicted parental income, respectively. In particular, we rank respondents relative to other survey respondents born in the same birth year. Similarly, we rank fathers relative to all other fathers with children born in the same year. Notably, we rank respondents and their fathers on the condition that we have a minimum of 100 observations in a given birth year for the relevant sample. Our baseline analysis sample ends up including individuals born in every year between 1911 and 1979. In our baseline approach, we use the population-adjusted weights when creating ranks.

Whenever we consider secondary samples of individuals, we re-rank respondents (and their fathers) so that individuals are compared to the other individuals in that sub-sample. We use the population-adjusted weights that correspond to that sub-sample when ranking.

E.7 Benchmarking two-sample two-stage least squares approach to OLS using the NLS & PSID surveys

In Appendix D, we compare our estimates of mobility (using retrospective information about the parents) to estimates of mobility that use actual (self-reported) parental income. For the NLS, we follow Davis and Mazumder (2022), using their preferred sample as well as their measures of child actual income and parental actual income. Non-missing predicted parental income indicates that the respondent provided information on retrospective parental occupation, race, and childhood region. To keep the sample consistent across specifications, we restrict the sample to respondents with available child income, parental actual income, and predicted parental income. We use two samples: one that only includes respondents with information about the father's occupation, and one that incorporates respondents with non-working fathers or with working mothers.

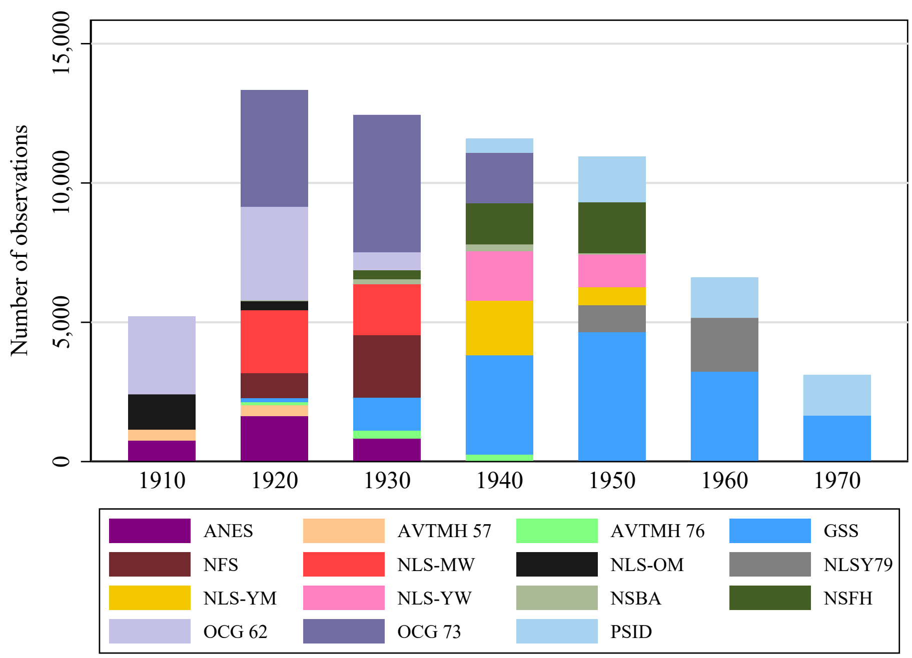
Appendix figure D.3 explores the differences in OLS estimates using actual vs. predicted income in the PSID. The usual restrictions are made (i.e., respondent is aged 30–50 and US-born). Respondents are further required to: (1) link to at least one parent present in an earlier PSID wave (using the PSID's Family Identification Mapping System), (2) to have participated in the survey at least once from 1997–2015 (when retrospective questions begin to be asked), and (3) to identify as the head/reference person or wife/spouse/cohabitating partner at least once during the same time frame (retrospective questions are only asked of these members). Finally, like with the NLS, the analysis sample consists of respondents with available child actual income and **both** kinds of parental income (i.e., actual and predicted) for their father or their mother.

E.8 Comparison to modern data

In Section 7 of the paper, we compare the mobility patterns of white men, white women, Black men, and Black women to the mobility patterns of these groups today. To make this comparison, we use publicly available data from Chetty *et al.* (2020) at `https://opportunityinsights.org/data/`. These data indicate the average household income rank of children growing up at the 25^{th} percentile of the income distribution by race and gender.

Next, we consider the within- and between-group components of the modern intergenerational mobility based on equations (4) and (5). For the IGE, we combine population shares as well as the mean and median incomes by race from Chetty *et al.* (2020). Specifically, we use Online Appendix Tables VI and IX to retrieve sample sizes and to parametrize the lognormal distributions of household income for the parents and children in the full population and for each subgroup. We do an analogous exercise for rank-rank correlations using publicly available data from Opportunity Insights on income distributions and population shares for the six racial and ethnic subgroups for which there are data available. We calculate that for the U.S. IGE and rank-rank correlation to fall to the Danish or Canadian level of 0.2 without between-group racial convergence in family income, the within-group IGE slopes (rank-rank correlations) would have to fall to around 0.04 (0.1). To simplify the calculation, we assume that all groups would have the same within-group slopes (Chetty *et al.* (2020) show that within-group rank-rank slopes are quite similar across groups, ranging from the mid-twenties to the low thirties, with the higher within-group mobility of Asian individuals being the exception).

Figure E.1: Survey data per birth cohort



Sources: This figure combines the 15 surveys, showing the number of respondents in each birth cohort in our baseline sample.

Table E.1: Survey cohorts and samples

Survey	Cohorts	Sample
American National Election Survey, 1956–1970	1910–1930	Representative
Americans View Their Mental Health, 1957	1910–1920	Representative
Americans View Their Mental Health, 1976	1920–1940	Representative
General Social Surveys, 1977–2018	1920–1970	Representative
Occupational Changes in a Generation, 1962	1910–1930	Representative & male
Occupational Changes in a Generation, 1973	1920–1940	Representative & male
National Fertility Survey, 1970	1920–1930	Ever-married women ages 30–44
National Longitudinal Survey of Mature Women, 1967	1920–1930	Representative & female, ages 30–44
National Longitudinal Survey of Older Men, 1966	1910–1920	Representative & male, ages 45–50
National Longitudinal Survey of Young Women, 1988	1940–1950	Representative & female, ages 34–46
National Longitudinal Survey of Young Men, 1981	1940–1950	Representative & male, ages 30–40
National Longitudinal Survey of Youth, 2002	1950–1960	Representative, ages 37–45
National Survey of Black Americans 1979–1980	1920–1950	Representative & Black Americans
National Survey of Families and Households 1987–1988	1930–1950	Representative
Panel Study of Income Dynamics, 1997 & 2017	1940–1970	Representative

Notes: This table reports the cohorts and sample for each of the 15 surveys in our baseline sample. "Representative & male" and "Representative & female" refers to having representative samples by race within an all-male or all-female survey, respectively. "Representative & Black Americans" refers to representative samples (e.g., in terms of age groups) within the Black-American population.

Table E.2: Sample size across successive restrictions, by survey

Surveys	(1) Ages 30–50 & U.S.-born	(2) +Available weight & relevant cohort	(3) +Non-missing income & father occupation
American National Election Survey, 1956–1970	3,781	3,625	3,218
Americans View Their Mental Health, 1957	1,049	930	782
Americans View Their Mental Health, 1976	804	799	665
General Social Surveys, 1977–2018	20,909	19,338	14,432
National Fertility Study, 1970	3,502	3,449	3,137
National Longitudinal Survey of Mature Women, 1967	4,846	4,782	4,090
National Longitudinal Survey of Older Men, 1966	2,115	2,088	1,630
National Longitudinal Survey of Youth, 2002	4,604	4,298	2,880
National Longitudinal Survey of Young Men, 1981	3,162	3,144	2,622
National Longitudinal Survey of Young Women, 1988	3,451	3,421	2,964
National Survey of Black Americans, 1979–1980	732	732	439
National Survey of Families & Households, 1987–1988	5,103	5,062	3,639
Occupational Changes in a Generation, 1962	10,341	10,260	6,780
Occupational Changes in a Generation, 1973	14,975	14,858	10,913
Panel Study of Income Dynamics, 1997 & 2017	7,858	5,733	5,060

Notes: The first column lists the original number of respondents ages 30–50 and U.S.-born in each survey. For the PSID, this column also imposes the restriction of keeping one member per household. Columns 2–3 show the change in sample size as we implement several restrictions. Column 2 restricts the sample to those born in the 1910–1979 birth cohorts as well as those who had a non-missing survey weight. Column 3 shows the number of respondents that meet the restriction of being in the baseline sample (i.e., having non-missing family income, race, region, and father occupation).

Table E.3: Number of observations, by cohort and survey

Surveys	1910	1920	1930	1940	1950	1960	1970
American National Election Survey, 1956–1970	754	1,632	832	—	—	—	—
Americans View Their Mental Health, 1957	392	390	—	—	—	—	—
Americans View Their Mental Health, 1976	—	117	288	260	—	—	—
General Social Surveys, 1977–2018	—	140	1,184	3,554	4,659	3,231	1,664
National Fertility Study, 1970	—	904	2,233	—	—	—	—
National Longitudinal Survey of Mature Women, 1967	—	2,249	1,841	—	—	—	—
National Longitudinal Survey of Older Men, 1966	1,267	363	—	—	—	—	—
National Longitudinal Survey of Youth, 2002	—	—	—	—	952	1,928	—
National Longitudinal Survey of Young Men, 1981	—	—	—	1,967	655	—	—
National Longitudinal Survey of Young Women, 1988	—	—	—	1,773	1,191	—	—
National Survey of Black Americans, 1979–1980	—	10	166	241	22	—	—
National Survey of Families & Households, 1987–1988	—	—	326	1,485	1,828	—	—
Occupational Changes in a Generation, 1962	2,794	3,338	648	—	—	—	—
Occupational Changes in a Generation, 1973	—	4,185	4,928	1,800	—	—	—
Panel Study of Income Dynamics, 1997 & 2017	—	—	—	509	1,644	1,452	1,455

Notes: This table lists the number of respondents in each survey and birth cohort in our baseline sample of respondents (analogous to Appendix Figure E.1). A dash indicates that zero survey respondents were born in that decade.

Table E.4: Additional details about sampling, weights, & available variables

Surveys	Sampling level	Racial oversampling	Weights available	Father edu. available	Childhood region available	Household size available
American National Election Survey, 1956–1970	HH	✓	✓		✓	✓
Americans View Their Mental Health, 1957	HH				✓	✓
Americans View Their Mental Health, 1976	HH			✓	✓	✓
General Social Surveys, 1977–2018	HH	✓	✓	✓	✓	✓
National Fertility Study, 1970	HH	✓	✓			
National Longitudinal Survey of Mature Women, 1967	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Older Men, 1966	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Youth, 2002	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Young Men, 1981	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Young Women, 1988	Ind.	✓	✓	✓		✓
National Survey of Black Americans, 1979–1980	HH	✓		✓	✓	✓
National Survey of Families & Households, 1987–1988	Ind.	✓	✓	✓	✓	✓
Occupational Changes in a Generation, 1962	HH		✓	✓	✓	
Occupational Changes in a Generation, 1973	HH	✓	✓	✓	✓	✓
Panel Study of Income Dynamics, 1997 & 2017	Ind.	✓	✓	✓	✓	✓

Notes: In the column denoting sampling level,"HH" signifies the selection of one person per household and "Ind." indicates that multiple household members were eligible for selection into the survey. In the case of the NLS surveys, it is also possible that several household members were interviewed *across* surveys (e.g., a mother and daughter were interviewed for the NLSMW and NLSYW surveys, respectively). In the third column, "racial sampling" refers to the oversampling of Black respondents. Oversampling occurs in 3 ANES cross sections and in 2 GSS cross sections. Weights are only available in five out of seven ANES cross sections. A survey receives a checkmark in the "childhood region available" column if it asks respondents where they were born and/or where they grew up with sufficient detail to identify the four Census regions. A survey receives a checkmark in the "household size available" column if it includes sufficient information to calculate the number of individuals living in the respondent's household.

Table E.5: Summary statistics of respondent family income, by survey

Surveys	(1) Share bottom coded	(2) Share top coded	(3) Number of bins, family income
American National Election Survey, 1956–1970	0.02	0.06	10
Americans View Their Mental Health, 1957	0.04	0.03	11
Americans View Their Mental Health, 1976	0.04	0.09	11
General Social Surveys, 1977–2018	0.03	0.11	10
National Fertility Study, 1970	0.02	0.23	12
National Longitudinal Survey of Mature Women, 1967	0.09	0.02	11
National Longitudinal Survey of Older Men, 1966	0.04	0.12	11
National Longitudinal Survey of Youth, 2002	0.04	0.09	12
National Longitudinal Survey of Young Men, 1981	0.00	0.00	11
National Longitudinal Survey of Young Women, 1988	0.03	0.27	11
National Survey of Black Americans, 1979–1980	0.06	0.10	10
National Survey of Families & Households, 1987–1988	0.05	0.16	10
Occupational Changes in a Generation, 1962	0.10	0.04	10
Occupational Changes in a Generation, 1973	0.01	0.06	10
Panel Study of Income Dynamics, 1997	0.07	0.09	11
Panel Study of Income Dynamics, 2017	0.07	0.09	12

Notes: Columns 1 and 2 list the share of respondents whose total family income is in the bottom and top bin, respectively, in that survey. All shares are based on the baseline sample and are unweighted. Column 3 lists the number of bins for respondent family income in each survey. For the ANES and GSS, we report the median number of bins across survey years.

Table E.6: Relative weight of each survey, by birth cohort

Surveys	1910	1920	1930	1940	1950	1960	1970
American National Election Survey, 1956–1970	0.38	0.14	0.07	—	—	—	—
Americans View Their Mental Health, 1957	0.18	0.04	—	—	—	—	—
Americans View Their Mental Health, 1976	—	0.01	0.02	0.02	—	—	—
General Social Surveys, 1977–2018	—	0.01	0.10	0.31	0.43	0.49	0.53
National Fertility Study, 1970	—	0.11	0.20	—	—	—	—
National Longitudinal Survey of Mature Women, 1967	—	0.26	0.17	—	—	—	—
National Longitudinal Survey of Older Men, 1966	0.14	0.02	—	—	—	—	—
National Longitudinal Survey of Youth, 2002	—	—	—	—	0.10	0.27	—
National Longitudinal Survey of Young Men, 1981	—	—	—	0.15	0.06	—	—
National Longitudinal Survey of Young Women, 1988	—	—	—	0.18	0.10	—	—
National Survey of Black Americans, 1979–1980	—	0.00	0.01	0.02	0.00	—	—
National Survey of Families & Households, 1987–1988	—	—	0.03	0.13	0.16	—	—
Occupational Changes in a Generation, 1962	0.31	0.19	0.05	—	—	—	—
Occupational Changes in a Generation, 1973	—	0.22	0.34	0.13	—	—	—
Panel Study of Income Dynamics, 1997 & 2017	—	—	—	0.06	0.15	0.24	0.47

Notes: This table shows the relative weight of each survey in each birth cohort using the baseline sample of respondents and population-adjusted weights. Each cell of the table divides total weight for a *survey* in a given birth decade by total weight for the entire birth decade. A dash indicates that 0 survey respondents were born in that decade. "0.00" corresponds to observations that are given extremely low weights in that cohort (i.e., due to small numbers of respondents born in that decade).

Table E.7: Summary statistics, comparing 1936 survey fathers to Census fathers

	1930 Census	1936 Survey	1940 Census
Self-employed × white × non-South			
Age	40.31	40.95	40.68
Lives in Northeast	0.46	0.13	0.44
Lives in Midwest	0.39	0.57	0.41
Lives in West	0.14	0.31	0.16
Married	0.99	1.00	0.99
# of kids <18 in household	2.29	2.04	1.94
Owns his home	0.59	0.48	0.52
Family income, 1936$	—	2,369	—
Share in Census	0.05	—	0.04
Observations	5,161	166	4,439
Self-employed × white × South			
Age	40.19	42.14	40.06
Married	0.98	1.00	0.99
# of kids <18 in household	2.44	1.66	2.03
Owns his home	0.64	0.66	0.65
Family income, 1936$	—	2,298	—
Share in Census	0.01	—	0.01
Observations	1,419	35	1,539
Farmer × white × non-South			
Age	40.31	41.44	40.59
Completed 8th grade	—	0.83	0.76
Completed HS	—	0.15	0.13
Lives in Northeast	0.13	0.26	0.11
Lives in Midwest	0.72	0.54	0.75
Lives in West	0.15	0.20	0.14
Married	0.98	1.00	0.98
# of kids <18 in household	2.99	2.22	2.70
Owns his home	0.57	0.65	0.52
Family income, 1936$	—	1,365	—
Share in Census	0.10	—	0.07
Observations	10,186	316	7,791
Farmer × white × South			
Age	40.03	40.46	40.02
Completed 8th grade	—	0.43	0.42
Completed HS	—	0.07	0.07
Married	0.98	1.00	0.99
# of kids <18 in household	3.54	2.80	3.03
Owns his home	0.46	0.53	0.47
Family income, 1936$	—	1,118	—
Share in Census	0.08	—	0.06
Observations	8,266	205	6,904
Farmer × Black × South			
Age	40.52	41.44	39.75
Completed 8th grade	—	0.02	0.07
Completed HS	—	0.02	0.01
Married	0.96	1.00	0.98
# of kids <18 in household	3.89	3.02	3.85
Owns his home	0.19	0.06	0.18
Family income, 1936$	—	474	—
Share in Census	0.03	—	0.02
Observations	2,626	57	2,187

Notes: The sample in the table consists of white and Black men ages 30–50 with at least one child living in the household. All estimates are weighted. Census fathers come from 1% Census samples from Ruggles *et al.* (2021). In the 1936 survey, "Owns his home" refers to owning his own dwelling for self-employed fathers and owning any amount of acreage for farmer fathers. For all fathers in the Census, "owns his home" refers to owning his own dwelling. "Share in Census" refers to the share of fathers ages 30–50 with that occupation, race, and region.