Economic Inquiry

CHANGING ROLES OF ABILITY AND EDUCATION IN U.S. INTERGENERATIONAL MOBILITY

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Using data on young adults from the 1979 and 1997 National Longitudinal Survey of Youth, we investigate the changing roles of ability and education in the transmission of economic status across generations. We find that ability plays a substantially diminished role for the most recent cohort whereas education plays a much larger role. The first finding results primarily from a smaller effect of children's ability on status, the second from an increased correlation between parental status and educational attainment. A replication of the analysis by gender reveals that the changes in the role of ability are largely driven by men whereas the changes in education's role are largely driven by women. (*JEL* J62, I24)

I. INTRODUCTION

Intergenerational economic mobility is considered a measure of equality of opportunity. Empirical evidence suggests there are relatively low levels of mobility in the United States (see Solon 1999 for a review and Mazumder 2005 for a recent reassessment). Stated differently, there is a high degree of transmission of economic status between generations. Transmission of economic status also appears to have been fairly persistent over time in the United States; empirical measures of intergenerational mobility across different generations have been relatively constant (Chetty et al. 2014; Jäntti and Jenkins 2015; Lee and Solon 2009). However, constant *aggregate* measures of mobility do not imply that the roles played by underlying factors have been constant. Whether a low degree of mobility should be of concern to policy makers and how to address it, in part, depends on the factors that explain the intergenerational link and how the roles of these underlying factors have changed over time.

A number of recent articles focus on identifying what factors explain the transmission of economic status; these studies differ widely in both objectives and method. One set of studies attempts to assign causal interpretations to specific relationships—for example, what portion is financial or genetic in nature (Bowles and Nelson 1974; Cardak, Johnston, and Martin 2013; Lefgren, Lindquist, and Sims 2012; Liu and Zeng 2009; Sacerdote 2002; Shea 2000). A related literature is more descriptive in its goals and attempts to "account" for the transmission of status through key variables (Blanden, Gregg, and Macmillan 2007; Blanden et al. 2014; Bowles and Gintis 2002). And although there has been substantial work assessing whether mobility has changed over time (Aaronson and Mazumder 2008; Chetty et al. 2014; Lee and Solon 2009; Mayer and Lopoo 2004, 2005), much less work, mostly because of data limitations, has explored how the factors responsible for the transmission of economic status have changed. One exception is Blanden, Gregg, and Macmillan (2007) who explored potential changes in the United Kingdom. Our analysis augments the existing literature by

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ABBREVIATIONS

AFQT: Armed Forces Qualifying Test
ASVAB: Armed Services Vocational Aptitude Battery
CAT: Computer Administered Test
IGE: Intergenerational Elasticity of Income
IQ: Intelligence Quotient
NLSY: National Longitudinal Survey of Youth
OLS: Ordinary Least Squares

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exploring potential changes in the United States. Specifically, we focus on two factors identified in previous literature—cognitive ability and education—and evaluate whether their roles in the transmission of economic status have changed over time.[1,2]

There are reasons to suspect that the roles of education and cognitive ability may have changed. The United States has had an unprecedented surge in investment in primary and secondary education over the past few decades. Between 1972 and 1992 alone, total per pupil spending on public education increased 53%; at the same time, within-state spending became more equal across school districts (Mayer and Lopoo 2004; Murray, Evans, and Schwab 1998). A key theoretical model of intergenerational mobility (Solon 2004) suggests that, all else equal, an increase in the progressivity of public investment in children's human capital should lower the transmission of economic status across generations. However, the surge in public education investment did not occur in isolation; for example, returns to education also increased over this time period. Furthermore, Zhong (2013) shows that extensive education expansion can cause "over-education" and lead to more persistent immobility. In this article, we do not attempt to empirically estimate the direct relationship between U.S. public education investment and the transmission of status.[3] Rather, given empirical evidence that the transmission of economic status has been relatively constant over a period of increasing investment in public education, we seek to investigate whether the roles of education and cognitive ability in the transmission of status have also been constant. By analyzing the potentially changing role of educational attainment and cognitive ability in the transmission of status, we hope our analysis will provide some indirect insight into the relationship between the recent changes mentioned above and the transmission of status.

To evaluate whether the roles of ability and education in the transmission of status have changed over time in the United States, we use data on two groups of young adults separated by about 20 years—the 1979 and 1997 cohorts of the National Longitudinal Survey of Youth (NLSY). Cognitive ability is measured through Armed Forces Qualifying Test (AFQT) scores whereas education is measured as degree attainment. For economic status, we consider two potential measures: (1) a within cohort percentile ranking of income and (2) log income. The latter is the more traditional measure of economic status and, empirically, provides an estimate of the intergenerational elasticity of income (IGE). To identify the individual roles of ability and education and how these roles may have changed over time, we apply a decomposition method based on the ordinary least squares (OLS) omitted variable bias formula (Gelbach 2016). To be clear, our results are descriptive in nature and we do not use the decomposition to assign causal effects of cognitive ability or education. However, given comparable measures of ability and education across the two cohorts, the decomposition allows us to investigate whether the roles played by these measures of ability and education have changed over time.

Consistent with the recent literature (Chetty et al. 2014; Lee and Solon 2009), we find that aggregate measures of the transmission of economic status have not changed over time in the United States. However, by decomposing the aggregate effect, we find that ability plays a significantly smaller role in the more recent cohort and education plays a significantly larger role. In other words, while the "aggregate" estimate of the transmission of status has not changed over time, we find that *how* status is transmitted has changed. Auxiliary analysis suggests that the diminished role of ability is due primarily to a decrease in the returns to ability; moreover, the diminished role of ability exists both conditionally and unconditionally on educational attainment. The increased role of education is driven primarily by an increase in the relationship (i.e., correlation) between parental status and child's educational attainment; similarly, this increase in correlation persists even when conditioned on child's ability. In other words, the overall correlation between parental status and education seems to have increased over time and the increase is independent of any role of ability. A replication of the analysis by gender reveals similar qualitative trends for education but with women exhibiting larger increases. The diminished role of ability appears to be driven by large declines for men. We also find that the portion of

1. Although it is well known that non-cognitive skills play an important role in explaining economic status (Heckman, Pinto, and Savelyev 2013), data limitations prohibit us from investigating their possible changing roles.

2. For recent summaries of the intergenerational mobility literature, see Björklund and Jäntti (2009), Black and Devereux (2011), and Jäntti and Jenkins (2015).

3. Mayer and Lopoo (2008) note several challenges in empirically estimating this relationship.

the transmission that can be explained jointly by ability and education is substantially less for men than women in the 1997 cohort.

II. METHODS

A. Measuring Mobility

Economic mobility is generally modeled through the (inversely) related transmission of economic status. The theoretical model for the transmission of economic status is based on the premise that parental investment in a child's human capital is a determinant of the child's future wages (Becker and Tomes 1979, 1986).[4] The common empirical approach is to regress the log income of a child $\left[\ln\left(Y_i^{c}\right)\right]$ onto log parental income $\left[\ln\left(Y_i^{p}\right)\right]$, or:

$$(1)\qquad \ln\left(Y_i^{c}\right) = \alpha + \beta \ln\left(Y_i^{p}\right) + \varepsilon_i.$$

The value β is the IGE and $(1-\beta)$ is a measure of intergenerational economic mobility. To accurately estimate the IGE, income measures for both children and parents should reflect their permanent lifetime income status.

At a more fundamental level, empirical estimation of the transmission of economic status across generations is analogous to characterizing the joint distribution of parental and child permanent lifetime income. A joint distribution can be decomposed into its copula (the joint distribution where each marginal has been converted to a uniform distribution, e.g., income ranking), which determines the dependence structure, and its component marginal distributions (Sklar 1959). In the approach outlined above, the IGE (β) inherently combines characteristics of the copula and the shapes of the two marginal distributions of incomes. In other words, the IGE estimate is a mix of the transmission of ranking and changes in the marginal distributions of income between generations. Therefore, while the log income specification provides a useful empirical approach, it is hindered by the fact that changes in the marginals between generations can affect estimates of the IGE.[5] Given that our goal is to understand how the intergenerational relationship may have changed over time, and acknowledging that changes in inequality over time are well-documented (i.e., changes in the marginal distributions), our main results will come from an alternative specification that evaluates the correlation between child and parent income ranks (Chetty et al. 2014). Income rank measures are also less susceptible to life-cycle biases (Nybom and Stuhler 2015), a topic that we take up in greater detail in Section III.B. We will, however, report IGE estimates for comparison purposes and, when applicable, discuss why results differ between the two specifications.

Empirically, the correlation of ranks (ρ) is estimated from a regression of the child's percentile rank (R_i^{c}) on his/her parents' rank (R_i^{p}), or:

$$(2)\qquad R_i^{c} = \gamma + \rho R_i^{p} + \upsilon_i,$$

where rankings reflect permanent lifetime earnings rank. The rank–rank specification allows us to evaluate how the factors explaining the transmission of status have changed over time without entangling what the transmission of status implies for eventual income (a matter of intragenerational distribution).

B. Decomposition

Our goal is to separately identify the portion of the transmission of economic status explained by ability and education. We begin by deriving the portion explained jointly by ability and education. Consider the following two regressions:

$$(3)\qquad R_i^{c} = \gamma_b + \rho_b R_i^{p} + \sum_{j=1}^{K} \beta_b^{j} Z_i^{j} + \varepsilon_i$$

$$(4)\quad R_i^{c} = \gamma_f + \rho_f R_i^{p} + \sum_{j=1}^{K} \beta_f^{j} Z_i^{j} + \sum_{l=1}^{M} \theta^{l} W_i^{l} + \upsilon_i$$

where the Z_is are control variables (e.g., age and parental age) and the W_is are education and ability measures. Equation (3) will be referred to as the "base specification," while Equation (4) will be referred to as the "full specification." The portion of the transmission of economic status explained jointly by ability and education is simply the change in the coefficient on parental status between the full and base specifications or $(\rho_b - \rho_f)$.[6]

4. This is the predominant underlying model in most economic analysis, but it is certainly not the only one. See Goldberger (1989) or Mulligan (1999) for alternative discussions.

5. In an attempt to purge away such changes in the marginals one could report intergenerational correlation of income $\left(r = \beta\left(SD_{\ln(Y^p)}/SD_{\ln(Y^c)}\right)\right)$. This post-estimation scaling, however, does not allow the decomposition method to be a natural part of the estimation process.

6. Such "accounting" methods are common in decomposing effects in the economics literature (e.g., Blanden, Gregg, and Macmillan 2007; Hellerstein and Neumark 2008; Krueger 1993). Gelbach (2016) provides an extensive list in motivating his decomposition method.

To separately identify the portion of $(\rho_b - \rho_f)$ attributable to our factors, we use a decomposition presented by Gelbach (2016). The decomposition is based on the well-known omitted variable bias formula for least squares regression analysis. Specifically, letting Z be the full matrix of control variables, W be the full matrix of education and ability measures, 1 be a vector of ones (i.e., the intercept), and $X \equiv [1 \quad R^p \quad Z]$ then:

$$(5) \qquad \begin{pmatrix} \widehat{\gamma}_b \\ \widehat{\rho}_b \\ \widehat{\beta}_b \end{pmatrix} - \begin{pmatrix} \widehat{\gamma}_f \\ \widehat{\rho}_f \\ \widehat{\beta}_f \end{pmatrix} = \left(X'X\right)^{-1} X'W\widehat{\theta}.$$

Furthermore, if we let W^l be the lth covariate in W, $\widehat{\Gamma}^l = \left(X'X\right)^{-1} X'W^l$ be the OLS coefficients on X from a regression of W^l on X, and $\widehat{\theta}^l$ be the coefficient on W^l in the full specification, then the portion of the change between the base and full specification because of the lth covariate is $\widehat{\Gamma}^l\widehat{\theta}^l$. This decomposition method can be extended to "groups" of covariates, such as a vector of educational attainment dummy variables (Gelbach 2016).[7]

The use of a decomposition method over a sequential accounting approach warrants a brief discussion. A sequential accounting approach would first account for the change in the correlation explained by one factor (e.g., ability) and then add the other factor (e.g., education). The problem with a sequential accounting approach is order dependence. For example, if we first account for the change in the correlation explained by ability and then add education our results would differ from if we first account for education and then add ability. Gelbach (2016) provides a detailed example of how order dependence leads to different outcomes in the case of explaining the black-white wage gap with ability and education controls. On a more conceptual level, the above decomposition yields the "direct" roles played by ability and education in the transmission process.

To investigate how the roles of our key factors have changed between cohorts, we apply the decomposition method to a model that includes both cohorts (i.e., a cohort-covariate interacted model). Specifically, we estimate:

$$(6) \qquad R_i^c = \gamma_b + \alpha_b C_i + \rho_b R_i^p + \rho_b^c R_i^p \cdot C_i + \sum_{j=1}^{K} \beta_b^j Z_i^j + \sum_{j=1}^{k} \beta_b^{jc} Z_i^j C_i + \varepsilon_i$$

$$(7) \qquad R_i^c = \gamma_f + \alpha_f C_i + \rho_f R_i^p + \rho_f^c R_i^p \cdot C_i + \sum_{j=1}^{K} \beta_b^j Z_i^j + \sum_{j=1}^{k} \beta_b^{jc} Z_i^j C_i + \sum_{l=1}^{M} \theta^l W_i^l + \sum_{l=1}^{M} \theta^{lc} W_i^l C_i + \upsilon_i.$$

where C_i is an indicator variable that takes the value of 1 if individual i is in the 1997 cohort.[8] The focus for the 1979 cohort is the decomposition of $(p_{\mathrm{b}} - p_{\mathrm{f}})$ while the focus for the 1997 cohort is the decomposition of $\left[\left(p_{\mathrm{b}} + p_{\mathrm{b}}^{\mathrm{c}}\right) - \left(p_{\mathrm{f}} + p_{\mathrm{f}}^{\mathrm{c}}\right)\right]$.

Before we discuss the data, a brief caveat is in order for appropriate interpretation of our results. In an ideal setting, our results would be based on clearly identified structural models and our estimates would represent causal parameters. Although much work is being done to identify causal links in intergenerational mobility (e.g., Bowles and Nelson 1974; Cardak, Johnston, and Martin 2013; Lefgren, Lindquist, and Sims 2012; Liu and Zeng 2009; Sacerdote 2002; Shea 2000), modifying such analysis to the current setting faces several empirical issues (Blanden et al. 2014).[9] Our objective is to understand potential changes over time in the portion of the transmission explained by specific covariates. Therefore, while we do not use the decomposition to assign causal effects of cognitive ability or education, our accounting method allows us to

7. Our approach is similar to, but differs from, that of Blanden, Gregg, and Macmillan (2007) and Blanden et al. (2014). At the most basic level, our approach multiplies parameters (on say AFQT) from a full earnings equation (including parental status) times the parameter on parental status from a model explaining AFQT which includes all control variables. The approach used by Blanden, Gregg, and Macmillan (2007) and Blanden et al. (2004) does not include parental status in the first equation and does not include all controls in the second. As a result, our accounting method sums to the difference between the estimated parameter on parental status in our full and base models while theirs would not.

8. The base and full specifications include interaction terms between cohort and select variables in Z. In Equations (6) and (7), these variables are denoted as the first k variables in Z.

9. Consider, for example, the potential endogeneity of education. First, data challenges exist in identifying valid instrumental variables for both generations. Second, even if valid instrumental variables were available, they would only identify causal effects for specific sub-populations; there is no reason to suspect these would be the same for both generations or that the parameters from our auxiliary regressions would align with those populations to give meaningful results.

recover parameters to analyze whether the roles played by our measures of ability and education have changed over time.

III. DATA AND MEASUREMENT

A. Data

The data used in this analysis come from the 1979 and 1997 NLSY. The 1979 NLSY is a panel survey of youths aged 14–22 in 1979. It includes a cross-sectional representative survey ($n = 6{,}111$), an over sample of minorities and poor whites ($n = 5{,}295$), and a sample of military respondents ($n = 1{,}280$). The 1997 NLSY is a survey of youths aged 12–18 in 1997. It includes a cross-sectional representative survey ($n = 6{,}748$) and an over sample of minorities ($n = 2{,}236$). We use the cross-sectional representative survey and over sample of minorities for both the 1979 and 1997 cohorts[10]; we exclude the over sample of the military and poor whites from the 1979 cohort which were discontinued in 1984 and 1990, respectively.

The sample is limited to individuals who reported living with a parent for the first 3 years of the survey and with reported parental income for those years.[11] A key variable of interest is parental status based on this 3-year average.[12] The outcome of interest is the individual's economic status based on their most recent reported wage and salary income between 1988 and 1991 for the 1979 cohort and between 2009 and 2012 for the 1997 cohort with all incomes deflated to 1982–1984 dollars using the consumer price index.[13] These years for individuals' incomes were selected since the 2013 survey is the latest available wave for the 1997 cohort and 1988–1991 are the years of the 1979 cohort which most closely align in age with these data for the 1997 cohort (the mean age is 29.1 for our sample of the 1979 cohort and 29.0 for our sample of the 1997 cohort). The sample is further limited to individuals not enrolled in school over the period of interest, aged 26–32,[14] and with available AFQT scores. With these restrictions, the final 1979 cohort sample includes individuals born between 1960 and 1965 with a median birth year of 1963. The final 1997 cohort sample includes birth years 1980–1983 with a median birth year of 1983. Summary statistics for the two cohorts are reported in Table 1.

The measure of ability used in our analysis is test scores from the AFQT. An important issue when selecting an ability measure is

TABLE 1
Summary Statistics

Variable	1979 Cohort	1997 Cohort
Parental income	29,467	36,382
	(17,334)	(27,438)
Child's income	17,022	16,197
	(12,314)	(13,670)
H.S. diploma	0.52	0.40
Two-year degree	0.09	0.10
Four-year degree	0.20	0.29
Master's degree or higher	0.05	0.11
AFQT	163.2	170.3
	(30.6)	(30.3)
Age	29.1	29.0
	(1.67)	(1.47)
Parental age	45.8	43.1
	(6.22)	(5.27)
Black	0.24	0.18
Hispanic	0.17	0.16
Male	0.57	0.53
Two bio-parent home	0.78	0.64
Number of siblings	2.96	1.32
	(1.51)	(1.02)
N	2,098	1,754

Note: Standard errors are in parentheses. Incomes measured in 1982–1984 dollars.

10. Our general findings hold when analyzed on the cross-sectional representative sample only.

11. For the 1979 survey, parental income is identified through a comparison of total household income and respondent's income. For the 1997 survey, parental income is identified using total parental reported income. We exclude individuals who lived with a spouse or child during these years.

12. It is well known that parental transitory income shocks can lead to significant downward bias in measurements of intergenerational mobility (Mazumder 2005). However, as long as this bias is relatively stable over the cohorts, this should not cause significant distortions in the estimated changes in mobility over time. Moreover, related studies have settled on 3-year averages as a compromise between better measurement of parental income and sample size (e.g., Lee and Solon 2009; Mayer and Lopoo 2008; Mazumder 2014).

13. Income rank is calculated "within sample." Results are nearly identical when rank is calculated using U.S. Census data. The U.S. Census and NLSY data impose different top-coding practices, and therefore we would have to make arbitrary decisions about how to deal with these differences in calculating ranks. Thus, since results are essentially unaffected, we report our results based on the within sample ranking measure.

14. The oldest individuals in the 1997 cohort were 32 for the last year of reported income. Chetty et al. (2014) show that by age 30 the parental-child percentile rank correlation stabilizes, and that there is minimal deviation by age 26. However, while results from Nybom and Stuhler (2015) indicate that the rank specification is less susceptible to life-cycle bias than the log income specification, life-cycle bias may still be a concern. We discuss this issue in greater detail in Section III.B.

comparability across cohorts. The AFQT scores were constructed from the Armed Services Vocational Aptitude Battery (ASVAB), which was administered to both the 1979 and 1997 cohorts. The two cohorts took different versions of the ASVAB and therefore the original AFQT scores are not directly comparable. The 1997 cohort took a computer administered test (CAT) while the 1979 cohort took a paper and pencil (P&P) version. In addition, the test was administered at different ages for the two cohorts. The AFQT scores used here were made comparable across cohorts through a two-step process detailed by Altonji, Bharadwaj and Lange (2010). First, a mapping from the P&P version to the CAT version is used to make the raw scores equivalent. This mapping is constructed by Segall (1997) and based on a sample of individuals randomly assigned the P&P or CAT version between 1988 and 1992. Second, an equi-percentile mapping is used across age groups to create age-consistent scores (Altonji, Bharadwaj, and Lange 2010). The equi-percentile mapping puts both cohorts into cohort-specific 16-year-old score distributions (age 16 is the age group with the greatest overlap between the two cohorts).[15] We then rescale the AFQT scores by subtracting the mean and dividing by the standard deviation to ease interpretability of results.

While the constructed AFQT scores provide a comparable measure across cohorts it is worth discussing what AFQT scores actually measure. Some argue AFQT scores are proxies for intelligence quotient (IQ) scores whereas others draw serious doubts to this interpretation (Ashenfelter and Rouse 2000). Although it may be appealing to interpret AFQT scores as a measure of IQ, it is also not entirely clear what IQ scores measure. For example, there have been large gains in IQ scores over time in nearly every country on record (Flynn 2004). Flynn (2004) argues that these differences are too large to uncautiously equate IQ with "intelligence." Therefore, we interpret AFQT scores as some combination of innate ability and accumulated human capital as a youth that is valued in the labor market. However, for ease of expression, we will refer to AFQT scores as our measure of "ability."

Education attainment is measured using a set of indicator variables: less than a high school diploma or general educational development (omitted indicator variable), high school diploma, associates degree, college degree, and master's degree or higher. The endogenous nature of education in explaining economic status is well documented and we acknowledge that our measures of education likely incorporate unobserved characteristics. However, similar to Blanden, Gregg, and Macmillan (2007) and Bowles and Gintis (2002), while we do not attempt to control for this endogeneity, we also do not attempt to assign causal interpretations. Rather, our objective is to investigate potential changes in the roles played by these measures of education (whatever they encompass but consistently measured across cohorts) over time. In other words, our focus is on whether the roles have changed, not necessarily clearly defining the causal aspect of these roles.

We use two indicator variables for race—Hispanic and black. Controls for household structure include an indicator variable for if the child lived with two biological parents (at age 14 for the 1979 NLSY and during the first round of the survey for the 1997 NLSY), number of siblings (reported siblings for the 1979 cohort and number of household members under 18 for the 1997 cohort), and number of siblings squared. Other control variables include a cohort indicator (equal to one for the 1997 cohort), sex, age of parents (calculated as the average age of parents in the household in the first three years of the survey), age of parents squared, respondent's age, and age squared. Our cohort indicator is interacted with sex, race, education, ability, and parental economic status.

B. *Life-cycle Bias*

One concern when evaluating lifetime earnings outcomes in young adults is the potential for life-cycle bias.[16] Given heterogeneous wage profiles and the tendency for those with the highest lifetime earnings to have the steepest wage profile in early adulthood, regressions based on young adult earnings may downward bias estimates.[17] Furthermore, as we are comparing earnings outcomes over time, changes in life-cycle bias

15. We are grateful to Altonji, Bharadwaj, and Lange for making the constructed scores publicly available on Fabian Lange's website http://www.econ.yale.edu/fl88/.

16. See Haider and Solon (2006) for a theoretical discussion as well as an application to U.S. data, Böhlmark and Lindquist (2006) for a replication of these results with Swedish data, Chetty et al. (2014) for related discussion, and Nybom and Stuhler (2015) for a generalization of these results to multiple dependence measures.

17. Life-cycle bias could exist even though children's earnings are the dependent variable; this bias differs from classical measurement issues in right-hand-side variables that lead to attenuation.

across cohorts could also potentially cloud our results.[18]

One way to limit potential life-cycle bias is to use incomes on individuals in their mid-30s. The life-cycle bias literature consistently finds that mid-30s is when life-cycle bias is at its smallest, and in some cases, negligible (Böhlmark and Lindquist 2006; Chetty et al. 2014; Haider and Solon 2006; Nybom and Stuhler 2015). The oldest individuals in the 1997 cohort are 32. Therefore, an averaged earnings measure would use earnings at ages identified as more susceptible to life-cycle bias than a single income observation based on the most recently reported earnings (i.e., earnings at oldest age available). However, with potential stochastic earnings shocks, a single income observation may have more classical measurement error (i.e., higher variance in estimates) than an averaged earnings measure. This then implies a bias/variance tradeoff between using a single (most recent) income observation and an averaged earnings measure. Given findings in the life-cycle bias literature, we chose to minimize life-cycle bias and use the most recently reported earnings. However, we reestimated the decomposition using an averaged income measure to evaluate the robustness of our findings. Our main results were unchanged. Furthermore, to test the robustness of our results to the age at which incomes were reported, we reevaluated the decomposition using the oldest half of the data set (i.e., respondents age 29 and up). Again, our general findings held. While this does not mean life-cycle biases are absent or unimportant, we believe these results suggest that our main findings are not unduly influenced by such biases.[19]

18. Böhlmark and Lindquist (2006) find that the pattern of life-cycle bias is fairly constant over time for young Swedish men born between 1929 and 1950. While they find large shifts in the pattern for women, they note this finding is likely due to the influx of women entering the workforce in Sweden over that period.

19. Nybom and Stuhler (2015) suggest a procedure to reduce attenuation bias in rank correlations. Although the correction is not specifically intended to correct for life-cycle bias but rather classical measurement error in rankings, simulations suggest that it may be instructive nonetheless. The bias correction is essentially equal across our two cohorts, and therefore, does not affect our general findings about the changing roles of education and ability. However, the correction does suggest that the magnitudes of our main rank–rank decomposition results may be downwardly biased by 10–15%.

TABLE 2
Regression Results

	Percentile Rank		Log Income	
	Base	**Full**	**Base**	**Full**
Parental status	0.265***	0.151***	0.322***	0.186***
	(0.022)	(0.022)	(0.038)	(0.037)
Cohort × parental status	−0.012	−0.022	−0.035	−0.053
	(0.033)	(0.034)	(0.054)	(0.054)
AFQT		0.054***		0.165***
		(0.008)		(0.032)
Cohort × AFQT score		−0.031***		−0.041
		(0.011)		(0.048)
H.S. diploma		0.073***		0.291***
		(0.017)		(0.069)
Cohort × H.S. diploma		0.025		0.077
		(0.025)		(0.112)
Two-year degree		0.147***		0.516***
		(0.024)		(0.085)
Cohort × 2-year degree		−0.002		0.042
		(0.037)		(0.131)
College degree		0.215***		0.615***
		(0.022)		(0.081)
Cohort × college degree		0.017		0.127
		(0.032)		(0.125)
Master's degree		0.298***		0.860***
		(0.032)		(0.098)
Cohort × master's degree		0.017		0.085
		(0.043)		(0.140)

Notes: Dependent variable is child's economic status (percentile rank or log income). Robust standard errors for OLS estimates are in parentheses. All regressions control for cohort, parental age, parental age squared, presence of two biological parents, number of siblings, number of sibling squared, race, sex, age, age squared, and interactions between cohort and sex and race.

***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

IV. RESULTS

A. *Measures of Economic Mobility*

The first and third columns of Table 2 present measures of the transmission of economic status based on percentile rank and log income, respectively, using our base specification. When using the percentile rank measure, the estimated correlation between parental rank and children's rank is around 0.27, slightly smaller than recent estimates (e.g., Chetty et al. 2014). We find no change in the relation between parents' and children's rank over time (i.e., cohort × parental rank variable is not statistically significant). From the log income specification, the estimated IGE is 0.32 and the interaction term is not statistically significant. Our IGE estimate is somewhat lower than previous literature; for example, Mayer and Lopoo (2008) argue that the consensus is around 0.40.

The finding that the transmission of economic status has not change over time is not surprising. Two predictions from the traditional model of the transmission of economic status are: (1) an increase in the returns to human capital should increase the parental-child correlation of status and (2) an increase in the progressivity of government investment in human capital should decrease the correlation of status (Solon 2004). Given returns to human capital have increased (Autor 2014; Goldin and Katz 2007) and U.S. investment in human capital has become more progressive (e.g., standardization), it is not surprising that we find no change in the aggregate transmission of status over time. However, while "aggregate" estimates of the transmission of status are unchanged, we need to apply the decomposition to evaluate whether the underlying factors responsible for transmission of status (e.g., ability, education) have changed over time. We begin by evaluating the joint role played by ability and education on children's status.

B. The Roles of Ability and Education on Children's Status

The second and fourth columns of Table 2 present results based on percentile rank and log income, respectively, using our full specification (i.e., includes AFQT score, education measures, and cohort interaction terms). As expected, ability and education are highly correlated with status and the inclusion of these variables reduces the coefficient on parental status from 0.27 to 0.15 when using the rank measure and from 0.32 to 0.19 when using log income.

The AFQT score and all education measures have positive effects on both outcome measures. Furthermore, in the percentile rank model, the interaction of cohort and AFQT score is negative and statistically significant (Table 2, column 2). The resulting AFQT effect for the 1997 cohort is less than half the effect for the 1979 cohort. Conditional on parental status and education, scoring one standard deviation higher on the AFQT corresponds to an average increase in income rank of about 5.4 percentiles for the 1979 cohort. A similar improvement for the 1997 cohort corresponds to an average increase in income rank of only about 2.3 percentiles. This decreased role of ability is consistent with recent findings that returns to cognitive ability have diminished over time (Castex and Dechter 2014).[20] Most of the education–cohort interaction terms are positive in both models, but none are significant; this result is somewhat unexpected given previous research suggesting

TABLE 3

Decomposition Results

		1979 Cohort	1997 Cohort	Difference (1997–1979)
Percentile rank	AFQT	0.046***	0.022***	−0.024**
		(0.008)	(0.008)	(0.012)
	Education	0.068***	0.102***	0.034**
		(0.009)	(0.011)	(0.015)
	Combined	0.115***	0.124***	0.010
		(0.011)	(0.012)	(0.016)
	Total	0.265***	0.253***	−0.012
		(0.022)	(0.024)	(0.033)
Log income	AFQT	0.063***	0.046***	−0.017
		(0.014)	(0.014)	(0.020)
	Education	0.073***	0.109***	0.035*
		(0.012)	(0.014)	(0.019)
	Combined	0.136***	0.154***	0.018
		(0.016)	(0.018)	(0.024)
	Total	0.322***	0.287***	−0.035
		(0.038)	(0.033)	(0.054)

Notes: The effect of education represents the effect from all measures of education. Bootstrapped standard errors (based on 1,000 replications) are reported in parentheses.

***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

increasing returns to education (Autor 2014; Castex and Dechter 2014; Oreopoulos and Petronijevic 2013). We speculate this may be because of the relatively young age at which we measure earnings.

C. Decomposition—Roles of Ability and Education

The top and bottom portions of Table 3 report decomposition results for the percentile rank and log income measures, respectively. In each section, the first row reports the portion explained by ability alone (AFQT), the second row reports the portion explained by education alone, the third row reports the portion explained jointly by ability and education, and the fourth row reports the aggregate transmission of status estimate (i.e., coefficient(s) from the base specification in Table 2). Columns report the decomposition results for the 1979 cohort, 1997 cohort, and difference between the two cohorts (1997–1979), respectively.

For the percentile rank measure, ability accounts for about 0.05 points (17%) of the intergenerational transmission for the 1979 cohort and 0.02 points (9%) for the 1997 cohort. This decrease in the role of ability over time is statistically significant. Conversely, education plays a statistically significant larger role in the 1997 cohort by explaining 0.10 points (40%) compared to 0.07 points (26%) for the 1979 cohort. Together, the decreased role of ability

20. Standard mincer equations reconfirm these findings for our data.

and increased role of education offset each other such that there is no significant change in the combined role of ability and education over time (Table 3, row 3). Ability and education together explain about 0.12 points of the transmission for both cohorts.

Similar patterns emerge when log income (i.e., IGE) is the measure of interest, but the drop in the role of AFQT across cohorts is not statistically significant. The portion explained by ability is about 0.06 log points (20%) for the 1979 cohort versus 0.05 log points (16%) for the 1997 cohort. The portion explained by education is 0.07 log points (23%) for the 1979 cohort and 0.11 log points (38%) for the 1997 cohort, a difference that is statistically significant. As with the results from the percentile rank measure, the portion explained jointly by ability and education does not differ across cohorts (0.14 points and 0.15 points).

Our estimates of the portion explained jointly by education and ability are fairly similar to, although somewhat lower than, previous research. Bowles and Gintis (2002) find that the combined effect of inherited cognitive skills and education attainment explains at most three-fifths. Mulligan (1999), although not specifically decomposing the transmission mechanism, finds AFQT score and various educational measures account for a little over half of the estimated income transmission. Our finding that education plays a larger role over time in the United States is consistent with previous analysis in the United Kingdom. Blanden, Gregg, and Macmillan (2007) found an increase in the role of education in the transmission of economic status in the United Kingdom between 1956 and 1970.[21] They also find evidence of a slight decline in the role of ability over time, but the magnitude of the decline for the United Kingdom was considerably lower than the decline identified here for the United States.

D. Auxiliary Analysis

Our decomposition results suggest that the roles of ability and education in the transmission of economic status have changed over time in the United States; for the most recent cohort, ability plays a diminished role while education plays a substantially larger role. Here, we take a closer look at the pathways through which these changes may have occurred. For brevity, we focus on our preferred specification with percentile income rank as the outcome measure.[22]

The estimated change in the portion of the transmission explained by each factor (ability, education) can occur through two pathways: (1) a change in the effect of the factor of interest on the child's percentile income rank and/or (2) a change in the relationship between parental income rank and the factor of interest. Regression results in Table 2 (column 2) provide insight into the first pathway. A negative and significant coefficient on the cohort–AFQT interaction term suggests a change over time in the effect of ability on income rank; that is, the decrease in the role of ability over time is at least partially driven by the first pathway. For education variables, all cohort interaction terms, while in general positive, are insignificant. In other words, we do not find strong evidence that the increased role of education came through the first pathway. To gain insight into the second pathway, we estimate auxiliary regressions. We regress our factors of interest (AFQT, education) onto parental income rank and all controls included in X. Auxiliary regression results are provided in the top panel of Table 4.

Column 1 in the top panel reports the relationship between parental income rank and ability (AFQT).[23] Parental income rank is positively correlated with children's measured ability. However, we fail to find evidence of a change in this correlation over time (i.e., second pathway). Therefore, our finding that ability explains a smaller portion of transmission over time appears to be driven primarily by the first pathway—lower returns in the labor market for measured ability.

Column 2 in the top panel of Table 4 reports the relationship between parental income rank and education. For ease of interpretation, we focus on results from a regression on a single measure of years of schooling[24]; the bottom panel

21. However, in percentage terms, education does not explain a larger portion in their study because the parent–child correlation in their sample has increased substantially.

22. Auxiliary analysis for the log income model is available upon request.

23. Note that decomposition results reported in Table 3 are derived from the coefficients in the first column of Table 4 and the coefficients in column 2 of Table 2. For example, the portion explained by AFQT in 1979 (0.046) is the product of the coefficient on parental rank in column 1 of Table 4 (0.835) and the coefficient on AFQT in column 2 of Table 2 (0.054); minor discrepancies are due to rounding.

24. As a result, the decomposition reported in Table 3 does not match the product of respective coefficients as in the ability analysis.

TABLE 4
Auxiliary Results

		Outcome of Interest			
	Dependent Variable	**AFQT**	**Education**	**Child's Status**	**Education**
General education	Parental Status	0.835*** (0.068)	2.035*** (0.193)	0.183*** (0.023)	0.891*** (0.177)
	Cohort* Parental status	0.107 (0.097)	1.067*** (0.273)	0.012 (0.033)	0.994*** (0.252)
	AFQT Score	—	—	0.097*** (0.007)	1.367*** (0.055)
	Cohort* AFQT score	—	—	−0.035*** (0.010)	−0.074 (0.080)
		Diploma	**Associates**	**Bachelors**	**Masters**
Detailed education	Parental Status	−0.168*** (0.041)	0.005 (0.024)	0.265*** (0.035)	0.076*** (0.022)
	Cohort* Parental status	−0.115** (0.058)	−0.065* (0.035)	0.045 (0.059)	0.134*** (0.031)

Notes: Robust standard errors are in parentheses. All regressions control for cohort, parental age, parental age squared, presence of two biological parents, number of siblings, number of sibling squared, race, sex, age, age squared, and interactions between cohort and sex and race.

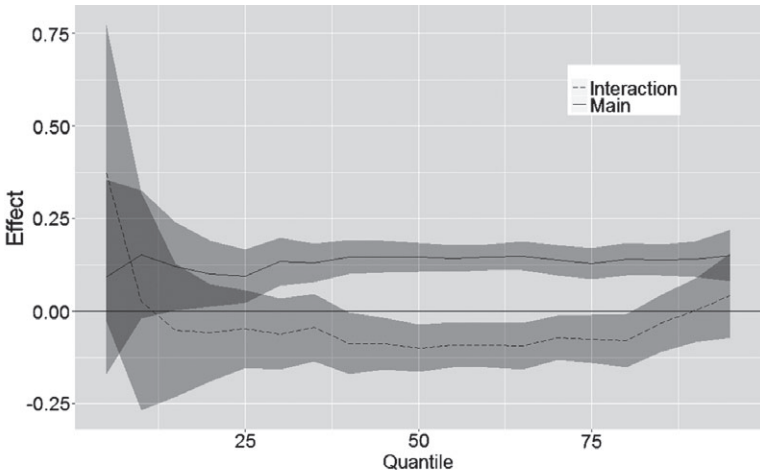
***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

of Table 4 provides degree-specific regressions for those interested. Similar to the ability regression, parental income rank is positively correlated with children's educational attainment. This relationship, however, appears to be increasing over time. The coefficient on the interaction term is positive, suggesting that parental status has a larger effect on education in the 1997 cohort (a 52% increase). Together with the lack of evidence for the first pathway, these results suggest that the increase in the portion explained by education is due primarily to the change in the relationship between parental status and educational attainment (i.e., second pathway).

Our main decomposition evaluates the independent roles of education and ability in the transmission of status. Given the large drop in the role played by ability, one might wonder if this drop is simply because of an increased dependence between ability and education. In other words, has the role of ability actually decreased over time or has its role simply been subsumed into education's role? The answer to the latter question is no. We show this by decomposing the "full" role played by ability when education controls are excluded. Column 3 (top panel of Table 4) reports estimates from the full specification model without education measures. In the 1979 cohort, the full role of ability (i.e., not controlling for education) is the coefficient on AFQT in column 3 (0.10) multiplied by the coefficient on parental percentile rank in column 1 (0.84). Similarly, the full role of ability in the 1997 cohort is the sum of the AFQT and cohort–AFQT coefficients in column 3 (0.10−0.04) times the sum of the parental rank and cohort-parental rank coefficients in column 1 (0.84−0.11). As a result, the full effect of ability accounts for about 0.08 points (30%) of the transmission of status in the 1979 cohort compared to 0.06 points (23%) in the 1997 cohort. Therefore, even unconditional on education, ability plays a smaller role. This drop (in absolute terms) is quite different from the findings by Blanden, Gregg, and Macmillan (2007) for the United Kingdom—the full effect of ability accounted for nearly identical portions across their two United Kingdom cohorts.

Auxiliary analysis above suggested that the increased role of education in the transmission of status was because of an increase in the correlation between parental status and educational attainment. This too could be from multiple sources: (1) an increase in the direct relationship between parental status and education (conditional on AFQT), (2) an indirect effect through an increase in the link between parental status and AFQT, or (3) an indirect effect through an increase in the link between AFQT and education (conditional on parental status). Column 4 (top panel of Table 4) evaluates the first source. The positive and significant coefficient on the cohort-parental status interaction term (0.99)

FIGURE 1
Quantile Regression Results for AFQT and Cohort Interaction



Notes: This figure depicts the direct AFQT effect and the AFQT–cohort interaction effect for quantile regressions that parallel our full specification model for describing log incomes (see Table 2, column 4 for mean results). Shaded regions represent 95% confidence intervals.

indicates an increase in the direct relationship between parental status and education (conditional on AFQT) over time. Notably there has been more than a 100% increase in this correlation between the two cohorts (0.89 in 1979 to 1.89 in 1997). The insignificant coefficient on the cohort-parental status interaction term in column 1 provides little evidence for the second source. Similarly, the insignificant coefficient on the cohort–AFQT interaction term in column 4 provides little evidence for the third source. Therefore, our results suggest that the increase in correlation between parental status and education over time is primarily because of an increase in the direct relationship between parental status and education (i.e., independent of ability). In other words, the relative importance of parental status to ability in determining educational outcomes is higher for the 1997 cohort. This finding of a decline in the relative importance of ability in determining U.S. educational outcomes is consistent with recent findings by Galindo-Rueda and Vignoles (2005) for Britain.

E. Rank-Log Differences

Decomposition results for both the percentile rank and log income specifications identify an increased role of education in the transmission of status for the most recent cohort. However, while both the percentile rank and log income specifications identify a drop in the role of AFQT across cohorts, this finding is only significant in the percentile rank specification. To provide insight into this difference, we evaluate quantile regressions that parallel the full specification model for log income reported in Table 2, column 4. Figure 1 depicts the estimated AFQT and AFQT–cohort interaction effects from this model across quantiles.[25]

Consistent with the percentile rank findings, the role of AFQT is fairly constant across quantiles for the 1979 cohort and substantially lower in the 15th–80th quantiles for the 1997 cohort. However, the story is quite different in the tails; the AFQT effect is higher for the 1997 cohort in the tails.[26] The positive cohort effects in the tails counteract the decreased role of AFQT on the mean, resulting in an insignificant cohort interaction term in the log income specification. The increases in log income in the tails correspond to small changes in ranking, and thus, do not counteract the percentile rank specification to the same degree.

25. Recall that the diminished role of AFQT in the percentile rank model was driven by lower returns in the labor market for measured ability (i.e., diminished effect on child's status).

26. We caution the reader from over-interpreting the point effects on the tails in Figure 1. As indicated by the (shaded) 95% confidence intervals, the tails are not precisely measured.

F. Results by Gender

Labor market performances and educational trends of men and women have evolved differently over the past 20 years (Autor 2014). Furthermore, changes in traditional gender roles over time may lead to different trends in intergenerational mobility for sons and daughters (Mayer and Lopoo 2004). To evaluate whether such differences and changes affect the role of ability and education in the transmission of status, we reexamine the data by gender. We will refer to the previously reported results (men and women combined) as the "main results," and focus on our preferred specification with percentile income rank as the measure of status.

Base and full regression results for men and women are provided in Table 5. The rank–rank correlations for men and women in the base specification are 0.29 and 0.23, respectively, and neither men nor women show a change in measured economic mobility over time (i.e., insignificant interaction term). However, the point effects of these interaction terms are of some interest as the negative effect for men (−0.05) and positive effect for women (0.04) counteract to close the point gap in the main baseline model (−0.01; Table 2).

The full specification results illustrate the joint importance of ability and education in the transmission of economic status. The rank–rank correlations are reduced from 0.29 to 0.16 for men and from 0.23 to 0.14 for women; these reductions are similar to the main results (0.27 to 0.15). Also consistent with the main results, the AFQT score and all education measures have positive effects; the only insignificant effect is a high school diploma for women. The interaction of cohort and AFQT score is negative for both men and women but only significant for men. Interestingly, the AFQT–cohort interaction effect for men (−0.05) essentially offsets the direct AFQT effect (0.05). Similar to the main results, most of the education–cohort interaction terms are positive, but none are significant.

Decomposition results by gender are reported in Table 6. Over time, education and ability combined explain higher amounts of the transmission of status for women—0.09 points for the 1979 cohort and 0.15 points for the 1997 cohort. Conversely there appears to be a drop for the total amount explained by men (0.13 vs. 0.10), but these changes are only statistically significant for women. Also, the total portion explained for men is smaller than the total portion explained for women in the 1997 cohort (significant at the 5% level). So while men and women (in the 1997 cohort) appear to have similar degrees of status correlation with their parents, substantially less of this link can be explained by education or measured ability for men (39% for men vs. 56% for women).

TABLE 5
Regression Results by Gender

	Men Only		Women Only	
	Base	**Full**	**Base**	**Full**
Parental income rank	0.294***	0.163***	0.229***	0.140***
	(0.030)	(0.031)	(0.034)	(0.034)
Cohort × parental Income rank	−0.052	−0.017	0.037	−0.025
	(0.044)	(0.046)	(0.048)	(0.048)
AFQT		0.050***		0.066***
		(0.010)		(0.012)
Cohort × AFQT		−0.051***		−0.001
		(0.015)		(0.017)
H.S. diploma		0.087***		0.021
		(0.020)		(0.027)
Cohort × H.S. diploma		0.043		0.022
		(0.033)		(0.041)
Two-year degree		0.150***		0.100***
		(0.038)		(0.035)
Cohort × 2 year degree		0.003		0.017
		(0.054)		(0.052)
College degree		0.228***		0.164***
		(0.027)		(0.036)
Cohort × college degree		0.005		0.048
		(0.044)		(0.050)
Master's degree		0.249***		0.331***
		(0.043)		(0.045)
Cohort × master's degree		0.055		−0.045
		(0.060)		(0.060)

Notes: Dependent variable is child's percentile income rank. Robust standard errors for OLS estimates are in parentheses. All regressions control for cohort, parental age, parental age squared, presence of two biological parents, number of siblings, number of sibling squared, race, age, age squared, and interactions between cohort and race.

***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

Men exhibit a large drop in the role of ability over time (Table 6, row 1). Ability accounted for 0.05 points (17%) of the transmission of status in the 1979 cohort and explains essentially none of the transmission in the 1997 cohort; this reduction is significant at the 1% level. For women, the estimated change is sightly positive (0.04 vs. 0.05) but the difference is imprecisely measured. Therefore, it appears that the main results regarding ability are primarily driven by the change observed for men. Similarly, our main results identify a drop in the "full" effect of ability over time. Men show a significant drop from 0.09 to 0.03 points across cohorts (derived from Table 7, columns 1 and 3), but we find no such change in the full effect of ability for women.

On the other hand, women exhibit larger increases in the portion of the transmission explained by education (Table 6, row 2 in *Women*

TABLE 6
Decomposition Results by Gender

	Ind. Variable	1979 Cohort	1997 Cohort	Difference
Men only	AFQT Score	0.050***	−0.003	−0.053***
		(0.012)	(0.012)	(0.017)
	Education	0.081***	0.098***	0.018
		(0.013)	(0.016)	(0.021)
	Combined	0.131***	0.095***	−0.036
		(0.014)	(0.017)	(0.023)
	Total	0.294***	0.242***	−0.052
		(0.030)	(0.033)	(0.044)
Women only	AFQT Score	0.042***	0.053***	0.011
		(0.010)	(0.012)	(0.015)
	Education	0.046***	0.096***	0.050**
		(0.013)	(0.016)	(0.021)
	Combined	0.088***	0.150***	0.061**
		(0.016)	(0.019)	(0.025)
	Total	0.229***	0.266***	0.037
		(0.034)	(0.035)	(0.048)

Notes: Dependent variable is child's percentile income rank. The effect of education represents the effect from all measures of education. Bootstrapped standard errors (based on 1,000 replications) are reported in parentheses.

***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

only). The portion explained by education increases from 0.05 points (20%) in 1979 to 0.10 points (36%) in 1997; this increase is significant at the 5% level. For men, education explains 28% in the 1979 cohort and 40% in the 1997 cohort (Table 6, row 2), but this increase is not statistically significant. Auxiliary results for men and women (Table 7) reveal similar findings to the main results; the decrease in the role of ability in men is mainly driven by the decreased return to measured ability and the increased role of education for women is mainly driven by an increased correlation between parental status and educational attainment. Regarding the role of parental status on education (Table 7, column 2), we observe a significantly larger increase in the relationship over time for women than for men (78% increase for women; 40% increase for men); this difference underlies the relatively larger changes in the role of education for women observed in the main decomposition.

V. CONCLUSION

Using a data set of two cohorts separated by 20 years, we investigated the changing roles of ability and education in the transmission of economic status across generations in the United States. Two measures of economic status were considered—a percentile income rank and the more traditional log income. To identify the individual roles of ability and education and how these roles may have changed over time, we applied a decomposition method based on the OLS omitted variable bias formula.

Consistent with recent literature, we find that the correlation between parent and child economic status has not changed over time. We do, however, find that the roles of ability and education in this transmission have changed. Ability plays a substantially diminished role whereas education plays a substantially larger role for the most recent cohort. Further analysis suggests that the diminished role of ability can be attributed mostly to a reduced effect of ability on status. The increased role of education is mostly attributed to an increased effect of parental status on educational outcomes—in particular, its effect independent of any effect on ability. When we reanalyze the data by gender we find that the decreased role of ability is driven by men while the increased importance of education appears largest in women. We also find that while the overall measures of mobility for men and women are similar in the most recent cohorts, education and ability jointly explain a smaller portion of the transmission of economic status for men than for women.

A large body of existing literature has evaluated the correlation of status between generations, and in recent analysis, has found no change in the transmission of status over time in the United States. Our results confirm these recent findings but also provide a more detailed picture of what underlies this point estimate. While the "aggregate" estimate of the transmission of status has not changed over time, we find that *how* status is transmitted has changed. For example, for a child born in 1960, we find that 17% of the correlation between parental and children's income rank is explained by measured ability. For a child born in 1980, only about 9% of the correlation can be explained by ability. We speculate that the vast changes in education policies over the last 30 years and higher demand for higher skilled workers have both played a role in the changes documented here.

Although we are cautious to not over-interpret our findings, we believe that our findings have important normative implications regarding the assessment of economic mobility. First, with increasing inequality over time, changes in rank require larger jumps in income. Even with stable percentile rank mobility, increasing inequality may imply greater consequences of parental status on children's welfare. Second,

TABLE 7
Auxiliary Results by Gender

		Outcome of Interest			
	Dependent Variable	**AFQT**	**Education**	**Child's Status**	**Education**
Men only	Parental income	0.965***	2.393***	0.206***	1.117***
	Rank	(0.097)	(0.255)	(0.031)	(0.233)
	Cohort × parental	0.081	0.951***	0.005	0.908***
	Income rank	(0.138)	(0.365)	(0.045)	(0.336)
	AFQT	—	—	0.089***	1.320***
		—	—	(0.009)	(0.068)
	Cohort × AFQT	—	—	−0.058***	−0.058
		—	—	(0.013)	(0.100)
Women only	Parental income	0.638***	1.535***	0.158***	0.609**
	Rank	(0.095)	(0.294)	(0.033)	(0.270)
	Cohort × parental	0.183	1.198***	0.017	1.032***
	Income rank	(0.133)	(0.412)	(0.047)	(0.382)
	AFQT	—	—	0.111***	1.446***
		—	—	(0.012)	(0.096)
	Cohort × AFQT	—	—	−0.000	−0.117
		—	—	(0.016)	(0.133)

Notes: Robust standard errors are in parentheses. All regressions control for cohort, parental age, parental age squared, presence of two biological parents, number of siblings, number of sibling squared, race, age, age squared, and interactions between cohort and race.

***Statistically significant at the 1% level; **statistically significant at the 5% level; *statistically significant at the 10% level.

Autor (2014) notes that what might be of greater concern is *what* determines inequality and how this is passed on between generations. Constant aggregate measures of mobility over time might lead one to conclude that the United States is equally meritocratic today as in the past; our results suggest that such an interpretation might be misleading. Cognitive ability, a (rough) measure of merit, has a substantially diminished role in our most recent cohort. This diminished role appears to have been replaced by an increased role of education, and notably, through an increased relationship between parental incomes and educational attainment; in other words, we find an increase in the relative importance of parental status in determining U.S. educational outcomes. The diminished role of ability in the transmission of status is primarily driven by men and the increased role of education is primarily driven by women. Therefore, our findings that the underlying factors that explain intergenerational economic mobility have changed over time, and to differing degrees across genders, should caution researchers and policymakers from over-interpretation of aggregate mobility measures.

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