Online Appendix for “The her in inheritance”

Appendix 1: Signatures and literacy data

Bride and groom raw signature variables, marriages 1800–1969

	Bride								
Groom	NA	Signed	Did not sign	Unclear	Not observed	Undeclared	X, etc.	Writer signed	Unspecified
NA	12,534	0	0	0	0	0	0	0	0
Signed	3	1,507,295	58,421	274	1,125	68	4,145	76	2,230
Did not sign	0	103,296	308,250	91	186	116	27	8	3,805
Unclear	0	795	106	94	6	1	2	0	20
Not observed	0	2,047	167	4	358	13	2	0	8
Undeclared	0	138	80	0	8	127	0	0	1
X, etc.	0	2490	40	2	4	2	2276	9	37
Writer signed	0	117	7	1	2	0	6	550	2
Unspecified	0	4,191	3,135	13	13	4	40	7	103,591

Sources: Project Balsac (2020).
Note: Here, I cross-tabulate the raw signature variables for each marriage. Signatures were often both declared by the record writer (typically, the priest) and observed by Project BALSAC. If at least one of the two was the case, I code the individual as signing the record. If they use X or another symbol or the record writer signed on their behalf, I coded them as not signing the record. Otherwise, I coded the variable as missing. Note that in the main paper, I assign each individual a variable based on if they signed their first marriage.

Table A2: Twenty most common nineteenth century occupations ranked by literacy

HISCO	Occupation	Translation	Mean signed	Rank	Percent	Mean year
06105	Medecin	Doctor	0.99	18	0.01	1875
45125	Commis marchand	Merchant clerk	0.98	17	0.01	1882
41025	Marchand	Merchant	0.94	5	0.03	1867
79100	Tailleur	Tailor	0.87	13	0.01	1869
93120	Peintre	Painter	0.75	16	0.01	1876
77310	Boucher	Butcher	0.71	15	0.01	1873
77620	Boulanger	Baker	0.62	12	0.01	1868
95410	Menuisier	Carpenter	0.55	3	0.05	1861
83110	Forgeron	Blacksmith	0.52	6	0.02	1863
77120	Meunier	Miller	0.50	20	0.00	1862
80110	Cordonnier	Shoemaker	0.50	4	0.03	1865
76145	Tanneur	Tanner	0.44	19	0.00	1856
98135	Navigateur	Navigator / sailor	0.44	7	0.02	1864
95135	Maçon	Mason	0.34	14	0.01	1852
98620	Charretier	Carter	0.33	8	0.02	1870
61110	Cultivateur	Farmer	0.32	1	0.48	1863
43220	Voyageur	Fur trader	0.20	10	0.01	1866
99910	Journalier	Day laborer	0.19	2	0.15	1863
64100	Pecheur	Fisherman	0.14	11	0.01	1874
62105	Laboureur	Laborer	0.05	9	0.01	1819

Sources: Project Balsac (2020).
Note: Observations are grooms at time of their marriage. Occupation titles are taken from the most common within a HISCO code (Van Leeuwen et al., 2004). Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Percent is the percentage of all non-indeterminate occupations with that HISCO code. The average year is the average year of marriage. In Quebec, *journalier* refers to workers paid by the day regardless of if they work in agriculture.

Appendix 2 Alternative measures of status

Figure A1 estimates the ratio measure estimate of the degree of assortment using several different occupational status scores. All the occupational scores give roughly the same picture of the overall level and trend of assortment. Moreover, the occupational status score used in the main results based on imputed 1901 earnings is about in the middle of the distribution of estimates.

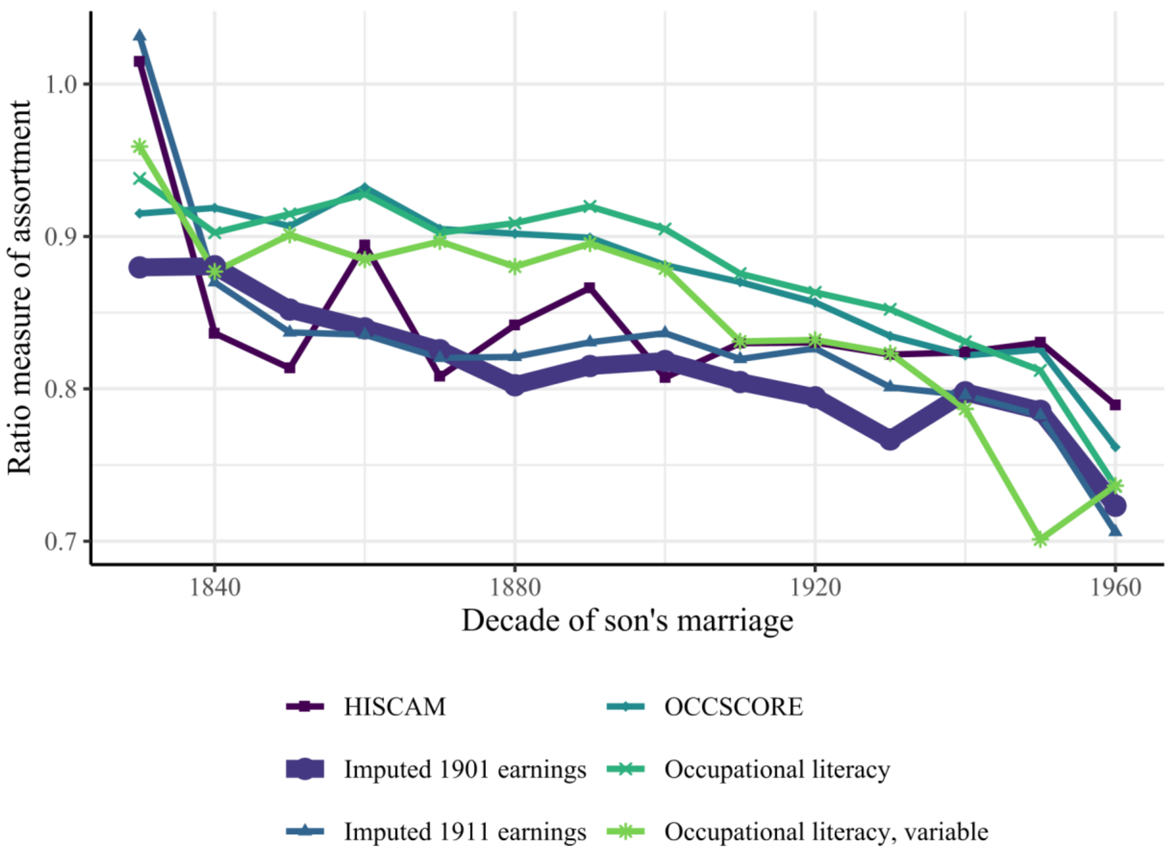

Figure A1: Alternative occupational status scores

Note: 1901 imputed earnings are imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). 1901 imputed earnings with signature also uses literacy (proxied by signatures) to impute the earnings. OCCSCORE is the IPUMS imputed earnings score, which is based on 1950 US Census earnings (Minnesota Population Center, 2019). HISCAM is the universal HISCAM score, a social distance-based ranking of nineteenth century occupations (Lambert et al., 2013). Occupational literacy scores are the share of men with that occupation in the 1890's in the vital records who could sign their name. Variable occupational literacy scores are computed for each decade using the method in Song et al. (2020): for each occupational category and decade, the score is the sum of the percentile rank of each educational group (signed and not signed) weighted by the share of the occupation in that category. This is essentially a reweighted average signature rate by occupational category that accounts for the varying rate of signatures over time. All the measures roughly agree.

Appendix 3 Occupations in the vital records

Figure 1 shows that the vital records accurately measured female literacy. But what about occupational status? Four extracts of Canadian censuses from 1881 to 1911 and data compiled by Long (1958) for 1920–1960 provide external points of comparison (Canadian Families Project, 2002; Dillon et al., 2008; Gaffield et al., 2009; Inwood and Jack, 2011; Minnesota Population Center, 2019).

Figure A2 shows the employment rate of women by marital status in the vital records compared to the censuses and the data compiled by Long (1958). Here, I reweight the BALSAC vital records data to match the age distribution in the other sources. Compared to the other sources, the vital records underestimate the formal employment rate of married women and almost entirely omit unmarried women with occupations. This is especially damning as the other sources very likely underreport female employment as well. One pattern, however, is clear. While unmarried women often worked outside the home, married women did not begin to report formal employment in substantial numbers until the second half of the twentieth century.

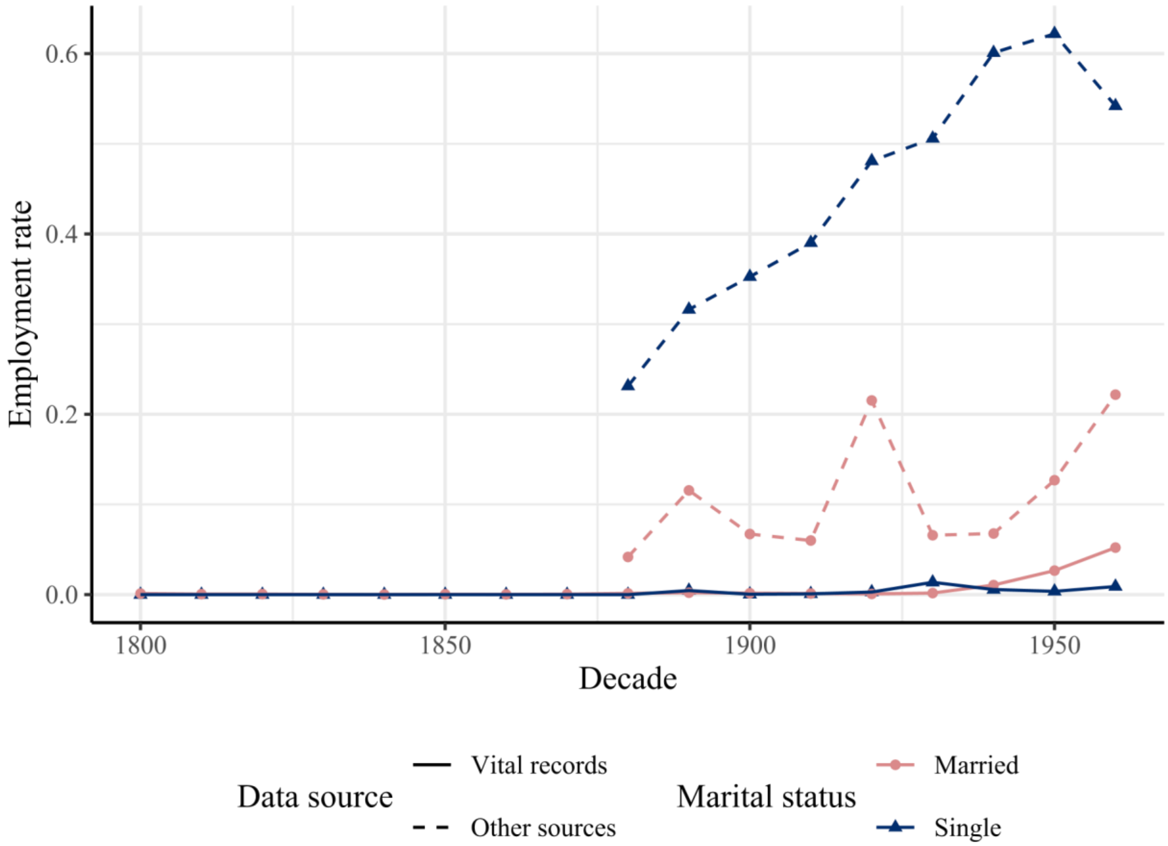

Figure A2: The vital records do not record formal female employment
Sources: Canadian Families Project (2002), Dillon, et al. (2008), Gaffield et al. (2009), Inwood and Jack (2011), Killingsworth and Heckman (1986), C. D. Long (1958), Minnesota Population Center (2019), and Project Balsac (2020). *Note:* The vital records are from the BALSAC database. The other sources are varied. For the vital records, a woman is counted as formally employed if she had an occupation listed in any record. She is assigned a year equal to the median of all the years in which she is observed. Then, I compute the average employment rate for each decade, reweighted by age to match the age distribution in the census data. Before 1920, the other sources are census extracts, and the employment rate is the fraction of women aged at least 16 with an occupation. After 1920, the other sources are data compiled by C. D. Long (1958), with the employment rate for married women calculated as an average of the rate for currently married women and the rate for widowed or divorced women weighted by the relative frequencies of the two categories in the censuses.

Appendix 4 Alternative ratio methods

In this appendix, I consider a popular correlation in the literature: that between the father of the groom and the father of the bride.

Using the symmetry of correlation, we can rewrite equation 5 as θ_2:

$$FG_i = \beta G_i + \mu_{F_i} \tag{1}$$

then substitute in equation 6:

$$FG_i = \gamma\beta^2 FB_i + \beta\gamma\nu_i + \beta\epsilon_i + \mu_{F_i} \tag{2}$$

This suggests two additional ratio methods. The first is the ratio of the correlation between fathers and fathers-in-law divided by the correlation of grooms and fathers-in-law. Under analogous assumptions to the previous section, this will be an estimate of β that is robust to classical measurement error. This approach is developed further in Clark, Cummins, and Curtis (2022).

A second ratio method would be to take the ratio of the correlation between fathers and fathers-in-law divided by the correlation of grooms and father squared. If there is no measurement error, this will be an estimate of γ. However, as the denominator is squared, there is no set of reasonable assumptions where the attenuation bias cancels. Under analogous assumptions to the previous section, the ratio will be biased downwards.

Appendix 5: Model of marriage intergenerational mobility

Espín-Sánchez, Gil-Guirado, and Vickers (2022) outline a simple model of marriage and intergenerational mobility. Here, I adapt their framework to illustrate the conditions under which assortment decreases intergenerational mobility. The key conditions are that parents assort on their individual abilities and these abilities matter independently for child outcomes. Later, I empirically test if these conditions hold.

For each child (of any gender) i, let C_i be their status. Let MC_i and be the status of their mother and FC_i be the status of their father. Assume C_i is related to MC_i and FC_i by a specific functional form:

Assumption 6 *Functional form for inheritance:* $C_i = \beta_M MC_i + \beta_F FC_i + e_i$

Then, write the assortment of the parents of child *i*:

$$Corr(FC_i, MC_i) = \lambda \tag{3}$$

Assumption 7 *Equal variance 3*: $Var(C_i) = Var(MC_i) = Var(MF_i)$

Using assumption 7 and the definition of Pearson's correlation coefficient, as before, Equation 13 can be rewritten as a linear association:

$$FC_i = \lambda MC_i + u_i \tag{4}$$

By the symmetry of correlation:[1]

$$MC_i = \lambda FC_i + n_i \tag{5}$$

By substitution,

$$C_i = (\beta_M + \lambda\beta_F)MC_i + \beta_F u_i + e_i \tag{6}$$

[1] Note that $u_i \neq n_i$ by construction. While we can invert Equation 14 to get $MC_i = 1/\lambda\,(FC_i - u_i)$, estimating this equation by OLS will result in coefficient of approximately λ. Intuitively, this must be true as correlation is symmetric. In terms of the mechanics of linear regression, it is true because FC_i will be correlated with u_i.

and

$$C_i = (\beta_F + \lambda\beta_M)FC_i + \beta_M n_i + e_i \quad (7)$$

The model is illustrated below in Figure A3. The solid nodes represent individuals whose status we observe. The dashed nodes represent the individuals whose status we do not observe. The diagram shows how these dashed nodes still have direct causal links to other nodes.

Note that if both $\beta_f > 0$ and $\gamma > 0$, then mothers contribute to the observed correlation between FC_i and C_i. If the parameter of interest is β_f, the direct influence of the father, then the observed correlation is a biased estimate. It is biased upwards due to the omitted variable of the mother's status. (If either $\beta_f = 0$ or $\beta_f = 0$, then there is no bias). Even if mothers are not observed, they thus could be driving results in the typical intergenerational mobility regression used in the literature.

To summarize, γ, the degree of assortment, is part of the equation determining the intergenerational correlation between fathers and sons. As long as $\beta_m > 0$— that is, if the status of mothers has a direct effect on that of sons — assortment will slow social mobility, increasing overall inequality. Together, all three main empirical findings show that assortment had long mattered for intergenerational mobility.

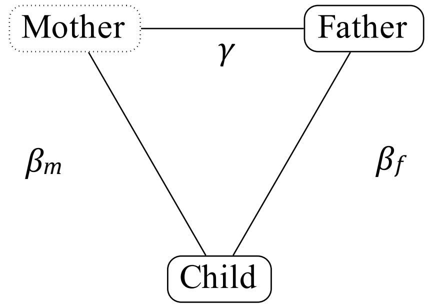

Figure A3: Simple model of assortment and mobility

Note: Solid lines represent a direct causal relationship. Solid nodes represent an individual with an imperfect (yet observed) measure of status. Dashed nodes represent an individual with unobserved status. The variables represent the true correlation over the relationship, but the observed correlation will differ due to omitted variable bias.

Appendix 6: Evidence that grooms did not match with fathers-in-law

I can directly test if the matching is between husbands and fathers-and-laws. For fathers-in-law who die before 1849, are their sons-in-law who married before their death have different human capital than those married after? As shown in Table A3, there appears to be no difference. In other words, if husbands are matching with their fathers-in-law, they don't seem to mind if their father-in-law is deceased before their marriage.

Table A3: Marriage matching and father-in-law's death

	Dependent variable: Groom's signature	
	OLS	2sDiD
	(1)	(2)
Father of bride dead	0.00	0.00
	(0.00)	(0.00)
N	73,218	53,455
Bride family FE	X	X
Bride sib order FE	X	X

Note: *p<0.10; **p<0.05; ***p<0.01. Standard errors in parentheses. Column 1 has family-clustered standard errors. Column 2 is estimated using two-stage difference-indifferences and has bootstrapped standard errors with 50,000 replications (Butts and Gardner, 2022; Gardner et al, 2024). Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. As deaths are only observed before 1849, only the sons-in-law of men who died before 1849 are included.

Appendix 7 Correlations underlying the ratio method

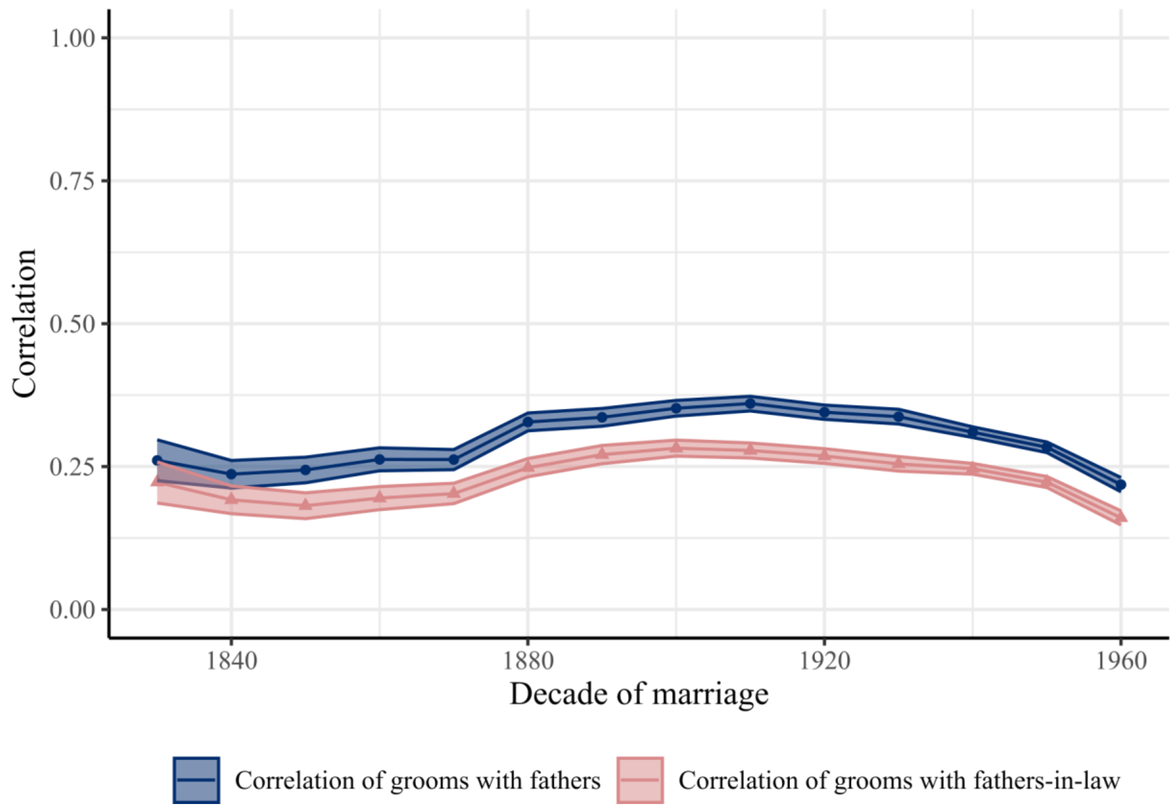

Figure A4: Correlations used to compute ratio
Sources: Project Balsac (2020).
Note: 95% bootstrapped confidence intervals shaded (10,000 replications). Imputed earnings are the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Spearman's rank correlations are used (which is equivalent to the correlation of the ranks).

Appendix 8 Robustness of estimates of sorting

Nybom and Stuhler (2017) method

Using occupational status scores introduces several potential sources of bias. The first is classical measurement error, as the true socioeconomic status of the individuals is necessarily measured with error when using occupation as a proxy.

One more standard method to account for measurement error is IV regression. IV can be used in the case of classical measurement error if a second measure of the independent variable is available (Solon, 1992; Ward, 2023). This addresses measurement error on the right-hand side which, for simple linear regression models, is sufficient. However, when estimating correlations, both variables are normalized according to the observed distributions. This introduces measurement error on both the right- and left-hand side. As the ratio method uses correlations, a different error correction procedure is necessary. Nybom and Stuhler (2017) proposes one such method.

The procedure computes the correlation of $\tilde{x}^*$ and $\tilde{y}^*$, the rank of the true variables, when only $\tilde{x} = \tilde{x}^* + \tilde{u}$ and $\tilde{y} = \tilde{y}^* + \tilde{v}$ are observed, where $\tilde{u}$ and $\tilde{v}$ are the errors in rank. Assume the true variables x^* and y^* are measured with classical measurement error. The resulting errors in rank will be non-classical but can still be addressed using instruments.

First, define λ_y and λ_x as the linear projection of the observed ranks on the true ranks. Then:

$$\tilde{y} = \alpha_y + \lambda_y \tilde{y}^* + \widetilde{w}_y \tag{8}$$

and

$$\tilde{x} = \alpha_x + \lambda_x \tilde{x}^* + \widetilde{w}_x \tag{9}$$

where $\widetilde{w}_y$ and $\widetilde{w}_x$ are now uncorrelated error terms.

If $\rho(x^*,y^*)$is the true Spearman's correlation coefficient, then (after noting that all the ranked variables have the same variances) the observed Spearman's correlation is:

$$\rho(x,y) = \frac{Cov(\tilde{x},\tilde{y})}{\sqrt{Var(\tilde{x})Var(\tilde{y})}} = \frac{\lambda_x\lambda_y Cov(\tilde{x}^*,\tilde{y}^*) + \lambda_x Cov(\tilde{x}^*,\tilde{w}_y) + \lambda_y Cov(\tilde{y}^*,\tilde{w}_x)}{\sqrt{Var(\tilde{x})Var(\tilde{y})}} =$$

$$\lambda_x\lambda_y\,\rho(x^*,y^*) \tag{10}$$

Then, assume we observe additional measures of status $\tilde{x}_2 = \tilde{x} + \tilde{u}_2$ and $\tilde{y}_2 = \tilde{y} + \tilde{v}_2$. Assuming $Cor(\tilde{u},\tilde{u}_2)$ and $Cor(\tilde{v},\tilde{v}_2) = 0$, that is the rank error is uncorrelated across multiple observations of the same individual, then:

$$Cor(\tilde{x},\tilde{x}_2) = \lambda_x^2 \tag{11}$$

and:

$$Cor(\tilde{y},\tilde{y}_2) = \lambda_y^2 \tag{12}$$

Thus, with the three sets of correlations above, $\rho(x^*,y^*)$ can be calculated:

$$\rho(x^*,y^*) = \frac{\rho(x,y)}{\sqrt{Cor(\tilde{x},\tilde{x}_2)Cor(\tilde{y},\tilde{y}_2)}} \tag{13}$$

and standard errors can be computed through bootstrapping.

Figure A5 below computes the ratio measure used in Figure 6 but first estimates both correlations using the error correction procedure described above. The instruments used are the occupational status for the occupation reported second closest chronologically to the first marriage.[2] As one would expect if attenuation bias were a concern, the two correlations are larger (Figure A6). However, they are both larger by a similar degree. Thus, the resulting ratio is very similar to that in Figure 6.

[2] Recall, the dataset often contains individuals who are not the primary subject of the vital event. For example, a man might have an occupation reported at his child's wedding.

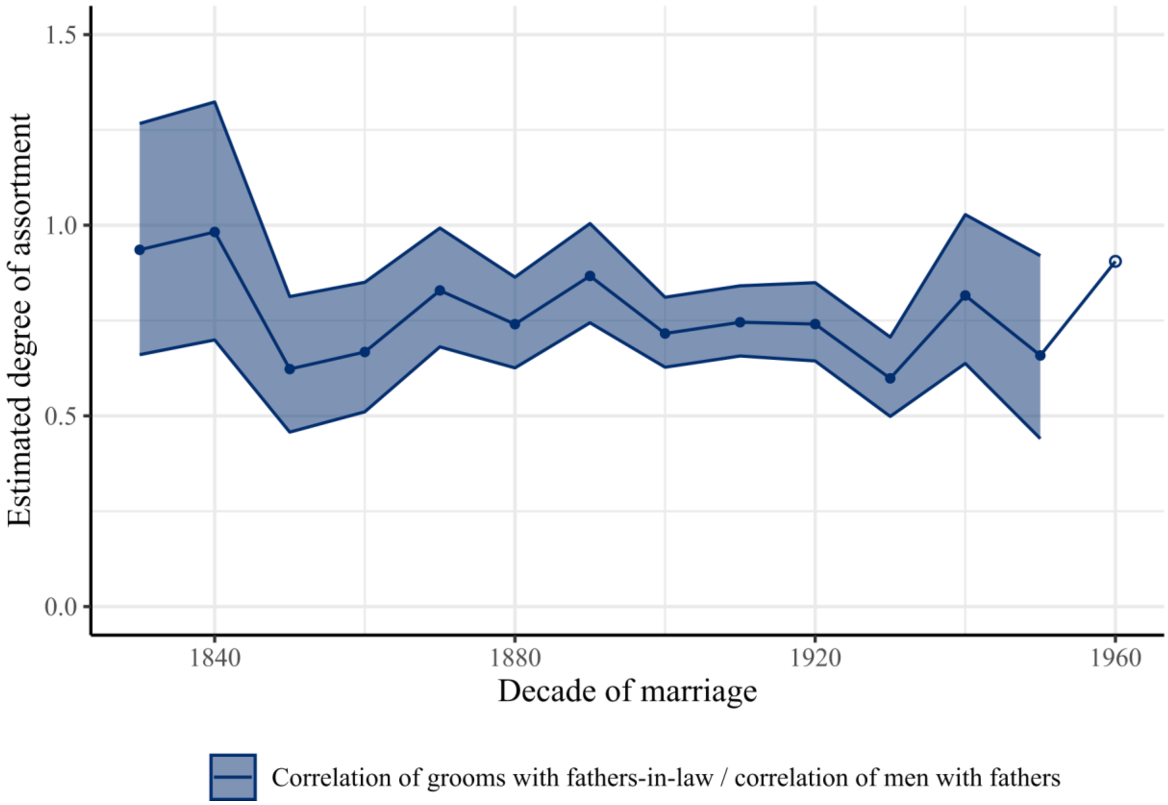

Figure A5: Estimated degree of marital assortment, error corrected
Sources: Project Balsac (2020).
Note: 95% bootstrapped confidence intervals shaded (10,000 replications). Earnings scores are the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Spearman's rank correlations are used (which is equivalent to the correlation of the ranks). To reduce attenuation bias, the correlations are adjusted by procedure proposed by Nybom and Stuhler (2017). This method, similar to instrumental variable regression, employs an additional measure of imputed earnings (using the second-closest occupation to the individual's first marriage) for each individual.

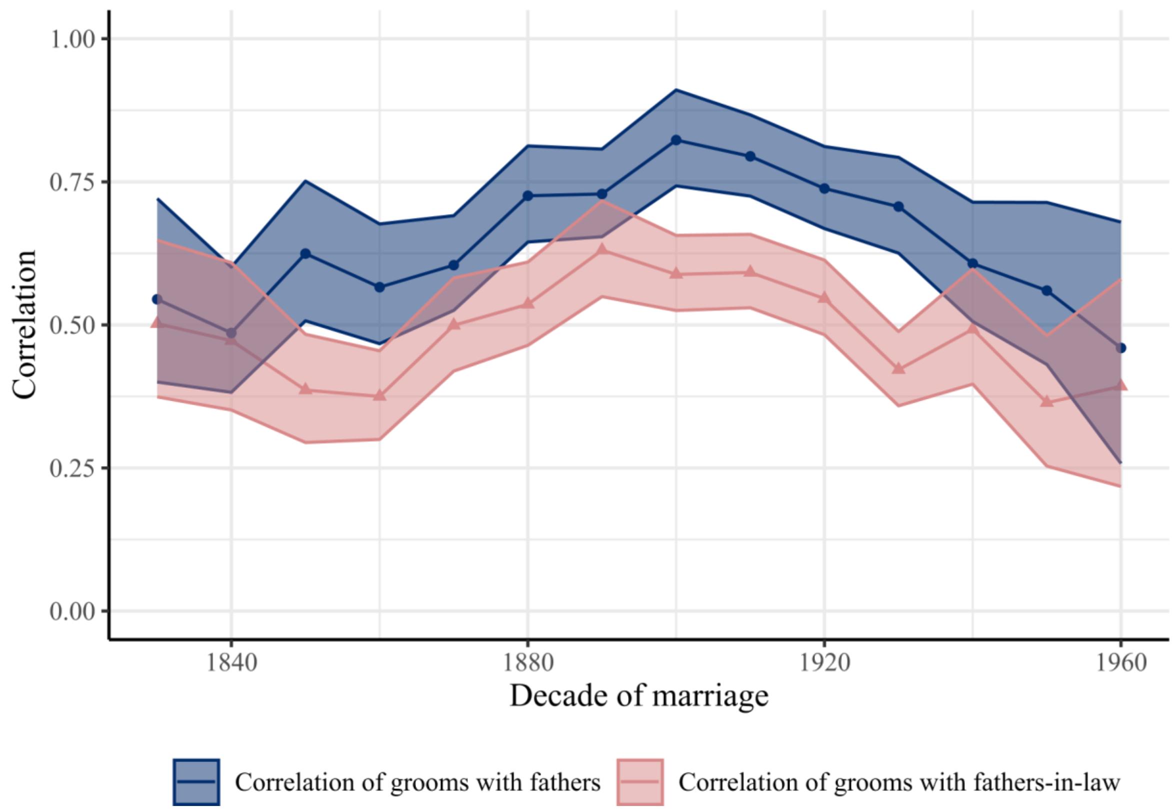

Figure A6: Correlations used to compute ratio, error corrected
Sources: Project Balsac (2020). Note:95% bootstrapped confidence intervals shaded (10,000 replications). Earnings scores are the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). To reduce attenuation bias, the correlations are adjusted by procedure proposed by Nybom and Stuhler (2017). This method, similar to instrumental variables regression, employs an additional measure of imputed earnings (using the second closest occupation to the individual's first marriage) for each individual.

Simulating within-occupation measurement errors

A second form of measurement error arises when comparing two individuals with the same occupation. In this case, the measurement error is because both are assigned the same status scores (Espín-Sánchez et al., 2019).

One way to estimate a lower bound robust to the second source of measurement is to simulate the underlying distribution of within-occupation status. For example, for the correlation between fathers' and sons' occupational earning scores, each individual with a given occupation can be assigned a draw from a log-normal distribution fit to the earnings data. This is a lower bound as fathers and sons likely have correlated earnings even after controlling for occupation. Figure A7 shows the ratio measure of occupational status using this randomization method (and Figure A8 shows the underlying correlations).

A third form of measurement error is sample selection bias, which arises if the underlying status is correlated with the probability of reporting an occupation that has a score (de la Croix and Goñi, 2021). I do not address that here.

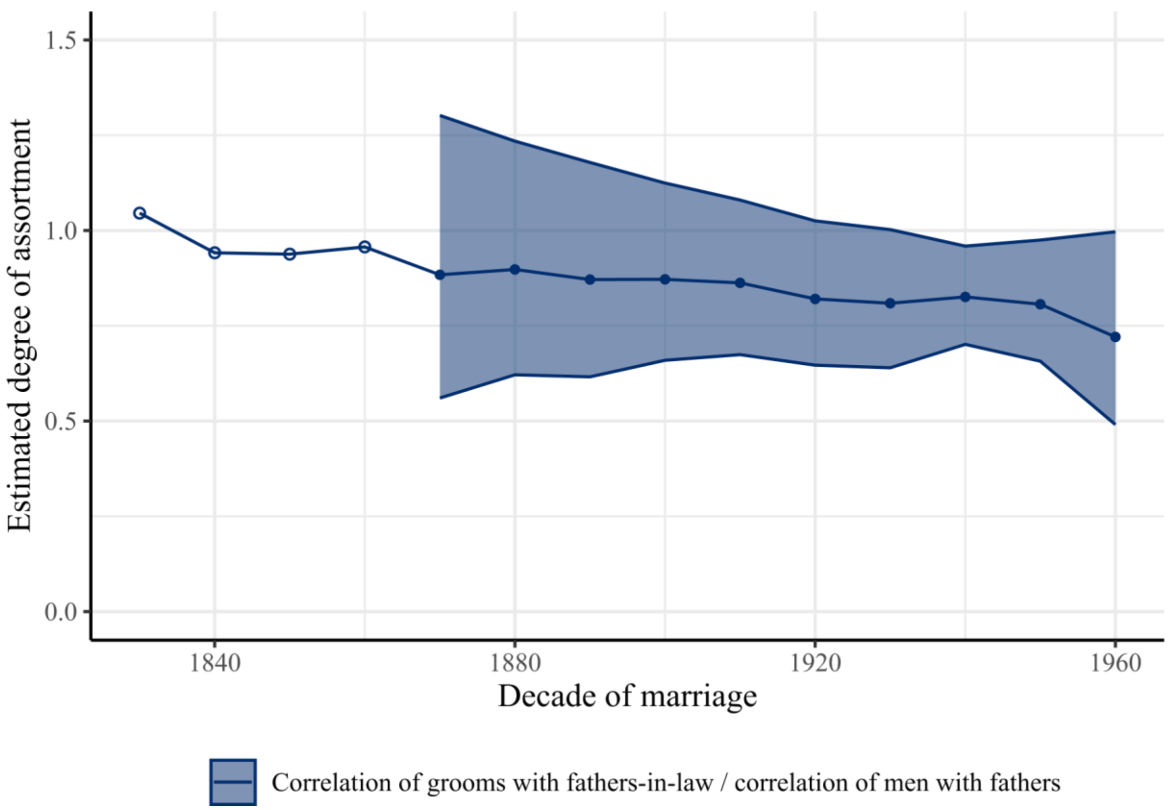

Figure A7: Estimated degree of marital assortment, resampled earnings
Note: 95% bootstrapped confidence intervals shaded (10,000 replications). Confidence intervals exceeding 1.5 are not displayed to aid comparisons with Figure 7. Resampled earnings are draws from a log normal distribution fit on the earnings scores (Espín-Sánchez et al., 2019). The earnings scores are imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Spearman's rank correlations are used (which is equivalent to the correlation of the ranks). The overall magnitude and trend remain very similar.

Figure A8: Estimated degree of marital assortment, randomized occupational earnings
Note: 95% bootstrapped confidence intervals shaded (10,000 replications). Resampled earnings are draws from a log normal distribution fit on the earnings scores (Espín-Sánchez et al., 2019). The earnings scores are imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Spearman's rank correlations are used (which is equivalent to the correlation of the ranks). The correlations are much smaller, which is expected as the resampling method is a lower bound.

Directly comparing fathers and fathers-in-law

Figure A9 estimates just the correlation between fathers and fathers-in-law using the same error correction model mentioned as before. This measure is more typically used in the literature (Craig et al., 2023). However, it is not a direct measure of the correlation between spouses. If the matching is at least partially on the characteristics of the groom and bride, the true correlation between spouses will likely be higher than this correlation. Regardless, the trend follows a very similar pattern over time to my preferred ratio method.

Figure A9: Father-father-in-law correlations

Note: 95% bootstrapped confidence intervals shaded (10,000 replications). The earnings scores are the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Resampled earnings are draws from a log normal distribution fit on the earnings scores (Espín-Sánchez et al., 2019). To reduce attenuation bias, the correlations are adjusted by procedure proposed by Nybom and Stuhler (2017). This method, similar to instrumental variable regression, employs an additional measure of imputed earnings (using the second-closest occupation to the individual's first marriage) for each individual. Spearman's rank correlations are used (which is equivalent to the correlation of the ranks).

Appendix 9 Robustness of sorting on individual human capital

Occupational status

Tables A4 and A5 replicate the analysis in Table 2 using earnings scores instead of signatures.

Selection into identification

The estimates in Table 3 are identified using family fixed effects. This means that families where one child signed and one child did not sign are the ones driving the results. The estimated coefficients are an average treatment effect of individuals in these "treated" families being able to sign. It is possible that these families have unusual characteristics.

One method of estimating this population-wide effect is to estimate the effect separately for each treated family and use a weighted average of the effects (Miller, 2024). The weights are inverse propensity scores, estimated from a logistic regression of an indicator for being treated regressed on observed family characteristics using the entire sample and normalized to sum to one. For this to be a true average treatment effect, the method does come at the cost of several strict assumptions.[3] It is also reweighting based only on observables; any unobservable characteristic that makes these families unique will not be accounted for. Regardless of assumptions, it is still a useful exercise to see if the estimates are robust to reweighting.

Here, I estimate the propensity scores using indicator variables for the mother's signature, decade of first marriage, borough of first marriage, denomination, and the number of married children. Missing values are included as an additional category for each indicator variable. As shown in Table A6, there is still a positive and significant marriage premium for literacy.

[3] The assumptions: 1. There is no selection into treatment within groups. 2. Conditional on observables, there is no selection into treatment between groups based on heterogenous effects. 3. The logistic regression is the correct functional form. 4. There is a non-zero probability of treatment for every value of observable.

Table A4: Family fixed effects, spouse earnings score

	Panel A: Groom's log earnings score			
	(1)	(2)	(3)	(4)
Bride's signature	0.21***	0.04***	0.04***	0.03***
	(0.00)	(0.00)	(0.00)	(0.00)
N	997,453	997,453	85,184	997,453
Family FE		X	X	X
Sample restriction			X	
Controls				X

Sources: Project Balsac (2020).

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Column 2 is my preferred specification. In Column 3, to illustrate the size of the identifying variation, the sample is restricted to just families where at least one sibling signed and one did not. Note that after adding family fixed effects the estimates are close to symmetrical across gender.

Table A5: Family fixed effects, father-in-law's earnings score

	Panel A: Groom's father's log earnings score			
	(1)	(2)	(3)	(4)
Bride's signature	0.12***	0.02***	0.02***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
N	879,845	879,845	67,009	879,845
Family FE		X	X	X
Sample restriction			X	
Controls				X

	Panel B: Bride's father's log earnings score			
	(1)	(2)	(3)	(4)
Groom's signature	0.14***	0.03***	0.03***	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
N	851,979	851,979	74,295	851,979
Family FE		X	X	X
Sample restriction			X	
Controls				X

Sources: Project Balsac (2020).

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Column 2 is my preferred specification. In Column 3, to illustrate the size of the identifying variation, the sample is restricted to just families where at least one sibling signed and one did not. Note that after adding family fixed effects the estimates are close to symmetrical across gender.

Table A6: Marriage selection, reweighting for selection into identification

	Panel A		
	Groom Signature (1)	Groom Earnings score (2)	Groom's father Earnings score (3)
Bride's signature	0.41***	0.04**	0.02*
	(0.01)	(0.02)	(0.01)
N	1,850,379	997,453	879,845
Family FE	X	X	X

	Panel B		
	Bride Signature (1)		Bride's father Earnings score (3)
Groom's signature	0.37***		0.05***
	(0.01)		(0.01)
N	1,843,748		893,398
Family FE	X		X

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). Reweighted estimates are constructed by estimating the effect separately for each family and then taking the weighted average of the effects. The weights are inverse propensity score weights constructed by running a logistic regression of an indicator for if a family had at least one child who signed and one who did not on indicator variables for the parent's signatures, the mother's decade of first marriage, the mother's borough of first marriage, and the number of married children of the same gender in each family. Missing values are included as an additional category for each indicator variable in the logistic regression.

Appendix 10 Robustness of estimates of effects of parents

Occupational status

Table A7 replicates the analysis in Table 3 using the occupational status of the daughter's husband or the son as the measure of child outcomes.

Directly comparing half-siblings

One downside of the father fixed effects approach is that it relies on observing a measure of the ability of the mother. As shown in the Columns 3 and 4 of Table 4, the identifying variation is quite small. Very few parents had two spouses, one of which was literate and one of which was not. Hence, not all the coefficients are significant at the 5 percent level.

Fortunately, there is another test using parents with more than one marriage that only relies on the characteristics of the children. Consider a pair of children who could be either half-siblings or full siblings. If they share both a mother and a father and the abilities of mothers matter directly, their outcomes should be more correlated than if they share only a father. Again, there is a concern that the event resulted in a second marriage could have harmed the children of the first marriage. Again, assuming the penalty is a constant, fixed effects can control for it.

I estimate the regression:

$$C_{i,F} = \alpha C_{j,F} \times I\left(MC_{i,F} = MC_{ij,F}\right) + \delta_{mar_F} + \epsilon_{i,j,F} \tag{27}$$

Where $C_{i,F}$ is a characteristic of child i with father F and mother $MC_{i,F}$, $C_{j,F}$ is a characteristic of their half- or full sibling j, $I\left(MC_{i,F} = MC_{ij,F}\right)$is an indicator that is one if the children share a mother, δ_{mar_F} are fixed effects to control for the marriage number of the father, and $\epsilon_{i,j,F}$ is an error term.

The results are shown in Table A8. Full siblings are more strongly associated than halfsiblings. For example, a daughter signing her name was associated with a 60-percentage point increase in the probability her half-sister could sign her name. However, it was associated with a seventy-two-percentage point increase in the probability her full sister could sign her name. As before, the results are very similar regardless of if I allow mothers or fathers to vary and if I look at daughters or sons.

Table A7: Parental human capital and earnings scores

	Panel A: Son-in-law's earnings score				
	(1)	(2)	(3)	(4)	(5)
Signature of mother	0.06***	0.01		0.01	
	(0.00)	(0.01)		(0.01)	
Signature of father	0.14***		0.02*		0.02
	(0.00)		(0.01)		(0.01)
N	853,451	864,852	860,723	7,680	3,542
Adj. R Squared	0.05	0.38	0.38	0.26	0.27
Mother FEs			X		X
Mother's mar. no. FEs			X		X
Father FEs		X		X	
Father's mar. no. FEs		X		X	
	Panel B: Son's ln earnings score				
	(1)	(2)	(3)	(4)	(5)
Signature of mother	0.06***	0.02***		0.02**	
	(0.00)	(0.01)		(0.01)	
Signature of father	0.15***		0.03		0.03
	(0.00)		(0.02)		(0.02)
N	795,217	806,117	802,127	6,720	2,131
Adj. R Squared	0.06	0.44	0.44	0.32	0.25
Mother FEs			X		X
Mother's mar. no. FEs			X		X
Father FEs		X		X	
Father's mar. no. FEs		X		X	

Sources: Project Balsac (2020).

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). In Columns 4 and 5, to illustrate the size of the identifying variation, the sample is restricted to just parents who had at least one spouse who signed and one who did not. Controls include marriage year and sibling marriage order fixed effects (as birthdates are not reported after 1849).

Table A8: The effect of parental human capital on half vs. full siblings

	Panel A: Controlling for father			
	Daughter Signature (1)	Son Signature (2)	Son-in-law ln earnings (3)	Son ln earnings (4)
That of (half) sib.	0.60***	0.62***	0.26***	0.33***
	(0.01)	(0.01)	(0.01)	(0.01)
” × same mother	0.12***	0.08***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
N	2,050,712	1,838,040	721,719	649,203
Marriage number FE	X	X	X	X

	Panel B: Controlling for mother			
	Daughters Signature (1)	Sons Signature (2)	Sons-in-law ln earnings (3)	Sons ln earnings (4)
That of (half) sib.	0.65***	0.64***	0.29***	0.28***
	(0.01)	(0.01)	(0.02)	(0.02)
” × same father	0.06***	0.06***	0.01	0.10***
	(0.01)	(0.01)	(0.02)	(0.02)
N	1,974,765	1,770,143	695,604	625,937
Marriage number FE	X	X	X	X

Sources: Project Balsac (2020).

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual’s occupation in 1901 Canadian dollars (see text).

Appendix 11 Additional Discussion

Matching matters for father-son intergenerational correlations

If women directly matter for the outcomes of their children and marriages are assortative, the correlation between fathers and sons will be partially determined by the mother.[4] In the simple model above, the association between fathers and sons is:

$$C_i = \left(\beta_m + \gamma\beta_f\right)FC_i + \epsilon_i \tag{19}$$

$\beta_m + \gamma\beta_f$ should not be interpreted as the direct effect of the father. If the parents matched on individual characteristics, the mother increases the association through the $\gamma\beta_f$ term. Changes in the observed rates of intergenerational mobility, even if women are not observed, could be driven by changes in marriage matching (γ) or in how strongly mothers influence their children (β_f).

To demonstrate this, Table A9 estimates the intergenerational elasticity of imputed earnings separately for more and less assorted parents. The less assorted parents are those where only one parents was literate and the more assorted parents those where both parents were either literate or illiterate. The elasticities for the less assorted parents are 0.31 for the sons and 0.26 for daughters (using their husbands' imputed earnings as a proxy). For the more assorted parents, the elasticities are 0.43 for sons and 0.37 for daughters. The more strongly assorted parents have higher estimated rates of intergenerational mobility. It is possible that the more and less assorted families are not directly comparable and that the difference is due to some other omitted variable; a concern I address below.

[4] Espín-Sánchez, Gil-Guirado, and Vickers (2022) makes this point as well.

Table A9: Father-son intergenerational elasticities, more and less assorted marriages

	Son's earnings score		Daughter's husband's earnings score	
	(1)	(2)	(3)	(4)
Father's earnings score	0.31***	0.43***	0.26***	0.37***
	(0.01)	(0.00)	(0.01)	(0.00)
N	77,944	395,627	83,019	421,290
Parents differ on signature	X		X	
Parents the same on signature		X		X

Sources: Project Balsac (2020).
Note: *p<0.10; **p<0.05; ***p<0.01. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text).

Matching matters for multigenerational mobility

Many recent studies consider correlations across more than two generations (Clark, 2014; Espín-Sánchez, Gil-Guirado, and Vickers, 2022; Long and Ferrie, 2018; Olivetti, Paserman, and Salisbury, 2018; Solon, 2018). Here too, overlooking the role of mothers and grandmothers can lead to misleading conclusions when using standard approaches.

I am able to estimate multigenerational mobility with the Quebec data, as shown in Table 6 below. Note that when estimated separately, the intergenerational elasticities between grandfathers and grandchildren seem to be the same regardless of if the grandfathers are maternal or paternal. However, when the partial elasticities are estimated controlling for the log imputed earnings of the other grandfather and of the father, there is a larger coefficient for the maternal grandfathers.

Should we interpret this as maternal grandfathers being more important to the outcomes of grandchildren? The answer is no. If it is directly related to the mother's true status, a grandfather's observed status will have a coefficient biased upwards as the mother is omitted. Likewise, if it is directly related to the father's true status, it will have a coefficient biased upwards if the father is omitted. Controlling for the father's observed status will reduce the bias from omitting the true status of the father much more than it would reduce the bias from omitting that of the mother. As one would expect the maternal grandfather to be more strongly correlated with the mother, we would therefore expect a larger coefficient than the paternal grandfather after controlling for the father. This is what we observe in Table A10.

This exercise demonstrates how caution must be taken in interpreting intergenerational correlations without accounting for the role of women. It would at first seem plausible to have found evidence that maternal grandfathers mattered more for the outcomes of children than paternal grandfathers. However, it is merely an artifact of omitted variable bias and measurement error.

Table A10: Grandfather-grandson intergenerational elasticities

	Panel A: Son's earnings score			
	(1)	(2)	(3)	(4)
Mother's father's earnings score	0.28***		0.22***	0.12***
	(0.00)		(0.00)	(0.00)
Father's father's earnings score		0.28***	0.23***	0.10***
		(0.00)	(0.00)	(0.01)
Father's earnings score				0.36***
				(0.00)
N	439,068	429,426	263,803	164,696
	Panel B: Daughter's husband's earnings score			
	(1)	(2)	(3)	(4)
Mother's father's earnings score	0.25***		0.20***	0.13***
	(0.00)		(0.00)	(0.00)
Father's father's earnings score		0.25***	0.20***	0.11***
		(0.00)	(0.00)	(0.00)
Father's earnings score				0.29***
				(0.00)
N	489,868	479,496	292,671	181,443

Sources: Project Balsac (2020).
Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. Earnings scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text).

Robustness of effect of sorting on intergenerational elasticity

One concern with Table 5 is that families where only one parent was literate were selected on some omitted factor that decreases intergenerational mobility. One way to overcome this endogeneity is to find a variable that changes the degree of assortment of the parents' marriage and only matters for the outcome of the children through the degree of assortment. One plausible variable that meets these criteria is the fraction of the mother's older siblings who are female (Abramitzky et al., 2011; Caron et al., 2017; Dillon, 2010). As I do not observe ages in most of the sample, I instead consider the sex composition of the mother's siblings who got married before her.

The gender of children should be, at least at birth, as good as random, especially as there is no evidence of parity-dependent fertility control (Clark, Cummins, and Curtis, 2020). Why should this matter for sorting? One could imagine a scenario where a set of sisters has multiple potential suitors of similar characteristics in their neighborhood or social network. As more of the sisters marry, the remaining sisters will have to be less picky. It is possible that older sisters have a different effect on younger sisters compared to older brothers.[5] However, if it merely changes the status of the younger sister, who then match accordingly, it shouldn't introduce bias.

As shown in Table A11, the sex composition decreases the association between the signature rates of spouses and decreases the intergenerational elasticity between fathers and sons. This is exactly what we would expect if the mother directly mattered for the outcomes of children.

[5] In preliminary research I have conducted for another project, I find that before 1849, the fraction of older siblings that are male increases the rate of infant mortality for younger sisters.

Table A11: Sex composition, assortment, and intergenerational elasticities

	Mother signed	Son's earnings score
	(1)	(2)
Father signed	0.64***	
	(0.00)	
Share female	0.01	0.12***
	(0.01)	(0.04)
Father signed × share female	-0.01**	
	(0.01)	
Father's earnings score		0.43***
		(0.00)
Father's earnings score × share female		-0.02***
		(0.01)
N	519,887	519,887

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earning scores are the natural logarithm of the imputed annual earnings for the individual's occupation in 1901 Canadian dollars (see text). The share female is the fraction of the mother's siblings who married before her that were female.

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