Is Education the Great Equalizer?

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Abstract

We investigate the role of education in equalizing differences in socio-economic status (SES) across groups determined by two at-birth "lotteries:" birthplace and genetics. Birthplace and genetics are strongly related to long-term SES and education is believed to be a way to overcome disadvantages on such initial endowments. We ask how the effects of a compulsory schooling law-induced increase in secondary education vary with the quality of an individual's birth neighborhood and their polygenic score for educational attainment. We use a regression discontinuity framework and a large sample that allows for well-powered estimates of such interactions. While the law change reduced differences in educational attainment across birthplace and genetic groups, it increased existing differences in middle age SES. In particular, the extra education benefited those with high genetic scores the most, doubling the gradient between the polygenic score and SES. Our findings suggest that compulsory schooling policies, while equalizing educational attainment, might have limited ability in reducing lifecycle SES inequalities by genetics and birthplace.

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"Education then, beyond all other devices of human origin, is a great equalizer of the conditions of men -- the balance wheel of the social machinery."

Horace Mann, pioneering American educator, 1848

"In America, education is still the great equalizer."

Arne Duncan, U.S. Secretary of Education, 2011

1. Introduction

Education is considered by many to be the “great equalizer” and education policy an important tool to fight poverty, reduce inequality and improve socio-economic mobility. In a world where initial endowments matter for later life economic success, education would be a way to “level the playing field” and to ensure that children from disadvantaged backgrounds have a fair shot at success. In this paper, using a change in compulsory schooling law as a natural experiment, we find just the opposite. Education exacerbates socio-economic differences across groups based on two proxies for endowments that are significantly related to later life outcomes and determined at birth: birthplace and genetics.

Birthplace and genetics are important predictors of long-term SES. In a series of influential papers, Chetty and co-authors find that outcomes such as earnings, college attendance, fertility and marriage patterns depend on the quality of the neighborhood in which children live (e.g. Chetty and Hendren 2018a and 2018b). Children living in counties with less concentrated poverty, less income inequality and better schools have better later-life outcomes, especially if they come from poor families. Genetics also play an important role in predicting SES. Lee et al. (2018) use newly developed methods in behavior genetics to conduct a large-scale genetic association analysis of educational attainment. The individual-level genetic index (generally called polygenic score) resulting from their analyses predict 11-13% of the variance in educational attainment and 7-10% of the variance in cognitive performance. Importantly, though perhaps unintuitively, this polygenic score reflects not only cognitive and non-cognitive skills, but also the community and household in which a child grows up and various other internal and external factors.

We ask whether education reinforces or equalizes the relationship between these initial endowments and SES. In particular, we ask how the effects of an increase in education vary with the quality of an individual's birth neighborhood and with their polygenic score for educational attainment. To deal with the endogeneity of education, we explore a well-known compulsory schooling age reform in the UK that generated as-good-as-random variation in education (Clark and Royer 2013; Barcellos, Carvalho and Turley 2019).

In 1972, England, Scotland, and Wales increased the minimum age at which students could drop out of school from 15 to 16 years. The reform affected only students born on or after September 1, 1957, generating a discontinuity in the relationship between education and date of birth. We explore such discontinuity to estimate the causal effect of secondary education on middle age SES, using a credible regression discontinuity design (RDD). We stratify the RDD analysis by predetermined birthplace and genetic groups and document how the effects of the education reform and the resulting extra education vary across birthplace and genetic groups.

Theoretically, it is not clear how the returns of such exogenous increase in education would vary with initial endowments. On the one hand, education might compensate for environmental and genetic disadvantages or there might be diminishing returns in the production of SES, suggesting that education might equalize SES differences across birthplace and genetic groups. On the other hand, returns to education might be higher in neighborhoods with more economic activity, or "smarter" people might benefit more from a given amount of education, suggesting that education might reinforce initial disadvantages. Therefore, the sign of these interactions are an empirical matter.

We use data from the UK Biobank, a study that collected measures of SES between 2006 and 2010; 34-38 years after the policy change. The data was collected using in-person interviews and self-administered questionnaires for more than half a million individuals ages 40 to 69. Moreover, the cohort was fully genotyped. The data includes exact date of birth and geographical coordinates of birth that we used to merge to local historical SES measures. In a verbal interview, UK Biobank respondents reported their occupation and answers were coded in 400+ detailed occupation categories. We use the 2009 Annual Survey of Hours and Earnings (ASHE) to match such categories to median wages at each occupation. This is our main measure of middle age SES in this paper. Our main analytic sample contains approximately 100 thousand people born in England, Scotland, and Wales between September 1, 1947 and August 31, 1967 who left full time education by age 18 and who are of classified as having "European ancestry."[2] The unprecedented availability of SES and genetic measures for such a large sample permits well-powered estimation of gene-by-education and birth environment-by-education interactions described here.

Consistent with the literature on the importance of initial endowments, we find that both the educational attainment polygenic score (EA PGS) and birth neighborhood quality are strongly associated with wages in mid-life. A one standard deviation (SD) in the PGS is associated with a 2.7% increase in wages and a one SD in birth neighborhood quality is associated with a 1.8% increase in wages, both relationships are highly significant (p-value<0.001). We also find that the compulsory schooling reform disproportionally increased the average education of those from less advantaged birthplaces and with lower PGS such that it reduced education differences across birthplace and genetic groups. For example, in our main analytic sample, the reform induced students in the bottom PGS tercile to stay in school for 0.38 extra years, among students in the top tercile the increase was only 0.28 years. Similar differences are seen across the distribution of neighborhood quality. Because the law change required students to stay in school until age 16, it disproportionally affected students from the bottom terciles of the PGS and neighborhood quality distributions, who were less likely to stay in school until age 16 before the reform. Therefore, the change in compulsory schooling had the effect of equalizing education across these groups.

Based on our reduced form estimates, the reform's largest benefits in terms of middle age SES are seen for those with high PGS, despite disproportionally increasing education among groups with low PGS. Wages in the top PGS tercile increased by 2.4%, the increase in the bottom PGS tercile was 1%. We see a similar positive interaction with birthplace quality, but the magnitude is smaller and the coefficients are not always significant. What is clear is that we see no evidence of the reform closing existing gaps in wages across birthplace and genetic groups.

Our two-stage least squares estimates indicate that the extra education induced by the reform more than doubled the gradient between the PGS and middle age wages among policy compliers. For individuals in the highest PGS tercile of the score distribution, one extra year of education

[2] As we explain below, the compulsory school reform we study did not affect the probability of staying in school past age 18, therefore we restrict our sample accordingly. Moreover, the polygenic score we use was developed using European samples and is not applicable to other ancestry groups, hence the European descent restriction.

induced by the reform increased wages by approximately 9-11%. In comparison, the returns to education among the bottom tercile were 3-5%. Our analyses suggest it is unlikely that differences in the characteristics of the marginal student populations affected by the reform across genetic groups explain this finding. Results are virtually identical in models with both genetic and neighborhood interactions.

Our findings question the usefulness of compulsory schooling policies as a tool to equalize the existing differences in SES across birthplace and genetic groups. If anything, in our context, the increase in school leaving age had the effect of further deepening SES differences across genetic groups. Around the world, compulsory schooling laws are among the most common types of education policy, but they might fall short from the ideal of being the "great equalizer." Our results suggest that, if a main goal of education policy is to reduce economic inequality resulting from initial endowments, alternative policies, perhaps more targeted at barriers faced by disadvantaged populations, are needed.

2. Background and Data

A. The 1972 Raising of the School Leaving Age

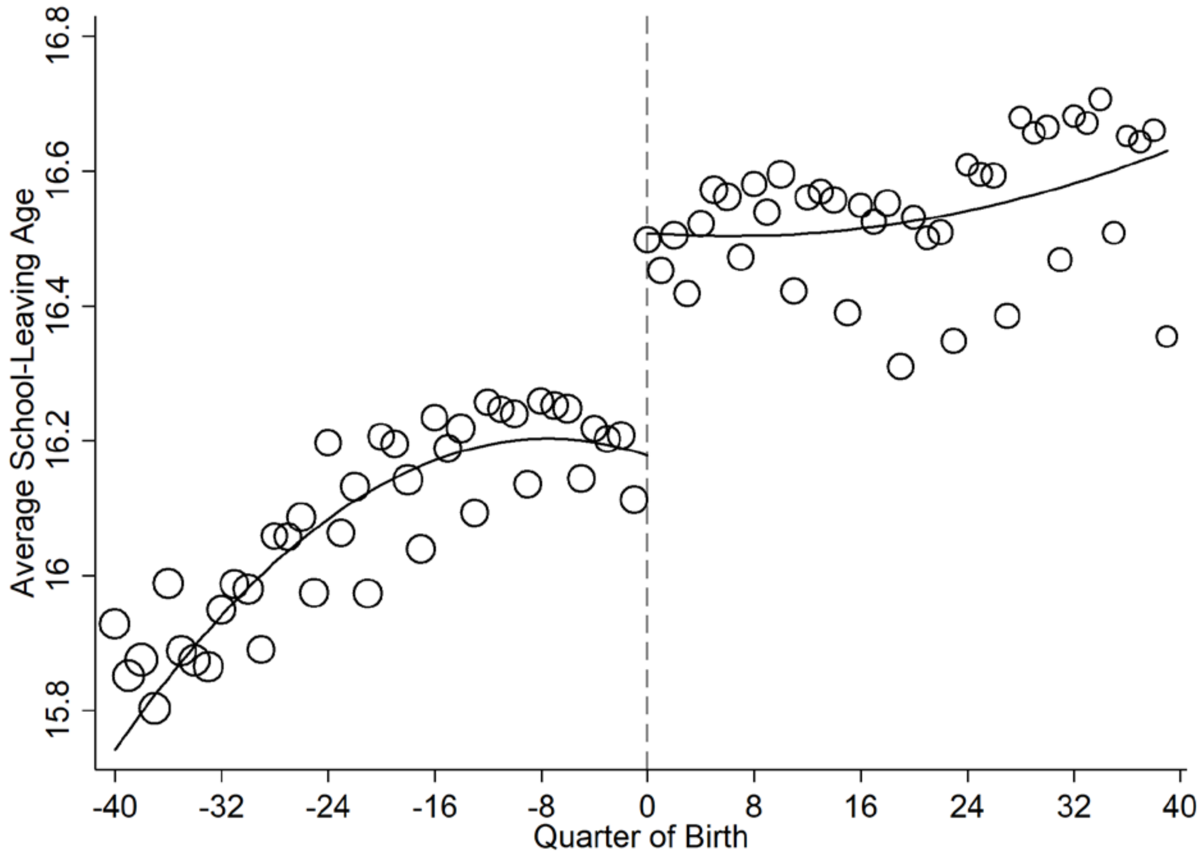
The British compulsory schooling laws specify the maximum age by which children must start school and the minimum age at which they can leave school. In this paper, we exploit the 1972 Raising of School Leaving Age (ROSLA) legislation, which increased the minimum school-leaving age from 15 to 16 years of age in England, Scotland, and Wales. These laws and their implementation have been extensively documented in other studies (see Clark and Royer 2010, 2013) so we only include a brief summary of its main features here.

The UK's 1944 Education Act raised the minimum school-leaving age from 14 to 15 years of age in England, Wales, and Scotland and gave the Minister of Education the power to further raise it to 16 years when conditions allowed. The Minister did so in January 1972 for Scotland (Statutory Instrument No. 59)[3] and in March 1972 for England and Wales (Statutory Instrument No. 444)[4]. Both changes took effect in September 1, 1972, implying that those who were 15 or younger before that date (born on September 1, 1957 or later) had to stay in school until at least age 16 in the three

[3] http://www.legislation.gov.uk/uksi/1972/59/pdfs/uksi_19720059_en.pdf
[4] http://www.legislation.gov.uk/uksi/1972/444/pdfs/uksi_19720444_en.pdf

countries (hereafter, we use the term "stayed in school until age 16" to refer to those who stayed in school until *at least* age 16). Infrastructure investments, such as school building to absorb the additional students, preceded the 1972 ROSLA but key elements of the school system did not change with the policy.

Figure 1: School Leaving Age (SLA) by Quarter of Birth



Notes: The figure shows average school leaving age of study participants among those who left school by 18 years old by quarter of birth. The dashed vertical line marks the first birth cohort affected by the 1972 school-leaving age reform. Cohorts born to the right of the line had to stay in school until age 16 while cohorts born before could leave at age 15. The curves show quadratic polynomials in quarter of birth that capture birth cohort trends. The circumference of each circle reflects the number of participants born in that quarter. *N* =104,578.

The main effect of the reform was to keep students who were dropping out of school at age 15 in school until age 16, with much smaller effects on the probability of staying in school until age 17 or 18.[5] Consistent with other papers, we find that the reform did not affect the probability

[5] Before the 1972 ROSLA, age 15 was the minimum school leaving age in the UK and compliance with the law was high.

of staying in school past age 18 or of entering college (see Appendix B Table 1).[6] Therefore, in order to focus the analysis on the affected population and improve power, we follow previous papers and restrict our analytical sample to those who left school ages 18 or younger (Banks and Mazonna 2012; Banks, Carvalho and Perez-Arce 2019).

Figure 1, which displays the average school leaving age (SLA) (y-axis) by quarter of birth (x-axis) for our sample, shows that the policy generated a discontinuous relationship between these two variables. There is a large jump at the September 1, 1957 cutoff marked by the vertical dashed line. We estimate that the policy increased average SLA among those who left school by age 18 by 0.3-0.33 years – see Table 4.

Notice there is a cyclical drop in school leaving age, corresponding to those born between June and August. This phenomenon is not specific to our data and has been noted by others. According to Clark and Royer (2013), "when the minimum leaving age became age 16, students had to stay until part way through grade 10. Grade 10 finishes with the "O level" exam period and, technically, students finish when they complete their last exam. Since the exam period starts in late May and finishes in mid-June, starting in 1972, students born in late June, July, and August could leave at 15, technically younger than the minimum leaving age (16)" (pg. 2 of Online Appendix). We include calendar month of birth dummies in our regressions to control for this seasonality.

B. Data

We use data from the UK Biobank, a large, population-based prospective study initiated by the UK National Health Service (NHS) (Sudlow et al. 2015). Between 2006 and 2010, invitations were mailed to 9.2 million people between the ages of 40 and 69 who were registered with the NHS and lived up to about 25 miles from one of 22 study assessment centers distributed throughout the UK (Allen et al. 2012) – see Appendix Figure C1.[7] The sample is formed by 503,325 individuals who agreed to participate (i.e. a response rate of 5.47%). Although the sample is not nationally

[6] Appendix B Table 1 shows that while the ROSLA may have had a small effect on the probability of staying in school until 18 or older, it had no effect on staying until 19 or older. Therefore, our strategy of focusing on those leaving school by age 18 is appropriate.
[7] The NHS has contact details for an estimated 98% of the UK population.

representative, our estimates have internal validity because there is no differential selection on the two sides of the September 1, 1957 cutoff – see Appendix A.[8]

In a verbal interview, respondents were asked about their occupation (or last occupation if retired); responses were mapped in more than 400 detailed categories. We use the 2009 Annual Survey of Hours and Earnings (ASHE) to match such categories to median wages at each occupation. We used gender-specific wages that were calculated among full-time employees and included all labor earnings (such as bonus, tips, overtime, etc.). This is our main measure of middle age SES. For simplicity, we refer to this measure of occupation-based wages simply as wages.

Exact date and approximate coordinates of birth are also available in the UK Biobank data. Exact date of birth increases the precision of RDD estimates and coordinates of birth allow us to link participants to local historical SES measures at their neighborhood and time of birth, as we describe below. Every participant was genotyped, we use this information to construct polygenic scores for educational attainment (EA PGS).

B.1. Genetic Heterogeneity

Using individual-level genetic information available in the UK Biobank, we summarize the genetic factors related to educational attainment using a linear index known as polygenic score (PGS). A person's genome is determined at conception and immutable; therefore, the PGS we will use in our analyses is predetermined with respect to the 1972 ROLSA. Below, we describe what genetic data represent and how a PGS is constructed.

Human DNA is made up of twenty-three pairs of long molecules, called *chromosomes*. While humans are all identical for 99.9% of their DNA, there are tens of millions of locations in the genome where individuals differ by a single genetic marker. These locations are called *Single Nucleotide Polymorphisms*, or SNPs. At the vast majority of SNPs, people can have one of two possible genetic variants. The variant that a person has is called their *allele*. In genetic data, one of the two possible alleles is arbitrarily chosen as the *reference allele*. Because individuals have two copies of each chromosome, they will either have 0, 1, or 2 copies of the reference allele. The number of reference alleles that an individual has at a SNP is called their *genotype* for that SNP.

[8] When the predetermined variable is continuous, we test whether the pre- and post-reforms distributions are different. When the predetermined variable is dichotomous, we conduct a test of difference in means.

The first step in producing a PGS is conducting a *Genome Wide Association Study* (GWAS). GWASs scan the entire genome and estimate associations between individual genotypes and outcomes of interest. Specifically, a GWAS is a series of regressions of some outcome onto the genotype of each SNP, one at a time, and a set of covariates. These covariates normally include sex, age, and the first several principal components of the genetic data. These principle components are included to account for ancestry-related omitted variable bias (see section B.2 for more details on this bias).

A *polygenic score* (PGS) is a weighted sum of SNP genotypes:

$$S_i = \sum_j g_{ij} w_j, \tag{3}$$

where S_i is the polygenic score for individual i, $g_{ij} \epsilon \{0,1,2\}$ is the count of the number of reference alleles for individual i at SNP j, and w_j is the weight for SNP j. [9] The weights in a PGS are derived from estimated coefficients in GWAS. In this paper, we use GWAS coefficients based on Lee et al. 2018, the largest currently available GWAS for educational attainment (EA).[10] There are several methods for producing PGS weights from GWAS coefficients, but each of them transform the GWAS coefficients in a way that is meant to account for the correlation structure that exists in the genome. We use a Bayesian method called LDpred (Vilhjálmsson et al., 2015). The PGS using this GWAS and LDpred can explain 12-13% of the variation in education (Lee et al., 2018).

B.2 Interpreting PGS Results

PGSs reflect associations only and should not be interpreted as causal effects of genetics on education or as capturing exclusively direct genetic effects. For example, a major potential source of bias in studies using genetic data is population stratification. This is a form of omitted variable bias where certain groups may differ in the average level of some outcome for reasons unrelated to genetics, and certain alleles may be more common in those groups by chance. This would lead to a correlation between the genotype and the unobserved group assignment, and

[9] GWASs for educational attainment have shown that education is associated to a large number of genetic markers, each with a small influence. Polygenic scores are a way to aggregate these many influences and construct a genetic marker that is sufficiently predictive to use in empirical applications.

[10] In this case, we use a version of the GWAS coefficients from Lee et al. (2018) that omit the subset of individuals from the UK Biobank that we use in our analysis. Specifically, we conduct a GWAS in the UK Biobank that exactly matches the specification in Lee et al., except that we exclude individuals that are born within 10 years 1 September 1957. This is done to avoid overfitting.

therefore would produce spurious correlations between genotypes (or polygenic scores) and the outcome.[11] Previous work has shown that these biases in PGS results are substantially larger than the direct effect of the PGS (Martin et al. 2017).

For this reason, samples in genetic research are nearly always restricted to individuals with approximately homogeneous genetic ancestry. Groups with homogeneous genetic ancestry are usually defined as a set of people who have tightly clustered first and second principle components of their genetic data. Due to Euro-centric bias in data collection, in most genetic data sets, the largest tightly clustered group usually contain only individuals who self-identify as "White" or "Caucasian." Homogeneous clusters containing mostly individuals self-identifying this way are usually labeled as having "European ancestry." Restricting the sample this way has two implications. First, PGSs based on currently available GWAS coefficients are substantially more predictive in other European ancestry samples (Martin et al, 2019). Second, PGS-based results only apply to groups with European ancestries and may not generalize to groups with African, Asian, Hispanic, or other non-European ancestries. The lack of generalizability means individuals with European ancestries have accrued much of the benefit of previous genetics research. For this reason, there is an urgent need to expand genetic samples to include individuals with diverse ancestries. That said, the reform studied in this paper only affected those born in the UK in the 1950s and 1960s. Because these individuals were predominantly of European ancestry, this restriction only made us drop a small fraction of our sample (less than 5%). However, it will be crucial expand the ancestral diversity of genetic samples in order to validate that our results generalize to modern populations.

Even restricting to approximately homogeneous samples, there is still risk that subtle population stratification can remain in the data. To account for this, the first several principle components of the genetic data are usually included as control variables in genetic studies. Empirically, it has been shown that restricting the sample and controlling for genetic principle components removes nearly all of the bias due to population stratification (Price et al. 2006). In our analyses, we include specifications that do and do not control for principle components. In

[11] The canonical example of population stratification is chopstick use (Price et al. 2006). Consider a GWAS of whether an individual regularly uses chopsticks and a sample that includes individuals of Asian and non-Asian backgrounds. This GWAS would find many SNPs that are associated with chopstick use, but each of these associations would correspond to SNPs that have alleles that are more common in Asian populations rather than to SNPs that represent any sort of genetic pathway between genes and the outcome.

analyses that omit the principle components, the PGS should be interpreted as not only representing pathways influenced to genetic factors but also pathways related to their ancestry. This could include cultural priorities and differences in resources that are available to their community. In the results below, we find little difference when controlling for the first 20 principal components of the genetic matrix, meaning that there is not much stratification in our analytic sample.

Another source of omitted variable bias when using PGSs is parental background. For example, imagine that a person's educational attainment can be influenced by their own genes and by the educational attainment of their parents. Since, parental genes affect both offspring genes and parental educational attainment, genetic studies that omit parental genotypes or educational attainment as covariates will be biased. Kong et al. 2018 quantify this bias using a dataset that contains parental and offspring genetics. They estimate that about half of the variation in educational attainment explained by a polygenic score is due to a direct genetic effect; the residual is driven by the parental effects described above. Therefore, our results using the EA PGS are going to be in part reflecting these intergenerational effects. Using siblings in the UK Biobank data, we show results that are consistent with the results in Kong et al. 2018, see Table 2.

Table 1: Log Wages and Educational Attainment PGS among pre-reform cohorts.

	Dependent Variable: Ln Wages							
	All Sample		Compliers Only		All Sample		Compliers Only	
PGS	0.027	0.027	0.018	0.016				
	[0.003]***	[0.002]***	[0.005]***	[0.003]***				
Top PGS Tercile					6.131	6.136	6.014	6.023
					[0.007]***	[0.005]***	[0.012]***	[0.009]***
Middle PGS Tercile					6.114	6.108	6.035	6.017
					[0.007]***	[0.005]***	[0.012]***	[0.009]***
Bottom PGS Tercile					6.068	6.071	5.976	5.983
					[0.006]***	[0.005]***	[0.011]***	[0.009]***
Constant	6.098	6.099	6.005	6.005				
	[0.005]***	[0.005]***	[0.009]***	[0.008]***				
Bandwidth	4 yrs	10 yrs	4 yrs	10 yrs	4 yrs	10 yrs	4 yrs	10 yrs
Observations	21,881	59,460	6,161	20,586	21,881	59,460	6,161	20,586

Notes: specification includes pre-cohort trends, linear for 4-year bandwidth and quadratic for 10-year.

Table 1 shows pre-ROSLA (i.e. cohorts born before Sept 1 1957) associations between wages and the EA PGS in all our analytic sample and among the ROSLA complier population only. Pre-reform compliers are approximated in this case as those who were born before September 1957 and left school before age 16. All of these individuals would have been constrained to obtain additional education if they had been born after September 1957 instead. Among these individuals, those with relatively high EA PGS exhibit higher wages. A one standard deviation (SD) increase in the PGS is associated with a 1.6-1.8% increase in wages. In other words, those at the top tercile of the PGS distribution have, on average, 4% higher wages than those at the bottom tercile. These associations are highly significant (p-value < 0.001). As expected, when we use the whole sample, these associations are even larger.[12]

Table 2: Log Wages and EA PGS among siblings from pre-reform cohorts

	Dependent Variable: Ln Wages					
	All Sample OLS		Sibling Sample OLS		Sibling Sample Fixed Effects	
PGS	0.072	0.072	0.076	0.075	0.039	0.039
	[0.001]***	[0.001]***	[0.003]***	[0.003]***	[0.007]***	[0.007]***
Constant	6.251	6.247	6.222	6.233	6.222	6.222
	[0.001]***	[0.006]***	[0.003]***	[0.024]***	[0.000]***	[0.000]***
PCs	No	Yes	No	Yes	No	No
Observations	192,131	192,131	11,524	11,524	11,524	11,524
Number of fam					5,639	5,639

As explained above, part of this association might be due to environmental factors (such as population stratification or home environment) shared by people with similar genetic backgrounds. To explore this possibility, we take advantage that the UK Biobank contains approximately 20,000 sibling pairs. Table 2 presents analyses similar to the ones on Table 1 among siblings born within 10 years of the ROSLA birth cutoff. The first two columns present OLS results

[12] This is among our analytic sample of those who left school by age 18. If we calculate this association lifting this restriction, 1 SD increase in the PGS is associated with a remarkable (and highly significant) 7.2 increase in wages.

among all sample (with and without principal components, PCs, controls), columns 3-4 present OLS results among our much smaller siblings sample, and columns 5-6 present family fixed-effects results. The results show that the association between wages and the EA PGS is large and very similar among the whole sample and the sibling sample (with or without controlling for PCs). A one SD increase in the PGS is associated with 7.2% higher wages. Moreover, in the within family estimates these associations are still strongly significant but the magnitudes fall by approximately half. This is consistent with the results by Kong et al. 2018 and the existence of biases related to parental background, as described above. However, it is important to note that the PGS is still highly predictive of wages within families.

B.3. Neighborhood Heterogeneity

We use the approximate coordinates of birth available in the UKB to link each individual to SES conditions at their neighborhood and time of birth. In particular, we link 1961 Census data on local authority level SES measures, such as average education and occupation levels, home ownership and mortality.[13] As our main measure of neighborhood SES, we use the fraction of adults in the local authority who stayed in school until age 15 or later. We chose to use an educational variable since it was most related to our treatment variable, and we chose the threshold of age 15 since that was the age that maximized the variation in neighborhood characteristics in our sample. Importantly, this variable was chosen before assessing its association with our measures of SES and education. Fortunately, this measure captures a large fraction, of the potential neighborhood variation at this level of aggregation, and it captures nearly all of the variation explained by all of our historical neighborhood variables collectively.[14]

[13] Historical local authority level data comes from http://www.visionofbritain.org.uk/, which makes local-level Census data available. In our data, we have 1637 different local authorities, each with on average 39K people (median 19K people).
[14] See Appendix Figures D1-D4 for the histograms of this variable among the whole sample and compliers only.

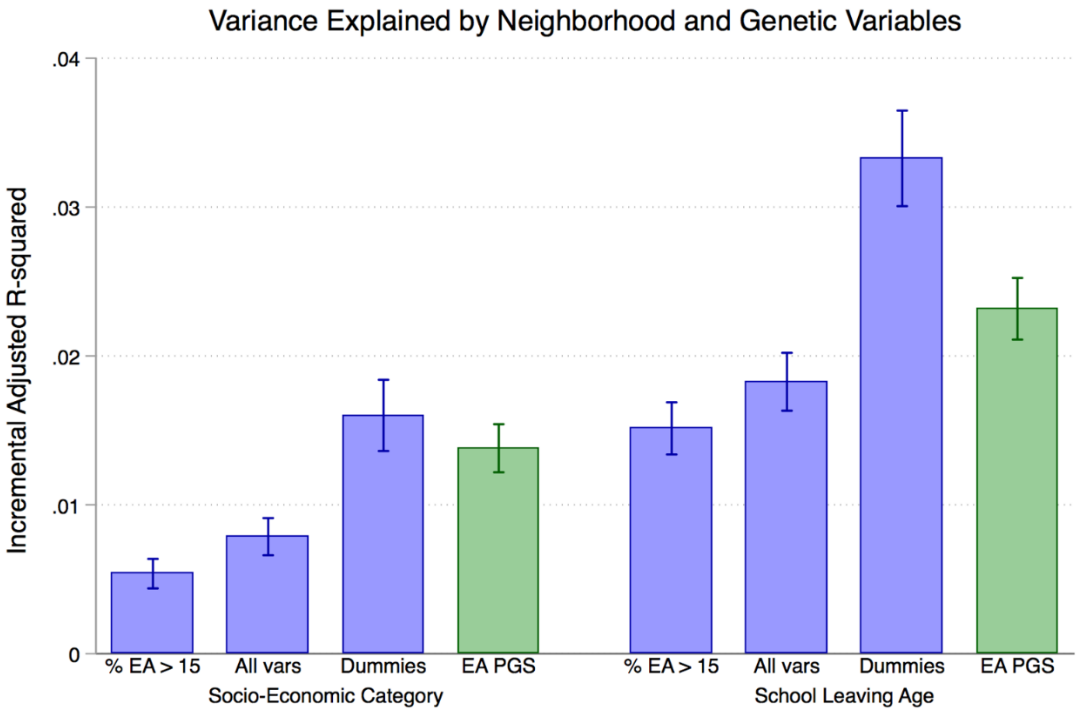


Figure 2 shows the incremental variance explained by different neighborhood and genetic variables for SES and school-leaving age (SLA). Incremental variance explained is defined to be the difference in R2 between a model that includes only our covariates[15] and a model that includes our covariates and the neighborhood or genetic variables. One can see that, in both cases, the percent staying in school until age 15 explains about 2/3 of the total variation explained by all the 1961 Census variables and 1/3 of the variation explained by a large set of local authority dummies (which can be interpreted as the upper bound of the total variation that can be explained at this level of aggregation). Moreover, the EA PGS explains about 1.5% of the variation in SES and 2.3% of the variation in SLA. Note that 2.3% is smaller than the 12-14% that is seen in some data sets. This is because SLA is only measured in the UK Biobank for those who never attended college, so our sample is restricted to this more narrow sample of individuals. Estimates of the

[15] The covariates in the case include date of birth, date of birth squared, an indicator of whether an individual was born after 1 September 1952, the interactions of the date of birth variables and this indicator, and sex. We restricted to those born within 10 years of the ROSLA reform and weighted our regression with a triangle kernel. The 95%-confidence intervals were calculated using a bootstrap with 1000 replications.

heritability[16] of EA in the UK Biobank sample are as a result substantially smaller than in other samples with better measures of EA.

Table 3: Log Wages and Birth Neighborhood Quality among Pre-Reform Cohorts

	Dependent Variable: Ln Wages							
	All Sample		Compliers Only		All Sample		Compliers Only	
RegEdu15	0.018	0.016	0.007	0.005				
	[0.003]***	[0.002]***	[0.005]	[0.003]				
HIGH					6.129	6.128	6.018	6.014
					[0.007]***	[0.006]***	[0.013]***	[0.010]***
MID					6.098	6.100	6.001	6.000
					[0.007]***	[0.006]***	[0.012]***	[0.010]***
LOW					6.084	6.087	6.000	6.000
					[0.007]***	[0.006]***	[0.012]***	[0.010]***
Constant	6.100	6.102	6.005	6.004				
	[0.006]***	[0.005]***	[0.010]***	[0.009]***				
Bandwidth	4 yrs	10 yrs	4 yrs	10 yrs	4 yrs	10 yrs	4 yrs	10 yrs
Observations	18,863	51,183	5,288	17,777	18,863	51,183	5,288	17,777

Notes: specification includes pre-cohort trends, linear for 4-year bandwidth and quadratic for 10-year.

The fraction staying in school until age 15 at an individual's neighborhood of birth is also positively associated with middle-age SES: individuals born in higher SES neighborhoods exhibit higher wages later in life (Table 3). A one SD increase in birth neighborhood quality is associated with 1.8% increase in wages. The association is still positive but smaller and not significant among our complier sample (p-value=0.119). Those at the top quality tercile have wages that are on average 1.4-1.8% higher than those at the bottom quality tercile. Below we investigate whether an increase in secondary education brought by a change in compulsory schooling laws mitigated or reinforced the relationships documented in Tables 1 and 3.

[16] Heritability is the total amount of variation in an outcome that can be explained by genetic factors. As a result, it may be thought of as an upper bound of the predictive power of a PGS.

3. Regression Discontinuity (RD) Model

We use a Regression Discontinuity Design (RDD) to investigate whether an exogenous increase in education can mitigate SES disadvantages related to genetic makeup and neighborhood of birth. The RDD compares the outcomes of individuals born just before and just after September 1, 1957, controlling for cohort trends. Intuitively, individuals born on August 31, 1957 and individuals born on September 1, 1957 were comparable (e.g., in terms of their childhood health and SES) before the reform. In other words, the SES of those born on August 31, 1957 provides a counterfactual of the SES those born on September 1, 1957 would have had had they not been forced to stay in school until age 16. For this reason, any later-life SES differences between these two groups can be attributed to the causal effect of the additional year of schooling. In Appendix A, we offer evidence that those born just before and just after September 1, 1957 were comparable before the reform. For example, we show that the two groups are genetically similar. Genetic markers are useful to test the RDD assumption because genotypes are objectively measured, determined at conception, and immutable.

To investigate whether the effect of education on SES varies with genetic makeup and neighborhood of birth, we compare the discontinuous changes in outcomes of groups with different PGSs and birth neighborhood quality, accounting for the differences in the fraction of individuals affected by the reform in different groups. We start by estimating our "first stage" regressions that estimate how the effect of the 1972 ROLSA on education varied by (predetermined) birth neighborhood and genetic groups:

$$SLA_i = \alpha_0 + \alpha_1(Post_i \times RegEdu15_i) + \alpha_2 Post_i + \alpha_3 RegEdu15_i + \\ + f(DoB_i) + \boldsymbol{x}_i'\boldsymbol{\alpha}_4 + \varepsilon_i, \qquad (1)$$

And

$$SLA_i = \beta_0 + \beta_1(Post_i \times PGS_i) + \beta_2 Post_i + \beta_3 PGS_i + \\ + f(DoB_i) + (Post_i \times \boldsymbol{PC}_i')\boldsymbol{\beta}_4 + \boldsymbol{PC}_i'\boldsymbol{\beta}_5 + \boldsymbol{x}_i'\boldsymbol{\beta}_6 + u_i, \qquad (2)$$

where SLA_i stands for school leaving age, our measure of the educational attainment of individual i; $Post_i$ is 1 if individual i was born on or after September 1, 1957 (and 0 otherwise); $RegEdu15_i$ is the fraction of adults staying in school until age 15 according to the 1961 Census at individual i's neighborhood of birth; PGS_i is the EA PGS; $\boldsymbol{PC}_i$ is a vector of the first 20 principal components of the genotypic data; DoB_i is individual i's date of birth; and the vector $\boldsymbol{x}_i$ contains predetermined characteristics—namely age, age-squared, gender, month and country of birth. Date of birth is measured in days relative to the cutoff, such that $DoB = 0$ for someone born on September 1, 1957. The function $f(\cdot)$ captures birth cohort trends in educational attainment, which are allowed to differ on either side of the September 1, 1957 cutoff. In regressions where we look for genetic heterogeneity, we include $Post_i \times \boldsymbol{PC}_i'$ and $\boldsymbol{PC}_i'$ to correct for population stratification, as explained above. The coefficients α_1 and β_1 estimate how the effect of the 1972 ROSLA on educational attainment vary with neighborhood of birth and genetics.[17]

We restrict the data to study participants born in England, Scotland, or Wales within 4 (or 10) years of September 1957 – that is, born between September 1, 1953 and August 31, 1961 – and use a linear (quadratic) polynomial in date of birth to capture cohort trends (i.e., function $f(\cdot)$ in equations (1 and 2)).[18] In Appendix Table B1, we show that the 1972 ROSLA had no effect on the probability staying in school past age 18, therefore, to improve power, we restrict our sample to those who left school ages 18 or younger. As described above, we also restrict the sample to participants of European ancestry, for which the existing PGSs are applicable. Our final sample with these restrictions has approximately 42 thousand (104 thousand for 10-year bandwidth) observations. We use triangular kernel weights that give greater weight to study participants born closer to the cutoff. The set of predetermined characteristics include gender, age in days (at the time of the baseline assessment) and age squared, dummies for ethnicity, dummies for country of birth, and dummies for calendar month of birth (to control for seasonality).[19]

Notice that even though previous work studying the 1972 ROSLA clustered standard errors by month-year of birth (e.g., Clark and Royer 2013; Davies et al. 2017), we do not need to cluster our standard errors because our data include exact date of birth. As Lee and Card (2008) discuss, in applications where the running variable is only reported in coarse intervals (e.g., month-year of birth), researchers have to choose a particular functional form for the model relating the outcomes of interest to the running variable. The deviation between the expected value of the outcome and the predicted value from a given functional form is modeled as a random specification error, which is incorporated in inference by clustering the standard errors for different values of the running

[17] The inclusion of predetermined controls in equations (1) and (2) is not needed for identification but can improve the precision of estimates.
[18] We opt to use 4-year bandwidths because it was the closest integer number to the optimal bandwidth for the direct effect of the 1972 ROLSA according to the algorithm in CCT. We present results using a 10-year bandwidth and quadratic trends for robustness.
[19] Because participants were surveyed for the baseline assessment between 2006 and 2010, date of birth and age are not perfectly collinear.

variable. This specification error should be negligible in our context because our data include day-month-year of birth. We get virtually identical standard errors estimates irrespective of whether we cluster by date of birth or not.[20]

Our main estimating equation use the 1972 ROSLA birth cutoff to instrument for the effects of education on SES. Formally, we estimate the following regressions:

$$lnW_i = \delta_0 + \delta_1(SLA_i \times RegEdu15_i) + \delta_2 SLA_i + \delta_3 RegEdu15_i + \\ + f(DoB_i) + \boldsymbol{x}_i' \boldsymbol{\delta}_4 + \varepsilon_i, \quad (3)$$

and

$$lnW_i = \gamma_0 + \gamma_1(SLA_i \times PGS_i) + \gamma_2 SLA_i + \gamma_3 PGS_i + \\ + f(DoB_i) + (SLA_i \times \boldsymbol{PC}_i')\boldsymbol{\gamma}_4 + \boldsymbol{PC}_i'\boldsymbol{\gamma}_5 + \boldsymbol{x}_i'\boldsymbol{\beta}\boldsymbol{\gamma}_6 + u_i, \quad (4)$$

where lnW_i is log wages and all the other variables and specification choices are defined as above. To account for the endogeneity of SLA_i and for the differential impacts of the reform on the education of groups with different PGSs and neighborhood qualities, we estimate equations (3) and (4) through two-stages least squares (2SLS), using the reform as an instrument. The 2SLS estimates the effect of staying in school one more year *among those affected by the reform* (i.e., those who would have dropped out at age 15 in the absence of the reform). In other words, our results cannot be explained by the fact that individuals with different EA PGSs and from different neighborhoods were differentially affected by the reform. We will also present results from a secondary, more non-parametric specification, were we create indicators for whether an individual is in the bottom, middle, or top tercile of the distribution of the PGS and neighborhood quality:

$$lnW_i = \delta_0 + \delta_1(SLA_i \times bottomEdu15_i) + \delta_2(SLA_i \times middleEdu15_i) + \delta_3(SLA_i \times \\ topEdu15_i)\delta_2 SLA_i + \delta_4 topEdu15_i + \delta_5 middleEdu15_i + \delta_6 SLA_i + f(DoB_i) + \boldsymbol{x}_i'\boldsymbol{\delta}_7 + \varepsilon_i, \\ (5)$$

and

$$lnW_i = \gamma_0 + \gamma_1(SLA_i \times bottomPGS_i) + \gamma_2(SLA_i \times middlePGS_i) + \gamma_3(SLA_i \times topPGS_i) + \\ \gamma_4 topPGS_i + \gamma_5(middlePGS_i) + \gamma_6 SLA_i + f(DoB_i) + (SLA_i \times \boldsymbol{PC}_i')\boldsymbol{\gamma}_7 + \boldsymbol{PC}_i'\boldsymbol{\gamma}_8 + \boldsymbol{x}_i'\boldsymbol{\beta}\boldsymbol{\gamma}_9 + \\ u_i, \quad (6)$$

[20] Results available upon request.

4. Results

A. Effects of the Compulsory Schooling Change on Average Education and Wages

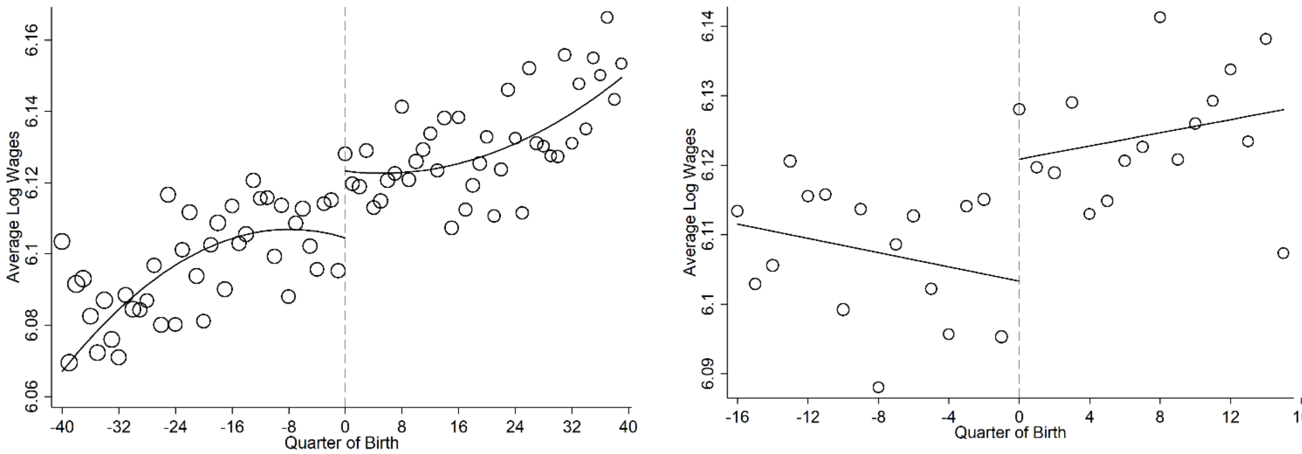
We begin by exploring how the 1972 ROSLA affected overall education and wages. As explained above, we focus on participants who left school age 18 or younger, since this is the population affected by the law change. Consistent with Figure 1, Table 1 shows that the reform made affected cohorts stay on average 0.3 years longer in school. The reform also increased wages by 1.7-1.8 percent (see Figure 3). Two-stage least squares estimates imply that one extra year of secondary education increase middle age wages by 5.5-5.6 percent. These are consistent with estimates in Grenet (2013) which estimated 6-7% return using the education induced by the 1972 ROSLA and pooled data from the Quarterly Labour Force Survey (QLFS), which contains individual-level wages. These effects are highly significant and robust to the use of 4- versus 10-year bandwidths and quadratic versus linear polynomials on date of birth.

Table 4: Effects of the 1972 ROSLA on Education and Wages

	First Stage		Reduced Form		2SLS	
	SLA	SLA	lnW	lnW	lnW	lnW
Post	0.305	0.330	0.017	0.018		
	[0.021]***	[0.020]***	[0.007]**	[0.007]***		
SLA					0.055	0.056
					[0.024]**	[0.021]***
Bandwidth	4 years	10 years	4 years	10 years	4 years	10 years
DoB Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	42,221	104,578	42,221	104,578	42,221	104,578
Mean pre-reform	14.94	14.95	6.003	6.003	6.003	6.003

Notes: Regression discontinuity estimates of effects on school leaving age (SLA) and log wages (lnW) using 10- and 4-year bandwidths. Effects among participants who left school age 18 or younger.

Figure 3: Effect of the 1972 ROSLA on Wages, 4- and 10-year Bandwidth



B. Effects of the Compulsory Schooling Change on Education by Birth Neighborhood and Genetic Groups

Next, we document how the reform differentially affected the education of groups with different initial endowments, measured by the EA PGS and birth neighborhood quality, and what these differential effects mean for inequalities in education across groups. Figure 4 shows cohort trends in school leaving age (left) and fraction leaving school at age 16 or later (right) by polygenic score tercile. As expected, before the reform those with lower PGS dropped out of school earlier. When the reform kicks in all three groups are affected, but the bottom group has a larger discontinuous jump in education. Estimates in Table 5 (columns 1 and 2) imply that the jump is about 1/3 or 0.1 years larger for the bottom than the top tercile.

The increase in SLA comes mostly from an increase in the fraction of students staying in school until age 16. Among cohorts born before September 1957, those with lower EA PGSs were less likely to stay in school until age 16. The reform had a strong Gene by Enviroment (“GxE”) effect: the difference in the fraction staying in school until age 16 between the bottom and top EA PGS terciles fell from 18.4 percentage points before the reform to 3.1 percentage points afterwards. Because almost everyone stayed in school until at least age 16 after the reform, there was little variation in educational attainment at this level left after the reform to be explained by the EA PGS.

Figure 4: Effect of the 1972 ROSLA on School Leaving Age and Fraction Leaving School Age 16 or Later, by Educational Attainment Polygenic Score

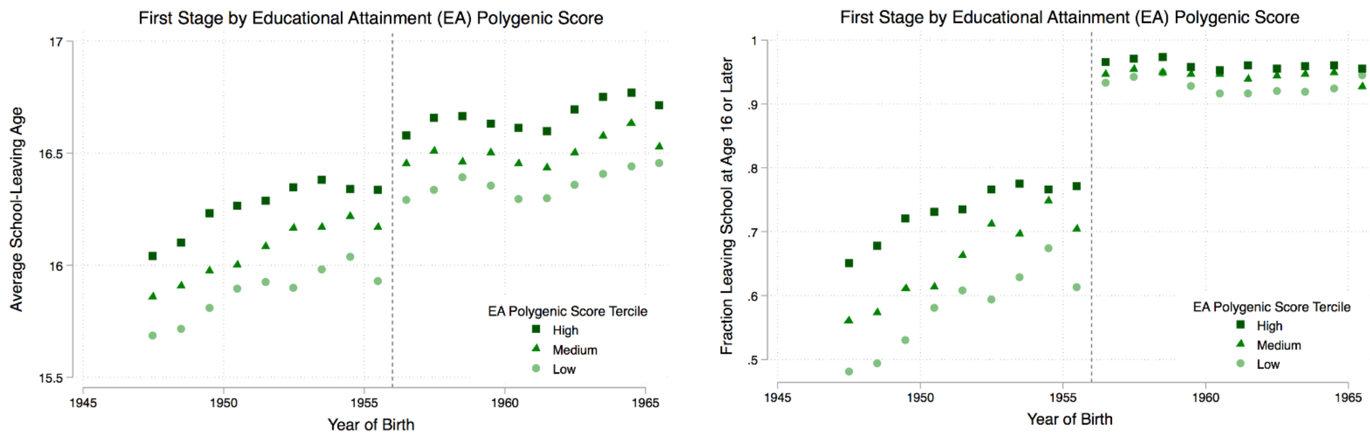


Figure 5: Effect of the 1972 ROSLA on School Leaving Age and Fraction Leaving School Age 16 or Later, by Birth Neighborhood Quality

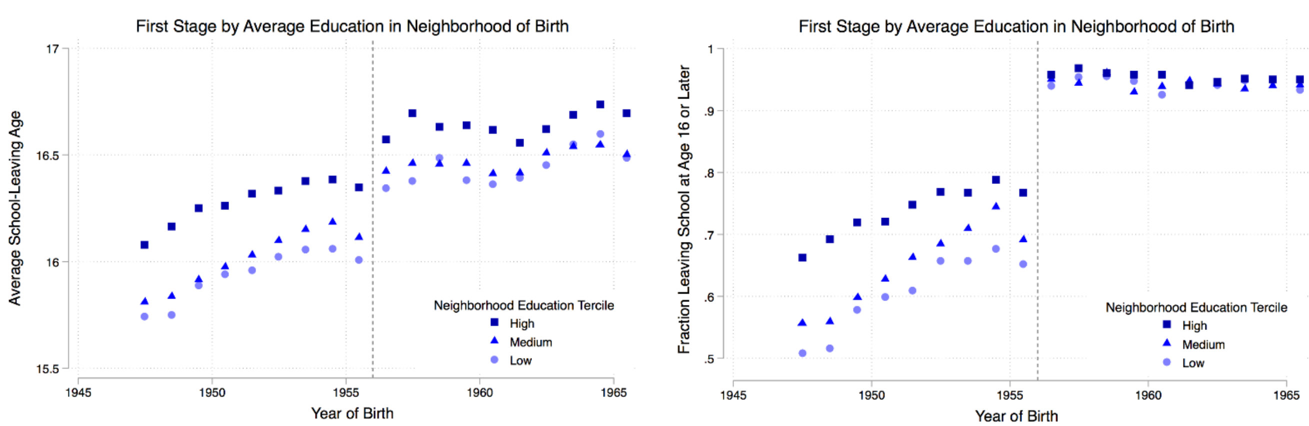


Figure 5 presents the education cohort trends by birth neighborhood education. We see similar trends. Those from low education neighborhoods on average drop out sooner than those from high education neighborhoods and, for these reason, are most affected by the reform. While smaller than the differences across PGS groups, the differences in the effect of the reform by neighborhoods are meaningful (0.06 years) and highly significant – see Table 6, Panel B, columns 1 and 2.

Because the ROSLA required students to stay in school until age 16, it disproportionally affected students from the bottom terciles of the PGS and neighborhood education distributions, who were less likely to stay in school until age 16 before the reform. Therefore, the change in compulsory schooling had the effect of equalizing education across these groups. In the next subsection we investigate whether the reduction in education differences translated in a reduction in middle age SES differences across groups.

C. Effects of Education on Wages by Birth Neighborhood and Genetic Groups

Tables 5 to 7 present our main results. They show results based on the continuous model described by equations (3) and (4) in Panel A and on the terciles model described by equations (5) and (6) in Panel B. They present first stage (columns 1-2), reduced form (columns 3-4) and 2SLS (columns 5-6) results for both 4-year bandwidths (with linear trends) and 10-year bandwidths (with quadratic trends). Table 5 documents heterogeneity by EA PGS, Table 6 by birth neighborhood quality and Table 7 investigates both interactions jointly (continuous models only).

Columns 3 and 4 of Table 5 shows the reduced form estimates of the effects of the 1972 ROSLA on log wages. Despite disproportionally increasing the education of *low* PGS students, the reform had the effect of disproportionally increasing the wages of those with *high* PGS. As seen in Table 4, the reform had the average effect of increasing wages by 1.7%, Table 5 shows that the effect among those with a PGS 1SD above the mean was about 0.6-0.7% higher. These differences are only marginally significant using the 4-year bandwidth, but the magnitude of the coefficients is very similar across specifications.

Due to the first stage pattern, the differences in wages across PGS groups are more pronounced under the two-stage least squares estimates. In Table 1 above, we document that the pre-reform difference in wages among compliers in the top and bottom PGS terciles is 4%. The estimates in Table 5 imply that the extra education provided by the reform more than doubled this gradient. The estimates on columns 5 and 6 in panel B show that one extra year of education increases wages 5.5-6.2% more in the top PGS compared to the bottom PGS tercile. Similarly, estimates in Panel A show positive and highly significant interactions between SLA and the continuous PGS. In other words, not only the extra education did not equalize existing differences in wages, it actually quite steeply increased those differences across PGS groups.

We see similar patterns when we explore heterogeneity across birth neighborhoods, see Table 6. The interactions here are not significant with the 4-year bandwidth and only marginally significant with 10-years, but still coefficients are quite stable across specifications. In any case, these results should be interpreted with caution. However, one thing is clear, since all the interactions are positive, we see no evidence that education reduced differences in middle age wages across birth neighborhoods.

One possibility is that these two interactions are capturing the same phenomenon, if, for example, birth neighborhood education and PGS are highly correlated. As we discussed below, the PGS reflects many factors and it is possible that it captures in part the birth neighborhood environment. In order to test for this possibility, in Table 7, we add both interactions to the same model. In case of significant overlap, we would expect a reduction in the estimated interaction coefficients. That is not what we find. Estimates remain (both in magnitude and significance) virtually identical to the ones on Tables 5 and 6, suggesting that education affects these two margins independently.

Table 5: Effects of Education on Wages by Educational Attainment Polygenic Score

	First Stage		Reduced Form		2SLS	
	SLA	SLA	lnW	lnW	lnW	lnW
Panel A: Continuous Score						
PGS * Post	-0.043	-0.046	0.006	0.007		
	[0.011]***	[0.007]***	[0.004]*	[0.002]***		
PGS * SLA					0.026	0.025
					[0.013]**	[0.007]***
Post	0.319	0.343	0.014	0.016		
	[0.021]***	[0.019]***	[0.007]*	[0.007]**		
SLA					0.060	0.055
					[0.029]**	[0.021]**
PGS	0.181	0.186	0.028	0.028	-0.018	-0.015
	[0.009]***	[0.005]***	[0.003]***	[0.002]***	[0.020]	[0.012]
Panel B: Score Terciles						
Top * Post	-0.094	-0.099	0.014	0.014		
	[0.028]***	[0.018]***	[0.010]	[0.006]**		
Mid * Post	-0.063	-0.050	-0.007	0.006		
	[0.026]**	[0.017]***	[0.010]	[0.006]		
Top * SLA					0.062	0.052
					[0.035]*	[0.018]***
Mid * SLA					-0.016	0.021
					[0.031]	[0.016]
Post	0.363	0.383	0.013	0.010		
	[0.025]***	[0.022]***	[0.009]	[0.008]		
SLA					0.052	0.035
					[0.033]	[0.021]*
Top	0.393	0.410	0.064	0.065	-0.972	-0.803
	[0.021]***	[0.013]***	[0.007]***	[0.004]***	[0.571]*	[0.301]***
Middle	0.212	0.208	0.047	0.037	0.295	-0.307
	[0.020]***	[0.013]***	[0.007]***	[0.004]***	[0.504]	[0.257]
Bandwidth	4 years	10 years	4 years	10 years	4 years	10 years
DoB Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	42,221	104,578	42,221	104,578	42,221	104,578
Mean pre-reform	14.94	14.95	6.003	6.003	6.003	6.003

Notes: Regression discontinuity estimates of effects on school leaving age (SLA) and log wages (lnW) using 10- and 4-year bandwidths. See equations on section 3 of paper for regression specification details.

Table 6: Effects of Education on Wages by Birth Neighborhood Quality

	First Stage		Reduced Form		2SLS	
	SLA	SLA	lnW	lnW	lnW	lnW
Panel A: Continuous Neighborhood Quality						
RegEdu15 * Post	-0.014	-0.011	0.004	0.004		
	[0.009]	[0.005]**	[0.003]	[0.002]**		
RegEdu15 * SLA					0.019	0.016
					[0.012]	[0.006]**
Post	0.313	0.334	0.017	0.019		
	[0.021]***	[0.020]***	[0.008]**	[0.007]***		
SLA					0.073	0.065
					[0.030]**	[0.022]***
RegEdu15	0.122	0.121	0.018	0.016	-0.019	-0.015
	[0.008]***	[0.005]***	[0.003]***	[0.002]***	[0.021]	[0.011]
Panel B: Neighbrhood Quality Terciles						
Top * Post	-0.060	-0.065	0.010	0.011		
	[0.030]**	[0.019]***	[0.010]	[0.007]*		
Mid * Post	-0.043	-0.029	-0.005	-0.004		
	[0.027]	[0.017]*	[0.010]	[0.006]		
Top * SLA					0.054	0.045
					[0.039]	[0.020]**
Mid * SLA					-0.013	-0.008
					[0.034]	[0.017]
Post	0.345	0.363	0.015	0.016		
	[0.025]***	[0.021]***	[0.009]*	[0.008]**		
SLA					0.057	0.050
					[0.030]*	[0.021]**
Top	0.253	0.245	0.033	0.028	-0.874	-0.723
	[0.023]***	[0.014]***	[0.007]***	[0.005]***	[0.648]	[0.334]**
Middle	0.064	0.042	0.005	0.002	0.217	0.137
	[0.020]***	[0.013]***	[0.007]	[0.004]	[0.549]	[0.271]
Bandwidth	4 years	10 years	4 years	10 years	4 years	10 years
DoB Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	42,221	104,578	42,221	104,578	42,221	104,578
Mean pre-reform	14.94	14.95	6.003	6.003	6.003	6.003

Notes: Regression discontinuity estimates of effects on school leaving age (SLA) and log wages (lnW) using 10- and 4-year bandwidths. See equations on section 3 of paper for regression specification details.

Table 7: Effects of Education on Wages by Educational Attainment Polygenic Score and Birth Neighborhood Quality

	First Stage		Reduced Form		2SLS	
	SLA	SLA	lnW	lnW	lnW	lnW
Panel A: Continuous Score and Neighborhood Quality						
PGS * Post	-0.044	-0.046	0.006	0.007		
	[0.011]***	[0.007]***	[0.004]*	[0.002]***		
PGS * SLA					0.027	0.025
					[0.013]**	[0.007]***
RegEdu15 * Post	-0.014	-0.012	0.003	0.004		
	[0.008]*	[0.005]**	[0.003]	[0.002]*		
RegEdu15 * SLA					0.017	0.014
					[0.012]	[0.006]**
Post	0.320	0.343	0.016	0.017		
	[0.021]***	[0.019]***	[0.008]**	[0.007]**		
SLA					0.069	0.061
					[0.030]**	[0.022]***
PGS	0.176	0.180	0.027	0.027	-0.021	-0.016
	[0.009]***	[0.005]***	[0.003]***	[0.002]***	[0.021]	[0.012]
RegEdu15	0.113	0.111	0.016	0.014	-0.018	-0.014
	[0.008]***	[0.005]***	[0.003]***	[0.002]***	[0.021]	[0.011]
Bandwidth	4 years	10 years	4 years	10 years	4 years	10 years
DoB Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	41,692	103,350	41,692	103,350	41,692	103,350
Mean pre-reform	14.94	14.95	6.003	6.003	6.003	6.003

Notes: Regression discontinuity estimates of effects on school leaving age (SLA) and log wages (lnW) using 10- and 4-year bandwidths. See equations on section 3 of paper for regression specification details.

5. Discussion and Conclusions

Is education, and education policy, the great equalizer? In a world were initial endowments matter for long-term SES, we investigate how a change in compulsory schooling law affects differences in educational attainment and middle age wages across genetic and birthplace groups. Despite being among the most common types of education policy around the globe, little is known about the effectiveness of compulsory schooling in reducing lifecycle differences in SES. In the British context we study, we find that despite significantly equalizing differences in educational attainment, compulsory schooling falls short of equalizing differences in SES. In fact, if anything, the law change disproportionally benefited those with high EA PGS, resulting in an increase in wage differences across genetic groups.

Our two-stage least squares estimates imply that the extra education induced by the reform more than doubled the pre-reform gradient between wages and the EA PGS among compliers. Many different channels would be consistent with such result. First, it might be that high PGS students have higher cognitive and/or non-cognitive skills that would allow them to benefit more from a given amount of education. Second, high PGS students might attend better quality schools and, for that reason, benefit more from staying longer in school. Finally, students affected by the law change might differ, in terms of observable and unobservable characteristics, across PGS groups. If, within each group there is a distribution of returns to schooling rather than a single, constant return, then we might be simply estimating different margins of such distribution.

Table 8: Correlation EA PGS and predetermined characteristics among compliers

	male	birthplace_North	birthplace_East	RegEdu15	BMIscore	leg_length	trunk_length	height
Panel A: 4-year bandwidth								
EA PGS	-0.020	0.039	-0.029	0.021	-0.134	0.012	-0.008	0.004
Observations	6,161	6,096	6,096	5,288	6,161	6,150	6,150	6,155
pvalue	0.173	0.00686	0.0496	0.191	0	0.412	0.594	0.788
Panel B: 10-year bandwidth								
EA PGS	-0.016	0.038	-0.041	0.022	-0.125	0.010	0.001	0.007
Observations	20,586	20,361	20,361	17,777	20,586	20,547	20,550	20,560
pvalue	0.0577	6.68e-06	1.44e-06	0.0145	0	0.231	0.865	0.388

Note: linear pre-trends on date of birth included on Panel A. Quadratic pre-trends included on Panel B.

In order to shed light on the last point, Table 8 documents the correlation between the PGS and several haracteristics among our sample of pre-reform compliers. We focus on characteristics that are arguably predetermined in relation to the 1972 ROSLA, such as birthplace coordinates, a PGS for BMI, height, and leg and truck length (see Bogin and Varela-Silva 2010 for evidence that leg and truck length are markers of nutrition and health during childhood). Table 8 shows that, even in cases when these correlations are statistically significant, such as for BMI PGS and birthplace coordinates, such correlations are in magnitude small. A correlation with birthplace coordinate has been shown by other studies (Abdellaoui et al. 2019), but it's important to note that the correlation with birthplace SES (RegEdu15) is smaller. Overall, Table 8 suggests that compliers do not differ sufficiently across EA PGS groups to explain our results. One obvious caveat with this argument is that we only have a limited number of predetermined observable characteristics and compliers might differ across PGS groups more substantially in other, unobservable, dimensions.

If, in our context, an increase in compulsory schooling age was not able to equalize differences in SES, are there other contexts or educational policies that could? More research is needed to understand whether our results generalize to other contexts and margins of education. If they do, it would be important to ask whether other types of education policies, that might focus specifically on the barriers to education and economic mobility encountered by students from less advantaged backgrounds, would be able to fulfill the great equalizer promise.

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7. APPENDIX

APPENDIX A: Balance Tests

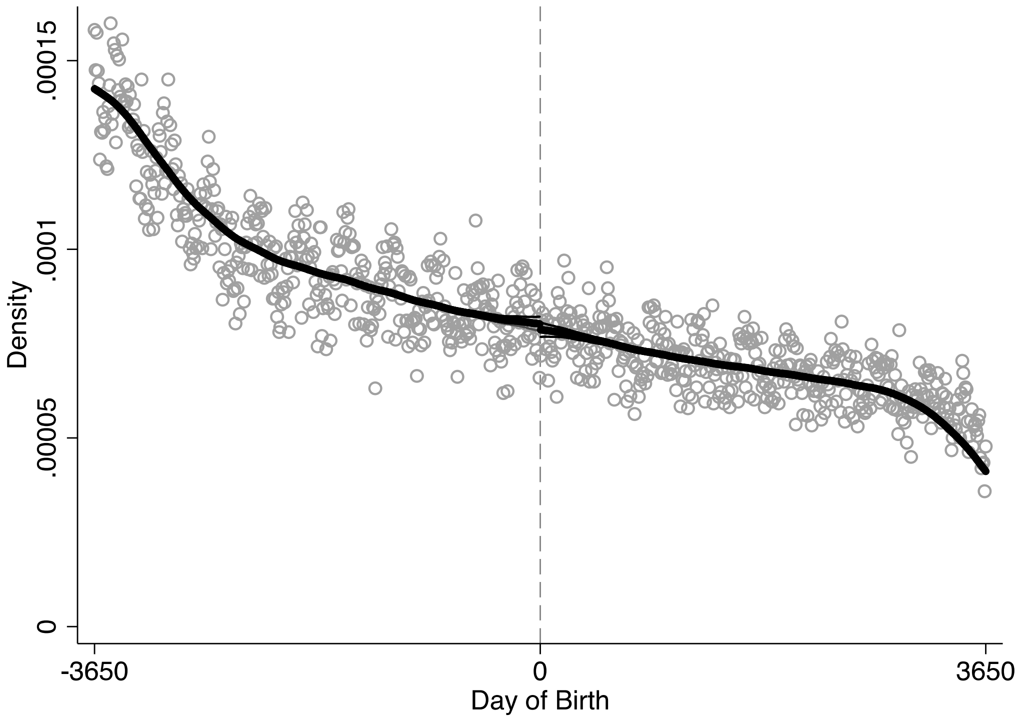
APPENDIX B: Effect of the ROLSA on staying in school past age 18

APPENDIX C: UK Biobank Assessment Centers map

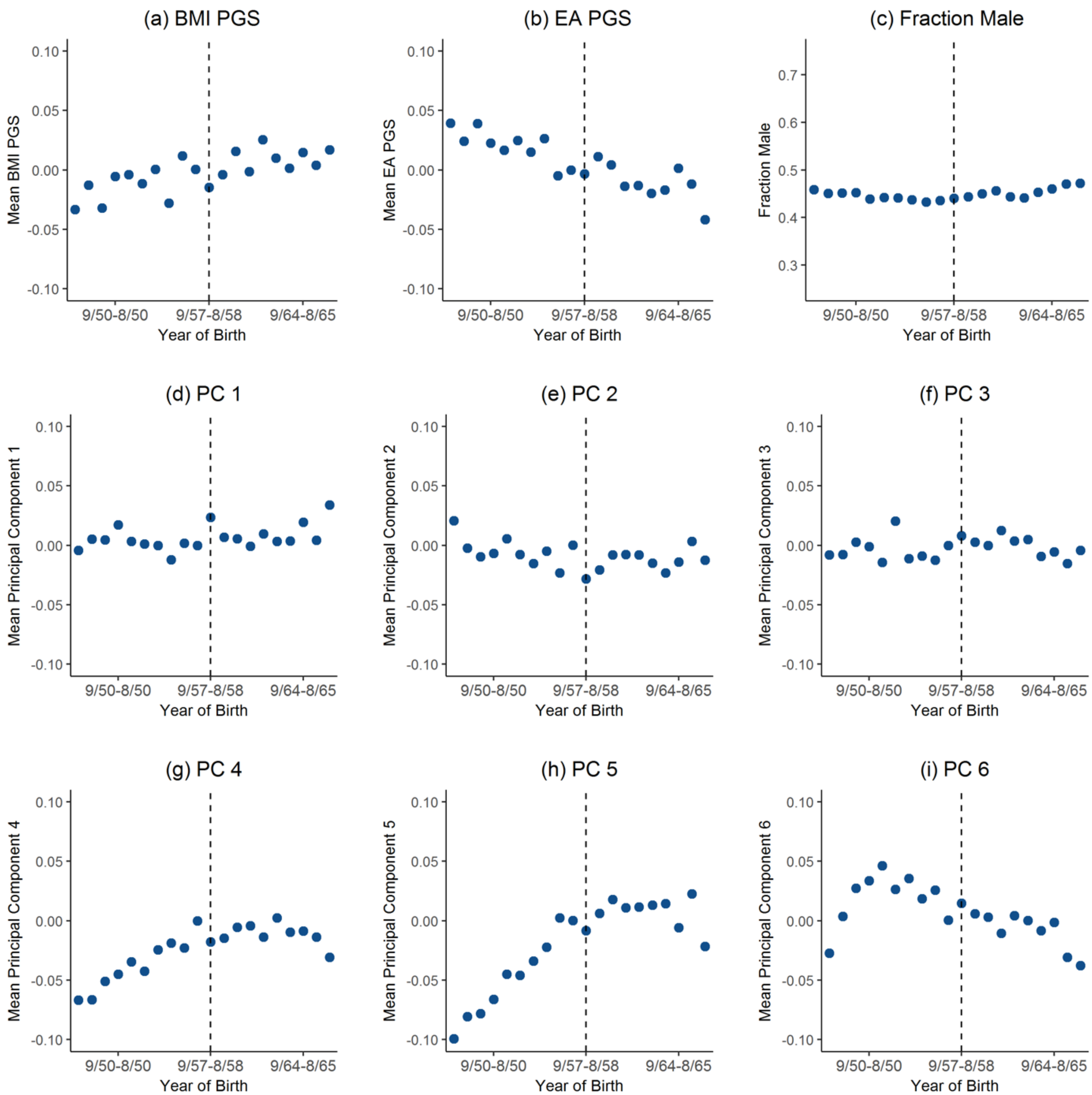
APPENDIX D: Histograms EA PGS and Neighborhood Education

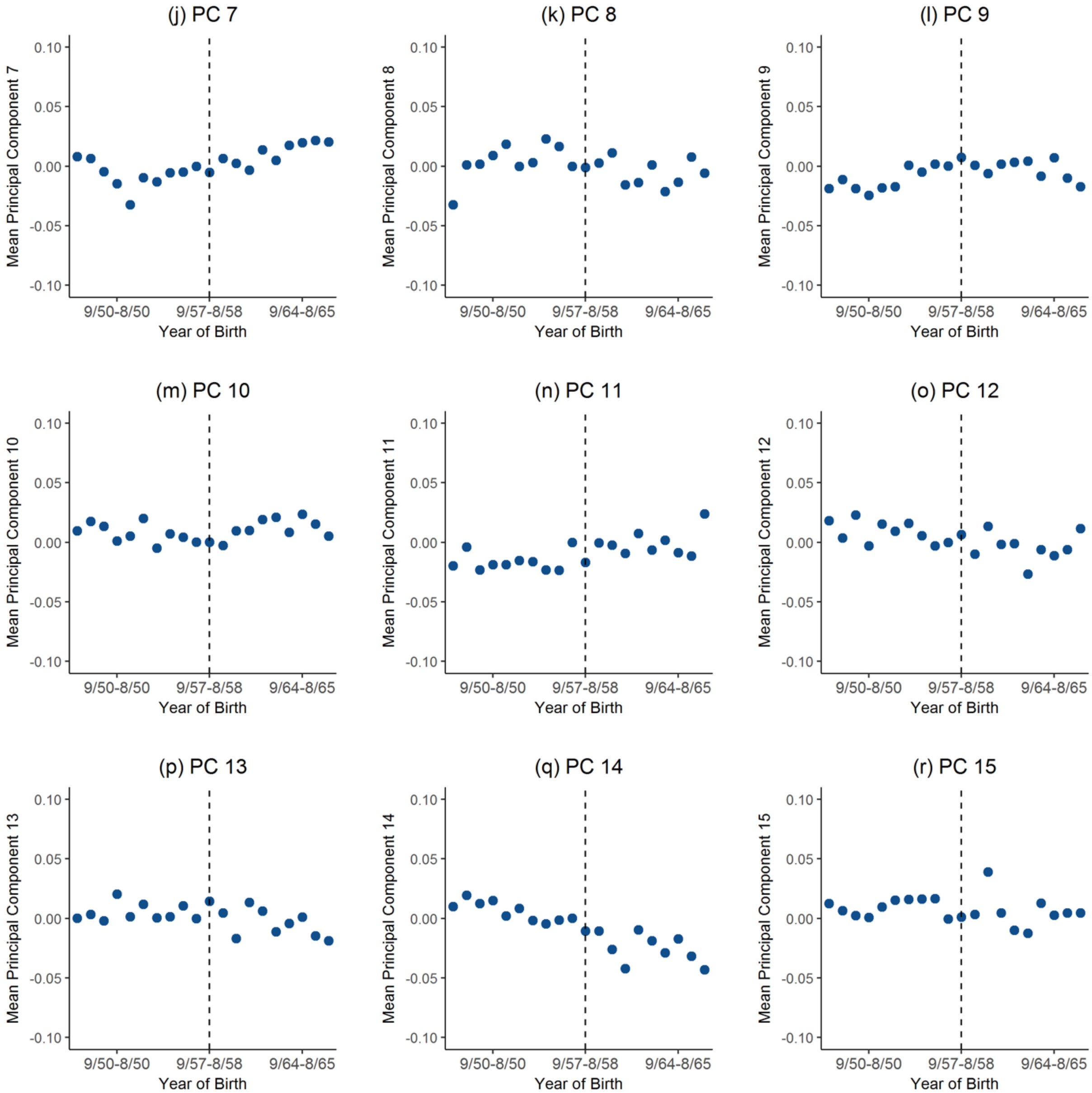
Appendix A

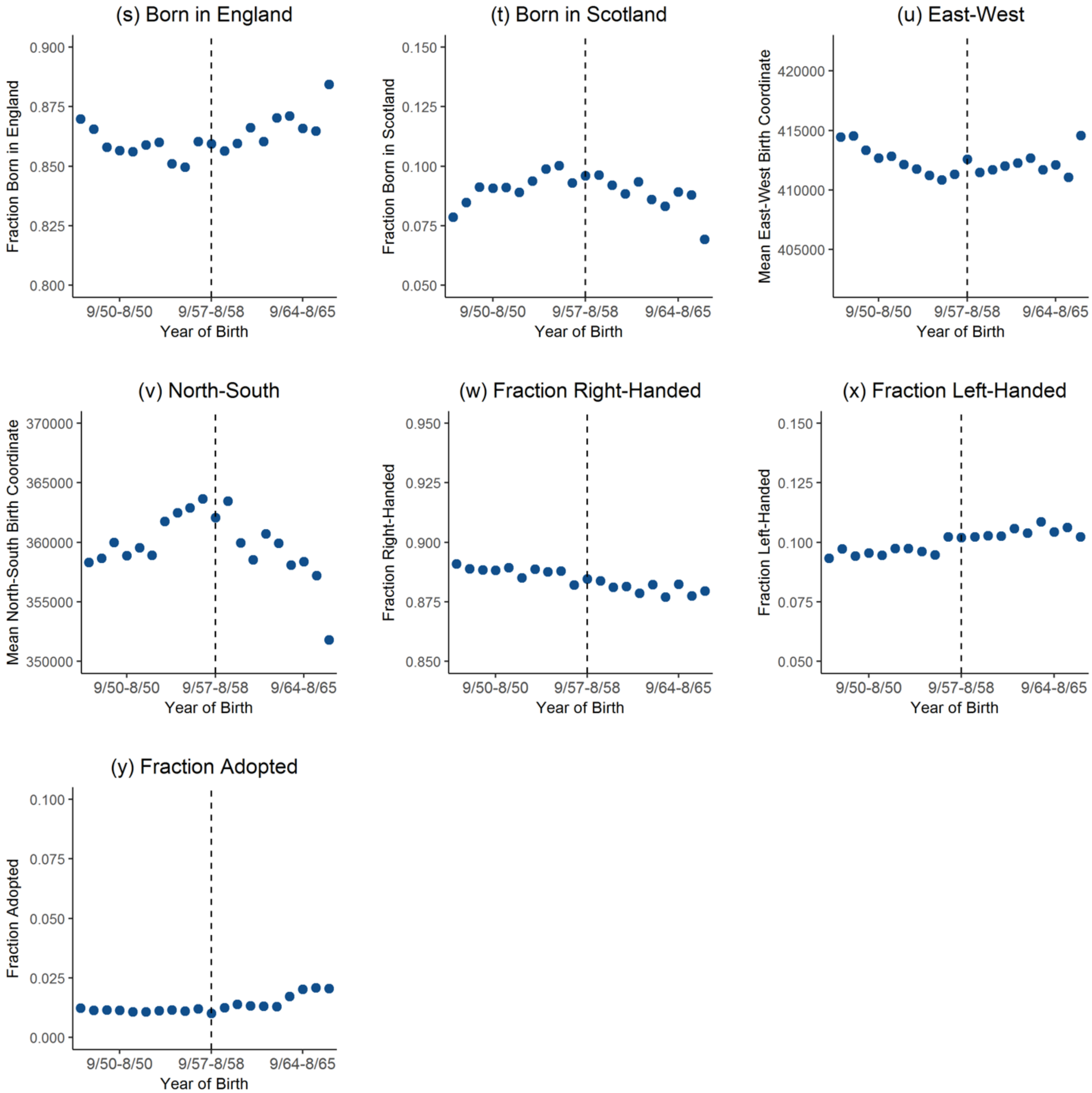
Appendix Figure A1: McCrary Test



Notes: The figure shows the fraction of study participants by day of birth. The dashed vertical line marks the first birth cohort affected by the 1972 school-leaving age reform. Cohorts born to the right of the line had to stay in school until age 16 while cohorts born before could leave at age 15. The curves show nonparametric birth cohort trends. The estimated discontinuity of the density is -0.0201 with a standard error of 0.0174. N = 271,234.


(a) BMI PGS
Mean BMI PGS
Year of Birth
(b) EA PGS
Mean EA PGS
Year of Birth
(c) Fraction Male
Fraction Male
Year of Birth
(d) PC 1
Mean Principal Component 1
Year of Birth
(e) PC 2
Mean Principal Component 2
Year of Birth
(f) PC 3
Mean Principal Component 3
Year of Birth
(g) PC 4
Mean Principal Component 4
Year of Birth
(h) PC 5
Mean Principal Component 5
Year of Birth
(i) PC 6
Mean Principal Component 6
Year of Birth
9/50-8/50
9/57-8/58
9/64-8/65
0.10
0.05
0.00
-0.05
-0.10
0.7
0.6
0.5
0.4
0.3


(j) PC 7
(k) PC 8
(l) PC 9
(m) PC 10
(n) PC 11
(o) PC 12
(p) PC 13
(q) PC 14
(r) PC 15
Mean Principal Component 7
Mean Principal Component 8
Mean Principal Component 9
Mean Principal Component 10
Mean Principal Component 11
Mean Principal Component 12
Mean Principal Component 13
Mean Principal Component 14
Mean Principal Component 15
0.10
0.05
0.00
-0.05
-0.10
9/50-8/50
9/57-8/58
9/64-8/65
Year of Birth



Appendix Figure A2: Balance Tests The figures show averages by year of birth. The dashed vertical line marks the first birth cohort affected by the 1972 school-leaving age reform. Cohorts born to the right of the line had to stay in school until age 16 while cohorts born before could leave at age 15. PC1 to PC 15 refers to the first 15 principal components of the genotypic data. "East-West" and "North-South" correspond to the latitude and longitude coordinates of place of birth. N = 253,567 for all variables with the following exceptions: birthplace coordinates (N = 249,897); right- or left-handed (N = 253,519); and adopted (N = 253,279).

Appendix Table A1: Balance Tests This table investigates whether predetermined characteristics are smooth around the September 1, 1957 cutoff. It reports the coefficient on an indicator for being born on or after September 1, 1957 (i.e., "Post") from regressions where the dependent variables is listed in the column. The regressions include a quadratic polynomial in date of birth, which is allowed to be different before and after September 1, 1957. Robust standard errors. The mean of Y corresponds to the mean of the dependent variable among those born in the 12 months before September 1, 1957.

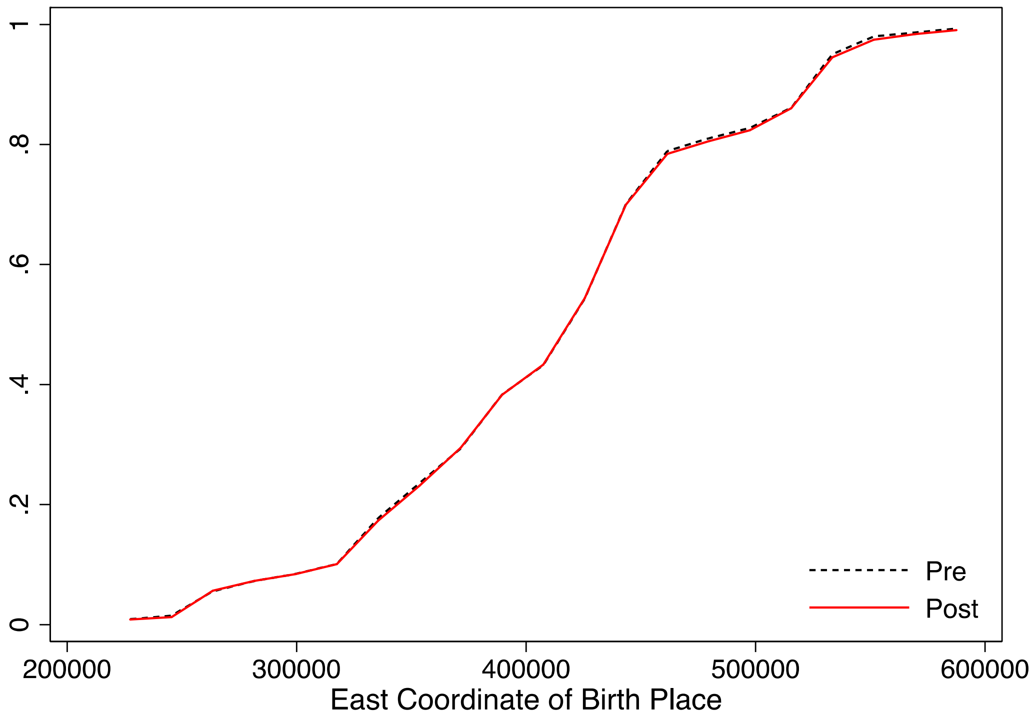
	BMI PGS	*EA PGS*	*Male*	*PC 1*	*PC 2*	*PC 3*	*PC 4*	*PC 5*	*PC 6*
Post	-0.02	0.01	0.01	0.03	-0.03	0.01	-0.02	-0.01	0.02
	[0.013]	[0.013]	[0.006]	[0.013]**	[0.013]**	[0.013]	[0.013]	[0.013]	[0.013]*
N	253,567	253,567	253,567	253,567	253,567	253,567	253,567	253,567	253,567
Mean of Y	0.000	0.000	0.436	0.000	0.000	0.000	0.000	0.000	0.000

	PC 7	*PC 8*	*PC 9*	*PC 10*	*PC 11*	*PC 12*	*PC 13*	*PC 14*	*PC 15*
Post	-0.01	0.00	0.00	-0.01	-0.01	0.01	0.01	-0.01	0.00
	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]
N	253,567	253,567	253,567	253,567	253,567	253,567	253,567	253,567	253,567
Mean of Y	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

	Born in England	*Born in Scotland*	*East Coor. Birthplace*	*North Coor. Birthplace*	*Right-Handed*	*Left-Handed*	*Adopted*
Post	0.00	0.00	977.14	-1348.92	0.00	0.00	0.00
	[0.005]	[0.004]	[1,035.757]	[2,154.922]	[0.004]	[0.004]	[0.001]
N	253,567	253,567	249,897	249,897	253,519	253,519	253,279
Mean of Y	0.861	0.093	411,341	363,640	0.882	0.102	0.012

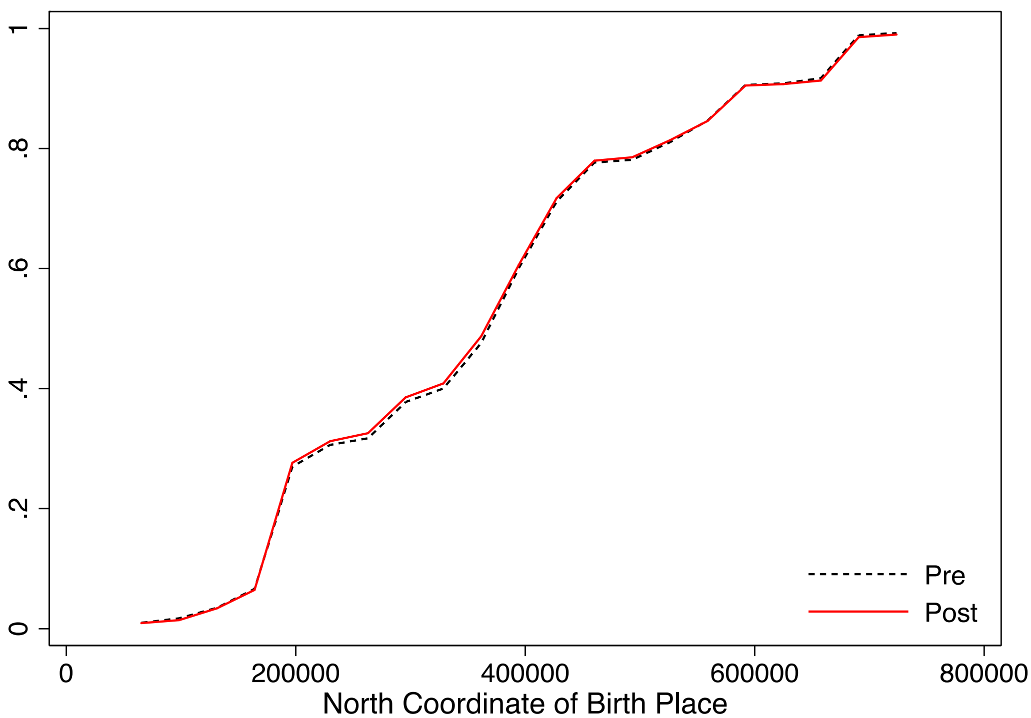
The p-value for a joint test of the null hypothesis that there is no discontinuity for any of the 25 variables above is 0.6921.

Appendix Figure A3: East Coordinate of Birth Place



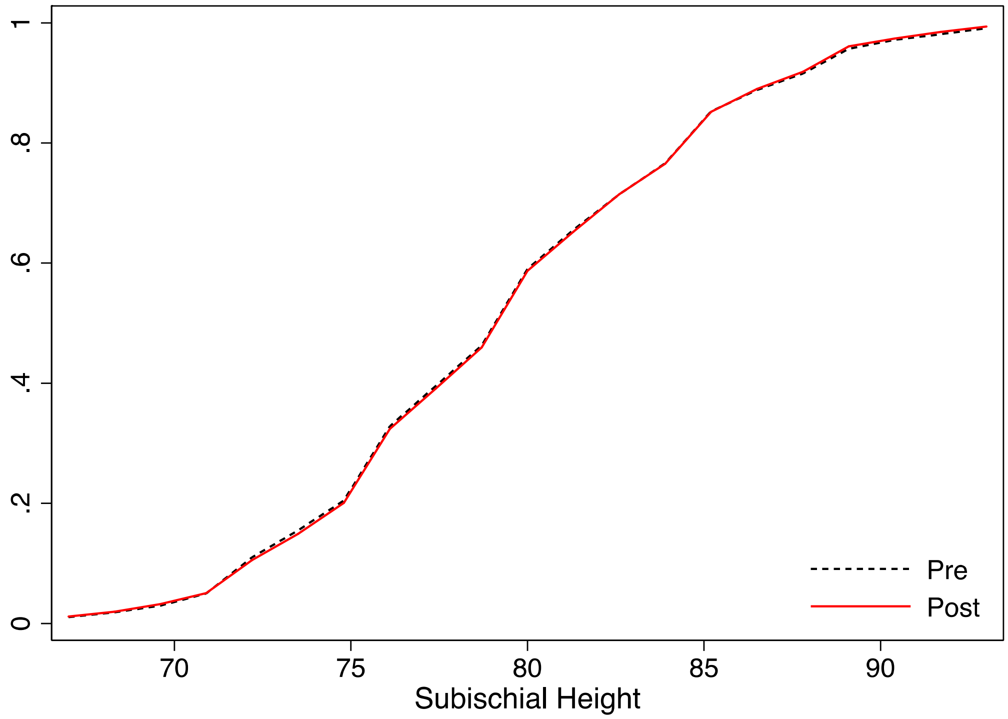
Notes: The figure shows the pre- and post-reform CDFs of east coordinate of place of birth. The *pre-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the left. The *post-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the right. N = 266,883.

Appendix Figure A4: North Coordinate of Birth Place



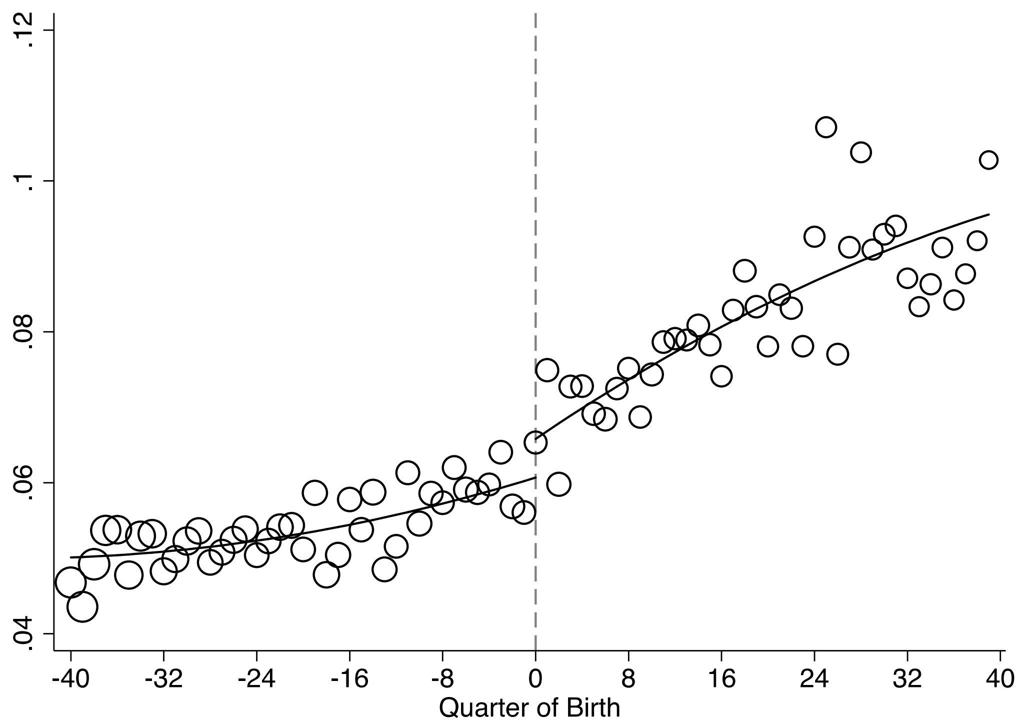
Notes: The figure shows the pre- and post-reform CDFs of north coordinate of place of birth. The *pre-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the left. The *post-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the right. N = 266,883.

Appendix Figure A5: Subischial Height



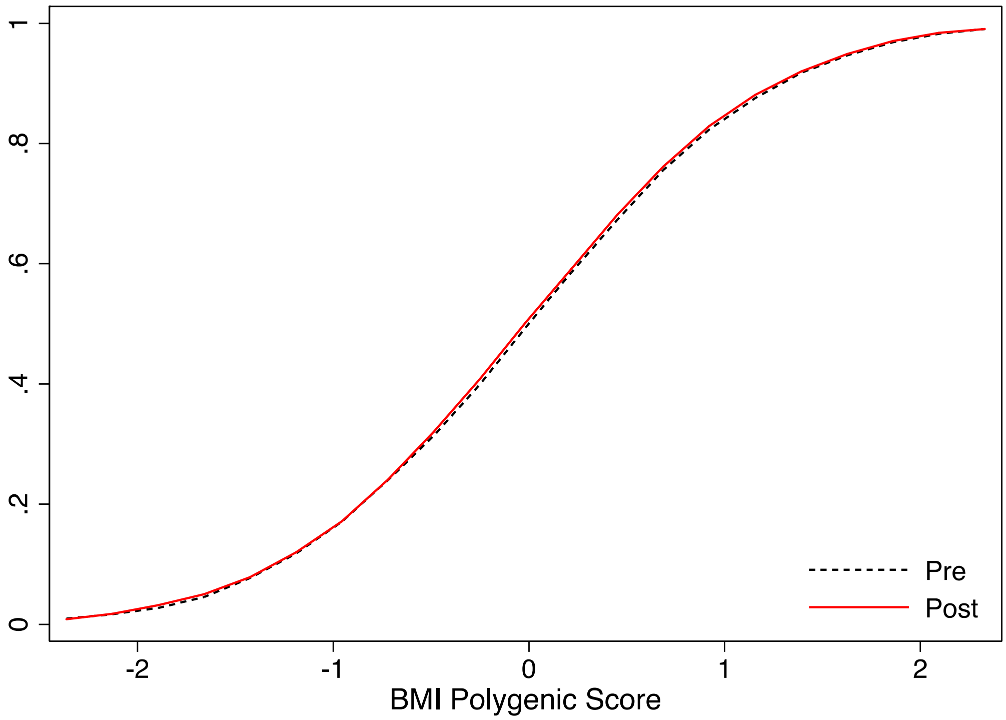
Notes: The figure shows the pre- and post-reform CDFs of subischial height. Subischial height is the difference between standing height and sitting height. The *pre-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the left. The *post-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the right. N = 271,173.

Appendix Figure A6: Fraction Missing Genetic Data



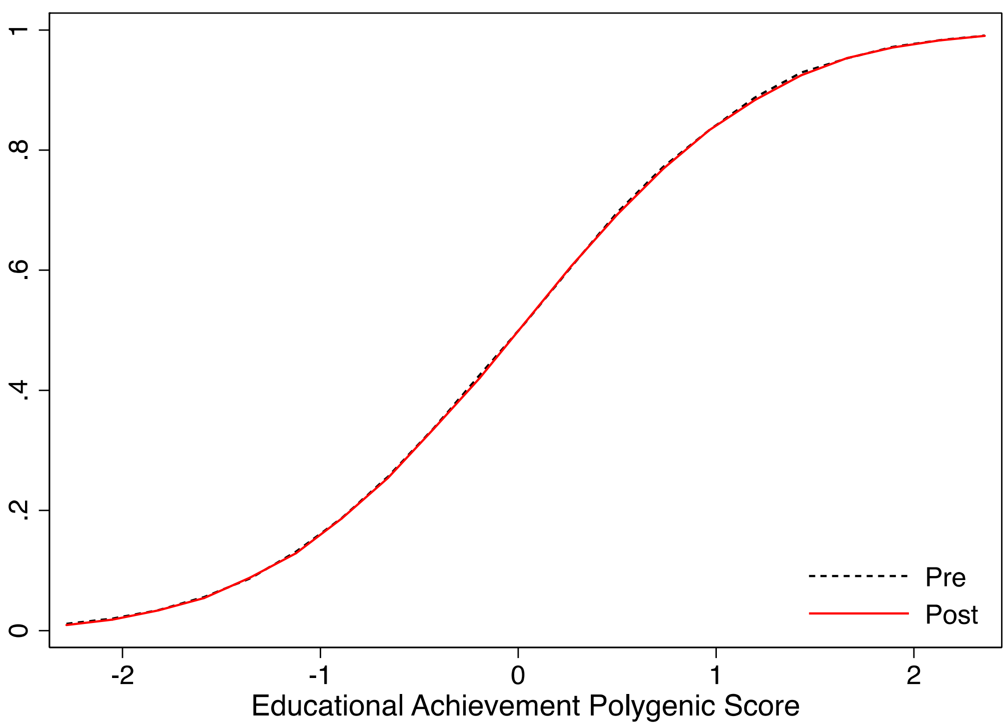
Notes: The figure shows the fraction of study participants with genetic data available by quarter of birth. The dashed vertical line marks the first birth cohort affected by the 1972 school-leaving age reform. Cohorts born to the right of the line had to stay in school until age 16 while cohorts born before could leave at age 15. The curves show quadratic polynomials in quarter of birth that capture birth cohort trends. The circumference of each circle reflects the number of participants born in that quarter. The discontinuity is 0.0044 with a standard error of 0.0031 (p-value of 0.14). The mean among those born in the 12 months before the cutoff is 0.0591. N = 271,234.

Appendix Figure A7: Body Mass Index Polygenic Score



Notes: The figure shows the pre- and post-reform CDFs of the polygenic score for BMI. The *pre-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the left. The *post-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the right. *N* = 253,715.

Appendix Figure A8: Educational Achievement Polygenic Score



Notes: The figure shows the pre- and post-reform CDFs of the polygenic score for educational achievement. The *pre-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the left. The *post-reform CDF* is the CDF in the limit when date of birth is converging to September 1, 1957 from the right. *N* = 253,715.

Appendix Table A2: Distributional Test

Coordinates of Birth Place			Polygenic Scores	
East	*North*	*Subischial Height*	*BMI*	*Educational Achievement*
0.65	0.21	0.63	0.32	0.92

Notes: The table show the p-values of tests of the equality of the pre- and post-reform CDFs. N = 266,883 (coordinates of place of birth); 269,173 (subischial height); and 253,715 (polygenic scores for BMI and educational achievement)

Appendix B

Appendix Table B1: Effect of the 1972 ROSLA on the Probability of Staying in School until age 18 or older (columns 1-2) and age 19 or older (columns 3-4)

	Pr(SLA>=18)		Pr(SLA>=19)	
Post	0.006	0.020	-0.003	0.004
	[0.008]	[0.007]***	[0.008]	[0.007]
Bandwidth	4 years	10 years	4 years	10 years
DoB Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	77,912	192,045	77,912	192,045
Mean pre-reform	0.550	0.550	0.457	0.457

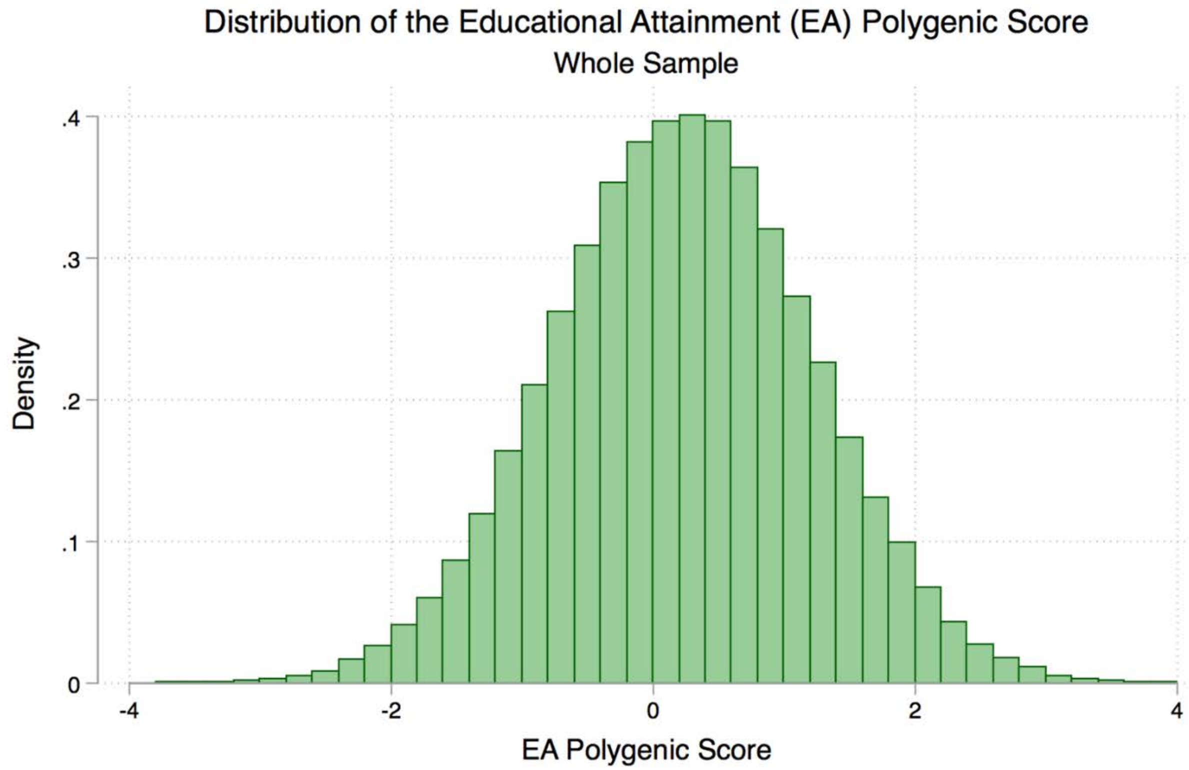
Appendix C

Appendix Figure C1: Map with Locations of UK Biobank 22 Assessment Centers

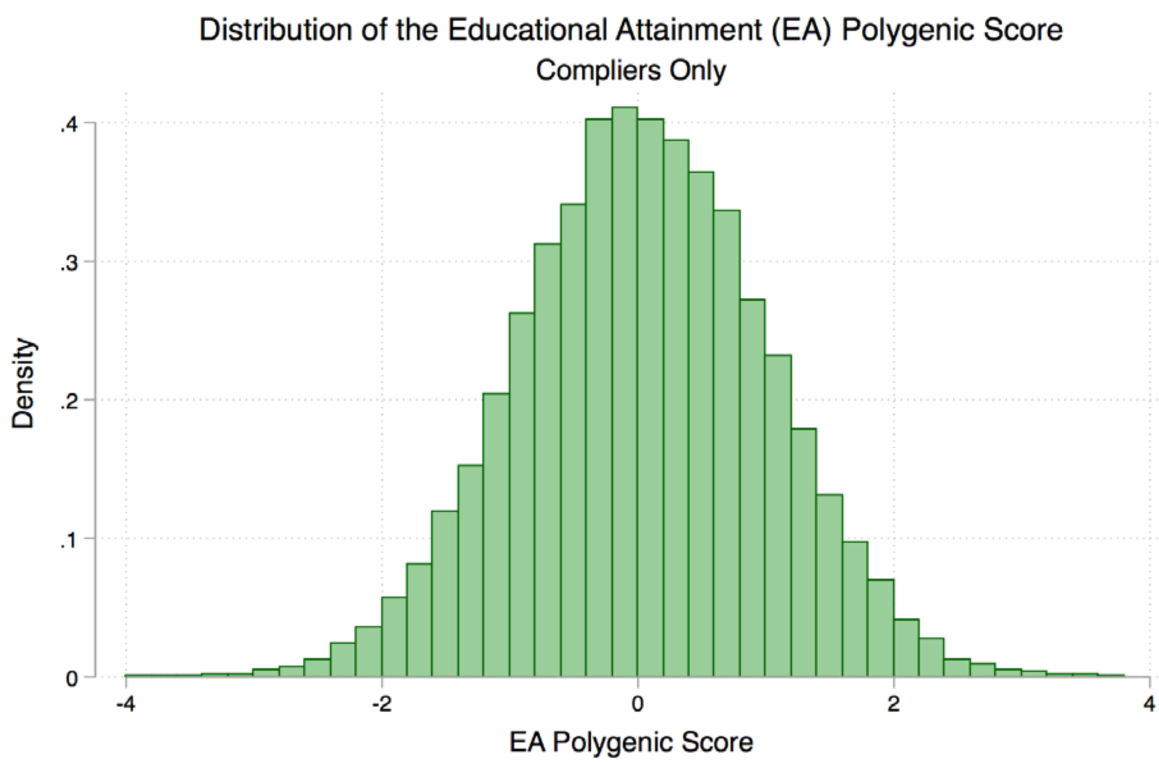
Notes: The figure shows the location of the 22 assessment centers (as well as the location of the pilot study).

Appendix D

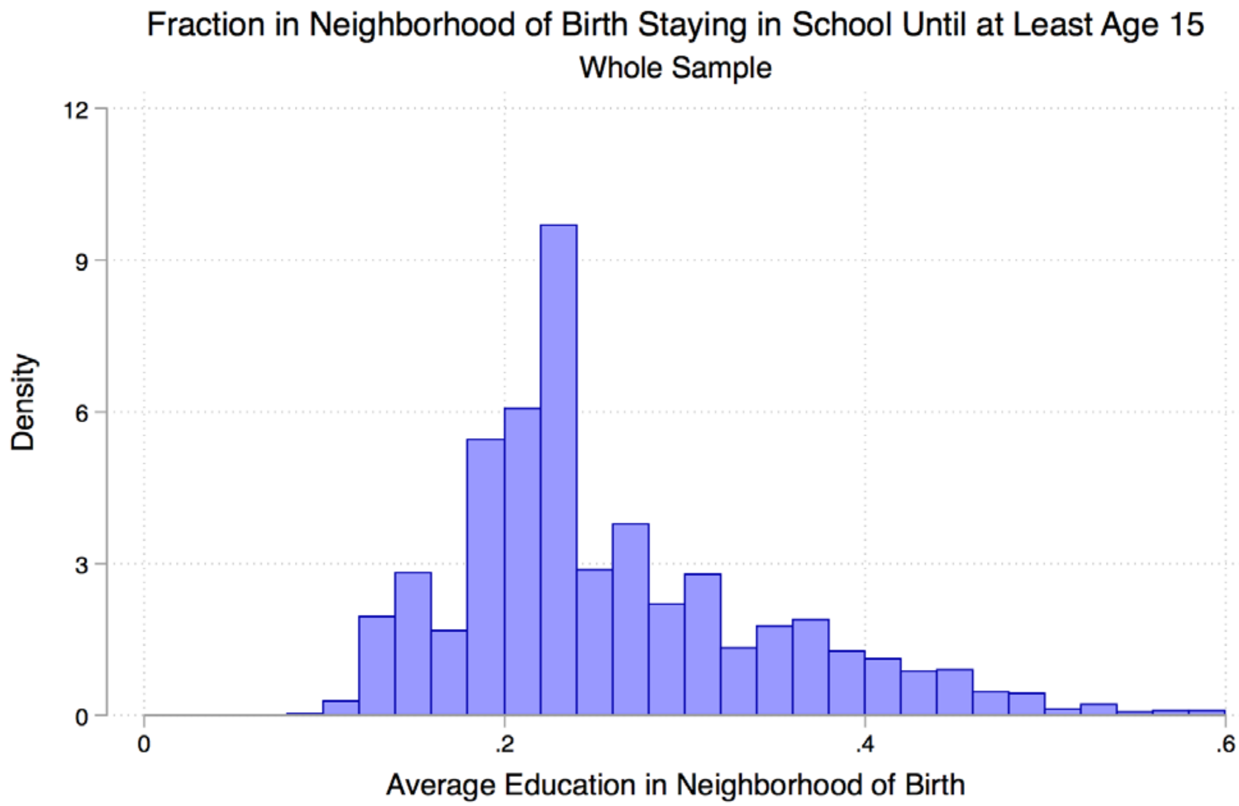
Appendix Figure D1: Histogram EA PGS, whole sample



Appendix Figure D2: Histogram EA PGS, compliers only



Appendix Figure D3: Histogram Neighborhood Quality, whole sample



Appendix Figure D4: Histogram Neighborhood Quality, compliers only