Online Appendix for Donovan and Herrington (2018) "Factors Affecting College Attainment and Student Ability in the U.S. since 1900"

Last update: July 17, 2018

This Appendix provides further details and extensions of the results provided in the main body of the paper. The table of contents provides a brief overview of the included results, and refer to the main text for the motivation and more details surrounding these exercises.

Table of Contents for Online Appendix

A College Cost Data

This appendix details the construction of annual college costs, which we take as exogenous to the model. The primary sources of the underlying data are the *Biennial Surveys of Education* (BSE), which were published every other year covering 1916–1918 through 1956–1958, and the *Digests of Education Statistics* (DES), which have been published annually since 1962. In some cases data were revised in later publications, so we take the latest published estimates available.

We calculate average out-of-pocket college costs per student each year, λ_t, equal to the total current fund revenue from student tuition and fees divided by the number of full-time equivalent students enrolled.[34] Importantly, we consider only public colleges and universities in this calculation. Additional costs paid for private tuition or fees could be interpreted as payment for higher quality education, consumption, or to satisfy personal preferences for education at a particular institution. In any case, such marginal costs are not relevant for the binary decision of whether to attend college, so we exclude them from consideration.

We consider birth cohorts 1900–1972, so these students graduated high school and made college decisions 1918–1990. Because college takes four years, the last cohort faced college costs through the 1993–1994 academic year. Therefore, we construct a time series for annual college costs that runs from 1918–1993 in the model. The numerator for λ_t is constructed as follows:

- 1961–1993: current revenues from student tuition and fees for all public 2-year and 4-year institutions of higher education, taken from various issues of the DES. Data for 1962, 1964, 1966, and 1978 are missing, so we linearly interpolate these values.

- 1957–1961: missing values are interpolated between the 1961 data described above, and the 1957 data described below.

- 1918–1957: current revenues from student tuition and fees for public colleges, universities, and professional schools, taken from various issues of the BSE. Data are

[34] Alternatively, one could use the total current expenditures rather than revenues, but this makes little difference quantitatively because revenues and expenditures track each other quite closely. In addition, the revenue data is preferable because it allows us to determine how much of costs are paid out-of-pocket by students for tuition and fees, and how much comes from other sources such as state, local, and federal governments, private gifts, endowment earnings, auxiliary enterprises (athletics, dormitories, meal plans, etc.), and other sources.

generally available every other year, and missing years are linearly interpolated.

The denominator for λ_t is constructed as follows:

- 1967–1993: number of full-time equivalent students enrolled, taken from various issues of the DES.

- 1953, 1955, 1957, 1963, and 1969: number of full-time equivalent students calculated from the following disaggregated data series on student enrollment: full-time, part-time, first professional, graduate, unclassified, and extension students. Consistency is verified by comparing the calculated versus published data in 1969, which are within one-half of one percent of each other. Missing years are linearly interpolated.

- 1918–1953: No published data are available for full-time equivalence, nor have we been able to obtain disaggregated data to estimate full-time equivalence as above. Rather, we use the published data for "resident college enrollment" in public colleges, universities, and professional schools, taken from various issues of the BSE. Data are generally available every other year, and missing years are linearly interpolated.

Finally, for the exercise in Appendix D.1 we subtract from the denominator the resident college enrollment of World War II Veterans in public institutions. These figures are available in the BSE issues covering 1944–1946 through 1954–1956. As above, the data are available every other year, and we linearly interpolate values for the missing years.

B Estimating the Joint Distribution of Assets and Ability

This appendix provides details on the procedure used to estimate the joint distribution of initial assets (parental transfers) and ability using the NLSY79 and HSB data sets.

B.1 Ability Distribution

Grade-Ability Relationship in NLSY We begin by considering the relationship between grades and ability in the NLSY. We first construct the variable GPA which is student high school grades on a $0-100$ scale. We assume the underlying distribution of GPA is lognormally distributed with mean and variance μ_g and σ_g. We next link this to ability, which in the NLSY is AFQT scores. We impose a linear relationship between log(GPA) and AFQT scores, and estimate the parameters of this relationship with the regression

$$AFQT_i = \alpha_0 + \alpha_1 log(GPA)_i + \varepsilon_i \tag{B.1}$$

which returns coefficient estimates $\alpha_0 = -8.00$ and $\alpha_1 = 1.91$, and error term standard deviation $\sigma_\varepsilon = 0.88$. By our assumption of lognormal GPA, it follows that $AFQT$ is normally distributed as $AFQT \sim N(\alpha_0 + \alpha_1\mu_{g,NLSY},\ \alpha_1^2\sigma_{g,NLSY}^2 + \sigma_\varepsilon^2)$.

Imposing the Relationship in HSB With the grades-ability link now estimated in the NLSY, we next impose this relationship on the High School and Beyond (HSB) data. This step is required because HSB includes student grades, but no information on underlying ability. In principal, we could compute the underlying AFQT score in HSB by directly imposing the estimates derived from equation (B.1). After assuming HSB GPA is lognormal, and computing $\mu_{g,HSB}$ and $\sigma_{g,HSB}$, this would imply

$$AFQT_{HSB} \sim N(\alpha_0 + \alpha_1\mu_{g,HSB},\ \alpha_1^2\sigma_{g,HSB}^2 + \sigma_\varepsilon^2)$$

where α_0, α_1 and σ_ε^2 are estimated from equation (B.1). Unfortunately, HSB does not ask for grades directly, but instead what bins they fall into. The bins are (1) 90-100, (2) 85-89, (3) 80 - 84, (4) 75-79, (5) 70-74, (6) 65-69, (7) 60-64, (8) lower than 60. We therefore must estimate the mean and variance of grades from these bins.

To this end, let $\bar{g}_j$ and $\underline{g}_j$ be the maximum and minimum grades in any grade bin j. Letting F denote the cumulative distribution function of the underlying (lognormal) grade distribution, each bin j includes mass

$$\widetilde{M}_j = F(\bar{g}_j) - F(\underline{g}_j).$$

Let M_j be the empirical mass in each grade bin calculated in the HSB data. We therefore choose $\mu_{g,HSB}$ and $\sigma_{g,HSB}$ to minimize the sum of the squared errors $(\mu_{g,HSB}, \sigma_{g,HSB}) \in \arg\min \sum_j (\widetilde{M}_j - M_j)^2$. The estimated parameters are $\mu_{g,HSB} = 4.404$ and $\sigma_{g,HSB} = 0.096$. Figure 12 plots the estimated bins along with their empirical counterparts, and shows that they match well. The sum of squared errors between the two discrete distributions is 0.004.

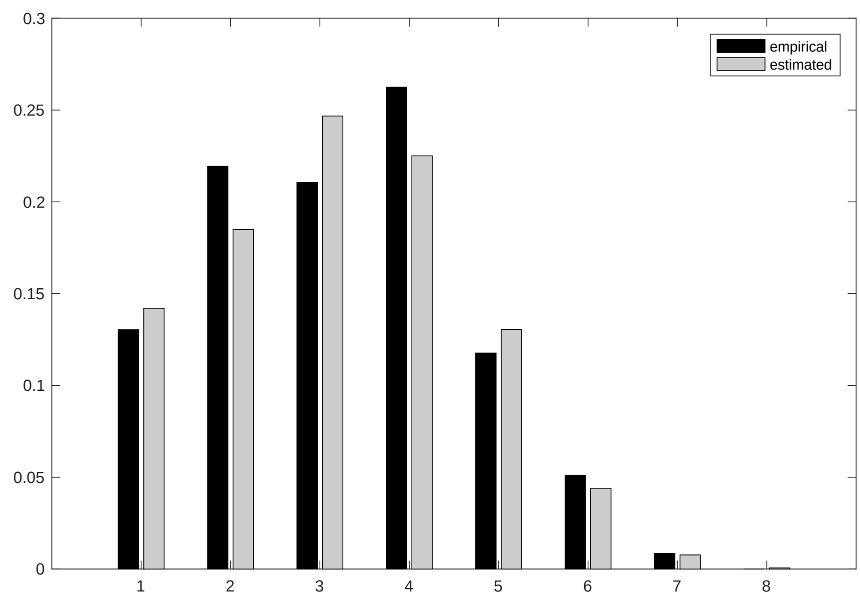


Figure 12: Data and Predicted Grade Bins in HSB

We now have the underlying grade distribution in HSB, which is distributed $logN(\mu_{g,HSB}, \sigma_{g,HSB})$. Imposing the estimated link between ability and grades in the NLSY onto the HSB data, we get that $AFQT_{HSB} \sim N(\alpha_0 + \alpha_1\mu_{g,HSB},\ \alpha_1^2\sigma_{g,HSB}^2 + \sigma_\varepsilon^2)$.

B.2 Transfer Distribution

Since we have the marginal distribution of $AFQT_{HSB}$, we now need to construct the unconditional distribution of transfers k_0. The empirical distribution of transfers has a large mass near zero. Therefore, we assume the marginal distribution of transfers follows a gamma distribution. We compute the distribution of unconditional transfers from parents to high school graduate children, then normalize by the average transfer. Denote $\widetilde{k}_0$ as the transfer normalized by the mean. The shape and scale parameters of the gamma distribution are chosen to minimize the sum of squared errors between that empirical c.d.f. and that of the estimated gamma distribution. The best fit parameters of the gamma distribution are a shape parameter of 0.24 and a scale parameter of 4.44. Figure 13 plots both together, and shows that the estimated distribution matches the data well.

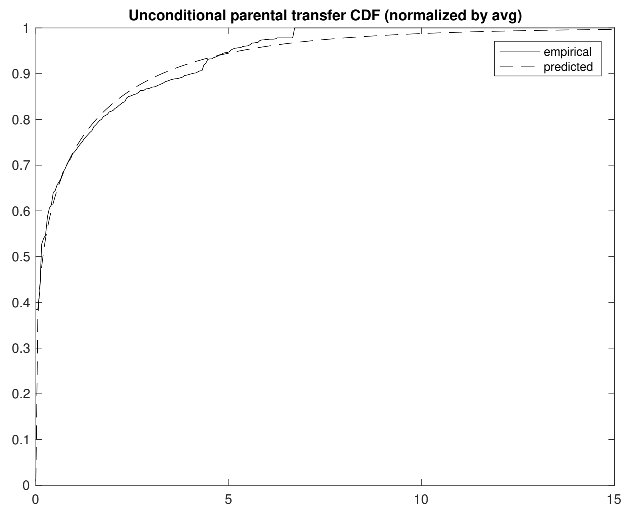


Figure 13: Cumulative Distribution Function for Transfers in HSB

B.3 Joint Distribution of Ability and Transfers

The last step is to compute the joint distribution of the two marginals we created above. We use a Frank copula to combine these two marginal distribution into the joint distribution required by the model. The Frank copula takes the form

$$C(u,v) = \frac{-1}{\rho}\log\left(1 + \frac{(\exp(-\rho u)-1)(\exp(-\rho v)-1)}{\exp(-\rho)-1}\right)$$

where ρ governs the dependence of draws. Our joint distribution of α and k can therefore be written as

$$H(\alpha,k) = C\left[F(\alpha), G(\widetilde{k}_0)\right]$$

where F and G are the cumulative distribution functions of the normal and gamma respectively. We are therefore left to calibrate ρ, which roughly implies a positively correlation between the two series when $\rho > 0$. Note, however, that while our above procedure gives the marginal distribution of AFQT in HSB, it does not provide individual-level estimates of AFQT. Moreover, we only have grade bins, not actual grade realizations. We therefore proceed as follows. We first assign each individual a random grade realization that is consistent with (1) the bin of their grades and (2) the estimated lognormal distribution of underlying grades. Conditional on that realization, we then use the implied relationship between grades and AFQT to draw an individual realization of AFQT. We then compute the Kendall rank coefficient. We repeat this simulation 1000 times, and the average Kendall rank coefficient is 0.04, implying that high school graduates with higher ability on average have higher initial asset holdings. This implies a copula parameter of $\rho = 0.35$.

B.4 Growth in Transfers over Time

Since the HSB data is for one cohort of high school graduates, we lastly need to make an assumption on the growth of transfers over time. We fix the parameters governing the joint distribution constructed above, then allow the actual dollar amount of transfers to evolve according to

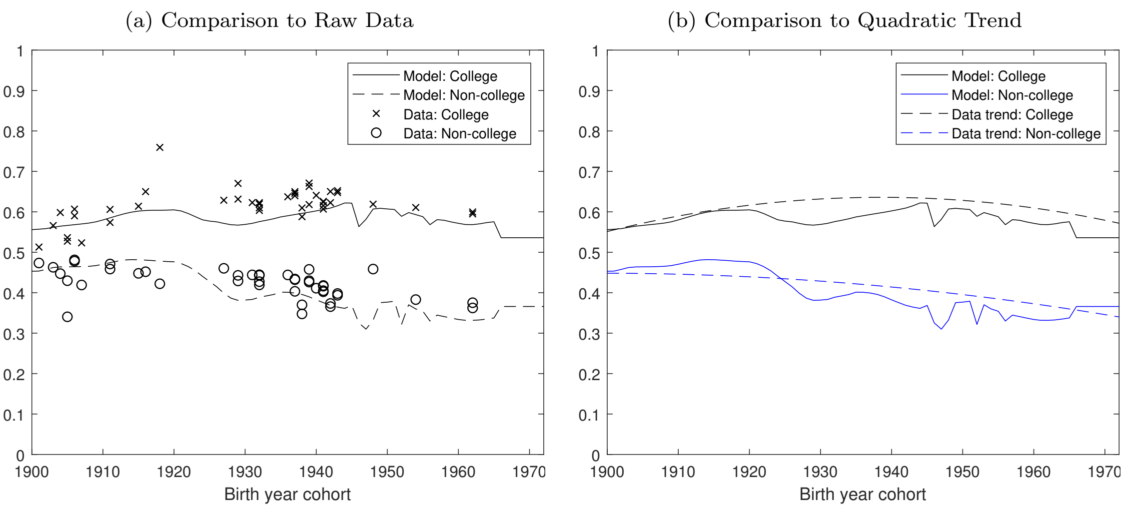
$$
\begin{aligned}
k_{0it} &= \left(\frac{\overline{k}_{0,1962}}{y_{1962}}\right) y_t \widetilde{k}_{0i} \\
&= \left(\frac{2225}{34473}\right) y_t \widetilde{k}_{0i} \\
&= 0.065 y_t \widetilde{k}_{0i}
\end{aligned}
$$

where y_t is average income in year t and $\overline{k}_{0,1962}$ is the average transfer in the HSB data. That is, we assume that the average transfer is always equal to 6.5 percent of average income (as in HSB), and therefore scales proportionally with average income over time.

C Further Quantitative Results

C.1 Ability Series for College and Non-College Individuals

Figure 14: Separate Ability Time Series, Model vs. Data



C.2 Time Series of Overall "Endogenous" Factors

Our results in the main text showed that we matched the overall trends of attainment. For further evidence that the "endogenous" portion of college completion matches well, we plot

$$\frac{\text{Number of college degrees awarded at year } t+4}{\text{Number of high school graduates at year } t}.$$

In the model, this is exactly equal to the fraction of high school graduates that eventually finish college (N_t^{grad}/N_t^{HS}), as all college completion is completed within four years. This empirical counterpart is available in Snyder (1993), though it naturally does not match perfectly given that some individuals take more than 4 years to finish college. However, it provides a reasonable approximation for comparison. Figure 15 plots this time series in both the model and data, and shows that our model matches the empirical time series well. Indeed, consistent with the results in Table 4, there is a substantial decline for cohorts before 1920, an immediate increase between 1920 and 1930, and a relatively small increase for post-1930 cohorts.

Note that Figure 15 shows why we under-predict attainment in the 1910 – 1920

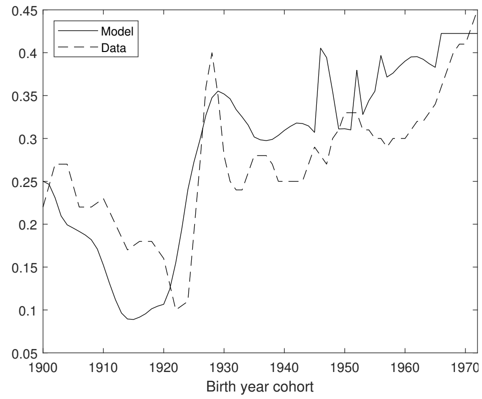


Figure 15: College Completion Rate Conditional on HS graduation

cohorts: we predict a decline in the conditional completion rate that predates the actual decline in the data.

C.3 Growth Accounting in Model vs Data

Table 4 reproduces the growth accounting exercise from Section 2, then computes the same numbers in the model-generated time series. While the model matches the long run trends well, it does underpredict college attainment growth for cohorts before 1920 and overpredicts attainment growth for cohorts after 1950.

Table 4: Attainment growth rate decomposition in data versus model

Panel A: Data	Log differences			Annualized Percent Change		
Birth years	γ^{col}	γ^{hs}	γ^{endog}	γ^{col}	γ^{hs}	γ^{endog}
1900 - 1972	2.13	1.59	0.54	3.00%	2.23%	0.76%
1900 - 1920	0.54	1.13	-0.59	2.72%	5.80%	-3.08%
1920 - 1950	1.41	0.50	0.91	4.82%	1.70%	3.13%
1950 - 1972	0.18	-0.04	0.22	0.80%	-0.19%	0.99%
Panel B: Model	Log differences			Annualized Percent Change		
Birth years	γ^{col}	γ^{hs}	γ^{endog}	γ^{col}	γ^{hs}	γ^{endog}
1900 - 1972	2.11	1.59	0.52	2.98%	2.23%	0.73%
1900 - 1920	0.27	1.13	-0.85	1.38%	5.80%	-4.17%
1920 - 1950	1.58	0.50	1.07	5.39%	1.70%	3.64%
1950 - 1972	0.26	-0.04	0.31	1.21%	-0.19%	1.40%

C.4 College Enrollment and Graduation Rates

Unlike in the data, the model allows a full decomposition of equation (2.2), in which γ^{endog} is split into enrollment and graduation rates as $\gamma^{endog} := \gamma^{enroll} + \gamma^{grad}$. Table 5 shows results of this more detailed decomposition.

Table 5: Growth decomposition of endogenous factors in model

	Log Differences				Annualized Percent Change			
Birth years	γ^{col}	γ^{hs}	γ^{enroll}	γ^{grad}	γ^{col}	γ^{hs}	γ^{enroll}	γ^{grad}
1900 - 1972	2.11	1.59	0.56	-0.03	2.98%	2.23%	0.78%	-0.05%
1900 - 1920	0.27	1.13	-0.93	0.08	1.38%	5.80%	-4.55%	0.39%
1920 - 1950	1.58	0.50	1.07	0.00	5.39%	1.70%	3.63%	0.01%
1950 - 1972	0.26	-0.04	0.42	-0.11	1.21%	-0.19%	1.32%	-0.52%

The striking result of this decomposition is the overwhelming importance of variation in college enrollment relative to variation in college graduation rates conditional on enrollment, as the latter accounts for only a small portion of changes in college attainment. Table 5 shows that during most of the period under consideration, the magnitude of γ^{enroll} dwarfs that of γ^{grad}.

D Additional Experiments in the Model

In this appendix we present the results of several additional experiments. First, we evaluate the model with an alternative time series for college costs that is adjusted for payments made on behalf of veterans through the WWII GI Bill. Next we consider heterogeneity in college costs. Then we incorporate returns to ability in the wage profiles to reflect the fact that higher (lower) ability students may experience higher (lower) returns to education. Finally, we incorporate ability selection at high school graduation and consider how this could affect the model results as high school graduation rates increased across cohorts. In several cases we find that these experiments can actually improve the model's fit to the data. Nevertheless, we chose not to include these features in the benchmark model because they complicate the interpretation of counterfactual exercises.

D.1 Adjusting College Costs for World War II GI Bill

The time series for college costs in our benchmark analysis computes the cost per student as the total tuition and fee revenue received by public colleges and universities divided by the number of full-time equivalent students at those institutions. In this section we discuss how various GI Bills impact our measure of costs and then evaluate how the World War II (WWII) GI Bill in particular affects the model's predictions for college attainment and ability sorting.

As is well-known, more than 2.2 million WWII veterans attended college using GI Bill funds. Another 1.2 million attended college on the Korean GI Bill, and 5.1 million on the Vietnam era GI Bill (see, e.g., Bound and Turner (2002)). By providing subsidized access to college for millions of returning veterans, these programs lowered the average out-of-pocket costs to attend college. In the WWII version, the Federal government made payments directly to schools on behalf of the veterans, but in the Korean and Vietnam versions the Federal government gave stipends directly to the veterans who then paid the schools. Because of this distinction, WWII GI Bill payments were reported as a separate revenue source in the data, and therefore were not included in our data on revenue from student tuition and fees. The Korean and Vietnam GI Bill payments, on the other hand, were included with student payments and therefore also in the calculation of our original college cost series. Thus, by including WWII veteran students in the denominator but excluding payments on their behalf from the numerator, our baseline college cost series understates the average costs actually faced by non-veteran students during the years 1945–1956.

In this experiment we make two changes to adjust for how the WWII GI Bill affects college costs faced by individuals in our model. Our goal in this exercise is not to assess the overall impact of the GI Bill; rather, we want to gauge the differential impact of college costs on women and non-veteran men compared to veteran men who were subsidized. Toward this end, we first compute an alternative cost series that applies to women and non-veteran men.[35] To do so, we subtract the number of WWII veterans enrolled in public colleges from the denominator of our original cost series, so we now calculate the average out-of-pocket tuition and fees per *non-veteran* student. From 1945–1956, the adjusted cost series is about 28% higher, on average, than the benchmark cost, reflecting the fact that women and non-veterans faced higher average costs since the GI Bill did not apply to them. This change affects the college costs for women and non-veteran men born 1923–1938 who attend college for at least one year during 1945–56. Second, we select a random group of men (independent of initial endowments (α, k_0)) to designate as "veterans" in the model and set their college costs to zero. This applies only to birth cohorts 1923–1929 because men born before 1923 would have been over age 23 already at the end of WWII, and those born after 1929 were too young to have served in WWII. We take the number of eligible veterans in each birth cohort from Table 2 in Turner and Bound (2003).

Figure 16 plots the results for aggregate college attainment and ability sorting in this experiment compared to the baseline model. We find only small changes in the aggregate college completion rate, which declines less than two percentage points relative to the baseline model across all affected cohorts.[36] The difference in average ability between college and non-college individuals also declines by several percentage points. This occurs because some lower ability veterans attend college using GI subsidies, while some higher ability women and non-veterans opt out in the face of higher costs.

[35] Only about 3% of veterans who attended college on the WWII GI Bill were women.

[36] Recall that individuals born after 1929 are still affected in this experiment because excluding GI Bill revenues during their college years causes us to underestimate the average costs they actually faced.

Figure 17 breaks down college attainment by sex. As seen in 17a, there is little change in overall attainment for men relative to the baseline. There are three main reasons for this. First, about half of men in the 1923–1929 cohorts are non-veterans, and their attendance declines because they face higher college costs in this experiment. Second, among the veterans who can receive free college education, many would have chosen to attend even without the subsidy, as in the baseline model. Finally, conditional on attendance these men still must graduate, and many of them do not. This is consistent with evidence in Bound and Turner (2002). Their Table 2 shows that about half of eligible veterans in the 1923–1929 birth cohorts utilized GI Bill benefits, but less than 15% actually completed a bachelor's degree.

Figure 17b shows the change in female college attainment in this experiment. Because females, like non-veteran men, face higher college costs in this model, they attend and complete college at lower rates. Much of the attainment growth observed for the late 1920s birth cohorts in the baseline model is dampened when we adjust for the fact that women did not benefit from GI Bill subsidies. In fact, this version of the model does a much better job replicating the data for women born during the 1925–1935 cohorts.

Finally, it is important to note some limitations from this experiment. While the model generates an increase in college attainment for veteran men, as expected, it can not be expected to account for the full effect of the GI Bill on aggregate college attainment. The main reason is that many affected veterans were older than 23 when they returned from the war. As shown, for example, in Bound and Turner (2002) roughly half of veterans who utilized GI Bill benefits were born 1915–1922. Moreover, the data series in Figure 1a is calculated as the number of college degrees conferred relative to the 23-year old population, so the spike in college attainment just after WWII in the data reflects, in part, college completion by older returning veterans. Because our model does not capture college attendance or completion at older ages, we can not speak to the total impact across all affected cohorts. Rather, we only intend to demonstrate the effect of GI Bill subsidies on a portion of affected veterans relative to non-veterans and women.

Figure 16: Experiment: Costs Adjusted for WWII GI Bill

(a) Attainment (b) Ability Sorting

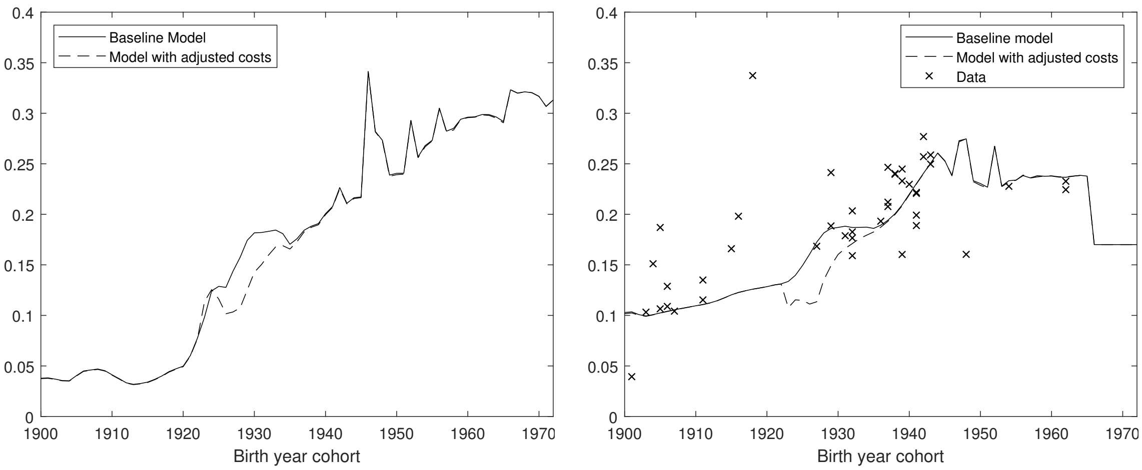
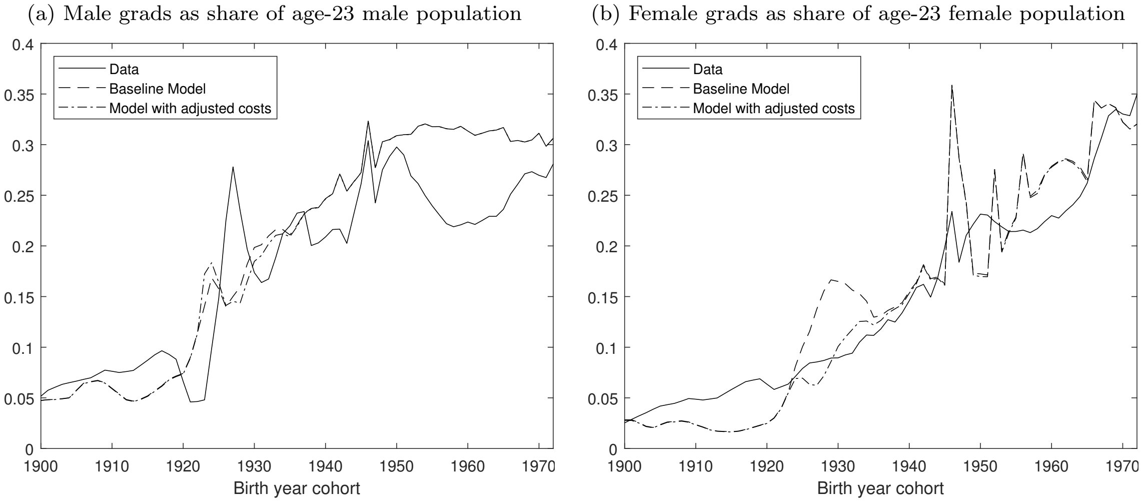


Figure 17: College Attainment by Sex in Model with Adjusted Costs

(a) Male grads as share of age-23 male population (b) Female grads as share of age-23 female population



D.2 Heterogeneity in College Costs

Our assumption throughout the main paper was that all individuals in a given birth cohort face the same price for college. The main reason for this assumption is that we focus on *whether* students attend college rather than *where* they attend, and we believe the relevant cost for students at this decision margin is the public in-state tuition. While many students choose to attend higher priced options like private or out-of-state public schools, we consider these decisions to be inframarginal. Nevertheless, even public college costs can vary substantially across and within states.

As a robustness exercise we now incorporate tuition heterogeneity in the form of a normally distributed random cost shock. Specifically, we define $\overline{\lambda_t} = \sum_{j=t}^{t+4} \frac{\lambda_t}{(1+r)^{j-t}}$ as the the present discounted value of 4-year college costs faced by the cohort making college decisions in year t in the benchmark model. In this experiment, individuals instead face costs $\widetilde{\lambda}_t = \sum_{j=t}^{t+4} \frac{\lambda_t+\varepsilon}{(1+r)^{j-t}}$, where $\varepsilon \sim N(0, \sigma^2_{\varepsilon,t})$. We assume that the shock is i.i.d. across individuals in a cohort and uncorrelated with the the initial asset and ability endowments. Note also that each individual receives a single shock that affects cost for all four years, so costs do not change unexpectedly after enrollment. We discipline the shock variance using state-level data for eleven school years spanning 1921–1956.[37] First, we calculate college costs for each state in the same manner as we did for the entire country in the benchmark—as the public university revenue from student tuition and fees divided by the resident college enrollment. We then compute the coefficient of variation across states in each year. The average across years is 0.55, meaning that the standard deviation in costs is about 55% of the average annual cost. We therefore set $\sigma_{\varepsilon,t} = 0.55\lambda_t, \forall t$.

Figures 18a and 18b show the results for college attainment and ability sorting in this experiment relative to the benchmark. Introducing cost heterogeneity does not affect the benchmark results substantially, and in fact it actually improves the fit with the data for many birth cohorts, particularly between 1910–20 and 1950–65 when average costs were historically high relative to income (as shown in Figure 3). For the 1910–20 cohorts, the college attendance rate was at a historical low because costs relative to income were high, and the college premium was near a historic low.

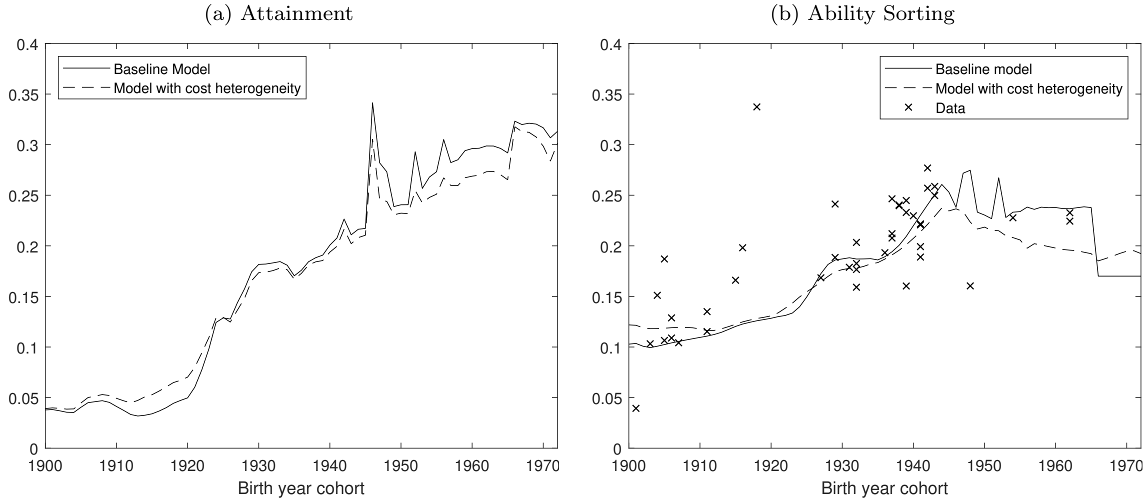
[37] Data are from various issues of the *Biennial Surveys of Education*. The state data were not reported in every issue, but we were able to collect it for 1921–22, 1925–26, 1927–28, 1929–30, 1943–44, 1945–46, 1947–48, 1949–50, 1951–51, 1953–54, and 1955-56.

For these cohorts, the negative cost shocks are particularly beneficial in boosting attendance and therefore completion. Indeed, in the data we observe many public universities that still charged zero or near-zero tuition during the 1930s and early 1940s, when these birth cohorts were making college decisions. Our average cost series in the benchmark does not capture this feature of the data, but by adding heterogeneity we have individuals who face very low college costs, as was true in reality.

For the 1950–65 birth cohorts, the story is basically reversed. College costs and the college premium were relatively high by historic standards, as was the college attendance rate in the benchmark model. In this case negative cost shocks have little effect on attendance because most people were already going, but positive cost shocks deter some individuals who would have otherwise attended at the average price. Hence we see attendance and completion rates fall relatively to the benchmark model. Whereas the benchmark model overpredicted college attainment by nearly five percentage points relative to the data for the 1950–65 birth cohorts, this experiment closes more than half of that gap. With cost heterogeneity, the model overpredicts the data by only two percentage points.

Figure 18: Experiment: heterogeneous college costs

(a) Attainment (b) Ability Sorting



D.3 Income Returns to Ability

In the benchmark specification we estimated wage profiles by cohort, sex, education, and potential labor market experience. Ideally we would like to also observe individual ability in the Census and ACS data, but this is not possible. Thus, in our wage profiles the return to education will also include the average returns to innate ability. Because ability selection across education levels has changed over time, this is also reflected in the average returns to education that we estimate. In an attempt to verify whether this assumption materially affects our results, we now adjust wage profiles by ability.

Our approach relies on estimates of the return to ability in Bowles, Gintis and Osborne (2001), who survey 24 other studies and report that a one standard deviation increase in measured cognitive skill is associated with an average wage increase of about 7%, or "roughly equivalent to a year of schooling." We incorporate this result by adjusting wage profiles for some college and college education up and down for abilities above and below the mean. Wage profiles for individuals of mean ability are unchanged. As a result, the education premia for some college and college education are higher (lower) for individuals with higher (lower) ability. For example, consider the final birth cohort in 1972. The average college premium (i.e., the increase in present value lifetime earnings for a college graduate relative to having only a high school degree) is about 68%. After adjusting for returns to ability, college graduates one standard deviation above the mean ability expect to earn a premium of almost 80%, while those one standard deviation below the mean ability expect a smaller premium of only 50%.

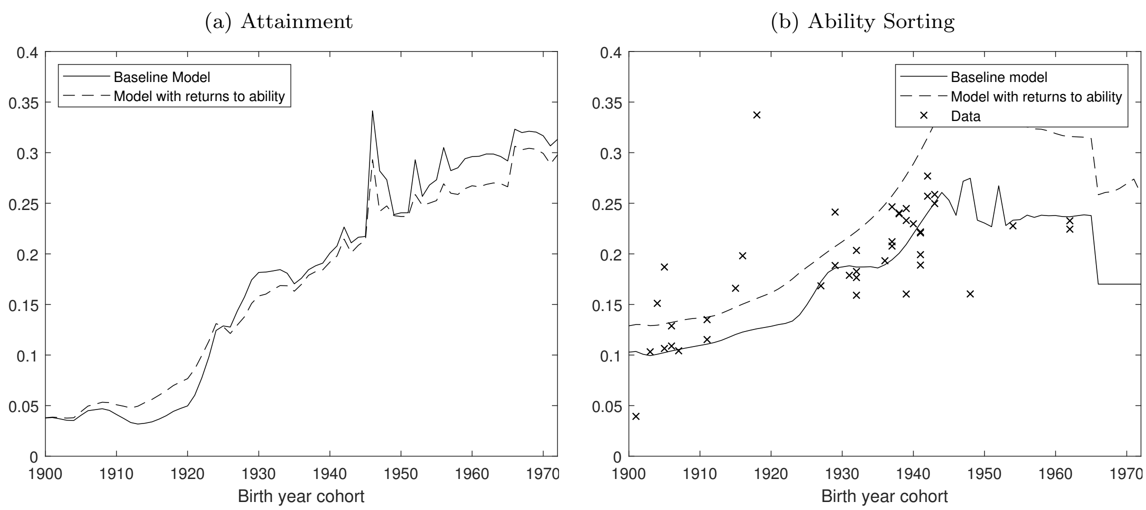
The results after incorporating these alternative wage profiles are shown in Figures 19a and 19b. College attainment in this experiment is generally quite similar to the benchmark, but ability differences are larger compared to the benchmark. The difference in average ability between college and non-college individuals is about three percentage points higher for pre-WWII birth cohorts, and about eight percentage points higher for post-WWII birth cohorts. The reason is straightforward. When returns to education are positively related to ability, higher ability individuals are more likely to attend and lower ability individuals less likely. Thus, the ability gap between college and non-college types grows larger.

As with heterogeneous costs the model fit for college attainment improves for the

1910–1920 and 1950–1965 birth cohorts. The intuition for this result is similar, except now the heterogeneity affects returns to college rather than costs, which also leads to an additional effect because the borrowing constraint adjusts. For the 1910-20 cohorts, recall that average returns were relatively low and costs relatively high. Individuals above mean ability now expect higher lifetime wages, and since the borrowing constraint is a share of expected lifetime earnings, they can borrow more to cover the high costs of college. Hence, more high ability individuals attend college. Individuals below mean ability expect lower lifetime earnings, and their borrowing constraint tightens. Some individuals who would have attended in the benchmark now opt out of college. However, fewer individuals below average ability would have attended anyway due to higher risk of non-completion, so the number of lower ability individuals opting out of college is dominated by the number of higher ability individuals opting in. Hence, we observe higher college attainment in Figure 19a and a larger ability gap in Figure 19b. Whereas the benchmark model predicted college attainment about three percentage points lower than the data over 1910–1920 cohorts, this experiment closes two-thirds of the gap and is only about one percentage point lower than the data.

For the 1950–65 birth cohorts, the average college premium is much higher, and costs are relatively high, as well. Now individuals above mean ability get higher returns, but their attendance decisions are unaffected. Most would attend regardless. However, individuals below mean ability are more affected. They expect lower average returns and, as a result, have tighter borrowing constraints. In the face of relatively high costs, this leads many more individuals below mean ability to forego college attendance. Hence, college attainment falls and the ability gap widens even further. As in the case of cost heterogeneity, this experiment is able to close about half of the gap between model and data for attainment over the 1950–65 cohorts.

Figure 19: Experiment: Returns to ability



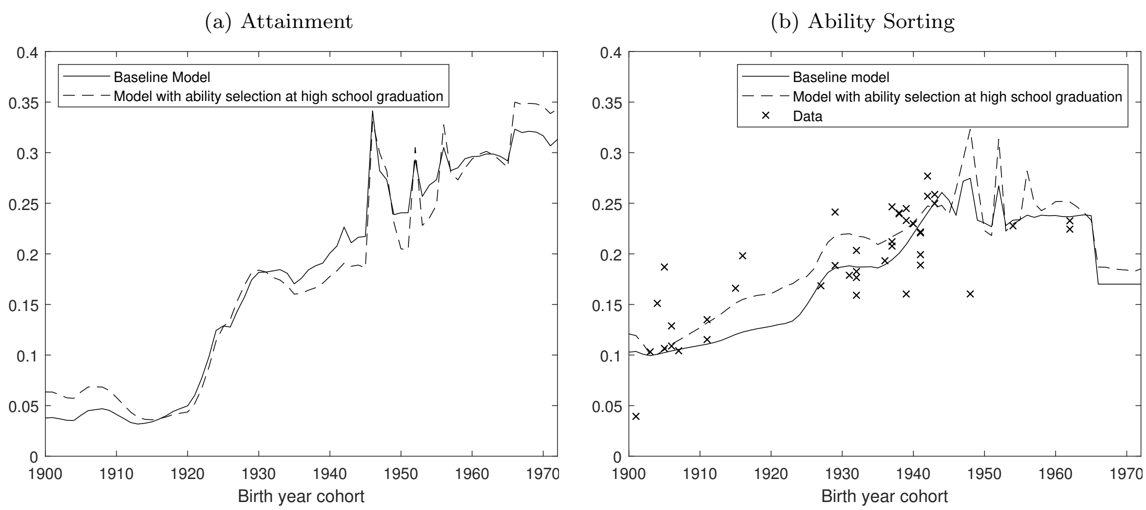
D.4 Ability Selection at High School Graduation

In the benchmark model we assume the ability distribution of high school graduates is constant across cohorts. This is a function of data availability, and we cannot compare raw ability levels across cohorts. Nevertheless, it is reasonable to ask whether the ability distribution of high school graduates may have changed over the cohorts we consider as the high school graduation rates increased.

Lacking reliable historical data about the extent of ability selection at high school graduation, we utilize NLSY79 data to estimate the relationship between ability ranking in the population and probability of high school graduation. We assume this relationship holds for all prior cohorts and adjust the ability distribution of high school graduates for each cohort so as to match the observed high school graduation rates. This procedure effectively selects out a larger share of lower ability individuals when the high school graduation rate is lower, so the remaining population of high school graduates has higher average ability.

Figures 20a and 20b show the college attainment and ability sorting results for this experiment. To obtain these results we first recalibrate; otherwise, the model overshoots attainment due to high school graduates having higher average ability. The new parameters are $\gamma = 0.05$ for the borrowing constraint, $\sigma_\varepsilon = 2.4$ for ability signal variance of the initial cohort, and $\sigma_\varepsilon = 0.55$ for the final signal variance. Overall we find that this approach generates very similar results to the baseline. The biggest difference is that the graduation rate conditional on attending starts out higher and falls steadily (from about 61% to 47%), whereas the benchmark model has conditional graduation rate that is relatively stable across cohorts (around 45% on average). In our view, the latter seems more consistent with the available historical evidence, which indicates a relatively stable graduation rate across cohorts.

Figure 20: Experiment: Ability selection at high school graduation



E Summary of Model Notation

Table 6: Summary of Model Notation

Symbol	Definition
N_{mt}	Number of males born into model (i.e., graduating high school) in year t
N_{ft}	Number of females born into model (i.e., graduating high school) in year t
a	Age of individual, where $a = 1, 2, ..., T$
s	Sex of individual, where $s \in \{f, m\}$
k_0	Initial asset endowment
α	Ability endowment
$\pi(\alpha)$	Annual probability of passing college, given ability α
θ	Signal of true ability, where $\theta = \alpha + \varepsilon$
ε	Error term on signal of true ability
$\sigma_{\varepsilon,t}$	Standard deviation of ability signals for high school graduates in year t
ν	Vector of variables that are informative about true ability, where $\nu = (k_0, \theta)$
C	Number of years required to graduate from college
e	Years of education completed by individual, where $e \in \{0, 1, ..., C\}$
λ_t	Annual cost of college in year t
γ	Individuals may not borrow more than a fraction γ of expected discounted future earnings
$\bar{k}$	Minimum asset level for individual, given age, sex, education, and γ