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School starting age and the social gradient in educational outcomes

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ABSTRACT
We provide evidence on the heterogeneous (positional) effects on school performance of two mandatory schooling reforms in Norway targeting achievement gaps based on family background and immigrant status. Both reforms effectively lowered school starting age by one year. Using high-quality register data, we examine children's grade point average (GPA) rank at age 15–16 and high school completion at age 21 as the main outcomes. Although not fully conclusive, our results suggest that the reforms neither had the intended effect of reducing socioeconomic achievement gaps or immigrant-native differentials, nor did they change the influences of relative age or gender.

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1. Introduction

In 1997, Norway implemented a reform reducing the school starting age from seven to six years old, and this led to the duration of compulsory schooling being extended from nine to ten years. A major aim of the reform was to counter differences in learning outcomes between children from different socioeconomic backgrounds (Ministry of Church, Education and Research 1993, 7). As children from economically disadvantaged families were strongly underrepresented in high-quality pre-school programs, it was hoped that a lower legislated school starting age could level the playing field. A particular concern was that many children from immigrant families did not participate in these programs, implying that they often started school at age seven with a considerable language disadvantage. To bridge the transition from kindergarten or the home environment to the school environment, the new first grade was to a large extent built on a kindergarten pedagogy, with a focus on play-oriented learning.

Nine years later – in 2006 – the reform was reformed, such that the first-grade curriculum was transformed to a more standard school pedagogy. Again, an important part of the motivation was to mitigate the socioeconomic achievement gaps (Ministry of Education and Research 2005, 3). A central worry was that the lack of formal learning in school lead to more socially skewed home-based learning, in effect nullifying the potential benefits of earlier school starting age and augmenting the social gradient in learning outcomes.

The lower school starting age in Norway has also fueled a debate about gender and relative age effects. This is based on two observations: firstly, that children born early in the year (and thus tend

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to be among the older in the class) perform better than those born later in the year (Dobkin and Ferreira 2010; Fredriksson and Öckert 2013; Peña 2017), and secondly, that girls tend to be more ready for school than boys and thus obtain an early learning advantage (Bertrand and Pan 2013; Cobb-Clark and Moschion 2017). The worry is that the lowering of school starting age may have exacerbated the achievement gaps related to relative age and gender. And in 2019, a government commission discussed the option of introducing a more flexible and individually adapted school starting age to offset differences in maturity (Ministry of Education and Research 2019).

In the present paper, we evaluate how the two reforms have affected relative school performance along the dimensions of i) socioeconomic background (parental earnings rank), ii) immigration background, iii) relative age, and iv) gender. In the study of socioeconomic background, we pay particular attention to whether or not one of the parents was a homemaker when the child was aged five, such that a realistic alternative to starting school at age six was to stay at home another year.

Our primary measures for school performance is individual rank in the distribution of grade point averages (GPA) obtained at age 15 to 16, adjusted for differences in grading standards identified through the use of externally graded exams, and high school completion by age 21. For the first of the two reforms, we also examine outcomes observed at higher ages, such as educational attainment and early labor market earnings.

Our analysis is built on a repeated differencing framework whereby we look for changes in the various outcome gradients for cohorts affected and unaffected by the reforms; i.e. the achievement gaps across family backgrounds, immigrant status, relative age (month-of-birth), and gender. Our results indicate that the two reforms had no robustly verifiable effects on any of the gradients in question. While there are some changes from cohort to cohort, and also some trends suggesting declining upward mobility for children born into disadvantaged families, there are no exceptionally large changes around the two reform years that could convincingly establish a pattern of reform effects. We also find no clear evidence of differential effects with respect to parents' labor market status. Note, however, that although we cannot confirm that any of the two reforms actually modified the social gradients, we cannot completely rule out such effects either.

Our paper speaks to an existing literature on the relationships between school starting age, socioeconomic achievement gaps, and social mobility. Using month of birth as a proxy for school starting age in Norway, Black, Devereux, and Salvanes (2011) find little evidence for heterogeneous effects on adult education and earnings across family backgrounds. Exploiting variation in school entry rules in England, Cornelissen and Dustmann (2019) find some support for positive effects of earlier school starting age on cognitive and noncognitive test scores, particularly for boys from disadvantaged family backgrounds. Suziedelyte and Zhu (2015) report similar findings based on Australian data, suggesting that an earlier school entry may narrow socioeconomic gaps in cognitive skills. By contrast, evaluating the rollout of a reform that transformed the first-year curriculum from a play-oriented to a learning-oriented curriculum in Brazil (a reform similar to the second of the two reforms evaluated in the present paper), Rosa, Martins, and Carnoy (2019) report particularly large positive effects for offspring from the higher social classes. Focusing on the relative age effect, Berniell and Estrada (2020) present evidence from Spain indicating that youngest-in-class children of college-educated parents experience a smaller relative age penalty than children with a more disadvantaged family background. Related to the role of school starting age, Havnes and Mogstad (2011, 2015) show that the expansion of high-quality childcare in Norway improved upward mobility for disadvantaged children. Still, Markussen and Røed (2023) show that the rapid expansion of childcare in Norway has not been able to prevent the overall socioeconomic achievement gaps from increasing.

A paper of particular interest is Drange, Havnes, and Sandsør (2016), which evaluated the first of the reforms examined in our paper. The authors consider the lowering of mandatory school starting age as an extension of universal childcare. Using a difference-in-differences (DiD) approach, the authors compare outcomes before and after the reform for children who would enter childcare as a result of the reform (the treatment group) with those who would have been in kindergarten regardless of the reform (the control group). Their findings point to negligible impact of the reform on children's school performance and academic tracking. Furthermore, they find little heterogeneity across subsamples by parental characteristics, family structure, and welfare dependency.

Our paper differs from Drange, Havnes, and Sandsør (2016) in several important aspects. To begin with, we evaluate the 1997 reform jointly with the 2006 reform, which made the first school year much more school-like. Second, whereas Drange, Havnes, and Sandsør (2016) focus on the absolute effects of the reform, our focus is entirely on the social gradient in outcomes and on the nature of socioeconomic and immigrant-native achievement gaps. This makes our analyses less vulnerable with respect to confounding factors affecting *average* school performance, obviously at the cost of not being able to identify any reform effects on this average. Third, we identify reform-induced changes in social mobility by leveraging rank-based family background measures that, in contrast to parental education, have the exact same distribution for all cohorts and arguably also a more stable socioeconomic interpretation. This reduces the risk that changes in achievement gaps arise from changes in the composition of parents along the trait used to define parental background. Finally, since all school-aged children were subject to the reforms after their implementation, the treatment-control approach does not apply in our context. Our alternative approach resembles a standard DiD strategy, whereby the first difference is that between a treatment and a control group and the second difference is that between post-reform and pre-reform observations. However, in our case, the treatment-control differential is replaced by the difference between socioeconomic groups. And to assess the likelihood that significant effects show up due to underlying trends or uncontrolled-for events, we rely on a rolling differencing approach to track the relevant achievement gaps over several years, offering insights into their underlying trends as well as on the (lack of) impacts of the reforms.

The rest of the paper is organized as follows. Section 2 outlines the institutional setting. Section 3 introduces the data and variables and describes the methodology and trends. Section 4 presents and discusses the results. Section 5 concludes the paper. Some supplementary analyses and robustness exercises are presented in Appendices A–E.

2. Institutional setting

Compulsory schooling in Norway applies to all resident children of eligible age. The school year starts in mid-August and ends in late June. Schools follow a common curriculum with no tracking, grade retention, or grade promotion. By default, students are assigned to free public schools according to their residential addresses (Markussen and Røed 2023).

A central idea behind the Norwegian schooling system is that all children can go to the same school, regardless of social background, family resources, intellectual ability, ethnicity, etc. This is sometimes referred to as the unitary school principle ('Enhetsskolen'), a concept introduced in the early twentieth century and enforced through subsequent policies (Nilsen 2010). An important aim of the unitary school is to foster diversity and ensure equality of opportunities for all, in particular across social and geographical groups.

The first reform we study, Reform 97, was launched in 1997. Prior to that, the duration of compulsory schooling was nine years, and school started in August the calendar year in which children reach the age of seven. In one of the central policy papers advocating the reduction of the school starting age, the motivation was explained as follows:

> School from the age of 6 will, in contrast to a voluntary offer in kindergarten, reach everyone with an equal educational offer, regardless of place of residence and the family's finances. It will be able to counteract the effects of the differences in learning ability and willingness to learn which are due to different growing-up conditions and social background. Children with an immigrant background will greatly benefit from being assured of a Norwegian-speaking environment and adapted education a year earlier than today (Ministry of Church, Education and Research 1993, 7; our translation).

Reform 97 lowered the age at which pupils start school from seven to six years, resulting in an extension of compulsory schooling from nine to ten years (Ministry of Church, Education and Research 1994). The rationale for the compulsory school extension was that lowering school starting age alone may impede learning due to pupils' lack of maturity (Thuen and Volckmar 2020). Since 1997, Norwegian children start school during the calendar year in which they reach the age of six, and they are comprehensively trained in the extended 10-year compulsory schooling (Ministry of Church, Education and Research 1994).

To adapt to the 10-year structure, a new curriculum was devised and launched between 1997 and 1999. The curriculum was divided into three stages: primary (grades one to four), intermediate (grades five to seven), and lower secondary school (grades eight to ten). In the primary stage, Reform 97 promoted the integration of the 'play' aspect of kindergarten and the 'learning' aspect of school. This primary stage was designed to ensure a smooth transition from nursery to formal education (Ministry of Church, Education and Research 1993, 1996).

Knowledge Promotion ('Kunnskapsløftet') was a reform implemented in the fall of 2006. It led to changes in the schools' curriculum, structure, and organization (Ministry of Education and Research 2004). The curriculum revision was partly a response to Norwegian children's disappointing performance in the PISA test in 2001 (Imsen, Blossing, and Moos 2017). In this context, the goal of Knowledge Promotion was to help all pupils develop basic skills that are necessary for an active participation in a knowledge-based society (Ministry of Education and Research 2006). In line with the principles of a unitary school, Knowledge Promotion advocated an inclusive learning environment, ensuring that everyone was given equal opportunities to develop their abilities (Ministry of Education and Research 2006). The policy paper highlighted that

> Evaluations of previous reforms also show that there are large differences in Norwegian schools, and that there are systematic differences between students as a result of gender and social and ethnic background. The aim of the Knowledge Promotion is for all pupils to acquire the basics skills and the competence they need to get by in life. Everyone should get the same opportunities to develop their abilities, regardless of social or ethnic background (Ministry of Education and Research 2005, 3; our translation).

Following Knowledge Promotion, the 10 years of compulsory schooling was restructured into two stages: primary school (grades one to seven) and lower secondary school (grades eight to ten). Grades one to nine of the 10-year compulsory school adopted the new curriculum from the 2006–2007 school year, while the 10th grade implemented the new curriculum from the 2007–2008 school year. Crucially, the curriculum reform introduced systematic reading and writing in the first grade. In other words, the pedagogy for the first year shifted from a play-oriented approach to a more learning-focused one (Ministry of Education and Research 2004). If we interpret the school starting age as the age at which children are exposed to structured learning, e.g. in the form of reading and writing, this pedagogy revision effectively reduced school starting age once again.

After compulsory school, virtually all students move on to a three or four year high school education, divided into an academic and a vocational track. Access to a high school education is a legal entitlement in Norway, but some schools and tracks are oversubscribed, in which case the GPA from compulsory school may be used to distribute the scarce slots. The dropout rate from high school is considerable (15–20%),[1] and many of those who do complete spend more than the three to four normed years.

3. Data and methods

3.1. Data

The study design follows the implementation of the reforms closely. We compare children born just before and just after relevant eligibility cutoff dates; children born before the cutoff dates constitute the control group, and those born after the cutoff dates represent the treatment group. The assignment of treatment is thus determined by birth date only. Parents could apply for earlier or later school start for their kids, but our analysis will be based on an intention-to-treat (ITT) strategy.

We use comprehensive registry data from Norway to conduct our analysis (Statistics Norway 2020a, 2020b, 2020c, 2020d, 2020e, 2020f, 2020g, 2020h, 2020i, 2020j, 2020k, 2020l). The sampling bias is minimized since the data cover the entire Norwegian population. Our sample period starts with the 1987 birth cohort, four years prior to the Reform 97 cohort, and ends with the 2002 cohort, three years after the Knowledge Promotion cohort. We work with two samples, one comprising native children only, and the other combining native and immigrant children. We rely on the native sample for analysis related to the social gradient, since parental information is missing for some first-generation immigrants. The native sample includes 871,971 children, 51.2% of whom are males, and for 94.1% of whom we can match both parents. We draw on the combined sample for the remaining analysis, namely, on immigration and relative age gradients. The combined sample contains 928,366 children, 51.2% of whom are males, and 6.1% of whom are immigrants from low- or middle-income countries. The emphasis on children from low- or middle-income countries reflects that these children likely experienced greater cultural and educational differences than children from high-income countries. The latter group turns out to behave similarly as natives and are thus included in the native group.

The outcomes, covariates, and designated reference groups are described in Table 1. Note that GPA is a composite measure of grades in all subjects, assigned at the end of compulsory school. We adjust for variation in local 'grading generosity' comparing average school-by-year GPA with corresponding results from externally evaluated exams, addressing findings in Falch and Naper (2013) and methodologically following Markussen and Røed (2023). This implies that GPA is adjusted downwards (upwards) for students going to schools that systematically give their students better (poorer) grades than predicted by externally evaluated tests.

We describe family background in terms of parental earnings rank during the parents' prime age. To begin with, we collect annual earnings between ages 40 and 46 for both parents. Next, we scale the earnings by the Basic Amount ('Grunnbeløpet'), a national wage index that tracks aggregate wage growth. Then, we rank the adjusted annual earnings and average the top three earnings (that can come from the mother and/or the father). The resultant ranking then captures the parents' earnings potential, which obviously will be highly correlated with other socially and

Table 1. Definitions of outcomes and covariates.

Variable	Definition
Outcomes	
GPA rank	Standardized grade point average ranked by birth year in percentiles[a]
High school completion†	= 1 if the individual has finished high school by age 21
Covariates	
Male†	= 1 if the individual is a male (reference category: females)
Parental earnings rank (PER)†	Indicators for the decile rank (1, 2–3, 4–7, 8–9, 10) of the highest three parental earnings between 40 and 46 by the child's birth year[b] (reference category: deciles 4–7)
Home-making parent†	= 1 if either parent earned less than two times the Basic Amount when the individual was five, = 0 if both parents were working and earned more than two times the Basic Amount (reference category: home-making parent)
Immigration status†	Indicators for whether the individual is a native, a second-generation immigrant, a first-generation immigrant arriving between one and five, or a first-generation immigrant arriving between six and nine. Immigrants refer to those from low- or middle-income countries, categorized as per the World Bank's classification in 2004 (World Bank 2021)[c] (reference category: natives)
Birth months†	Birth month indicators: January to February, March to April, May to August, September to October, November to December (reference category: May to August)

Notes: A † indicates a dichotomous variable that equals zero if the stated condition is not met. [a] We treat missing records as zero (i.e. minimum grade) and generate two separate ranks, one among natives and one combining natives and immigrants. [b] We rank only the parental earnings of natives, and missing parental earnings are excluded. [c] We categorize immigrants' source countries using World Bank's classification in 2004 because 2004 had marked the beginning of the expansion of the European Union; the income classifications of member countries have changed since then, but the immigrants in our sample have not experienced such changes before they migrated. Immigrants from high-income countries are categorized as natives.

genetically inheritable characteristics relevant for the offspring's educational performance, such as cognitive ability, self-control, ambitions, learning patterns (e.g. Fumarco and Schultze 2020), home environment (e.g.Ulvestad and Markussen 2023), social networks, and neighborhood characteristics (e.g. Markussen and Røed 2022). Hence, it arguably comes close to something we can interpret as social class background. To operationalize the earnings rank indicator in our analysis, we divide the population into five bins; i.e. decile 1 in the parental earnings rank distribution (bottom class), deciles 2–3 (lower class), deciles 4–7 (middle class), deciles 8–9 (upper class), and decile 10 (top class).

An important advantage of the earnings-rank-based measure of family background is that it by construction has the exact same distribution (and interpretation) for all cohorts. This ensures that any changes in the relationship between family background and offspring outcomes can be interpreted as genuine changes in the degree of intergenerational mobility. By contrast, when parental education is used to characterize family background, it is typically difficult to assess whether changes in the intergenerational associations reflect changes in mobility or changes in the sorting into attainment brackets within the parent generations. Consequently, we adopt parental earnings rank for the main analysis, and employ parental education as an alternative measure for robustness checks.

3.2. Methods

We apply a DiD-type strategy, where the purpose is to examine how the socioeconomic achievement gaps (the first difference) changed from the last cohort unaffected by a specific reform (Reform 97 or Knowledge Promotion) to the first cohort affected by it (the second difference). This strategy will ensure unbiased estimates of the causal reform effects provided there is no underlying trend in group-specific outcomes and no other events coinciding with the reforms that affected socioeconomic achievement gaps. Unfortunately, these assumptions are not particularly plausible in our case, as the data indicate the presence of trends as well as annual fluctuations in achievement gaps. The existence of a stable trend may potentially be dealt with by means of a triple difference strategy; e.g. comparing the change in achievement gaps from the last pre-reform cohort to the first post-reform cohort with the corresponding change lagged one cohort. However, the assumption of a stable common trend is also questionable in our case, particularly when it comes to rank-based outcomes, as declining rank for one group by construction implies improving rank for someone else and as ranks are bounded between 0 and 1.

Rather than seeking to provide specific 'preferred' estimates based on questionable identifying assumptions, we deal with these challenges by presenting a range of estimated 'treatment effects', both based on actual reform implementation and on sequences of placebo reforms. What we are looking for is then a pattern of estimates that stand out as different for the true reform years. This is arguably the most transparent way of communicating the evidence embedded in our data, making it possible for the readers to make their own assessment of whether or not the reforms did noticeably affect the socioeconomic achievement gaps. More specifically, we start with a window that contains cohorts born in 1987 and 1988, assuming that a (placebo) reform was enforced on the 1988 birth cohort. Then, we move the window to 1988 and 1989 birth cohorts, this time assuming the reform was enforced on children born in 1989. We repeat this exercise until the entire sample period is covered. The real reform years were 1991 and 2000, whereas the placebo reform years were the remaining years between 1988 and 2002.

For each real or placebo reform, let $S_i = \{X_{i1}, \ldots, X_{ik}\},\ k \geq 1$ denote the set of covariates for individual i and $\mathcal{P}(S_i)$ the power set of S_i. Then, for each subset $S_{ij} \subseteq \mathcal{P}(S_i)$ (excluding the empty set), we construct $\Pi(S_{ij})$ as the product of all elements in the subset. To estimate the relative effects of the reforms, we specify

$$y_i = \alpha + \beta \times \text{reform}_i + \sum_j \gamma_j \times \Pi(S_{ij}) + \sum_j \delta_j \times \Pi(S_{ij}) \times \text{reform}_i + \varepsilon_i, \tag{1}$$

where y_i denotes the outcome for child i, reform_i is an indicator variable that equals 1 if the child was born in the second year of the two-year period, and hence exposed to either the real or the placebo reform. S_i draws from parental earnings rank, immigration status, birth month, gender, and parental role (i.e. home-making or working), the former three constituting vectors with multiple indicators in X_i. We are interested in the parameter δ_j, the reform-induced changes in outcome compared to changes in the reference group. For example, to examine the heterogeneous reform effects by gender and parental earnings rank (PER), we specify

$$y_i = \alpha + \beta \,\text{reform}_i + \gamma_m \text{male}_i + \sum_{p=1}^{P-1} \gamma_p \text{PER}_{ip} + \sum_{p=1}^{P-1} \gamma_{mp} \text{male}_i \times \text{PER}_{ip}$$
$$+ \sum_{p=1}^{P} \delta_{mp} \text{male}_i \times \text{PER}_{ip} \times \text{reform}_i + \sum_{p=1}^{P-1} \delta_{\overline{m}p} (1 - \text{male}_i) \times \text{PER}_{ip} \times \text{reform}_i + \varepsilon_i,$$

where we use subscript m for males, $\overline{m}$ for females, p for parental earnings rank bin, $P = 5$, and the reference category is middle-class females.

We estimate ITT effects using year of birth, rather than actual school starting age. The reason is that while we can infer actual school starting age from observed data, it is likely endogenously determined as parents react to the reforms. For example, parents may hold off children born late in the year (such as December) to start a year later ('redshirting'), or enroll children born early in the year (such as January) a year sooner (Cook and Kang 2020; Deming and Dynarski 2008; Larsen and Solli 2017). In Figure 1, we plot the proportion of noncompliers across years using age at completion

$$\widehat{\text{School starting age}}_i = \begin{cases} \text{Age at completion}_i - 9 & \text{if birth year} < 1991 \\ \text{Age at completion}_i - 10 & \text{if birth year} \geq 1991. \end{cases}$$

Since there is hardly any grade retention or promotion in Norway, the age at completion (which is also the age a child receives the GPA assignment) will almost perfectly identify school starting age. We do observe that in the 1991-cohort, more children start later (2.2%) than in neighboring years, and fewer children start earlier (0.2%) than before. In the 2000-cohort, up to 3.6% of children enroll later than age 6, and 0.3% enroll sooner. It is important to bear in mind that the 1990 and 1991 birth cohorts started school at the same time in 1997. Although only the 1991 cohort were directly subjected to the reform, it appears probable that also the 1990 cohort may have been

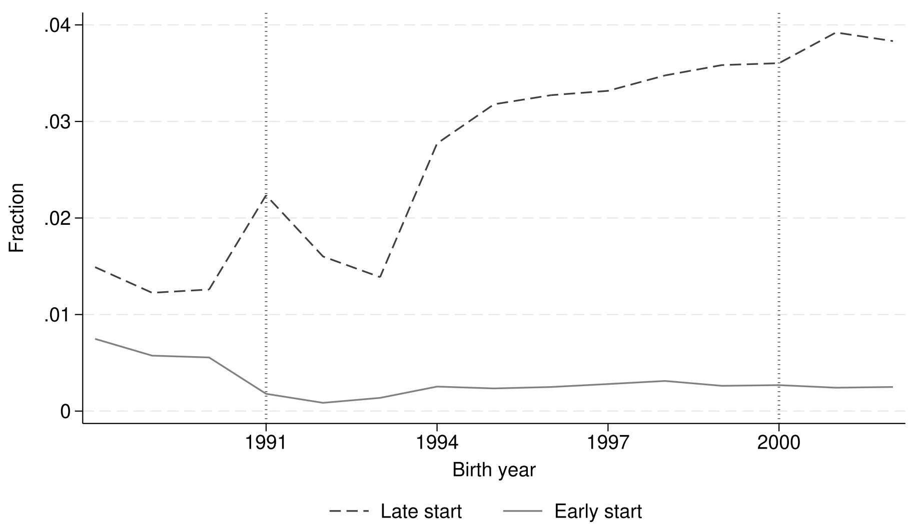


Figure 1. The proportion of children who start earlier and later than 6/7. We identify early and late starters based on age and grade at completion. The reform years, 1991 and 2000, are marked by the dotted vertical lines.

affected by it, as the doubling of the number of school starters put the educational system under stress and perhaps implied less resources and attention directed toward the oldest of the two cohorts.

3.3. Trends

Are there trends in the socio-demographic markers during the sample period? In Figure 2, we illustrate the trends in immigration, month of birth, gender, and parental roles for cohorts born between 1988 and 2002. The proportion of immigrant children (panel a) has increased over the sample period, especially second-generation immigrants. This was partly driven by the expansion of the European labor market (Hoen, Markussen, and Røed 2022). In panel (b), we observe that some months have consistently higher average birth rate than others, notably, January to February, and May to August. This seasonal variation in births can be attributed to factors such as climate and holiday patterns.

Panel (c) plots the trends in the proportion of males and females. Over the years, there are consistently more boys than girls, and the average gap is 2.5 percentage points. Towards the end of the sample, the gap has tightened but the difference is still well over one percentage point. Over time, there has been a continuous decline in the proportion of children with home-making parents at the child's age five and thus a potential for home care at age six (panel d), while the proportion with working parents has steadily climbed. Starting from the 1989 birth cohort, there are more children without a homemaker parent than there are with a homemaker parent. This trend may be credited to the rise in female labor force participation and the corresponding increase in preschool enrollment after 1990 (Black, Devereux, and Salvanes 2011).

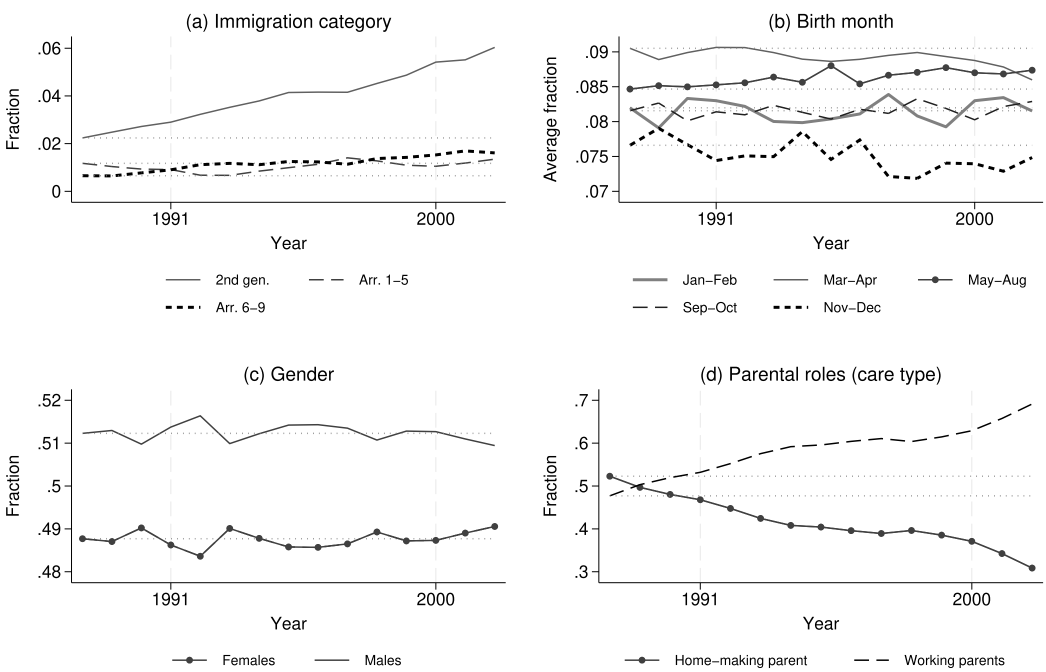


Figure 2. Descriptive trends among children born between 1988 and 2002. (a) Immigration categories: second-generation immigrants from low- or middle-income countries, first-generation immigrants from low- or middle-income countries arriving between ages one and five, and between ages six and nine. Due to high prevalence, we do not plot natives in panel (a). (b) Birth months: January to February, March to April, May to August, September to October, and November to December. (c) Gender: females and males. (d) Parental roles implying care type: home-making parent (home care) and working parents (kindergarten care).

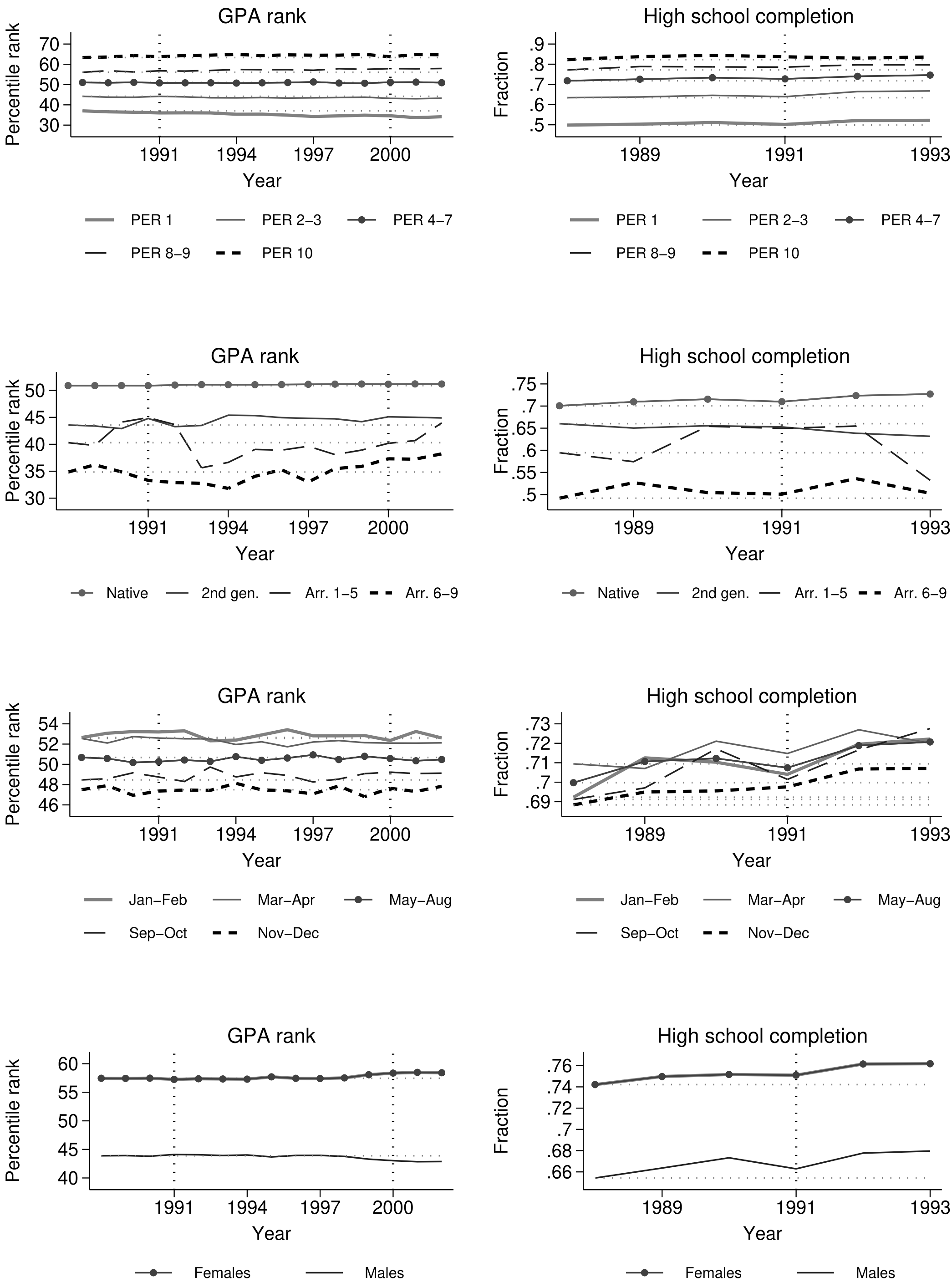


Figure 3. Average child outcomes by parental earnings rank (PER) deciles, immigration categories, birth months, and gender. We focus on births between 1988 and 2002 for GPA, which is assigned at age 15 to 16 for most children. For high school completion, which is assessed by age 21, we restrict attention to births between 1988 and 1993. Parents are ranked based on their top three earnings from ages 40 to 46 and the child's birth year. Children are categorized into five classes: bottom class (decile 1), lower class (deciles 2 to 3), middle class (deciles 4 to 7), upper class (deciles 8 to 9), and top class (decile 10). Concerning immigration categories, children are categorized into four immigration groups: natives, second-generation immigrants from low- or middle-income countries, first-generation immigrants from low- or middle-income countries arriving between ages one and five, and between ages six and nine. Immigrants from high-income countries are treated as natives. Regarding relative age, children are categorized into five groups based on their birth months: January to February, March to April, May to August, September to October, and November to December.

In Figure 3, we present average child outcomes by parental earnings rank, immigration category, birth month, and gender over the sample period. Some interesting patterns can be identified from the panels. First, there is a clear ranking in GPA and high school completion by social class (top row): children's outcomes improve with their parents' social standing. For GPA rank, the gap widens over the years, with bottom class offspring lagging behind. Second, native students attain better outcomes than immigrants (second row). The disadvantages are the most pronounced for first-generation immigrants who arrived after the age of six, despite steady improvements in their GPA rank and high school completion. Third, being old-for-grade (i.e. born earlier in the year) gives pupils a clear advantage in GPA, whereas being young-for-grade (i.e. born later in the year) means lower achievements (third row). The differences between intermediate birth groups are less distinct for high school completion. Fourth, there exist persistent achievement gaps between females and males (fourth row), with females consistently outperforming males under both measures.

These descriptive findings tentatively suggest that the reforms may have been ineffective at leveling the playing field. Had they succeeded, we should probably have witnessed breaks in the trends around the reform years and narrower achievement gaps in subsequent periods. We do not see any clear evidence in this direction.

In Appendix A, we illustrate the observed annual achievement gaps with confidence intervals. More specifically, we estimate the positional effects for each year, excluding the reform indicator and its interactions. For instance, for the parental earnings rank (PER) specification, this means

$$y_i = \alpha + \sum_{p=1}^{P} \delta_{mp}\text{male}_i \times \text{PER}_{ip} + \sum_{p=1}^{P-1} \delta_{\overline{m}p}(1 - \text{male}_i) \times \text{PER}_{ip} + \varepsilon_i,$$

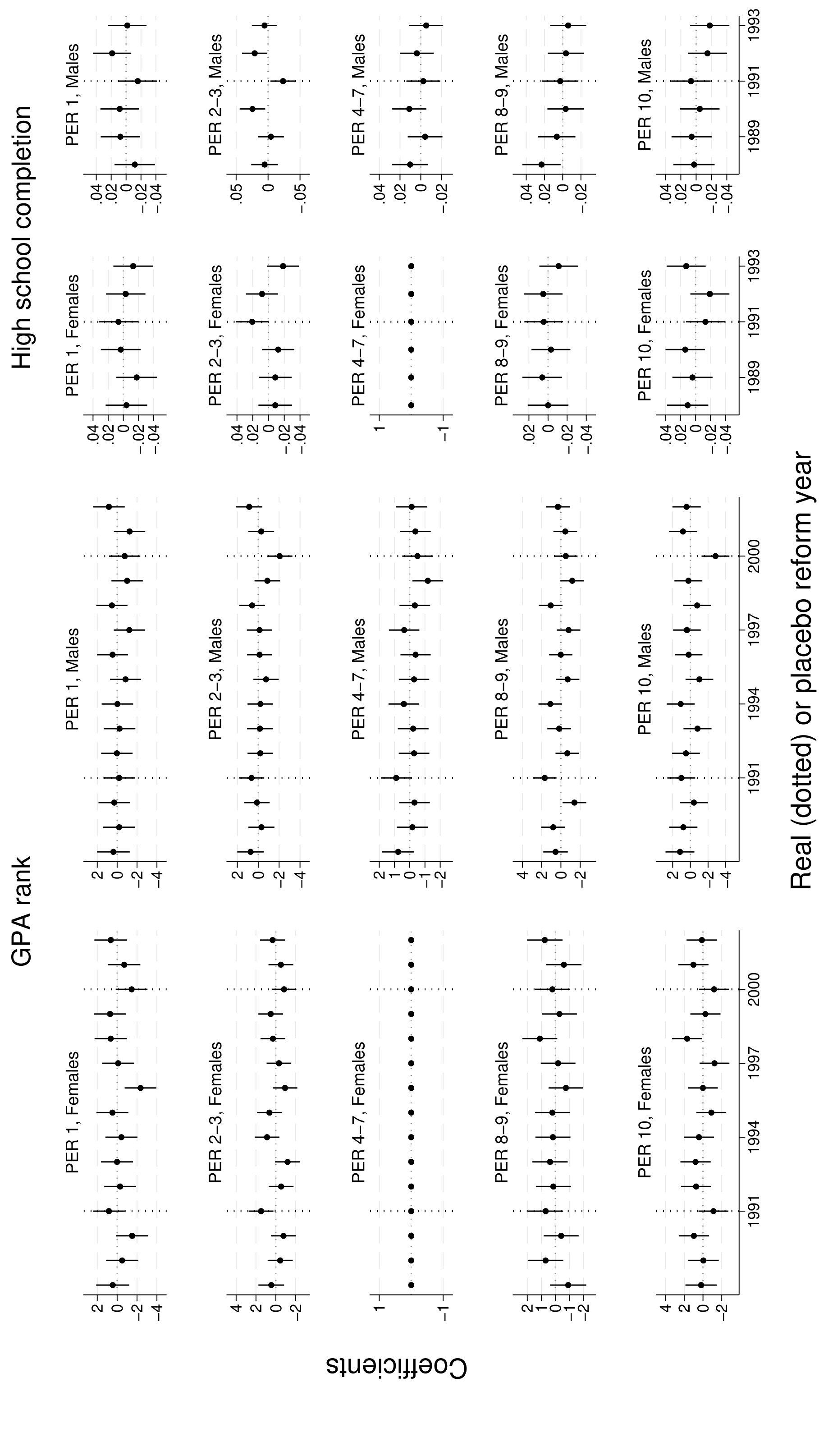
where we use subscript m for males, $\overline{m}$ for females, p for parental earnings rank bin, $P = 5$, the reference category is middle-class females, and we plot the δ coefficients. We observe that the GPA ranks of bottom- and lower-class children worsen over time, while those of upper- and top-class children steadily improve relative to middle-class females. Furthermore, second-generation immigrants become less likely to complete high school than native children over the sample period. Meanwhile, the school performance of males born throughout the year declines over time compared to females born mid-year.

4. Results

In this section, we report the interaction coefficients from Equation (1) for the real and the placebo reforms. These coefficients can be interpreted as the change from one cohort to another in the positional (relative) influence of background characteristics, *as if* a reform took place for the second of the two cohorts. For most of the years, we can think of this as effects of a placebo reform, capturing trends and fluctuations that are unrelated to the reforms evaluated in this paper. The motivation for doing this is to assess the risk of erroneously establishing reform effects had we relied on the true reforms only. Then, for two of the birth cohorts (1991 and 2000), the coefficients may capture effects of the true reforms (Reform 97 and Knowledge Promotion, respectively). These coefficients are marked on the figures by vertical dotted lines. If the two reforms really had non-negligible influences on the outcome gradients in question, we would expect to see some significant estimates in these particular years that stand out relative to the estimates obtained for the placebo reform years. For GPA rank, we examine the impact of both reforms, with children born in 1991 and 2000 being the first cohort subject to the respective reforms. For high school completion, we focus on Reform 97 with effective reform year 1991 due to the relatively young age of children from the Knowledge Promotion cohort.

4.1. Parental earnings rank

Has an early school start affected the social gradient? In Figure 4, we use parental earnings rank (PER) as the socioeconomic indicator to answer the question. Due to a lack of information on parental

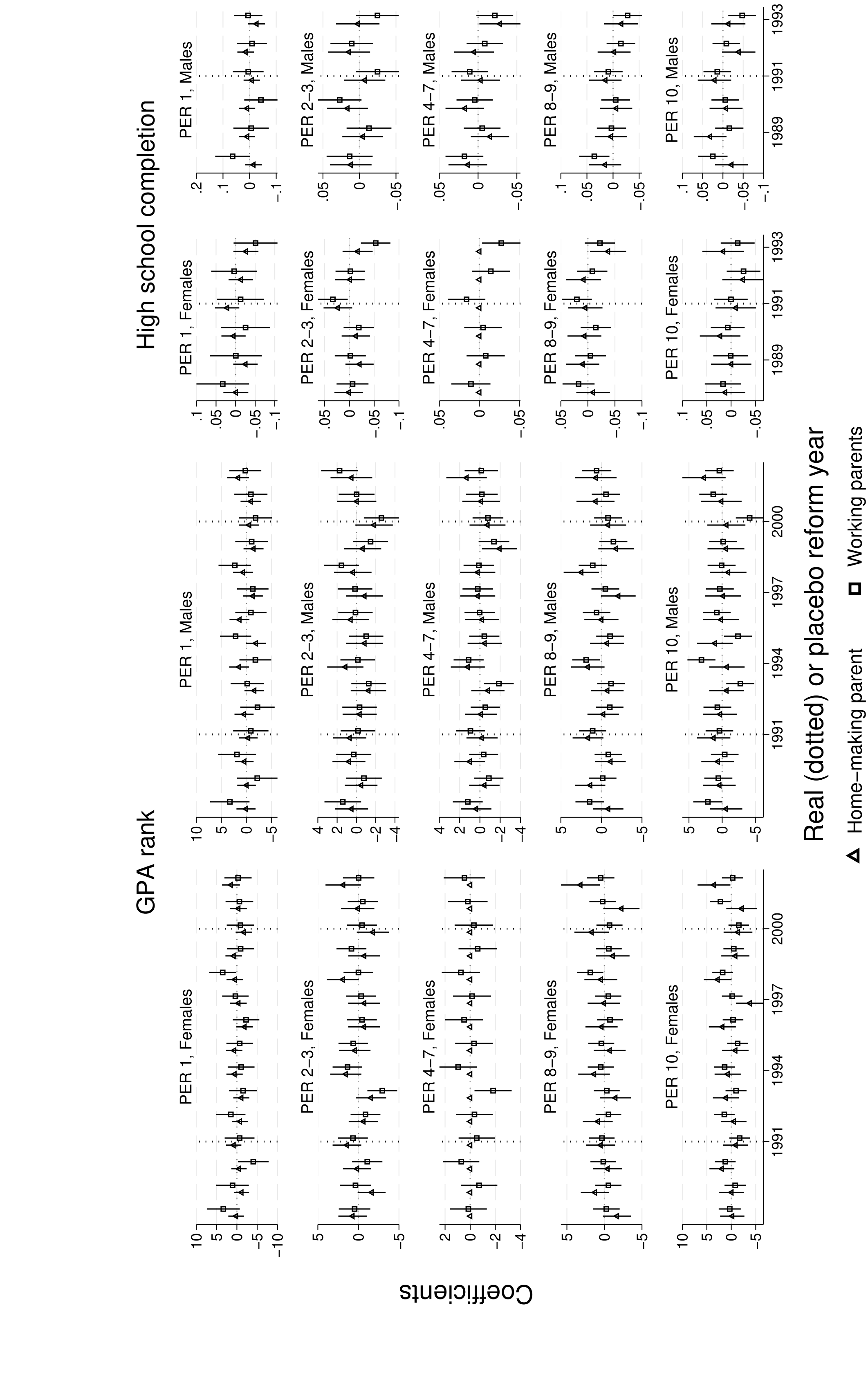

GPA rank
High school completion
PER 1, Females
PER 1, Males
PER 2–3, Females
PER 2–3, Males
PER 4–7, Females
PER 4–7, Males
PER 8–9, Females
PER 8–9, Males
PER 10, Females
PER 10, Males
Coefficients
Real (dotted) or placebo reform year
1991
1994
1997
2000
1989
1993

earnings for some first-generation immigrant children, we focus on native children in this exercise. As discussed in Section 3.1, examining the social gradient based on parental earnings rank has the great advantage that the distribution of parental traits is the same for all cohorts, mitigating the risk that changes in parent-offspring associations arise from changes in the composition of parents along the measured parental trait. The figure presents separate findings for GPA rank and high school completion, as well as for males and females. We estimate the gradient effects pooling all parents, treating middle-class females as the reference group. This implies that all standard errors and significance statements refer to a comparison with this particular group. Since the choice of reference group appears a bit arbitrary, this limits the value of focusing exclusively on the reported statistical significance of individual estimates. Our assessment of the reforms will therefore be based on the overall pattern of estimated coefficients, and the extent to which they together signal coherent reform effects.

Focusing on the effects estimated for the true reform years (indicated by the markers at the vertical dotted lines), we see a few changes in achievement gaps that appear to be borderline statistically significant. In particular, with the implementation of Reform 97, it seems that lower-class females improved their performance relative to middle- and top-class females and bottom-class males, both in terms of GPA rank and high school completion. There were also some improvements for middle- and upper-class males, whereas bottom- and lower-class males lost out in terms of high school completion. Hence, taken at face value, the estimates indicate that socioeconomic achievement gaps closed a bit among girls, whereas they expanded among boys. Had we used a triple difference framework here, it can be seen from Figure 4 that some of these effect estimates would have been even larger, as the apparent 'winning' classes in the 1991 (Reform 97) cohort were among the 'losing' classes in the 1990 cohort. However, this pattern also raises the suspicion that some of the changes in achievement gaps in the 1991 cohort represent a rebound of their opposite changes in the 1990 cohort. This could either reflect that Reform 97 in reality affected the 1990 cohort also, due to the doubling of the normal number of school starters in 1997, possibly with less attention directed to the oldest, or simply that particularly large (random) changes in one year will tend be reversed in the subsequent year (regression toward the mean). Viewed as a whole, we find it hard to identify a consistent pattern that can convincingly point toward reduced achievement gaps as a result of Reform 97. Moreover, as can be seen from Figure 4, there are a number of statistically significant changes in the gradients also in non-reform years, indicating that there are other fluctuations and trends represented in the data.

Figure 5 reports the results where we have distinguished between having a home-making parent and having working parents – a distinction more relevant for Reform 97 – and treat middle-class females with a home-making parent as the reference category. One might expect that children who had a homemaker parent would benefit disproportionally from Reform 97, particularly if resources are limited in the household. However, given the large changes in the fraction of home-making parents over time (see Figure 2, panel (d)), it seems plausible that the selection into this state has also changed considerably, along dimensions that are relevant for offspring outcomes (e.g. parents' cognitive and social abilities); hence any changes in the parent-offspring associations need to be interpreted with some care. The results are in any case not very different from those obtained unconditional on parents' employment status. We find that the number of statistically

← **Figure 4.** Reform-induced changes by parental earnings rank (PER) decile. Each coefficient comes from a regression on a two-year sample, where the second year is set to be the real or placebo reform year. The real reform years are 1991 and 2000. We present separate estimates for males and females. We focus on births between 1988 and 2002 for GPA, which is assigned at age 15 to 16 for most children. For high school completion, which is assessed by age 21, we restrict attention to births between 1988 and 1993. Parents are ranked based on their top three earnings from ages 40 to 46 and the child's birth year. Children are categorized into five classes: bottom class (decile 1), lower class (deciles 2 to 3), middle class (deciles 4 to 7), upper class (deciles 8 to 9), and top class (decile 10). The reference group comprises middle-class females. The markers pinpoint the estimated coefficients, and the vertical whiskers represent the 95 percent confidence intervals.


GPA rank
High school completion
PER 1, Females
PER 1, Males
PER 2–3, Females
PER 2–3, Males
PER 4–7, Females
PER 4–7, Males
PER 8–9, Females
PER 8–9, Males
PER 10, Females
PER 10, Males
Coefficients
Real (dotted) or placebo reform year
Home–making parent
Working parents
1989
1991
1993
1994
1997
2000

significant coefficients regarding the true Reform 97 effects is 0 (out of 18) when the presumed alternative to school start is home care and 1 (out of 20) when the alternative is kindergarten care. Overall, there is no clear evidence of changes in the social gradient in 1991 or 2000, and it holds regardless of parental earnings rank and care type.

Considering all the estimates reported in Figures 4 and 5 together, it is notable that the estimates for the true reform years do not stand out. Of the 84 true-reform coefficients, 9 or 10.7% are significant at the 5% level. This is not statistically different from the 6.0% (30 out of 504) for the placebo-reform coefficients (p-value = 0.10). Looking at the estimated real reform effects on GPA rank and high school completion together, it is also striking that they rarely convey a consistent story whereby, e.g. positive effects on GPA rank are matched by corresponding positive effects on high school completion.

Given the ambition of improving relative outcomes for the lower classes, it is notable that the coefficients for the bottom class (PER 1) are more often negative than positive (72.2% vs. 27.8% in real reform years, and 58.7% vs. 41.3% across all years), consistent with the presence of a negative trend in their relative performance that continued more or less undisturbed after the reforms. Meanwhile, both females and males report a nearly equal share of negative and positive coefficients, indicating that gender gaps are not closing.

4.2. Immigration background

Has legislating a lower school entry age reshaped the impact of immigration background? We answer this question using Figure 6, which includes native as well as immigrant children. Immigrant children are categorized into three groups: second-generation immigrants, first-generation immigrants arriving between ages one and five, and first-generation immigrants arriving between ages six and nine. We set the reference category to native females.

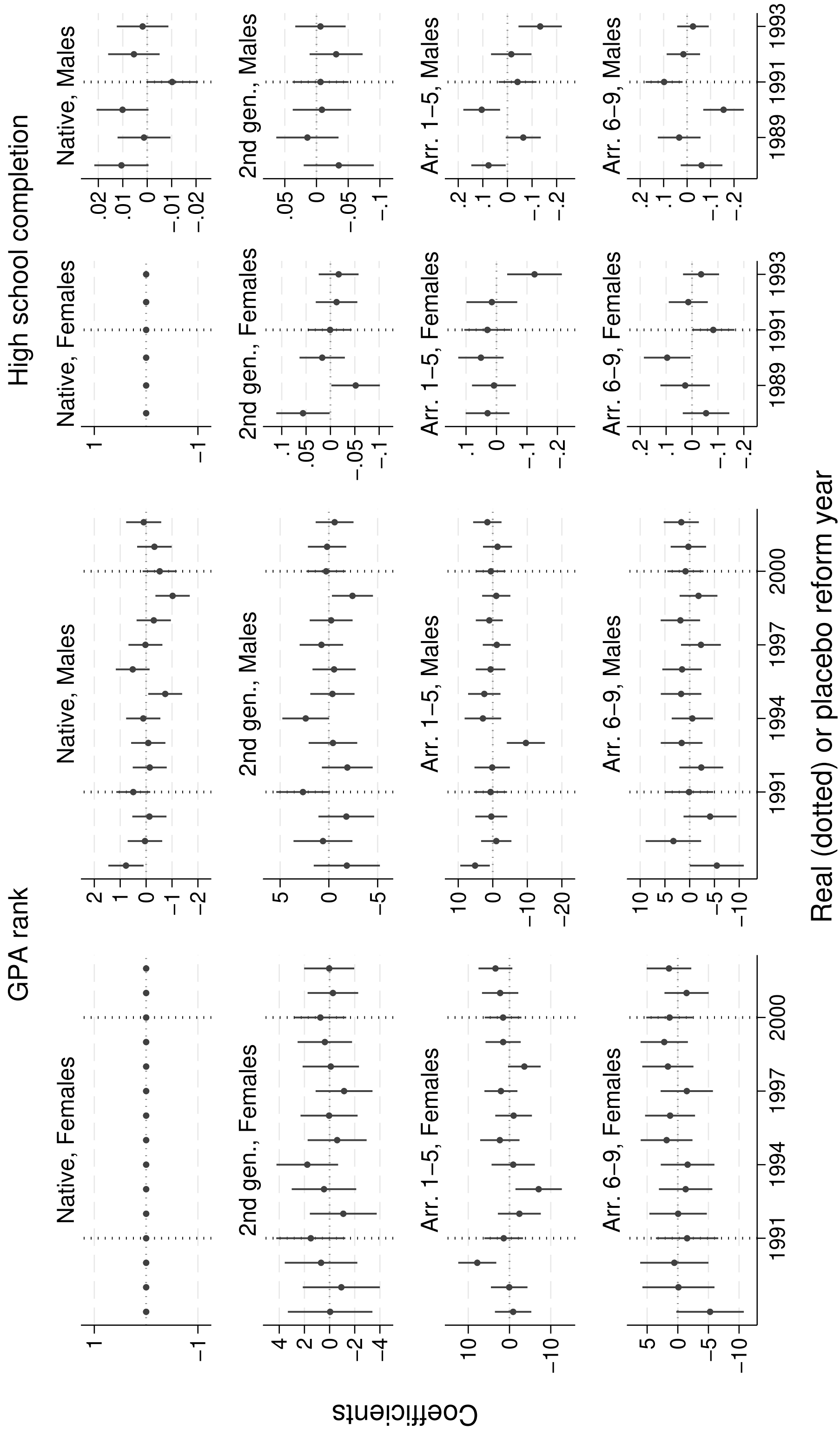
Similar to socioeconomic background, we recognize little effects on the immigration gradient in the real reform years. Viewed in isolation, the estimates indicate a favorable effect of Reform 97 on GPA rank for second-generation immigrants. However, this is not matched by corresponding effects on high school completion, casting some doubts on the findings' substantive significance. For first-generation immigrant groups, the estimated effect patterns appear a bit erratic, most likely reflecting differences in the composition of immigrant cohorts, e.g. with respect to the country of origin. Put differently, these significant effects could be spurious in nature.

In sum, the placebo reform years return a higher proportion of significant coefficients than the real reform years (13.5% vs. 4.8%), although the difference is not statistically significant (p-value = 0.26). The distinction between real and placebo reform effects remains non-existent when we examine GPA rank and high school completion separately (p-values = 0.22 and 0.62), or females and males separately (p-values = 0.30 and 0.53).

4.3. Relative age

Has commencing school sooner affected the influence of relative age? In Figure 7, we categorize birth months into five groups: January to February, March to April, May to August, September to

← **Figure 5.** Reform-induced changes by parental earnings rank (PER) decile. Each coefficient comes from a regression on a two-year sample, where the second year is set to be the real or placebo reform year. The real reform years are 1991 and 2000. We present separate estimates for children with a home-making parent, with working parents, males, and females. We focus on births between 1988 and 2002 for GPA, which is assigned at age 15 to 16 for most children. For high school completion, which is assessed by age 21, we restrict attention to births between 1988 and 1993. Parents are ranked based on their top three earnings from ages 40 to 46 and the child's birth year. Children are categorized into five classes: bottom class (decile 1), lower class (deciles 2 to 3), middle class (deciles 4 to 7), upper class (deciles 8 to 9), and top class (decile 10). A home-making parent is defined as either parent earning less than two times the Basic Amount when the child was five. The reference group comprises middle-class females with a home-making parent. The markers pinpoint the estimated coefficients, and the vertical whiskers represent the 95 percent confidence intervals.


GPA rank
High school completion
Native, Females
Native, Males
2nd gen., Females
2nd gen., Males
Arr. 1–5, Females
Arr. 1–5, Males
Arr. 6–9, Females
Arr. 6–9, Males
Coefficients
Real (dotted) or placebo reform year

October, and November to December.[2] We set the reference category to females born mid-year (May to August) and include native as well as immigrant children in the analysis.

Here, we would have expected to find some negative relative effects of both reforms on those born late in the year (the young-for-grades). However, with the exception of a negative effect of Reform 97 on high school completion for November-December-born boys, we see no clear pattern in this direction. The figure indicates that the reforms have not generated a notable impact on the relative age gradient. Of the 27 true reform coefficients, 2 are statistically significant at the 5% level, compared to 17 out of 162 of the placebo reform coefficients. The *p*-value of difference in proportions is 0.62. The lack of real versus placebo differential remains when we slice the comparison by gender, outcome, or reform. This indicates that statistically significant results arise in the real and placebo reform years alike, as observed with parental earnings rank and immigration background.

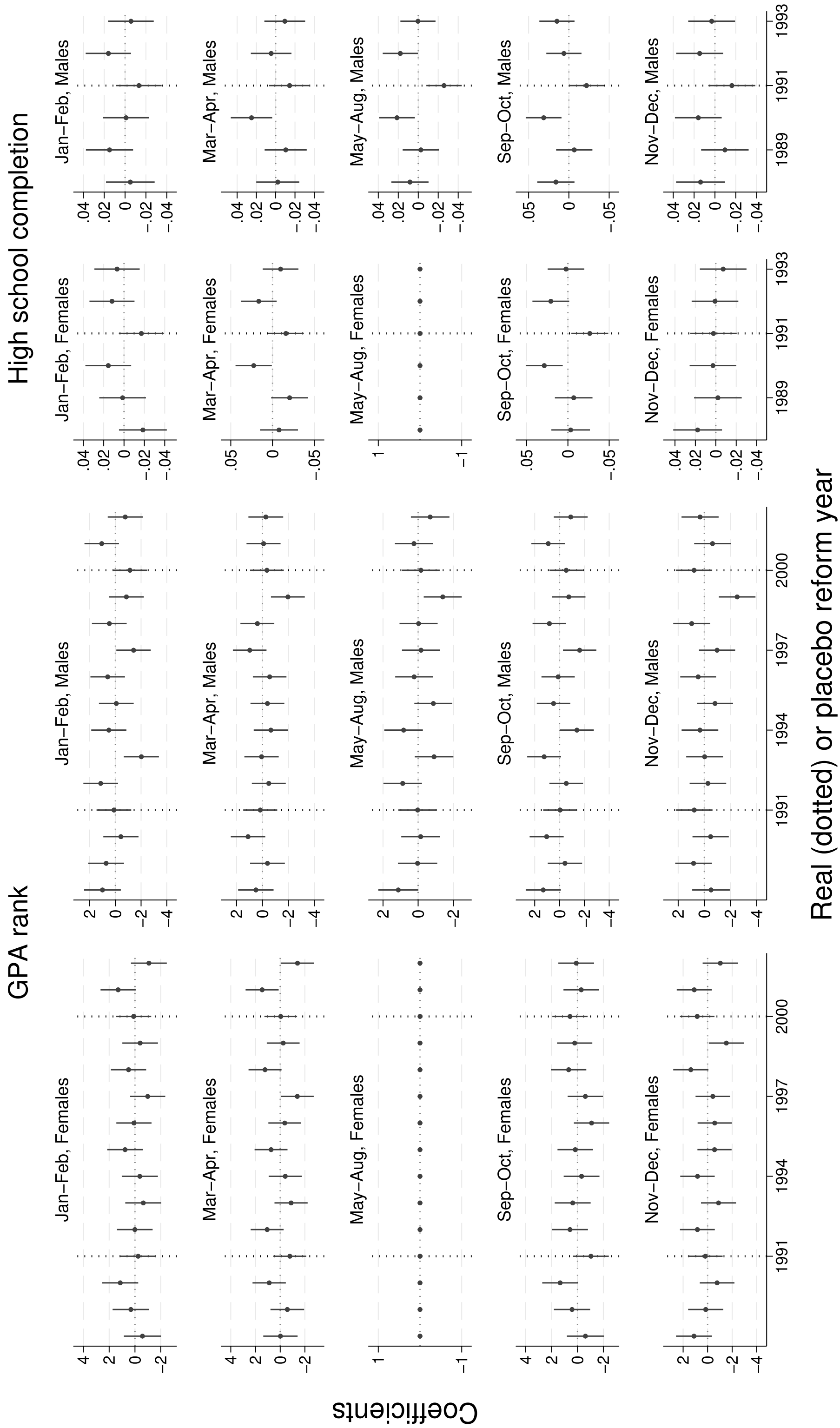
An alternative way of assessing the reforms' implications for relative age effects is to apply a regression discontinuity (RD) approach for offspring born in December one year and in January the next year. These offspring are born only up to a few weeks apart, but those born in December will normally start one year earlier in school. Hence, whereas the December-born will typically be the youngest in class, the January-born will be the oldest. In Appendix B, we provide an RD analysis for three different periods (avoiding the two reform years); i) before Reform 97, ii) between Reform 97 and Knowledge Promotion, and iii) after Knowledge Promotion. The analysis confirms that there are indeed large relative age effects. In particular, the oldest children in class typically rank 5 percentage points higher in the GPA distribution than the youngest in class. However, when we compare these discontinuities across the three periods by means of a difference-in-regression-discontinuities (DRD) analysis, we find no evidence that these relative age effects changed as a result of any of the two reforms, and this applies regardless of class background and gender. Hence, in the current context, it does not appear to be a greater disadvantage to be $5\frac{1}{2}$ rather than $6\frac{1}{2}$ when starting school than it was to be $6\frac{1}{2}$ rather of $7\frac{1}{2}$, and it has not become a greater disadvantage to be $5\frac{1}{2}$ rather than $6\frac{1}{2}$ after Knowledge Promotion than it was before.

4.4. Robustness checks

In Appendix C, we conduct several robustness checks to verify the lack of clear reform effects. So far, we have used parental prime-age earnings rank to capture family background. An alternative measure would be to use parental education. In Figure C.1, we repeat the social gradient analysis, this time using parental education level to represent socioeconomic status. We divide children into four groups based on the highest education level of the parent with the highest education: compulsory education, secondary schooling, bachelor's degree, or master's degree or above. We find that, akin to parental earnings, the parental education gradient is unresponsive to the reforms.

To assess the reforms' impact by alternative care mode, we have, up to this point, used parental employment at the child's age five to proxy for home-making and thus home care. What is also available to us is kindergarten coverage rate by municipality. In Figure C.2, we assign kindergarten coverage in a child's municipality at birth, and reevaluate the effect of an early school start on the social

← **Figure 6.** Reform-induced changes by immigration category. Each coefficient comes from a regression on a two-year sample, where the second year is set to be the real or placebo reform year. The real reform years are 1991 and 2000. We present separate estimates for males and females. We focus on births between 1988 and 2002 for GPA, which is assigned at age 15 to 16 for most children. For high school completion, which is assessed by age 21, we restrict attention to births between 1988 and 1993. Children are categorized into four immigration groups: natives, second-generation immigrants from low- or middle-income countries, first-generation immigrants from low- or middle-income countries arriving between ages one and five, and between ages six and nine. Immigrants from high-income countries are treated as natives. The reference group comprises native females. The markers pinpoint the estimated coefficients, and the vertical whiskers represent the 95 percent confidence intervals.


GPA rank
High school completion
Jan–Feb, Females
Jan–Feb, Males
Mar–Apr, Females
Mar–Apr, Males
May–Aug, Females
May–Aug, Males
Sep–Oct, Females
Sep–Oct, Males
Nov–Dec, Females
Nov–Dec, Males
Coefficients
Real (dotted) or placebo reform year

gradient. For each birth cohort, we compute the average coverage rate from age one to five, and define low coverage as the birth municipality having below-median kindergarten coverage. We observe no clear differences with respect to alternative childcare mode, perhaps with the exception of a slight decline in the relative performance of lower-class boys in high-coverage municipalities. However, children's school performance or high school completion vary from year to year, and we again fail to identify significant changes to the social gradient in the reform years.

With respect to immigration, we have thus far focused on immigrant children from low- or middle-income countries. What if we had considered all immigrants regardless of their origin country? In Figure C.3, we redefine immigrant status to comprise all immigrants (that additionally include immigrants from high-income countries). The proportion of immigrants increases from 6.1% to 7.8%. We establish similar findings as before, namely, there is insubstantial evidence of a change in the immigration gradient.[3]

By and large, these robustness checks point to the lack of clear results that were previously established. With few exceptions, lowering the school starting age irrespective of pedagogy does not flatten the socio-demographic gradients as the reforms intended.

4.5. Further results

In Appendix D, we examine the impact of lowering school starting age from seven to six has on the social gradient of two additional education outcomes: university degree and years of non-compulsory education by age 27. We define university degree as a binary measure that equals 1 if the individual has acquired at least a bachelor's degree by age 27. For non-compulsory education, we consider years of education above grade 9/10 by age 27. While there is an upward trend in university completion and non-compulsory education (see Figure D.1), it does not seem to be noticeably affected by Reform 97. As per Figures D.2, D.3, and D.4, an early school entry has little impact on the social gradient, the role of immigration status, or the effect of relative age, in keeping with the main results on high school completion.

In Appendix E, we turn attention to early-life labor market outcomes including full-time employment and earnings rank. We define full-time employment as a dichotomous measure that equals 1 if the individual earns over three times the Basic Amount at age 27. To construct earnings rank, we rank individuals' taxable earnings at age 27 by birth cohort. We observe that children from the bottom decile, immigrants arriving after school starting age, and young-for-graders are still among the weakest performing in the labor market (see Figure E.1). Yet, once again, we find no clear evidence of changes in the gradients that coincided with Reform 97 in Figures E.2, E.3, or E.4. That said, these estimated effects may have been dominated by other time-varying patterns such as cyclical fluctuations in employment.

5. Concluding remarks

Using population data from Norway, we study how two compulsory schooling reforms altered children's relative schooling outcomes along the dimensions of socioeconomic status, immigration status, relative age, and gender. The first reform, Reform 97, lowered school starting age from seven to six and extended compulsory education from nine to ten years. The second reform, Knowledge

← **Figure 7.** Reform-induced changes by birth month. Each coefficient comes from a regression on a two-year sample, where the second year is set to be the real or placebo reform year. The real reform years are 1991 and 2000. We present separate estimates for males and females. We focus on births between 1988 and 2002 for GPA, which is assigned at age 15 to 16 for most children. For high school completion, which is assessed by age 21, we restrict attention to births between 1988 and 1993. Children are categorized into five groups based on their birth months: January to February, March to April, May to August, September to October, and November to December. The reference group comprises females born between May and August. The markers pinpoint the estimated coefficients, and the vertical whiskers represent the 95 percent confidence intervals.

Promotion, introduced structured learning at age six, in some sense reducing school starting age again from seven to six. We use rolling difference-in-differences-type models to identify the relative effects of the reforms on children's GPA rank at age 15 to 16, as well as their high school completion by age 21. We find that the reforms did not noticeably alter the socio-demographic gradients, at least not in any systematic way. With respect to Reform 97, our results fit well into the findings by Drange, Havnes, and Sandsør (2016), who found no reform effect on average outcomes and also failed to identify significant effect heterogeneity with respect to family background. Our results also align with the findings reported by Markussen and Røed (2023) that the declining upward mobility for children born into disadvantaged families has continued undisturbed during the reform periods.

These findings contrast with one of the aims of both reforms, which was to narrow existing achievement gaps, especially with respect to socioeconomic status and immigrant background. For the reforms to enhance social mobility, they need to benefit socially disadvantaged children more than they do socially advantaged children. However, in our analysis, we have failed to establish clear evidence that either lower-class or immigrant children gained disproportionately from the reforms, irrespective of gender or the type of care they alternatively would have received. In this sense, the reforms have failed to deliver convincing results. However, in contrast to popular concerns and ongoing debate, the reforms did not exacerbate the relative age and gender differentials. Hence, one could say that the reforms neither achieved their intended goals nor generated their expected adverse side effects. We simply see no clear effects in any direction.

What could have contributed to the reforms' lack of systematic impact on the socio-demographic gradients? The reforms are comprehensive, involving multiple strategies, and the exact impact of each strategy is hard to pinpoint. In the case of Reform 97, it is difficult to separately identify the effects of lowering the school starting age, which took effect at age six, and extending compulsory schooling, which occurred at age 14 to 15. This is because the earliest measure is assigned at age 15 to 16 (i.e. GPA rank). In the case of Knowledge Promotion, the key reform strategy was the academization of the first-year curriculum, and although the reform was implemented at a particular point in time (2006), it may in practice have developed in a more gradual fashion. It is also important to recognize that the effects of certain strategies can counteract each other: students can maladjust to entering school at a younger age, but thrive in the extra year of compulsory education. Moreover, the implementation of Reform 97 implied that two birth cohorts started school at the same time, perhaps putting an exceptional pressure on limited school resources. This may have affected different children differently in ways that modulated pupils' positional responses to the reform. Meanwhile, another implication of Reform 97 was that children born 1992 and onward enjoyed easier access to kindergartens, as the reform freed up places previously occupied by the six-year-olds. The interplay of kindergarten and school starting age could thus be keeping relative gains and losses in check.

Notes

1. Looking at outcomes for the first two birth cohorts included in the present paper (those born in 1988 and 1989) we note that children with only compulsory education are more likely to either become social insurance claimants or earn less than those with higher levels of education. They are also more likely to work in skilled trades, such as manufacturing, construction, and trade/repair, or in service-oriented occupations, such as sales.
2. We recognize that starting early or late correlates systematically with birth month. When we use pupils' actual age at start rather than their default age to estimate the average treatment effect (instead of the ITT effect), we obtain very similar results. We do not report these results because using actual school starting age reintroduces the selection problems that motivated our ITT analysis in the first place.
3. When we exclude from our definition immigrants from other Scandinavian countries (Sweden and Denmark), the null findings remain.

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Disclosure statement

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Data availability statement

Administrative registers made available by Statistics Norway have been essential. Due to the confidential nature of these registers, we are unable to deposit data in a publicly available repository. Applications to access this microdata are assessed by Statistics Norway. Applications should contain the purpose of access, processing period, the person responsible, the datasets and variables required, where and how data will be stored, a list of people with access, the applicant's legal basis according to GDPR, a data protection impact assessment, and a description of the linking process. Access will be granted if the conditions set out in application guidelines are met. More information is available at https://www.ssb.no/en/data-til-forskning/utlan-av-data-til-forskere. Questions related to access should be directed to mikrodata@ssb.no.

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