The Long Term Payoffs of Having Privileged Peers.

Evidence from Siblings in Schools

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Abstract

By comparing siblings attending the same school at different points of time, we estimate the effects of schoolmates' average parental education on lifetime earnings and other medium and long-term outcomes and investigate whether these effects vary with individual parental education. We find that exposure to privileged peers increases lifetime earnings and the probability of completing tertiary education. These effects are mainly concentrated among "disadvantaged" students. Lifetime earnings increase also with the dispersion of peers' average parental education. These results suggest that school de-segregation policies can produce long-term benefits. The size of the estimated effects, however, is small.

Keywords: education peer effects, parental background, human capital production, long term outcomes

JEL codes: I21, J16, J24

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1. Introduction

Do social interactions at school affect only individual short-term outcomes, such as test scores or student behavior, or do they also influence long-term outcomes, such as lifetime earnings? Assessing long-term effects is important for the evaluation of education policies that affect peer composition in schools and classes, such as tracking. The concerns over the implications of these policies are likely to be stronger when peer effects do not fade over time but persist to affect adult outcomes (Carrell et al, 2016).

Peer interactions at school can affect individual outcomes in several ways. They can influence the development of skills and the attitudes toward education and individual aspirations. They can also provide information on alternative opportunities that affect individual choices and actions. In addition, the quality of weak social ties may affect labor market entry and careers.[1]

The empirical literature has considered various indicators of school peer characteristics, including the percentage of girls and immigrants in the class/grade/school attended by an individual, the average parental background of peers, and average peer ability.[2] In this paper, we focus on peers' parental education.

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[1] See Granovetter, 1983, for a definition of weak social ties.

[2] On the one hand, Lavy and Schlosser, 2011, and Black, Devereux and Salvanes, 2013, find that a higher share of girls affects the learning outcomes of both girls and boys. The choice of college major is also affected (Anelli and Peri, 2017). Gould, Lavy and Paserman, 2009, Brunello and Rocco, 2013, and Ballatore, Fort and Ichino, 2018, find a negative and significant effect of immigrant concentration in schools on natives' educational achievement. Another peer characteristic that has been shown to generate relevant spillover effects is ability (see Lyle, 2009; Lavy et al., 2012a; Lavy et al., 2012b; Booij et al., 2017). Additional contributions in this area are Hoxby, 2000; Whitmore,

Previous research has shown that the interaction with a higher percentage of classmates having a college educated mother decreases the likelihood of dropping out of high school and increases the probability of attending college (Bifulco et al, 2011). The literature has also looked at the effects of peer composition at school either on earnings in the twenties (between age 24 and 28 in Carrell et al, 2016, and at age 24 in Anelli and Peri, 2017) or on earnings in the late twenties and early thirties (at age 27 to 32 in Bifulco et al, 2014) or finally on earnings in a three-year window (ranging from age 31 to 33 to age 46 to 48 - Black, Devereux and Salvanes, 2013). Carrell et al., 2016, find that exposure to disruptive peers during elementary school reduces earnings at age 26 by 3-4 percent. In a similar vein, Black, Devereux and Salvanes, 2013, find that a one standard deviation increase in the father's earnings of peers increases male earnings by 1 percent.[3]

While interesting, this evidence may not be very informative of the effects of school peer characteristics on lifetime earnings – defined as earnings over the life cycle - because of the so called "life-cycle" bias (see Haider and Solon, 2006): unless age-earnings profiles for individuals exposed to different peer types are parallel, peer effects on earnings in a specific age window can deviate significantly from the effects on lifetime earnings.

The exposure to a privileged background[4] starts in the family and well before school. Surprisingly, a question that the literature has not addressed so far is whether and how

2005; Ammermueller and Pischke, 2009; Oosterbeek et al, 2014; Ciccone et al, 2015; Park, 2015; Eisenkopf et al, 2015; Feld and Zoelitz, 2017; Schøne et al, 2017.

[3] Conversely, Bifulco et al, 2014, and Anelli and Peri, 2017, find no statistically significant effect of the share of female schoolmates on labor market outcomes.

[4] In the parlance of this paper, we classify individuals as privileged or disadvantaged depending on whether their parents have above average education or not.

social interactions in the family and at school interplay in the determination of goals, expectations and skills, and eventually labor market outcomes. Using the framework introduced by Cunha and Heckman, 2007, exposure to a privileged background at home may foster / hamper the effects of later exposure to privileged peers in schools, depending on whether these interactions are substitutes or complements in the production of individual human capital and personality traits.

In this paper, we contribute to this area of research in three ways. First, we measure adult earnings using average earnings between age 31 and 40, which have been shown to minimize the gap between current and lifetime earnings (see Brenner, 2010; Bjorklund and Jantti, 2012; Nybom and Stuhler, 2016; Bhuller et al, 2017). By so doing, we are the first to study the effects of peer characteristics on the best available proxy of lifetime earnings.

Second, we investigate whether and how the effects of peer characteristics at school vary with family characteristics. We measure school interactions when individuals are aged 15 (normally attending the 9th grade) with the average parental education of schoolmates, and family interactions with parental education in the family. By so doing, we try to understand whether the eventual benefits of "good" peer characteristics are equally distributed or vary with the parental background of pupils, and to draw implications for de-segregation policies, that reallocate pupils to school with different peer compositions.

Third, we discuss both human capital accumulation and network effects as mechanisms explaining why peer characteristics may affect lifetime earnings. We measure the latter with the probability that individuals are employed at age 31 in the same firm where either a peer or a peer's parent works.

Our empirical approach departs from the standard in this literature, due to Hoxby, 2000, which consists of comparing *individuals* going to the same school at different points of time. Since this approach fails to purge our data from the effects of endogenous selection of families into schools, we turn to the comparison of *siblings* going to the same school at different points of time. We obtain internally valid estimates of causal effects at the price of restricting the estimation sample to families with at least two children going to the same school.

We find that assignment to more privileged peers increases both lifetime earnings and years spent in adult employment, between age 31 and age 40. The estimated effects, however, are often imprecise (for earnings) and small in size: a one standard deviation increase in peer quality raises earnings by 0.1 to 0.5 percent and employment by 0.4 to 0.5 percent. We show that a mechanism explaining our findings is that better peers increase the probability of completing tertiary education.

The diversity of peers' parental background also matters. While diversity can have both negative and positive effects – teaching may be more difficult but the variety of backgrounds could be enriching – we find that individuals in schools where the dispersion of peers' parental education is higher have higher lifetime earnings, a result which clearly supports school de-segregation policies.

We also find that the estimated effects of peers' parental education on lifetime earnings varies significantly with own parental education. While disadvantaged students benefit from interacting with more privileged peers, privileged students lose. The former group gains more in terms of higher education and is more likely to choose high-paying college majors (STEM, law or social sciences). The latter group tends to work more in the same firm as their peers and peers' parents, a fact which could be interpreted as sign of lower initiative and independence, with negative effects on adult earnings and career development.

The remainder of the paper is organized as follows: we introduce the data in Section 2 and discuss our empirical approach in Section 3. Results are presented and discussed in Sections 4. Conclusions follow.

2. The Data

Our data consist of administrative records drawn from registers of the Danish population. Since 1968, the civil registration system attributes a unique personal identifier to all residents, which we use to reconstruct families and track individuals across various registers. We merge these data with individual tax declarations, which include information on individual earnings, and with school registers to associate individuals to their schoolmates. These registers were introduced in the country in 1973 to monitor compliance with compulsory school reforms.

We have information on 18 cohorts of individuals born between 1958 and 1975, for which we observe labor incomes between 1989 and 2015 (age 31 to 40). We start with those born in 1958 because it is only from this cohort that linkages to parents (and therefore to siblings) are complete. Also, the cohort born in 1958 is the first being matched to the identifier of the school attended at the end of compulsory education. Our last cohort is the one born in 1975, because our earnings data end in 2015 and we wish to observe earnings until age 40. Overall, there are 1,007,939 individuals in our sample, and 859,681 non-missing observations for real earnings. For each individual, we observe her completed education at age 31, well after the completion of highest statutory education in Denmark. For those attending college, we also know the field of study. We broadly classify majors as high and low-wage on the basis of observed major-specific average lifetime earnings. STEM, law and social sciences belong to the former group, and the remaining majors to the latter.

The school registers allow us to link each individual to her schoolmates on October 31 of the calendar year when she turned 15, typically corresponding to enrolment in the 9th grade of compulsory education.[5] We define as school peers the individuals aged 15 who were born in the same year and are enrolled in the same school. This definition differs from the one used by Black, Devereux and Salvanes, 2013, who consider pupils attending the same grade, and is not at risk of being endogenous because of parents' strategic choice of school starting age.

Nonetheless, in our data the difference between definitions is marginal, because the vast majority of children in Denmark (close to 95% for our cohorts, see Bingley et al., 2017) start school at the prescribed age and there are very few grade retentions. In addition, since most Danish students complete primary and secondary education in the same school, our measure is a good proxy of peer composition throughout compulsory school.

We combine school registers with household information to obtain data on parental education, or the average number of years of education completed by the parents, and compute average parental education both in the family, *PE*, and in the school (at age 15), *E(PE)*. The latter is calculated leaving out individual *i*. Both variables are standardized to

[5] There are 1,442 non-special education schools in our sample. We have dropped close to two thousand individuals who attended seventeen special education schools. Results are qualitatively similar if we include them.

have mean equal to 0 and standard deviation equal to 1.[6] Figure 1 shows the distribution of *E(PE)*.

Household information is used also to identify siblings. We define as a family all individuals in the sample who are born from the same mother and father. In total, our sample consists of close to 600,000 families. When we use school-by-family fixed effects, we retain the sub-sample (252,121 families) with at least two siblings attending the same school. We also drop from the final estimation sample 452 families with siblings born in the same year and school-by-family groups with no within-group variation in *E(PE).*

Finally, pre-tax annual labor earnings in Danish kronas - or total income from labor - at 2012 prices are drawn from tax records. We approximate lifetime (log) real earnings with the average of (log) annual real earnings in the age window 31-40. By using averages of annual earnings we limit the measurement error associated with transitory income fluctuations. By centering averages in the 31-40 window, we minimize the life cycle bias.[7]

The summary statistics of the variables used in the empirical analysis are presented in Table 1. In our sample, 34 percent of the individuals have completed tertiary education at age 31, and 14 percent have a tertiary degree in a high-wage major; average log earnings are equal to 12.32 and 12.62 in the age ranges 25-30 and 31-40 respectively; years employed are on average 4.82 and 8.14 between age 25 and 30 and between age 31 and

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[6] Before standardization, average parental education *PE* and peers' *E(PE)* are equal to 10.743 and 10.739 years respectively.

[7] For each individual, we compute average earnings only if at least five valid observations in the age interval are available. We only retain measured annual earnings above 35,000 Danish Crowns (about 4,700 euro).

40 respectively. The percent of females in the sample and of girls in the school and cohort is 49 percent, and average enrolment by school and cohort is 47.24 pupils.

3. Empirical Methodology

3.1. Identification

Estimates of the causal effects of peer characteristics on individual outcomes need to consider that parents and pupils select into different schools. This problem is very relevant in Denmark, as in the country there is a long-standing tradition of free school choice. According to reports by the OECD, 1994, the Fraser Institute (see Rebanks Hepburn, 1999), and the World Bank (see Patrinos, 2001), the country has a large share of autonomous schools, and attendance is publicly subsidized with a generous system of vouchers. In addition, schools and municipalities have large autonomy in the choice of admission criteria. Therefore, the institutional context leaves plenty of space for parental selection and self-sorting in schools.

Several contributions in this literature - including Lavy and Schlosser, 2011, and Black, Devereux and Salvanes, 2013 – address self-selection by following the approach proposed by Hoxby, 2000, who suggests to identify peer effects using the variation in student composition across cohorts within schools, under the assumption that parents and their children only sort across schools based on the average composition of the school, not the demographic composition of the child’s cohort (see Bifulco et al, 2011).

If this identification strategy is valid, peer average parental education *E(PE)* is “as good as random” - conditional on school and cohort fixed effects and on school specific trends - and therefore uncorrelated with predetermined individual characteristics such as gender, parental education, the number of siblings and birth order. We verify whether this is the case by performing balancing tests for regressions of individual covariates on *E(PE)*, conditional on school and cohort fixed effects, school specific cohort trends and birth order dummies. The available covariates are: gender, parental education *PE,* the number of siblings, the age of the mother and the father at birth and a dummy for living in a parish in the top tertile of urbanicity at age 15. We add to these two cohort-by-school covariates, the share of girls and enrolment.

The estimated coefficients associated to the peer variable *E(PE)* are reported in the first column of Table 2.[8] We find that two of nine coefficients are statistically different from zero at the 0.05 level of confidence, and that additional two coefficients are statistically different from zero at the 0.10 level of confidence. In particular, there is evidence of positive sorting of individuals with higher parental education or living in highly urbanized parishes, in conflict with the identification assumption that characterizes Hoxby's approach.

To circumvent this problem, we propose an alternative identification strategy. While Hoxby's approach identifies peer effects by comparing *individuals* going to the same school at different points of time, we propose to restrict this comparison to *siblings*, who

[8] Guryan et al, 2009, and Caeyers and Fafchamps, 2016, observe that excluding the individual when computing the peer variable *E(PE)* mechanically creates a negative association between the individual attribute *PE* and *E(PE).* They recommend to address this exclusion bias by controlling for the composition of the population of peer candidates, after excluding the individual from the calculation. We include in the regression of *PE* on *E(PE)* the average parental education for the individual's cohort in the catchment area of the school (see Gibbons et al., 2008). The catchment area is empirically defined as the set of parishes where at least five percent of the total enrollment in each school - for the cohorts born between 1959 and 1975 - resides.

share the same family characteristics. In practice, rather than controlling for school and cohort fixed effects, we control for school-by-family fixed effects (in addition to gender and birth order dummies as well as sibling spacing) and restrict our sample to families with at least two kids who attend the same school at different points of time.

Conditional on these effects, peer average parental education *E(PE)* is uncorrelated by construction with individual characteristics such as average parental education *PE*, number of siblings and age of mother or father at birth, simply because no residual variation remains in the dependent variable after school by family fixed effects are included.[9]

Our identification strategy guarantees that the peer characteristic *E(PE)* is as good as random, at the price of excluding both families with single kids and families with siblings but with no sibling going to the same school at different points of time. Table A1 in the Appendix shows that our final sample retains 36.7 percent of the original sample of households. Figure 2 illustrates instead the distribution of individuals in the final sample by maximum spacing between siblings attending the same school. Modal maximum spacing is three years, and three quarters of the sample has maximum spacing below six years. Hence, conditional on controls for birth order and sibling spacing, there is limited room for uncontrolled variation in family conditions or for potential concurring trends in school composition and labor market outcomes among siblings (see van den Berg et al., 2014).

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[9] Conditional on spacing, the age of mother and father varies across families but not within families. Residual variation remains for gender and for living in a parish in the top tertile of urbanicity, because of mobility between parishes.

3.2. Estimation

We consider the following baseline empirical specification

$$Y_{icsf} = \pi_1 E(PE)_{cs,-i} + X'_{icsf}\lambda + \alpha_{fs} + \varepsilon_{icsf} \qquad (1)$$

where the indices *i*, *c*, *f* and *s* are for the individual, the cohort, the family and the school respectively; *Y* is the outcome; *E(PE)* is the schoolmates' average parental education, which we standardize to have zero mean and unit standard deviation; *X* is a vector of controls (with associated parameter vector λ), which includes: gender, birth order dummies, siblings age spacing, an urban / rural dummy, the share of girls in the school and cohort, enrolment by school and cohort, *E(PE)* in the parish of residence at age 15 and spacing trends, that can be specific either to a given cohort of firstborns or to a given school; α_{fs} is a school – by – family fixed effect; ε is the error term .

We estimate (1) by OLS and cluster standard errors two-way, by school and family (see Cameron, Gelbach and Miller, 2011). The third and fourth columns of Table 1 show the summary statistics for the relevant sub-sample. The peer variable E(PE), which has been standardized in the full sample, has mean equal to -0.11 and standard deviation equal to 0.97 in the estimation sample. A potential concern is that, once school by family fixed effects and additional controls are included, there is little remaining variation in the peer variable to identify its effects on individual outcomes. This is not the case in our data, however, as the standard deviation of the residuals of the regression of E(PE) on these controls is 0.24, about a quarter of the original variation (see Figure 3).[10]

[10] These results are in line with Black et al, 2013.

We also estimate an augmented version of (1) which includes the interaction of *E(PE)* with average parental education *PE*

$$Y_{icsf} = \pi_1 E(PE)_{cs,-i} + \pi_2 E(PE)_{cs,-i} * PE_f + X'_{icsf}\lambda + \alpha_{fs} + \varepsilon_{icsf} \qquad (2)$$

4. Results

We organize the presentation of our results in four sub-sections. First, we present the estimates of Eq. (1), the baseline specification. Second, we consider the interactions between peer characteristics and family characteristics – see Eq. (2). Third, we examine whether long-term outcomes are affected not only by the peers' average parental education but also by its dispersion. Additional results and sensitivities are relegated to the fourth sub-section.

4.1. The effects of peer characteristics on long-term outcomes

We present our estimates of Eq. (1) in Table 3. The table is organized in six columns: in column (1) we include as controls school-by-family fixed effects, gender, birth order dummies and sibling spacing; in column (2) we add a dummy for urbanicity, the share of girls and school enrolment in the school and cohort; in column (3) we also include school specific trends in spacing; in column (4) we replace these trends with cohort of the firstborn – specific trends in spacing; in column (5) we use the specification in column (2) but restrict our sample to 5 years of maximum spacing. Finally, in column (6) we replicate the specification in column (2) but include only the families where all siblings attended the same school.[11] These specifications try to account in different ways for the remaining uncontrolled heterogeneity between siblings, due to omitted sibling-specific

[11] We exclude, for instance, families with four siblings who have sent two siblings to one school and the other two siblings to another school.

factors or to potential concurring trends in school composition and labor market outcomes.

Results are qualitatively similar across different specifications. We find that a one standard increase in the peer characteristic *E(PE)* raises lifetime earnings by 0.1 to 0.5 percent and years spent in employment by 0.4 to 0.5 percent, depending on the specification. The estimated effects for earnings are statistically significant only in two specifications (columns (4) and (6)). For employment, they are always statistically significant at least at the 10 percent level. These are small effects but broadly in line with what found in the literature. For instance, Black et al, 2013, find that a one standard deviation increases in peers' fathers' earnings increase earnings in a three-year window by about 1 percent.[12]

Mechanisms explaining the positive effect of peer characteristic *E(PE)* on earnings and employment include educational attainment and network effects. On the one hand, higher education is expected to yield higher earnings. On the other hand, interacting with peers with privileged background could provide access to better employment opportunities via the strength of weak social ties (see Granovetter, 1983; Cappellari and Tatsiramos, 2015; Kramarz and Nordstrom Skans, 2014; Plug et al, 2018).

We explore mechanisms in Table 4 by using the baseline specification in column (1) of Table 3 to estimate the effects of *E(PE)* on: (i) completion of tertiary education; (ii)

[12] A comparison of the lifetime earnings effect of adding one standard deviation to peer quality with the one associated with adding one year of completed education shows that the former is much smaller than the latter, which is estimated to range between 9 and 10 percent (see Bhuller et al, 2017 and Brunello et al, 2017).

completion of a high-wage tertiary degree; (iii) earnings and employment in the age range 25 to 30; (iv) living at age 31 in the same region as at age 15; (v) being employed at age 31 by the same firm where a peer is employed; (vi) being employed at age 31 by the same firm where a peer's parent is employed.[13]

We estimate that a one standard deviation increase in *E(PE)* raises the probability of completing tertiary education by 1.7%, a statistically significant effect. There is also evidence that years spent in employment between age 25 and 30 increase by 0.77 percent, higher than the long - run effect. Finally, we find that a one standard deviation increase in the average education of schoolmates' parents raises by 4.7% the probability that an individual is employed in the firm where a peer's parent is working. Without further information, however, it is difficult to say whether working in the same firm as a schoolmate's better educated parent is a ticket for higher long-run pay. This would be the case if better educated parents match with high wage firms, which pay higher wages to all. An alternative interpretation is that individuals working in the same firm as their peers and their peers' parents have less initiative, independence and therefore end up in the long-run with lower wages. We conclude from this that human capital accumulation appears to be the most plausible mechanism explaining our results.

4.2. Do school peer effects vary with family characteristics?

We estimate Eq. (2) using the baseline specification in the first column of Table 3.[14] Results for average real earnings and years employed between age 31 and 40 are reported

[13] Results are qualitatively similar when we consider working with a peer with college-educated parents or with a peer's parent who has college education.

[14] The estimates of the other specifications are similar and available from the authors upon request.

in Table 5. We find that the effect of *E(PE)* is similar to the one estimated in Table 3, and that the interaction between *E(PE)* and *PE* attracts a negative sign, statistically significant in the case of earnings.[15]

The variation of the estimated marginal effect of *E(PE)* when *PE* varies between -2 and +2 and the associated 95% confidence intervals are shown in Figures 4 and 5. Figure 4 shows that a one standard deviation increase in the peer characteristic *E(PE)* reduces the long-term earnings of individuals with parental education *PE* above the mean, and increases them for those with parental education below the mean. The estimated effects range between -2.2 and +2.6 percent. Figure 5 illustrates that a marginal increase in *E(PE)* has small positive effects on years employed between age 31 and 40, that vary little with individual parental education.

The effects of *E(PE)* and its interaction with *PE* on medium-run outcomes, including educational attainment, earnings and employment between age 25 and 30 are reported in Table 6. Our evidence indicates that less privileged individuals (with *PE* below the mean) benefit more than the privileged (with *PE* above the mean) from interacting with "better" peers – who have parents with higher average education – in terms of a higher probability of attaining tertiary education, of choosing a high-wage major, and of higher real earnings between age 25 and 30. At the same time, the less privileged are less likely than the privileged to be employed in the same firm as a peer or a peer's parent, which may signal higher independence and initiative in the choice of jobs, and higher long-run earnings.

[15] Table A2 reports our estimates when we replace parental education *PE* with dummies for the bottom and top quartiles of *PE*. Results are qualitatively unchanged.

Conversely, privileged individuals do not benefit from interacting with “better” peers, because - by virtue of this interaction - they are less likely to complete tertiary education and to choose a high – wage college major. They also tend to work more in the same firm as their peers and their peers’ parents.

An additional reason why privileged students may suffer from interacting with peers with privileged background could be that their choices and behavior are affected by rank concerns. Elsner and Isphording, 2017, argue that students ranked higher among their peers have higher expectations about their future career and attend college more. While assignment to a school with marginally higher peer quality is unlikely to affect the relative rank of disadvantaged students, this may not be the case for privileged students, who, by facing more difficulties in attaining top rank, could lose their motivation and perform more poorly both at school and in the labor market.

4.3. The dispersion of peers’ parental education

Following Booji, Leuven and Oosterbeek, 2017, we examine whether individual outcomes are affected not only by the average parental education of peers but also by its standard deviation (see also Lyle, 2009). Conditional on the mean, a higher dispersion of individual backgrounds in the school may loosen social ties or reduce teacher effectiveness, which is typically higher in more homogeneous classes, with negative effects on human capital accumulation. On the other hand, more diversity of backgrounds indicates that different experiences and ideas are present in the school, which could be mind-broadening and enriching, with positive effects on human capital and earnings (Alesina et al, 2016).

Figure 6 shows that the relationship between *E(PE)* and the standard deviation of *PE* within the school, *SD(PE)*, is hump-shaped. With the exception of the extreme values of the mean, there is substantial variation in *SD(PE)* for each selected *E(PE)*. We explore the effects of *SD(PE)* on long-run individual outcomes by adding it as an additional regressor in Eq. (1).

Results are reported in Table 7 for the baseline specification used in column (1) of Table 3. We find that both a higher *E(PE)* and a higher *SD(PE)* increase average log real earnings between age 31 and 40. Only the second effect, however, is statistically significant, suggesting that a higher diversity of backgrounds in the school can be an asset. While a one standard deviation increase in average parental education increases earnings by 0.2 percent, a similar increase accompanied by a one standard deviation increase in the dispersion of parental education raises earnings by 0.54 percent.[16]

We next interact *SD(PE)* with parental education *PE* and show the results in Table 8. We also display the changes in the estimated marginal effect of *E(PE)* when *PE* varies between -2 and +2, and the 95% confidence intervals, in Figures 7 and 8. Conditional on *E(PE)*, we find that an increase in the dispersion of peers' parental education improves the lifetime earnings of those with a privileged background (*PE* positive) but has imprecise (negative) effects on disadvantaged males (*PE* negative).[17]

In the previous sub-section, we have shown that assignment to schools where peers have a better average parental background improves the earnings of the disadvantaged but penalizes the earnings of the privileged. The results in Table 7 indicate that the diversity

[16] In the full sample, the standard deviation of *SD(PE)* is equal to 0.24.

[17] The effects of *SD(PE)* on years employed are positive but imprecisely estimated.

of backgrounds can attenuate these effects. In particular, a one standard deviation increase in both *E(PE)* and *SD(PE)* increases the long-term earnings of individuals with disadvantaged background (*PE*=-1) by 0.6 percent and reduces the earnings of the privileged (*PE*=1) by 0.6 percent. These effects are 40 percent lower than those associated to a one standard deviation increase in *E(PE)* that does not alter *SD(PE)*.

4.4. Additional results

Our identification strategy relies on the comparison of siblings going to the same school at different points of time. This strategy is motivated by the fact that, in our sample, the Hoxby approach fails to pass some balancing tests, suggesting that residual sorting of individuals into schools remains even after controlling for school and cohort fixed effects. To see how our results would have changed had we used the Hoxby approach, we replicate the estimates in Table 5, which include both the peer effect and its interaction with parental education *PE*, using school and cohort fixed effects in place of school – by – family fixed effects. As shown in Table 9, results change drastically. First, the estimated effect of *E(PE)* using Hoxby's method is twice as large as the baseline and statistically significant at the 5 percent level. Most likely, this effect is biased upwards by endogenous selection into privileged schools. Second, the interaction with parental education is also positive and statistically significant, indicating that the privileged benefit more than the disadvantaged from allocation to privileged schools, contrary to what found using school-by-family fixed effects.[18]

[18] The estimates reported in Table 9 are based on the family-by-school fixed effects sample. Using the full sample produces similar results -– available from the authors upon request.

We only consider families who send at least two children to the same school at different points of time. This may be problematic if families change the school of their junior children when the peers of senior children are of lower quality in terms of their parental education. To verify whether this is the case, we consider all pairs of older and younger siblings in our sample, define a dummy equal to 1 if the pair goes to the same school and to 0 otherwise, and regress this dummy on the peer characteristic *E(PE)* for the older sibling and additional controls. As shown in Table 10, the probability of going to the same school is driven mainly by residence in the same parish and by whether the school of enrolment of the older (younger) sibling was available for the younger (older) one. Importantly, the coefficient associated to *E(PE)* turns out to be not statistically different from zero, suggesting that selection based on the quality of peers is not an issue in our data.

We have performed several additional sensitivities to validate the robustness of our findings. Results are not reported for brevity, but are available from the authors upon request. First, we have replaced average parental education with the maximum level of education attained by parents, with no qualitative difference. Second, to dispel concerns that – since many students attend a local school – our measures of school composition could be capturing neighbourhood composition effects, we have added to our regressions the average parental education in the parish where the individual was living at age 15 - a good approximation of neighbourhood composition (see Bingley, Cappellari and Tatsiramos, 2017). The coefficient associated with this variable is rarely significant, and our results on school composition effects are never affected. Third, as done by Booij et al, 2017, we have estimated the models reported in Tables 7 and 8 by adding the triple interaction between *E(PE)*, *SD(PE)* and *PE*, but found that this addition was never statistically significant.

5. Discussion

Our results have implications for de-segregation policies, promoting the reallocation of disadvantaged students from lower to higher quality schools, which are typically populated by peers with higher average parental education. These policies include vouchers, lotteries, "bussing" initiatives, measures to improve the quality of schools and the improvement of parental choice. The existing evidence (see for instance Guryan et al, 2004, and Billings et al, 2014) indicates that de-segregation in the US reduces the school dropout of blacks without affecting that of whites. Re-segregation, on the other hand, increases the inequality of school outcomes between whites and minorities.

We are aware that our estimates are derived from within-school and across-cohorts variation in peer composition and do not rely on policy variation. Yet, we believe that they can still be used as *ex-ante* evidence of the effects of policies aiming at changing school composition and therefore peer quality.

Our results imply that re-assigning a disadvantaged student (with $PE<0$) from a "low quality" ($E(PE)<0$) to a "high quality" ($E(PE)>0$) school improves the probability that he/she completes tertiary education and raises his/her lifetime earnings, approximated here with average real earnings between age 31 to 40. By virtue of this reassignment, average parental education in the "high quality" school marginally declines, with positive spill-overs on privileged students, who also benefit from the increase in the dispersion of average parental education. Perhaps more surprisingly, our results also indicate that reassigning privileged students to schools with lower average parental background would increase their long-term earnings, with positive spill-over effects for the less privileged in the same school.

6. Conclusions

Using Danish register data, we have studied the medium and long-run effects of school peer characteristics, which we have measured with the average parental education of schoolmates. Our key indicator of long-run labor market success is lifetime earnings, which we have approximated with average real earnings between age 31 and age 40. According to the relevant literature, this proxy minimizes the bias incurred when using earnings in an arbitrary age range in place of lifetime earnings.

In our empirical approach, we have departed from most of the literature in this area by comparing siblings rather than individuals going to the same school at different points of time. By so doing, we have been able to bypass the positive correlation between individual average parental education *PE* and the peers' average parental education *E(PE)* and defend the assumption that, conditional on school-by-family fixed effects, *E(PE)* can be considered as good as random.

Our identification strategy requires that we restrict the sample to families with at least two children who are going to the same school at different points of time. We have shown that the choice of using the same school is not driven by peer characteristics, but rather by residence in the same parish and by the availability of schools.

We have found that assignment to "high quality" schools – that is, schools with high *E(PE)* – has positive but small long term consequences on adult earnings and employment. We believe that the key mechanism explaining these effects is that good peers increase the probability of completing tertiary education.

We have also shown that the long - term effects of assignment to good peers are positive for the disadvantaged and negative for the privileged. Potential mechanisms include educational attainment – the privileged are more likely to complete tertiary education in low-wage majors– and network effects – the privileged tend to be employed by age 31 in the same firm as their peers and their peers' parents, which may indicate lower independence and initiative in the choice of jobs.

Finally, our evidence indicates that not only the peers' average parental background but also its diversity, measured by the standard deviation of parental background, matters for long-term outcomes. While in principle diversity can have both negative and positive effects, our results show that the latter dominate the former.

Our results have clear implications for school de-segregation policies. On the one hand, the diversity of parental backgrounds – typical of de-segregated schools – is found to improve long-term earnings. On the other hand, reassignment of disadvantaged students to high quality schools and of privileged students to lower quality schools is shown to improve the earnings of both groups. Since the size of the estimated effects is small, however, we cannot draw conclusions on the net benefits of these policies without considering their implicit and explicit costs.

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Tables and Figures

Table 1. Descriptive statistics. Full sample and the sub-sample for the estimates using school by family fixed effects.

	(1)	(2)	(3)	(4)
	Full sample		School-by-family fixed effects estimation sample	
	Mean	Std. Dev.	Mean	Std. Dev.
Long-run outcomes				
Average log real earnings, age 31-40	12.63	0.43	12.63	0.42
Years employed, age 31-40	8.14	3.14	8.29	3.03
Explanatory variables				
E(PE) by school and cohort	0	1	-0.11	0.97
SD(PE) by school and cohort	0	0.24	0.015	0.24
Explanatory variables				
PE	0	1	-0.04	1.01
Female	0.49	0.5	0.49	0.5
Birth order	1.39	0.63	1.68	0.72
Spacing	1.24	2.29	2.20	2.68
Share of girls by school and cohort	0.49	0.08	0.49	0.08
Enrolment by school and cohort	47.24	21.20	49.70	21.03

Notes: while columns (1) and (2) include families with a single child, with siblings born in the same year and with no within - group variation in E(PE), columns (3) and (4) exclude them. The number of observations is 1,007,939 in Columns (1) and (2), and 569,468 in Columns (3) and (4). For average log earnings in the age range 31-40, sample size is 859,681 and 493,629 respectively. For average log earnings in the age range 25-30, sample size is 874,886 and 501,637.

Table 2. Balancing tests. Reverse regressions of individual covariates on E(PE)

	(1)	(2)
Dependent variable	E(PE)	E(PE)
Female	0.004*	-0.001
	(0.002)	(0.003)
Average years of education of father and mother – PE	0.019***	*Absorbed*
	(0.007)	
Number of siblings	-0.001	*Absorbed*
	(0.003)	
Age of mother at birth	0.003	*Absorbed*
	(0.017)	
Age of father at birth	0.018	*Absorbed*
	(0.020)	
Lives in parish in top tertile of urbanicity at 15	0.003**	0.001
	(0.001)	(0.001)
Share of girls by school and cohort	-0.000	-0.002
	(0.002)	(0.002)
Enrolment by school and cohort	0.518*	-0.250
	(0.280)	(0.306)
School and cohort fixed effects & school specific cohort trends	Yes	No
School-by-family fixed effects	No	Yes

Notes: each regression includes the following variables: E(PE), birth order dummies and the fixed effects and trends listed at the bottom of each column. The second column includes spacing from the oldest sibling. The regression for parental education includes also as control for exclusion bias à-la Guryan et al 2009 the average parental education in the catchment area of the school for the cohort of the individual. Catchment areas are empirically defined as the set of parishes where at least 5% of total enrolment in each school for the 1958-75 cohorts resides. Column (1) includes singletons in terms of school-by-family groups, while Columns (2) does not. The number of observations is 1,007,939 in Column (1), and 569,468 in Column (2). There are 1,442 schools in the full sample used in Column (1). After including school-by-family fixed effects and dropping families where there are siblings born in the same year and school-by-family groups where there is no within-group variation in E(PE), we retain 1,435 schools in Column (2). There are 252,121 families and 253,398 school-by-family groups in the sample used in Column (2). Standard errors are clustered by schools in Column (1) and two-way clustered by school and family in Column (2). ***: p<.01; **: p<.05; *: p<.10.

Table 3. Marginal effects of E(PE) on long-run labor market outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)
	E(PE)					
Average log real earnings, age 31-40	0.003	0.003	0.001	0.005**	0.002	0.005**
Mean: 12.63	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Years employed, age 31-40	0.036**	0.036**	0.030*	0.032**	0.035*	0.043**
Mean: 8.29	(0.016)	(0.016)	(0.017)	(0.016)	(0.020)	(0.019)
School-by-family fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Gender, birth order dummies, and spacing	Yes	Yes	Yes	Yes	Yes	Yes
Urbanicity, share of girls and enrolment by school-cohort	No	Yes	Yes	Yes	Yes	Yes
School-specific trends in spacing	No	No	Yes	No	No	No
Cohort of first born in the family-specific trends in spacing	No	No	No	Yes	No	No
Max (spacing)	17	17	17	17	5	17
Includes only family where all siblings attend the same school	No	No	No	No	No	Yes

Notes: each regression includes the following variables: E(PE), gender, birth order dummies, spacing from the oldest sibling, and the fixed effects and trends listed at the bottom of each column. Urbanicity, the share of girls and enrolment by school-cohort are also included as additional covariates in Columns (2) to (6). Standard errors are clustered by school and family (two-way). There are 1,435 schools, 252,121 families and 253,398 school-by-family groups in the estimation sample. E(PE) is standardized (zero mean and unit standard deviation in the full sample). Total number of observations: 569,468, except for Column (5), where it is 402,158, and Column (6), where it is 391,369. ***: p<.01; **: p<.05; *: p<.10.

Table 4. Mechanisms. The marginal effects of E(PE) on medium-run outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Has tertiary education degree	Has tertiary degree in high-wage major	Average log income, age 25-30	Years employed, age 25-30	Employed in the same firm as a peer at age 31	Employed in the same firm as a peer's parent at age 31	Lives in the same region at age 15 and age 31
E(PE)	0.006**	0.000	0.001	0.038***	-0.002	0.009***	0.002
	(0.002)	(0.002)	(0.003)	(0.009)	(0.003)	(0.003)	(0.003)
Mean Outcome	0.35	0.14	12.34	4.90	0.19	0.19	0.72

Notes: each regression includes E(PE), gender, birth order dummies, spacing from the oldest sibling, and school-by-family fixed, as in column (1) of Table 3. Number of observations: 569,468, with the exception of columns (5) and (6), where it is equal to 282,268, because we observe firm IDs only for employees, and of column (7), where it is equal to 405,482 because of missing values in the region of residence at age 15 or 31. ***: p<.01; **: p<.05; *: p<.10.

Table 5. Marginal effects of E(PE) and its interactions with PE on long-run labor market outcomes

	(1)	(2)
	Average log real earnings, age 31-40	Years employed, age 31-40
E(PE)	0.002	0.035**
	(0.002)	(0.016)
E(PE)*PE	-0.012***	-0.006
	(0.002)	(0.012)
Mean Outcome	12.63	8.29

Notes: each regression includes E(PE), gender, birth order dummies, spacing from oldest sibling, and school-by-family fixed, as in Column (1) of Table 3. Total number of observations: 569,468. ***: p<.01; **: p<.05; *: p<.10.

Table 6. Mechanisms: marginal effects of E(PE) and its interactions with PE on medium-run outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Has tertiary education degree	Has tertiary degree in high-wage major	Average log income, age 25-30	Years employed, age 25-30	Employed in the same firm as a peer at age 31	Employed in the same firm as a peer's parent at age 31	Lives in the same region at age 15 and age 31
E(PE)	0.006**	-0.001	-0.001	0.038***	-0.001	0.009***	0.002
	(0.002)	(0.002)	(0.003)	(0.009)	(0.003)	(0.003)	(0.003)
E(PE)*PE	-0.005***	-0.006***	-0.016***	0.006	0.010***	0.004**	-0.001
	(0.002)	(0.001)	(0.002)	(0.007)	(0.002)	(0.002)	(0.002)
Mean Outcome	0.35	0.14	12.34	4.90	0.19	0.19	0.72

Notes: each regression includes E(PE), gender, birth order dummies, spacing from the oldest sibling, and school-by-family fixed, as in column (1) of Table 3. Number of observations: 569,468, with the exception of columns (5) and (6), where it is equal to 282,268, because we observe firm IDs only for employees, and of column (7), where it is equal to 405,482 because of missing values in the region of residence at age 15 or 31. ***: p<.01; **: p<.05; *: p<.10.

Table 7. Marginal effects of E(PE) and SD(PE) on long-run labor market outcomes

	(1)	(2)
	Average log income, age 31-40	Years employed, age 31-40
E(PE)	0.002	0.032**
	(0.002)	(0.016)
SD(PE)	0.014***	0.034
	(0.004)	(0.027)
Mean Outcome	12.63	8.29

Notes: see Table 5.

Table 8. Marginal effects of E(PE), SD(PE) and their interactions with PE on long-run labor market outcomes

	(1)	(2)
	Average log income, age 31-40	Years employed, age 31-40
E(PE)	0.002	0.033**
	(0.002)	(0.016)
E(PE)*PE	-0.012***	-0.004
	(0.002)	(0.012)
SD(PE)	0.009**	0.034
	(0.004)	(0.027)
SD(PE)*PE	0.008**	0.012
	(0.004)	(0.026)
Mean Outcome	12.63	8.29

Notes: see Table 5. SD(PE) is standardized to have zero mean (in the full sample) and is divided by the standard deviation of PE in the micro data, which sets the scale for the analysis.

Table 9. Marginal effects of E(PE) and its interactions with PE on long-run earnings.

Comparing results of Hoxby's approach and ours in the school-by-fixed effects sample.

	(1)	(2)
	School by family fixed effects	School and cohort effects (Hoxby's approach)
E(PE)	0.002	0.004**
	(0.002)	(0.002)
E(PE)*PE	-0.012***	0.002**
	(0.002)	(0.001)
Mean Outcome	12.63	12.63

Notes: See Table 5. Total number of observations: 569,468. ***: p<.01; **: p<.05; *: p<.10.

Table 10. Determinants of the probability of attending the same school as an older sibling.

	(1)
Younger sibling is a women	-0.003**
	(0.001)
Siblings of different gender	0.002
	(0.001)
Siblings of different gender * Younger sibling is a women	0.012***
	(0.003)
Spacing between sibling	-0.006***
	(0.002
Older sibling lives in parish in top tertile of urbanicity at age 15	0.017**
	(0.007)
E(PE) – older sibling	0.003
	(0.003)
Share of girls in school-cohort – older sibling	0.002
	(0.007)
Enrolment in school-cohort – older sibling	-0.001***
	(0.0001)
School attended by older sibling is closed for cohort of younger sibling	-0.444***
	(0.025)
School attended by younger sibling was not open for cohort of older sibling	-0.684***
	(0.009)
Siblings live in the same parish at age 15	0.364***
	(0.004)
Mean outcome	0.599
R-squared	0.604
Older sibling school f.e.	Yes
Older sibling cohort f.e.	Yes
Older sibling school specific cohort trends	Yes

Notes: each observation is an older sibling-younger sibling pair. The dependent variable is a dummy equal to 1 if the older and the younger sibling attend the same school, and to 0 otherwise. Standard errors are clustered by older sibling school. Total number of observations: 521,525. ***: p<.01; **: p<.05; *: p<.10.

Figure 1. Distribution of standardized average education in the school E(PE)

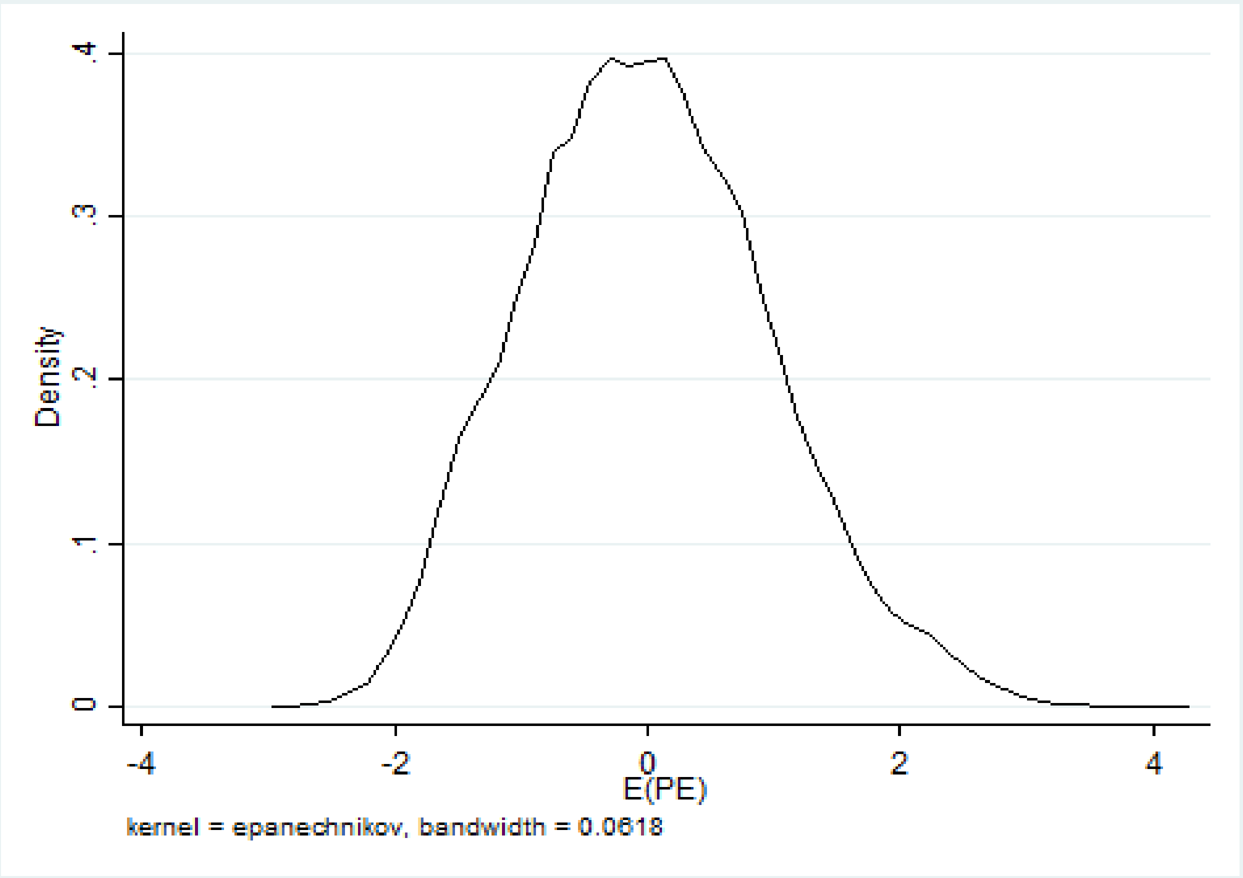
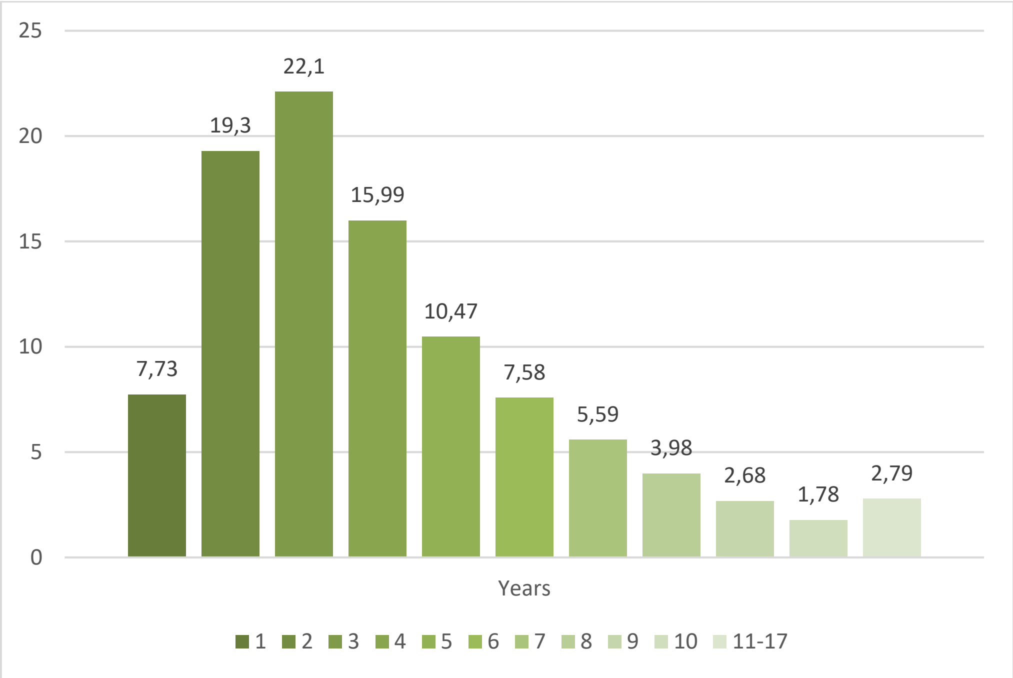


Figure 2. Maximum spacing between siblings attending the same school.



Notes: Calculations carried out in the school-by-family estimation sample. One observation per school-by-family group considered. Total number of observations: 253,398.

Figure 3. Distribution of the residuals of E(PE) after removing school-by-family fixed effects, birth order, gender and school specific spacing trends.

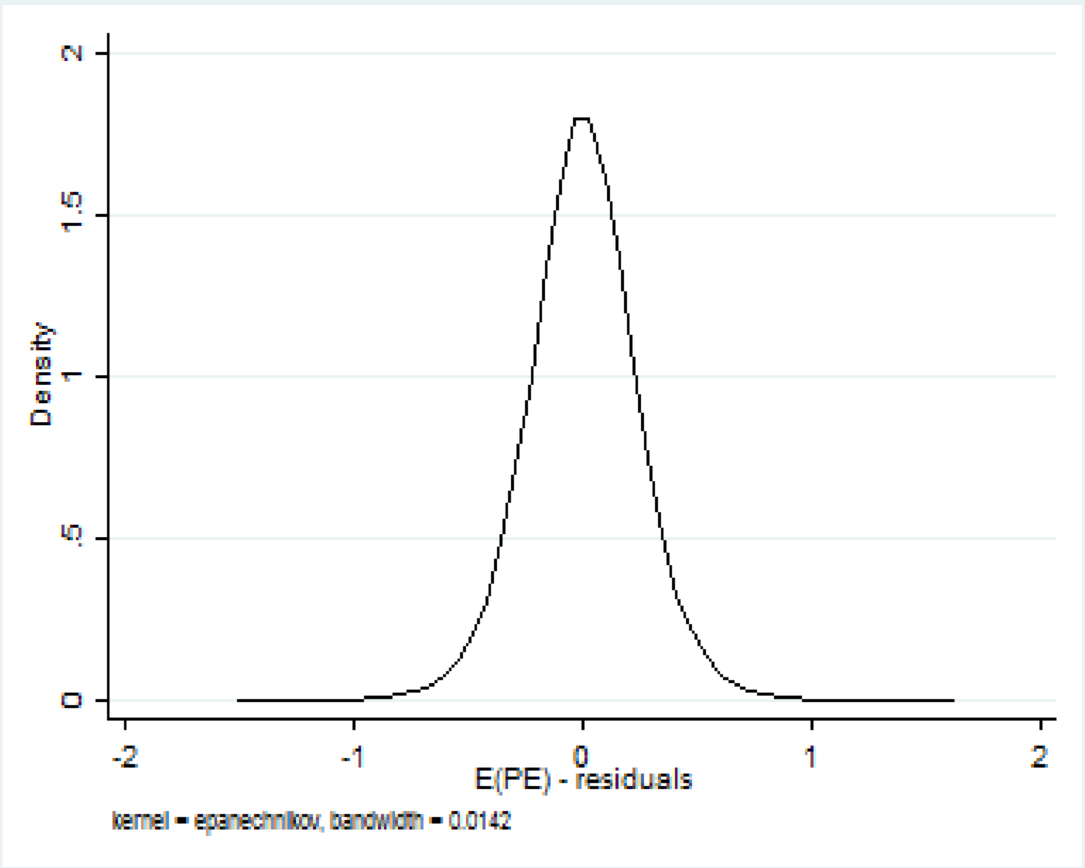


Figure 4. Marginal effect of E(PE) on average log real earnings between age 31 and 40.

For different values of individual PE.

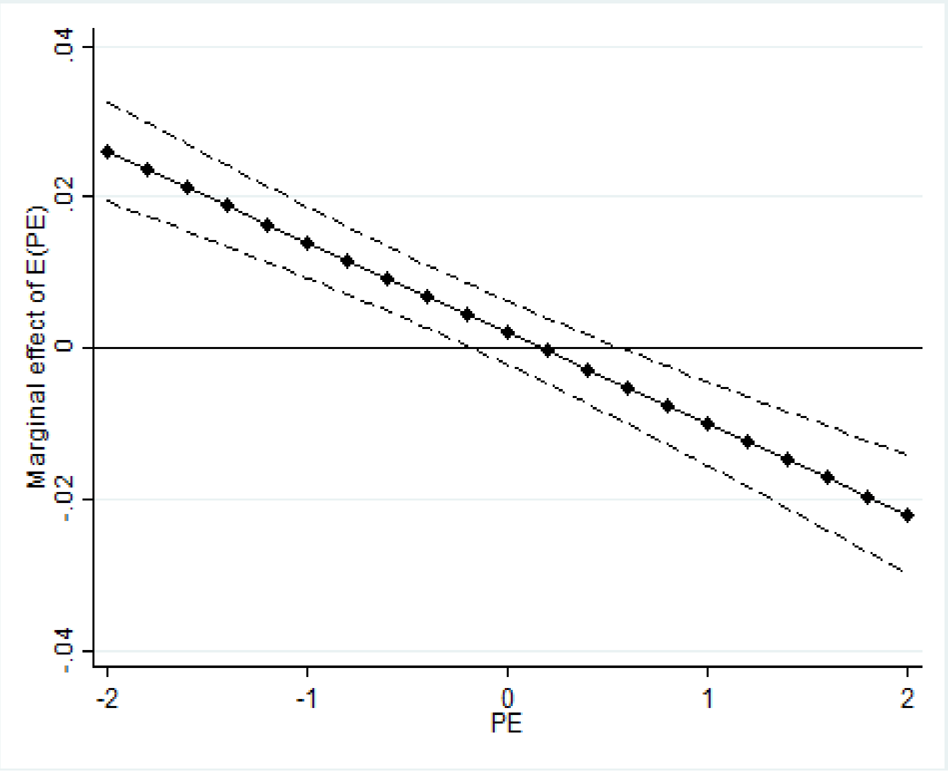


Figure 5. Marginal effect of E(PE) on years employed between age 31 and 40. For different values of individual PE.

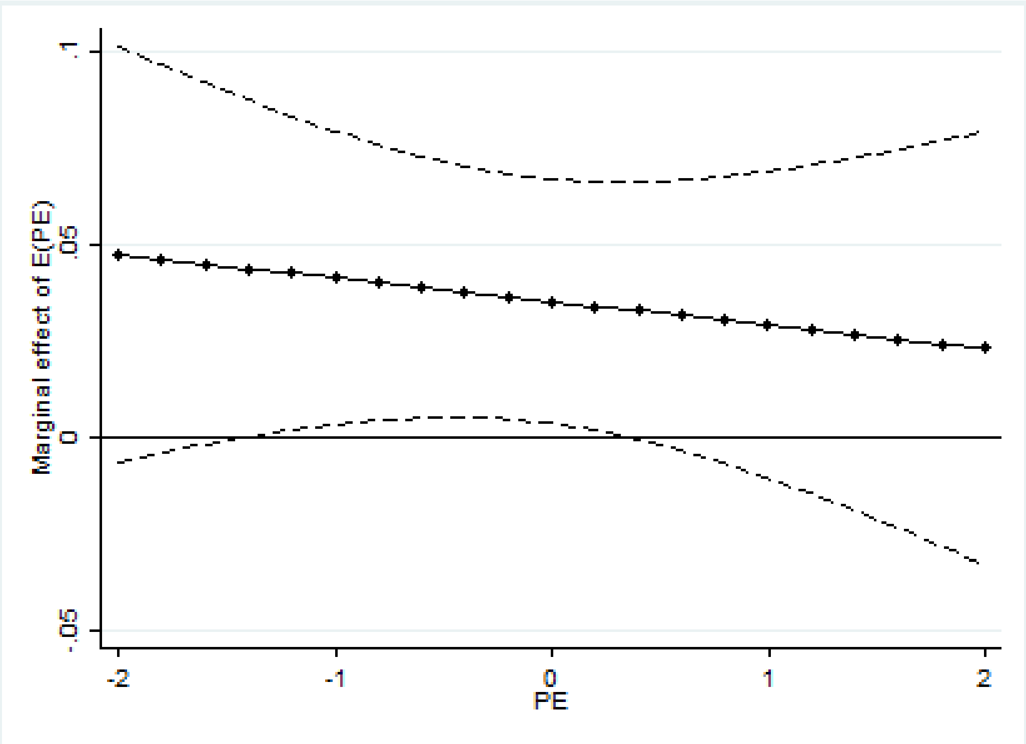


Figure 6. Mean and standard deviation of parental education in the school.

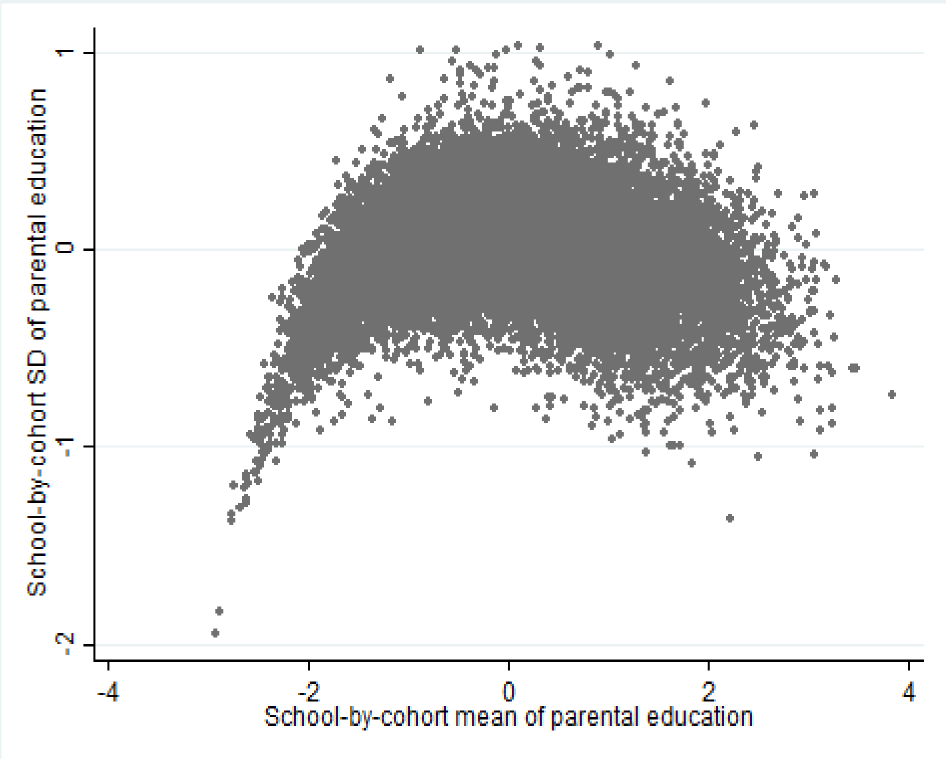


Figure 7. Marginal effect of SD(PE) on log real average earnings, age 31 to 40

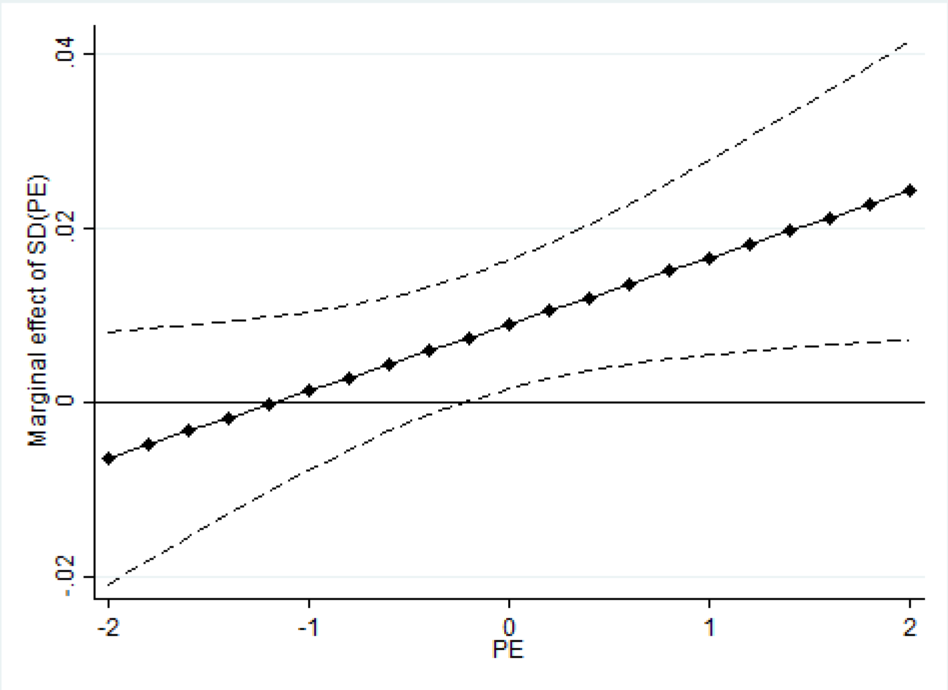
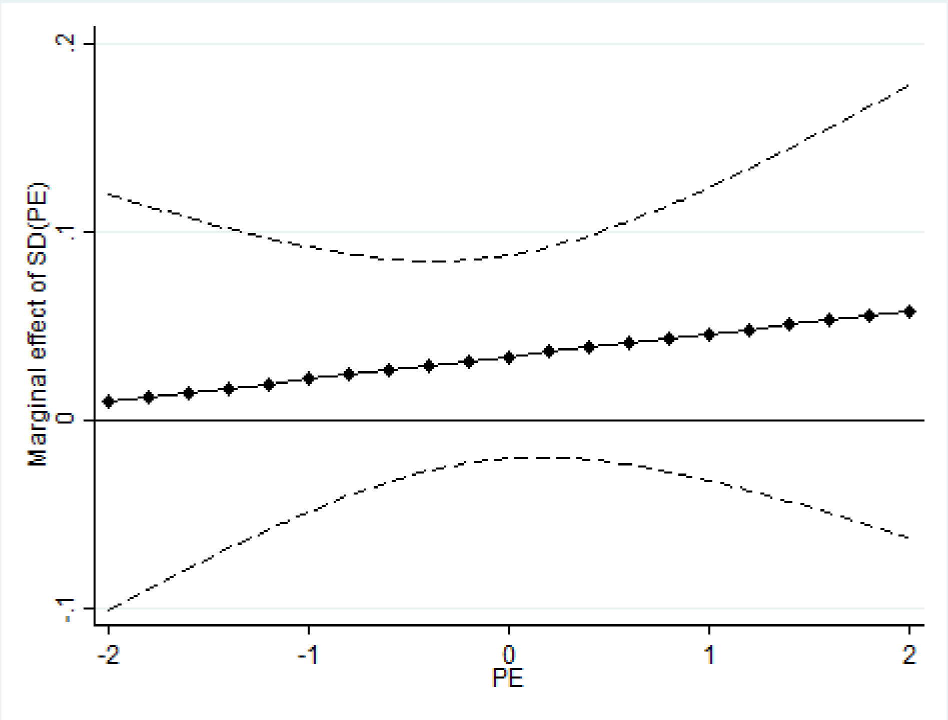


Figure 8. Marginal effect of SD(PE) on years employed, age 31 to 40.



Appendix

Table A1. Families by number of siblings attending the same schools.

	Frequency	%	Cumulated
One	437,357	63.3	63.3
Two	200,092	29.0	92.3
Three	45,741	6.6	98.9
Four and more	8,017	1.1	100

Notes: Total number of observations: 691,207. The total number of observations by family and school is larger than the number of families present in the school-by-family estimation sample because we drop 452 families where there are siblings born in the same year and school-by-family groups where there is no within-group variation in E(PE).

Table A2. Marginal effects of E(PE) and its interactions with PE on outcomes – discrete PE

	(1)	(2)
	Average log income, age 31-40	Years employed, age 31-40
1. E(PE)	0.002	0.029
	(0.003)	(0.019)
2. E(PE)*in bottom quartile of PE	0.007**	0.004
	(0.003)	(0.015)
3. E(PE)*in top quartile of PE	-0.023***	-0.000
	(0.004)	(0.034)
4. = 1. + 2.	0.009***	0.033
	(0.003)	(0.025)
5. = 1. + 3.	-0.020***	0.029
	(0.005)	(0.033)
Mean Outcome	12.63	8.29

Notes: see Table 5.