



Tracking in context: Variation in the effects of reforms in the age at tracking on educational mobility

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ABSTRACT

Previous research found that increasing the age at first tracking in an education system increased educational mobility. This research has implicitly assumed that these effects do not vary across contexts. Contrary to this assumption, we develop two hypotheses predicting such variation. The first hypothesis predicts that changes in the age at tracking increase educational mobility more for larger than for smaller changes in the age at tracking. According to the second hypothesis, reforms in the age at tracking only increase educational mobility if they occur in societies which put a high emphasis on equality of opportunity as a policy aim. We test these hypotheses by estimating the effects of reforms in the age at tracking, which occurred in five European countries (Belgium, Denmark, Finland, France, and Italy) in the 20th century, on educational mobility. We use data from the European Social Survey (ESS) and the Survey of Health, Ageing and Retirement in Europe (SHARE). The effects of the reforms are identified using a regression discontinuity design (RDD). A third hypothesis tests if the reforms increase educational mobility more among women than among men and if this is particularly the case in countries with a more gender egalitarian climate. Overall, the results reveal little cross-country variation in the effects of reforms in the age at tracking on educational mobility. In all analyzed countries there is an increase in educational mobility due to the reform in the age at tracking. In most countries, these effects do not differ between men and women.

1. Introduction

Educational systems can affect the intergenerational transmission of education (Breen and Jonsson, 2005; van de Werfhorst and Mijs, 2010). Previous research has identified one central institutional factor as influencing the intergenerational transmission of educational advantage: between-school tracking, which decreases educational mobility (e.g., Canaan, 2020). The extent to which tracking impacts educational mobility depends on the age at which between-school tracking takes place (Brunello and Checchi, 2007; Hanushek and Wößmann, 2006; Horn, 2009, 2013; Pfeffer, 2008).¹ A later age at tracking in an education system increases

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¹ In line with the previous European literature on this topic, the focus in this article is on between-school and not on within-school tracking (Chmielewski, 2014).

intergenerational educational mobility (van de Werfhorst and Mijs, 2010).

The most convincing empirical evidence in favor of causal effects of the age at (first) tracking on intergenerational educational mobility comes from research estimating the effects of educational reforms in the age at (first) tracking (e.g., van de Werfhorst, 2018, 2019). This literature has implicitly assumed that the effects of such reforms do not vary across contexts. In other words, the empirical models applied in this literature assume that reforms in the age at tracking increase educational mobility independent of the context in which these reforms are implemented. This is a strong assumption, which has not yet been empirically tested. There are, however, theoretical reasons to expect heterogeneity in the effects of reforms in the age at tracking on the intergenerational transmission of education by context.

In this study, we develop two expectations of such variation. First, the effects of the reforms may vary with the size of the change in the age at tracking. There may be reforms in the age at tracking, which are too incremental, to increase educational mobility. Research has often used a dichotomy of early and late tracking countries based on a cut-off at age 15 (e.g., Hanushek and Wößmann, 2006; Scheeren, 2022). There is, however, considerable variation in terms of age at tracking within both the early and the late age at tracking group. It seems, however, possible that reforms in the age at tracking could increase educational mobility more if they shift the age at tracking by more compared to fewer years.

Second, the effects of reforms in the age at tracking on educational mobility may be stronger in countries with stronger egalitarian values. In a climate of more egalitarian societal values there may be more possibilities for reforms to translate into actual outcomes for students from socio-economically disadvantaged backgrounds and the intended equalizing effects of the reforms might more easily materialize. This hypothesis was articulated by earlier research (van de Werfhorst, 2018:32) but it has so far not been empirically tested.

The present study tests whether the effects of reforms in the age at tracking on educational mobility vary across countries. There are two kinds of previous studies on the effects of age at tracking on educational mobility, neither of them allows us to answer this question. On the one hand, previous research investigated the effects of reforms in tracking age in single country studies (e.g., Cnaan, 2020 on France; Dronkers, 1993 on the Netherlands). Variation in results across different studies can be based on differences in the research design and the operationalization of variables. Yet, it is possible that these differences are due to contextual differences. On the other hand, previous comparative research has analyzed multiple countries in difference-in-differences designs and therefore, by design, excluded the possibility of cross-country variation in the effects of reforms in the age at tracking on educational mobility (Brunello and Checchi, 2007; Hanushek and Wößmann, 2006; Scheeren, 2022; van de Werfhorst, 2018, 2019). Our approach allows us to establish the role of contextual factors in the relationship between reforms increasing the age at tracking and educational mobility.

In addition to cross-country variation in the effects of reforms in the age at tracking on educational mobility, we analyze gender differences in the effects of these reforms. The investigation of gender differences is motivated by a previous study that found reforms in the age at tracking to increase educational mobility more among women than among men (Scheeren, 2022). Our analysis tests whether we can replicate this finding for each of the five age at first tracking reforms we analyze, or whether the effects of age at first tracking reforms on educational mobility vary according to a country's level of gender egalitarian values.

While we focus on the potentially positive effects of age at first tracking reforms on educational mobility, there could also be negative effects of these reforms on average educational attainment. This would be the case if there were a trade-off between equality and efficiency with respect to tracking (Coleman, 1993). This could be the case, for example, if the quality of teaching declined because of children spending more time in mixed ability classes (Borghans et al., 2020). While this mainly applies to test scores as an outcome, a decrease in average test scores as a result of the reform could also lead to a decrease in average educational attainment. However, Hanushek and Wößmann (2006) found that tracking led to higher average test scores while increasing educational mobility. Their study therefore casts doubt on the empirical existence of a trade-off between equality and efficiency.

We analyze reforms in five Western European countries, Belgium, Denmark, Finland, France, and Italy, that increased the age at first selection into different educational tracks. These countries have centralized educational systems and implemented reforms that increased the age at first tracking between two and five years. To this end we analyze data from the European Social Survey (ESS) and the Survey of Health, Ageing and Retirement in Europe (SHARE).

2. Background and theoretical considerations

2.1. Between-school tracking and educational mobility

From a functional perspective, between-school tracking has been developed to respond to increasingly differentiated needs from the labor market. From the perspective of conflict theory, however, for upper-class families differentiated tracks were a means to distinguish themselves from the lower classes and to emphasize social class differences in times of educational expansion (Bol and Herman, 2013; Collins, 1971).

Educational systems differ in their degree of selectivity (Allmendinger, 1989) and more stratified systems are associated with a lower degree of educational mobility (Pfeffer, 2008). Between-school tracking comes with a trade-off: While facilitating school-to-labor-market transitions, it tends to decrease educational mobility (Bol and Herman, 2013; Brunello and Checchi, 2007; Fischer et al., 2022; van de Werfhorst, 2018, 2019). The latter seems to be particularly the case when tracking occurs earlier in educational careers (for a review see van de Werfhorst and Mijs, 2010).

In an analysis using internationally comparative data from PISA, Montt (2011) estimated the impact of education policies, including between-school tracking, on the dispersion of achievement within countries as a measure of inequality. He found that between-school tracking was a key driver of inequality. Our study focuses on educational mobility, that is, the inverse of the

association between parental education and child education, rather than the overall dispersion of achievement, and differs in this respect from Montt (2011). A second difference is that we use educational attainment as the outcome instead of test scores. Finally, we use a different research design than Montt (2011).

Several theories and previous studies suggested that apart from the potential negative effects on educational mobility, tracking could also have positive consequences. These go back to the original justification of tracking when it was introduced. In the very beginning, tracking was implemented to respond to demands from the labor market for a more specialized workforce (Bol and Herman, 2013). Moreover, it has important advantages for school-to-labor-market transitions (Bol and Herman, 2013). It has also been argued that tracking might enhance teaching efficiency by targeting more homogeneous student groups (Borghans et al., 2020). Focusing more precisely on educational mobility, Coleman (in Kerckhoff, 1993) argued that tracking could lead to a higher average achievement and could ensure that children born with high abilities can develop their potential.

However, Hanushek and Wößmann (2006) found that early tracking reduced average achievement and decreased educational mobility. They concluded that for this reason there was no equality-efficiency trade-off with respect to tracking. Unlike Hanushek and Wößmann (2006) study, our analysis is limited to educational attainment, as we do not observe test scores in our data. Therefore, we cannot directly test the equality-efficiency trade-off in our data (although we observe as discussed below that in Belgium the reform in the age at tracking reduced average educational attainment).

2.2. Why should reforms in the age at tracking affect educational mobility?

Previous research identified the age at between-school tracking as a key characteristic of educational systems that influences the intergenerational transmission of education (Sørensen, 1970; van de Werfhorst and Mijs, 2010). Various studies compared educational mobility in societies with earlier and later tracking in the education system with the general conclusion being that increasing the age at tracking increased educational mobility (Bol and Herman, 2013; Brunello and Checchi, 2007; Hanushek and Wößmann, 2006).

Several factors explain why a higher age at (first) tracking contributes to increasing educational mobility. First, parental interference in education is more important for younger students than for older ones (Brunello and Checchi, 2007) and this seems to contribute to the finding that earlier tracking accentuates the impact of family background (Ammermüller, 2005; Hanushek and Wößmann, 2006; Schütz et al., 2008). Furthermore, at a younger age there is more uncertainty about students' abilities, which can lead to a misallocation to tracks when tracking happens early (Borghans et al., 2020). This is particularly the case for students from socioeconomically disadvantaged families. Increasing the age at the first selection might imply a better fit between students' abilities and the attended track (Borghans et al., 2020; Brunello and Checchi, 2007; Brunello et al., 2007; Canaan, 2020). This can be most important for children from socioeconomically disadvantaged families, whose parents tend to be more reluctant to intervene in the educational decision-making of their children (Lareau, 2011) and who are more likely to be allocated to lower tracks (Shavit and Westerbeek, 1998). At the same time, resulting from the reforms, some groups might experience disadvantages; for example, higher SES students or, as suggested by Pekkarinen et al. (2009), boys with lower educated parents. Therefore, it is important to establish if there are heterogeneous effects of the reforms on different groups of students.

Studies on the long-term effects of increases in the age at first tracking show positive long-lasting effects including higher earnings (Borghans et al., 2020; Canaan, 2020). Borghans et al. (2020) found that early tracking led to a decrease in higher education completion, and that it lowered earnings for low- and medium-ability students. These negative effects appeared to be largely driven by a misallocation to tracks when tracking happened early (Borghans et al., 2020). Postponing the age at tracking is likely to be important for the offspring of lower educated parents compared to their peers with parents with higher levels of education. This is also stipulated by the life course hypothesis (Crul and Vermeulen, 2003; van de Werfhorst, 2019), which argues that when secondary school tracks are assigned at younger ages, disadvantaged children cannot show their full potential.

On the other hand, tracking can enhance teaching efficiency because instruction is targeted towards a more homogeneous group of students and, therefore, earlier tracking may benefit educational performance (Borghans et al., 2020). As mentioned above, tracking also has important advantages for school-to-labor-market transitions (Bol and Herman, 2013). These, however, are not universal but likely dependent on contextual labor market structures.

2.3. Cross-country differences in the effects of reforms in the age at tracking on educational mobility

Whilst previous research made a theoretical and empirical case that reforms in the age at between-school tracking affected educational mobility, this research did not investigate cross-country variation in the effects of reforms in the age at tracking on educational mobility. We argue that there are two possible hypotheses predicting such cross-country variation.

First, we expect the effects of reforms in the age at tracking to increase with larger changes in the age at tracking. In other words, these reforms may increase educational mobility more if tracking age is increased for a larger number of years, but there may be no measurable increases in educational mobility if age at tracking is increased only by one or two years. As a result, a two-year change in the age at tracking may have less positive consequences for educational mobility than a five-year change in the age at tracking.

Second, there may be variation in the effects of reforms in the age at tracking on educational mobility across contexts based on the egalitarian values in a society. van de Werfhorst (2018:32) speculated that: "It may be that the reform towards comprehensive education was particularly effective in societies with a broader egalitarian ideology." In line with this reasoning, we test the hypothesis that reforms in the age at tracking only increase educational mobility in contexts of high egalitarian values.

2.4. Gender differences in the effects of reforms in the age at tracking on educational mobility

The effects of reforms of the age at tracking on educational mobility might depend on students' gender as well as on the gender egalitarian climate in a specific context in which the reform is implemented. Increases in age at tracking occurring in adolescence may coincide with the period when the male-female gap in academic performance widens to the benefit of women (DiPrete and Buchmann, 2013; Sammons, 1995; Scheeren et al., 2018). Due to the gender difference in the timing of puberty, later tracking may benefit girls more than boys. At age 14 to 16 girls are ahead of boys in terms of biological and psychosocial development (Pekkarinen et al., 2009; Scheeren et al., 2018).

With respect to educational mobility, Scheeren (2022) found that socioeconomically disadvantaged girls benefited from increases in the age at tracking more than boys. In other words, she found that the positive effects of age at tracking reforms on educational mobility were only there for women but not for men. Scheeren's (2022) analysis was based on a difference-in-differences approach and did not test whether the positive consequences of reforms in the age at tracking were concentrated among women with respect to each reform included in her analysis. We provide such a test.

In addition, we argue that the gender egalitarian climate in a society might moderate the effects of the reforms in the age at tracking differently for women and men. More specifically, we hypothesize that a more gender egalitarian climate in a society increases the concentration of the positive effects of the reforms in the age at tracking on educational mobility more among women than among men.

3. Background on the educational reforms analyzed in the present study and hypotheses

Our empirical analysis estimates the effects of reforms in the age at tracking on educational mobility. We include five countries which have undergone educational reforms in the age of tracking. To select the sample of countries included in our analysis, we relied upon the collection of European educational reforms done by Fort (2006) and Garrouste (2010). From the reforms described in these collections, we only selected reforms that changed the age at tracking and did not implement other radical changes in the education system at the same time. This was necessary for identifying and isolating the effect of tracking age reforms. Hence, several countries were excluded from the analysis as in addition to the reform in age at tracking other changes were implemented. The described criteria lead to including five countries into the study: Belgium, Denmark, Finland, France, and Italy. In the following, we describe the included reforms in detail. Table 1 gives an overview over each country included in the analysis, the year of the reform in tracking age, and the cohorts affected by the country-specific reforms, as well as the contextual factors that we expect to moderate the effects of the reforms.

Belgium 1971. In 1971, Belgium implemented a reform which changed the age at between-school tracking (Fort, 2006; Wielemans, 1991). The 1971 reform introduced a comprehensive secondary education system which replaced the four different types of secondary schools in place before the reform. For that reason, the reform postponed tracking from age 10 to 12. Wielemans (1991) reported several positive developments stemming from the reform: The years of schooling as well as participation in higher education increased and the proportion of dropouts was reduced.

Denmark 1958. Denmark restructured lower secondary education in 1958 (Arendt, 2005; Fort, 2006; Garrouste, 2010). The reform replaced the middle school from grade 6 to 9 with a comprehensive lower secondary education system. Therefore, the age at tracking was increased by the reform from age 11 to 14.² Following the years of the reform, educational attainment levels were higher compared to pre-reform years (Arendt, 2005).

Finland 1972–1977. This “Comprehensive School Reform” in Finland replaced the selective system in which students were split at age 11 into an academic and a vocational track by a single mixed-ability nine-year comprehensive school which students attend up to age 16 (Pekkarinen et al., 2009; Simola, 2002). The reform was implemented in different regions at different points in time (Fort, 2006; Österman, 2021; Pekkarinen et al., 2009; Simola, 2002), as described in Table S1 in the Online Supplement.

France 1977. The French reform analyzed in the present study is the so-called “Haby reform”. The reform merged the previously existing tracks and required all pupils to attend the same curriculum inside a *collège* (Duru-Bellatt and Kiefer, 2000). Effectively the reform delayed the distribution of pupils to vocational and general academic tracks by two years, from age 11 to age 13 (Canaan, 2020).

Italy 1963. With its “Middle School Reform”, Italy introduced the “scuola media unica”, the unified middle school (Flabbi, 1999; Fort, 2006; Shavit and Westerbeek, 1998). Before this reform, there have been two separate and hierarchical lower secondary school tracks, which have been unified into one by the reform. The reform changed the age at tracking from 11 to 14. Prior research reveals little effect of the reform on equalizing the educational selection process (Shavit and Westerbeek, 1998).

What are the expectations of our hypotheses for the effects of these educational reforms? The first hypothesis about cross-country variation in the effects of the reforms in the age at tracking predicts stronger effects of the reforms in Finland than in the other countries because the former reform increased age at tracking by five instead of fewer years (see Table 1).

The second hypothesis about cross-country variation in terms of social equality makes a different prediction: This perspective leads us to expect the strongest effects of the reforms in Finland, followed by Denmark and France, followed by Belgium and Italy. This is because these contexts show decreasing degrees of social equality (see Table 1).

Finally, with respect to gender inequalities in the effects of the reforms in the age at tracking, we report in Table 1 how the countries differed in the level of gender equality at the time of the reform (Dilli et al., 2019). Gender equality was particularly high in Finland and

² Parallel to the reform, a difference between village and city schools was eliminated. This implied that the number of lessons, which prior to the reform was lower the village schools, was adjusted to the number of lessons in city schools (Arendt, 2005).

Table 1

Overview of the reforms in tracking age and the Definition of cohorts.

Country (Year of Reform)	First Birth Year Affected by the Reform (Actual Implementation)	Increase in Age at Tracking	Before- Reform Cohort	After-Reform Cohort	Parties' standpoint regarding "Positive concept of social justice and the need for fair treatment of all people" ^{a,b}	Historical Gender Equality Index (Dilli et al., 2019)
Belgium (1971)	1960	10 → 12	1953–1959	1961–1967	2.79	66.22
Denmark (1958)	1948	11 → 14	1941–1947	1949–1955	5.16	65.96
Finland (1972) ^a	1961	11 → 16	1954–1960	1962–1968	16.22	71.94
France (1977)	1966	11 → 13	1959–1965	1967–1973	5.09	68.44
Italy (1963)	1950	11 → 14	1943–1949	1951–1957	0.85	65.61

Notes.

^a In Finland the reform was implemented in different regions at different time points as detailed in [Table S1](#) in the *Online Supplement*.^b Weighted by percentage obtained in election closest to the reform date (0–25).

France. We expect therefore that female educational mobility increased more than male educational mobility in Finland and France. In the other contexts, gender equality was lower and we, therefore, expect no gender differences in the effects of the reforms on educational mobility in Belgium, Denmark, and Italy.

4. Data and methods

4.1. Data and case selection

We use data from the European Social Survey (ESS; [European Social Survey Round 1-10 Data, 2020](#)) and the Survey of Health, Ageing and Retirement in Europe (SHARE; [SHARE-ERIC, 2022](#)). We use both data sources to increase the sample sizes. This is needed to improve the precision of our estimates. Whilst the ESS provides a sample which is representative for the adult population in each country, SHARE is representative of the population aged 50 years and older as well as their partners ([Börsch-Supan et al., 2013](#)). Both data sets cover the countries with reforms included in our analysis and the cohorts that have been affected by the reforms. We use data from waves 1 to 10 (2002–2020) from the ESS and waves 1 to 8 (2004–2017) from SHARE ([Börsch-Supan, 2019](#)). SHARE is a panel data set. However, with respect to each respondent we only use the information once, and we always use the most recent information to measure educational attainment.

We also analyzed each data source separately. However, these estimates were very imprecise; in other words, they had large standard errors, and we therefore decided not to interpret and report them.

4.2. Variables

Educational Attainment. We use three variables to measure final educational attainment. First, we use years of education. Years of education is based on country-specific codings of the ISCED-1997-classification of the level of education reported by the respondents, following [Fort \(2006\)](#). Second, we use a binary variable indicating the completion of upper secondary education. This corresponds to categories 3 and higher on the ISCED-1997-classification of levels of education ([UNESCO, 2006](#)). Third, we measure post-secondary education via a binary variable indicating if a respondent has obtained a university degree, including at a university of applied sciences. This corresponds to ISCED categories 5 and higher.³

Parental Education. We measure parental education using the highest educational degree of any parent. In all countries, we distinguish between the same three levels of parental education: (1) low, (2) middle, and (3) high. A low level of education refers to ISCED categories 1 and 2, a middle level of education refers to ISCED categories 3 and 4, and a high level of education refers to ISCED categories 5 and 6. Throughout the analysis the category parental middle education is used as the reference category. We use parental education as a measure of social origin because it has by far the largest number of valid responses. We do not report models using parental occupation as a measure of social origin because there are too many missing values on this variable.

Reform. Exposure to the reform is defined by respondents' year of birth. The distribution of men and women to the pre- and post-reform cohorts is described in [Table 1](#) above.

Control Variables. In the combined analyses of men and women, we control for gender. We report separate results for men and women to test the above-stated hypothesis about gender differences in the effects of reforms in the age at tracking on educational inequalities. In addition, we control for year of birth as a linear time trend. This variable takes up general time trends, which may

³ ISCED is an educational classification developed by UNESCO ([UNESCO 2006](#); ISCED 1997 distinguishes the following levels of education: (0) pre-primary education, (1) primary education, (2) lower secondary education, (3) upper secondary education, (4) post-secondary non-tertiary education, (5) first stage of tertiary education, and (6) second stage of tertiary education.

otherwise confound the causal effect of the reforms, as described in detail in the “Analytical Strategy” section.

Table 2 reports descriptive statistics for all five countries on all variables used in the analysis for all observations as well as for the before- and after-reform cohorts. Across countries, all three outcomes (i.e., years of education, attendance of upper secondary education, and attendance of post-secondary education) are higher for the after-reform cohorts compared to the before-reform cohorts.

4.3. Analytical Strategy

We analyze the effects of reforms in age at tracking on educational mobility using a regression discontinuity design (RDD). In other words, we estimate the effect of the reform—separately for each country—controlling for a time trend before and after the reform. This corresponds to a local linear approach of the RDD (Bueno de Mesquita and Fowler, 2021:247–9). We also tested a specification in which we control additionally for a square term of time (year of birth). Results of this alternative specification are reported in Table S2 in the Online Supplement and are virtually identical to those using the linear specification.

We estimate the following OLS and Linear Probability Models (LPMs):

$$\text{Educ}_c = a + b_1 \text{Edu}_p + b_2 \text{Ref} + b_3 \text{Edu}_p * \text{Ref} + b_4 t + b_5 \text{Ref} * t + b_6 G + \varepsilon \quad (1)$$

with Educ_c being respondent’s education (in terms of the three different variables), Edu_p being parental education and Ref being the indicator of whether a respondent was exposed to the reform. The interaction between Edu_p and Ref is the quantity of interest for the analysis, as it indicates the causal effect of the reform on intergenerational educational mobility. t is time (year of birth) and G a control for gender.

We consider three dependent variables to capture three nuances of potential effects of the reforms on educational mobility. The three dependent variables are years of education, upper secondary education completion, and post-secondary education completion. Years of education is a general measure of educational attainment, which provides estimates of the overall effect. Analyzing upper secondary and post-secondary education allows us to establish at what level of education the reforms impacted educational mobility. Upper secondary education is the level of education most directly affected by the reforms. Looking at post-secondary education allows us to determine the consequences of the tracking age reforms for a later transition in the educational career.

We control for a time trend t and an interaction between the reform and the time trend to estimate different trends before and after the discontinuity. We also control, besides gender, for the survey (ESS or SHARE) and the survey wave in case of the ESS (these controls are not shown in equation (1)).

The cohorts have been defined around the first birth year that has been affected by the respective reform (see Table 1), so that the seven birth cohorts just before the first cohort affected and the seven years just after are considered in the analysis.

Importantly, our research design assumes that there is no confounding event occurring at exactly the same time as the education reforms whose effects we are trying to identify. We carefully selected the reforms included in the analysis, based on the academic literature and additional reports and documents on the reforms, to ensure this was the case. The countries included in our analysis did not introduce other reforms to the education system at the same time as they delayed the age of first tracking. This is also the reason why only five countries remain in the analysis, whereas we started with more countries. However, there may have been events that occurred at the same time as these reforms in the countries we studied—and that affected educational mobility—that we were not aware of.

5. Results

The results of the analysis are reported in Tables 3–5. Each table refers to one dependent variable. We report and discuss the results separately for each of the five countries included in the analysis.

5.1. Years of education

Table 3 reports the results with respect to years of education as an outcome. Panel A shows the results for Belgium. In Belgium, the tracking age reform reduced educational attainment (Model 1). We will come back to this finding and its implications in the discussion. The reform increased educational mobility by reducing the gap in educational attainment between children with low and children with highly educated parents from 1.90 years of education to $1.90 - 0.72 = 1.18$ years of education.

An increase of educational mobility by, on average, 0.72 years of education is a strong increase in educational mobility. It corresponds to a $0.72 * 9\% = 6.5\%$ increase in terms of lifetime earnings.⁴ It also has other positive consequences such as an increase in cognitive ability in late adulthood (Schneeweis et al., 2014). This strong increase in educational mobility was similar for both men and women (Models 4 and 6). At the same time, the gap in educational attainment for the offspring from highly compared to the offspring of medium educated parents was unchanged by the reform in the age at tracking.

In Denmark (Panel B), there was no main effect of the tracking age reform on educational attainment (Model 1). However, with respect to changes in educational mobility, the same effects were observed as in Belgium. Educational mobility increased as the gap in the educational attainment between the children with low and the children with medium educated parents was reduced from 1.37

⁴ The return of 9 % for one year of schooling is taken from the meta-analysis by Psacharopoulos and Patrinos (2018).

Table 2
Descriptive statistics.

Variable	Belgium	Denmark	Finland	France	Italy
<i>Panel A: All</i>					
Years of education	13.24 (3.23)	12.72 (2.53)	14.02 (2.29)	13.12 (2.85)	9.27 (3.78)
Compulsory education	0.74 (0.44)	0.81 (0.39)	0.89 (0.32)	0.89 (0.32)	0.35 (0.48)
Post-secondary education	0.43 (0.50)	0.43 (0.50)	0.57 (0.50)	0.38 (0.49)	0.11 (0.32)
Low parental education	0.57 (0.50)	0.43 (0.49)	0.53 (0.50)	0.54 (0.50)	0.91 (0.29)
Medium parental education	0.22 (0.41)	0.41 (0.49)	0.24 (0.43)	0.32 (0.46)	0.06 (0.24)
High parental education	0.22 (0.41)	0.16 (0.37)	0.23 (0.42)	0.14 (0.35)	0.03 (0.17)
Male	0.46 (0.50)	0.51 (0.50)	0.50 (0.50)	0.47 (0.50)	0.46 (0.50)
<i>N (Combined Sample)</i>	4010	3494	2522	2936	3673
<i>N (ESS)</i>	1978	2522	0	2347	1097
<i>N (SHARE)</i>	2032	972	2522	589	2576
<i>Panel B: After-reform cohort</i>					
Years of education	13.35 (3.06)	13.01 (2.31)	14.41 (1.77)	13.65 (2.66)	9.97 (3.66)
Compulsory education	0.79 (0.41)	0.84 (0.37)	0.93 (0.25)	0.92 (0.27)	0.42 (0.49)
Post-secondary education	0.45 (0.50)	0.47 (0.50)	0.62 (0.49)	0.47 (0.50)	0.13 (0.33)
Low parental education	0.51 (0.50)	0.40 (0.49)	0.46 (0.50)	0.44 (0.50)	0.90 (0.30)
Medium parental education	0.25 (0.43)	0.42 (0.49)	0.27 (0.44)	0.38 (0.49)	0.07 (0.25)
High parental education	0.24 (0.43)	0.17 (0.38)	0.28 (0.45)	0.17 (0.38)	0.03 (0.17)
Male	0.46 (0.50)	0.50 (0.50)	0.51 (0.50)	0.48 (0.50)	0.46 (0.50)
<i>N (combined)</i>	1761	1805	1166	1218	1809
<i>Panel C: Before-reform cohort</i>					
Years of education	13.15 (3.36)	12.41 (2.71)	13.67 (2.61)	12.74 (2.91)	8.60 (3.78)
Compulsory education	0.71 (0.46)	0.78 (0.42)	0.85 (0.36)	0.86 (0.34)	0.29 (0.45)
Post-secondary education	0.41 (0.49)	0.39 (0.49)	0.53 (0.50)	0.32 (0.47)	0.10 (0.30)
Low parental education	0.61 (0.49)	0.45 (0.50)	0.59 (0.49)	0.61 (0.49)	0.91 (0.28)
Medium parental education	0.20 (0.40)	0.40 (0.49)	0.22 (0.41)	0.27 (0.44)	0.06 (0.23)
High parental education	0.20 (0.40)	0.15 (0.36)	0.19 (0.39)	0.12 (0.33)	0.03 (0.17)
Male	0.47 (0.50)	0.51 (0.50)	0.49 (0.50)	0.47 (0.50)	0.46 (0.50)
<i>N (combined)</i>	2249	1689	1356	1718	1864

Notes: Means and standard deviations (in brackets).

Sources: ESS, Waves 1 to 10 and SHARE, Waves 1 to 8.

years to $1.37 - 0.52 = 0.85$ years (Model 2). As in Belgium, this increase in educational mobility was found for both men and women (Models 4 and 6).

In Finland (Panel C), the increase in the age at tracking did not affect educational attainment at the population level (Model 1). The reform did increase educational mobility by improving the educational attainment for children with low educated parents more than for children with medium educated parents (Model 2). Contrary to Belgium and Denmark, the increase in Finland was, however, concentrated among men (Model 6). For men educational mobility increased whilst it was unchanged for women (Model 4).

In France (Panel D), the reform in the age at tracking did not affect educational attainment (Model 1). The reform did increase educational mobility by shrinking the gap in educational attainment between children with low and children with medium educated parents from 1.57 to $1.57 - 0.52 = 1.05$ years of education (Model 2). In France, the increase in educational mobility was concentrated among women (Model 4) and no improvement in educational mobility was observed for men (Model 6).

In Italy (Panel E), the reform did neither affect educational attainment at the population level (Model 1). The reform did considerably improve educational mobility (Model 2) but only for women (Model 4) and not for men (Model 6).

5.2. Upper secondary education completion

We turn to the question which levels of education were affected by the reforms in the age at tracking. Table 4 reports the results of LPMs predicting participation in upper secondary education. In Panel A, the results for Belgium are reported. In Belgium, the tracking age reform had no effect on the probability that someone completed upper secondary education (Model 1). However, educational mobility in terms of upper secondary education increased through the reform. The probability that a child with low educated parents achieved upper secondary education was 25 percentage points lower before the reform and only $25 - 7 = 18$ percentage points lower after the reform (Model 2) compared to a child with medium educated parents. This increase in educational mobility was not statistically significantly different between women (Model 4) and men (Model 6).

For Denmark (Panel B), there was neither an effect of the tracking age reform on educational attainment at the population level (Model 1). Similar to the findings with respect to the reform in Belgium, the reform in Denmark did improve educational mobility. Before the reform, children with low educated parents had a 20 percentage points lower probability to achieve upper secondary education than children with medium educated parents. Due to the reform, this difference changed to $20 - 8 = 12$ percentage points. As for Belgium, this increase in educational mobility was not statistically significantly different between women (Model 4) and men (Model 6).

Table 3
OLS regression models predicting years of education.

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
<i>Panel A: Belgium</i>						
Low parental education	−1.58* [−1.81,−1.34]	−1.90* [−2.23,−1.57]	−1.53* [−1.85,−1.21]	−1.86* [−2.29,−1.42]	−1.61* [−1.98,−1.25]	−1.96* [−2.45,−1.47]
High parental education	1.03* [0.74,1.32]	1.09* [0.69,1.49]	0.93* [0.54,1.31]	1.02* [0.49,1.56]	1.17* [0.74,1.61]	1.17* [0.57,1.76]
Reform	−0.48* [−0.89,−0.08]	−0.85* [−1.38,−0.31]	−0.67* [−1.21,−0.12]	−1.01* [−1.73,−0.29]	−0.26 [−0.86,0.35]	−0.68 [−1.47,0.12]
Male	0.25* [0.06,0.44]	0.25* [0.06,0.44]				
Reform X Low parental education		0.72* [0.24,1.19]		0.72* [0.09,1.35]		0.77* [0.05,1.49]
Reform X High parental education		−0.13 [−0.70,0.44]		−0.22 [−0.98,0.54]		0.01 [−0.86,0.87]
<i>N</i>	4010	4010	2148	2148	1862	1862
<i>Panel B: Denmark</i>						
Low parental education	−1.11* [−1.28,−0.94]	−1.37* [−1.62,−1.13]	−1.01* [−1.26,−0.76]	−1.30* [−1.66,−0.94]	−1.23* [−1.46,−0.99]	−1.46* [−1.79,−1.13]
High parental education	1.35* [1.12,1.57]	1.38* [1.03,1.72]	1.31* [0.97,1.64]	1.30* [0.80,1.80]	1.38* [1.06,1.69]	1.46* [0.99,1.93]
Reform	0.05 [−0.29,0.39]	−0.14 [−0.53,0.24]	0.18 [−0.31,0.68]	−0.04 [−0.61,0.52]	−0.08 [−0.55,0.39]	−0.24 [−0.77,0.29]
Male	0.13 [−0.02,0.29]	0.14 [−0.02,0.30]				
Reform X Low parental education		0.52* [0.18,0.86]		0.55* [0.05,1.05]		0.47* [0.01,0.94]
Reform X High parental education		−0.04 [−0.50,0.42]		0.03 [−0.64,0.70]		−0.14 [−0.77,0.49]
<i>N</i>	3494	3494	1720	1720	1774	1774
<i>Panel C: Finland</i>						
Low parental education	−0.65* [−0.85,−0.44]	−0.92* [−1.21,−0.64]	−0.48* [−0.74,−0.23]	−0.61* [−0.95,−0.26]	−0.84* [−1.17,−0.51]	−1.31* [−1.78,−0.85]
High parental education	0.96* [0.71,1.20]	0.99* [0.64,1.35]	0.84* [0.53,1.14]	0.87* [0.44,1.31]	1.03* [0.65,1.42]	1.05* [0.47,1.63]
Reform	0.10 [−0.27,0.47]	−0.22 [−0.70,0.26]	0.08 [−0.39,0.55]	−0.05 [−0.65,0.54]	0.15 [−0.42,0.73]	−0.40 [−1.15,0.35]
Male	−0.43* [−0.59,−0.26]	−0.42* [−0.59,−0.25]				
Reform X Low parental education		0.61* [0.19,1.03]		0.28 [−0.23,0.79]		1.00* [0.34,1.67]
Reform X High parental education		−0.04 [−0.53,0.45]		−0.06 [−0.66,0.55]		0.01 [−0.76,0.79]
<i>N</i>	2522	2522	1258	1258	1264	1264
<i>Panel D: France</i>						
Low parental education	−1.35* [−1.57,−1.14]	−1.57* [−1.86,−1.28]	−1.41* [−1.71,−1.12]	−1.68* [−2.07,−1.29]	−1.27* [−1.59,−0.96]	−1.43* [−1.86,−1.00]
High parental education	1.39* [1.09,1.69]	1.31* [0.88,1.73]	1.43* [1.01,1.86]	1.22* [0.62,1.81]	1.35* [0.92,1.78]	1.43* [0.82,2.04]
Reform	−0.11 [−0.55,0.34]	−0.40 [−0.92,0.12]	−0.36 [−0.97,0.25]	−0.76* [−1.47,−0.04]	0.15 [−0.50,0.79]	−0.02 [−0.78,0.73]
Male	−0.24* [−0.43,−0.05]	−0.23* [−0.42,−0.04]				
Reform X Low parental education		0.52* [0.08,0.95]		0.64* [0.04,1.23]		0.37 [−0.26,1.01]
Reform X High parental education		0.16 [−0.45,0.76]		0.46 [−0.39,1.31]		−0.19 [−1.05,0.67]
<i>N</i>	2936	2936	1549	1549	1387	1387
<i>Panel E: Italy</i>						
Low parental education	−4.10* [−4.57,−3.64]	−4.60* [−5.28,−3.93]	−4.56* [−5.19,−3.92]	−5.66* [−6.61,−4.72]	−3.60* [−4.29,−2.91]	−3.56* [−4.53,−2.58]
High parental education	1.26* [0.96,1.56]	1.81* [1.31,2.31]	1.80* [1.21,2.39]	1.64* [1.04,2.24]	0.73 [−0.14,1.60]	1.71* [1.01,2.41]

(continued on next page)

Table 3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
Reform	[0.49,2.03] 0.39 [-0.10,0.88]	[0.70,2.92] -0.46 [-1.47,0.55]	[0.74,2.87] 0.58 [-0.09,1.24]	[0.16,3.12] -1.28 [-2.67,0.10]	[-0.40,1.87] 0.15 [-0.58,0.88]	[0.03,3.40] 0.27 [-1.21,1.75]
Male	0.70* [0.48,0.92]	0.70* [0.48,0.92]				
Reform X Low parental education		0.95* [0.02,1.88]		2.01* [0.74,3.27]		-0.08 [-1.45,1.28]
Reform X High parental education		-1.15 [-2.69,0.40]		-0.08 [-2.21,2.05]		-1.76 [-4.03,0.51]
N	3673	3673	1973	1973	1700	1700

* indicates the 5% significance level.

The tracking age reform in Finland (Panel C) did not affect the probability to complete upper secondary education on average (Model 1). However, the reform improved educational mobility with respect to upper secondary education (Model 2). The point estimate is higher for women (Model 4) than for men (Model 6), but these differences are not statistically significant.

In France (Panel D), the increase in the age at tracking did not affect the probability to attain upper secondary education (Model 1). Educational mobility at the level of upper secondary education did considerably increase (Model 2). The gender differences were not statistically significant.

The tracking age reform in Italy (Panel E) did neither affect the probability of upper secondary education completion. At the same time, we again observe a striking gender difference in Italy. Educational mobility, measured as the difference in the probability of attaining upper secondary education between children with low and children with medium educated parents, did increase only for women (Model 4) and not for men (Model 6).

5.3. Post-secondary education completion

Our last set of results is reported in Table 5. This table reports the results of LPMs predicting post-secondary education completion. The findings are different from those of the two previous outcomes. In Belgium (Panel A), the reform increased educational mobility at the level of post-secondary education (Model 2). This effect is concentrated among women (Model 4).

Contrary to Belgium, no change in educational mobility was observed at the level of post-secondary education in Denmark (Panel B). This finding holds for the combined sample (Model 2) as well as for women (Model 4) and men (Model 6) separately.

In Finland (Panel C), no change in educational mobility was observed at the post-secondary level (Model 2). This finding also holds for the subsamples of women (Model 4) and men (Model 6). For men, there is a statistically significant decrease in educational mobility comparing children of medium and highly educated parents. However, this finding is probably a statistical artifact due to multiple hypothesis testing. It is not robust as it is not confirmed for the other outcomes and countries.

The tracking age reform in France (Panel D) did neither affect educational mobility at the level of post-secondary education. This finding holds for both the differences in educational attainment between children with low and medium educated parents and the differences in educational attainment between children with medium and highly educated parents.

The reform in Italy (Panel E) had also no effect on educational mobility at the level of post-secondary education. In other words, the differences in educational attainment between children with low, medium, and highly educated parents were unchanged by the tracking age reform in Italy.

6. Discussion and conclusion

Numerous studies have investigated the effects of age at tracking on educational mobility comparing differences across countries and within countries over time using difference-in-differences (DiD) research designs (Braga et al., 2013; Scheeren, 2022; van de Werfhorst, 2018, 2019). This research implicitly assumed that the effects of reforms in the age at tracking were the same independent from the context in which they were implemented. We have argued that there are good theoretical reasons to believe that the effects of the reforms in tracking age on educational mobility may vary across contexts. First, it is possible that reforms, which increase the age at tracking by more years have stronger consequences than reforms which increase the age at tracking by fewer years. Second, the general climate in terms of fostering educational equality may moderate the effects of reforms in the age at tracking (van de Werfhorst, 2019). In addition, we hypothesized that educational mobility may have increased due to the reforms more for women than for men and that this might have been particularly pronounced in more gender egalitarian societies.

Our results are generally more in line with the implicit expectation of prior studies, which assumed no cross-country variation in the effects of reforms in the age at tracking on educational mobility than with our hypotheses. Neither the size of the increase in the age at tracking (Hypothesis 1), nor the extent to which there are egalitarian values are prevailing in a society (Hypothesis 2) impact the effect of an age increase in tracking on educational mobility. In fact, it is astonishing how robust the positive effects of reforms in the age at tracking are for fostering educational mobility. In all five countries, the reforms improved the educational attainment of children with

Table 4

Linear probability models predicting upper secondary education.

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
<i>Panel A: Belgium</i>						
Low parental education	−0.22* [−0.25,−0.19]	−0.25* [−0.29,−0.21]	−0.24* [−0.28,−0.20]	−0.28* [−0.33,−0.22]	−0.20* [−0.24,−0.15]	−0.22* [−0.29,−0.16]
High parental education	0.06* [0.03,0.09]	0.07* [0.03,0.11]	0.05* [0.01,0.09]	0.07* [0.01,0.13]	0.07* [0.02,0.11]	0.07* [0.01,0.14]
Reform	−0.06* [−0.11,0.00]	−0.09* [−0.16,−0.02]	−0.09* [−0.17,−0.01]	−0.13* [−0.21,−0.04]	−0.02 [−0.10,0.06]	−0.05 [−0.15,0.05]
Male	0.00 [−0.02,0.03]	0.00 [−0.02,0.03]				
Reform X Low parental education		0.07* [0.01,0.13]		0.09* [0.01,0.17]		0.06 [−0.03,0.15]
Reform X High parental education		−0.03 [−0.09,0.03]		−0.05 [−0.13,0.03]		−0.01 [−0.10,0.08]
<i>N</i>	4010	4010	2148	2148	1862	1862
<i>Panel B: Denmark</i>						
Low parental education	−0.16* [−0.19,−0.13]	−0.20* [−0.24,−0.16]	−0.15* [−0.19,−0.10]	−0.20* [−0.26,−0.13]	−0.18* [−0.21,−0.14]	−0.21* [−0.26,−0.16]
High parental education	0.08* [0.06,0.11]	0.08* [0.04,0.12]	0.12* [0.08,0.16]	0.11* [0.05,0.18]	0.05* [0.01,0.08]	0.04 [−0.00,0.09]
Reform	0.01 [−0.04,0.07]	−0.02 [−0.08,0.04]	0.01 [−0.07,0.09]	−0.03 [−0.12,0.06]	0.02 [−0.05,0.09]	−0.01 [−0.08,0.06]
Male	0.07* [0.05,0.10]	0.07* [0.05,0.10]				
Reform X Low parental education		0.08* [0.02,0.14]		0.09* [0.00,0.18]		0.07 [−0.01,0.14]
Reform X High parental education		0.01 [−0.04,0.06]		0.01 [−0.08,0.09]		0.01 [−0.05,0.07]
<i>N</i>	3494	3494	1720	1720	1774	1774
<i>Panel C: Finland</i>						
Low parental education	−0.09* [−0.12,−0.07]	−0.12* [−0.16,−0.08]	−0.07* [−0.10,−0.03]	−0.08* [−0.13,−0.03]	−0.13* [−0.17,−0.08]	−0.17* [−0.24,−0.10]
High parental education	0.05* [0.02,0.07]	0.07* [0.03,0.10]	0.03 [−0.00,0.05]	0.04 [−0.01,0.09]	0.06* [0.02,0.10]	0.08* [0.02,0.14]
Reform	0.02 [−0.03,0.07]	−0.00 [−0.06,0.06]	−0.03 [−0.08,0.02]	−0.04 [−0.10,0.02]	0.08 [−0.01,0.16]	0.03 [−0.07,0.13]
Male	−0.07* [−0.09,−0.04]	−0.07* [−0.09,−0.04]				
Reform X Low parental education		0.06* [0.00,0.12]		0.03 [−0.03,0.10]		0.10* [0.01,0.19]
Reform X High parental education		−0.03 [−0.08,0.01]		−0.02 [−0.08,0.03]		−0.04 [−0.12,0.04]
<i>N</i>	2522	2522	1258	1258	1264	1264
<i>Panel D: France</i>						
Low parental education	−0.10* [−0.12,−0.07]	−0.14* [−0.17,−0.11]	−0.11* [−0.14,−0.07]	−0.15* [−0.19,−0.11]	−0.09* [−0.12,−0.06]	−0.13* [−0.17,−0.08]
High parental education	0.04* [0.02,0.07]	0.03 [−0.00,0.06]	0.05* [0.02,0.08]	0.04* [0.00,0.08]	0.04* [0.00,0.07]	0.02 [−0.03,0.07]
Reform	−0.03 [−0.08,0.02]	−0.09* [−0.14,−0.03]	−0.06 [−0.13,0.01]	−0.12* [−0.20,−0.04]	−0.01 [−0.08,0.06]	−0.05 [−0.13,0.02]
Male	0.01 [−0.01,0.04]	0.01 [−0.01,0.04]				
Reform X Low parental education		0.09* [0.04,0.14]		0.10* [0.03,0.17]		0.08* [0.01,0.15]
Reform X High parental education		0.03 [−0.01,0.07]		0.02 [−0.04,0.08]		0.03 [−0.03,0.09]
<i>N</i>	2936	2936	1549	1549	1387	1387
<i>Panel E: Italy</i>						
Low parental education	−0.49* [−0.54,−0.43]	−0.52* [−0.61,−0.44]	−0.52* [−0.60,−0.45]	−0.61* [−0.72,−0.50]	−0.45* [−0.53,−0.37]	−0.43* [−0.56,−0.31]
High parental education	0.11* [0.07,0.15]	0.17* [0.12,0.22]	0.16* [0.11,0.21]	0.18* [0.13,0.23]	0.07 [−0.02,0.16]	0.13 [0.04,0.22]

(continued on next page)

Table 4 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
Reform	[0.04,0.19] 0.04	[0.06,0.28] −0.02	[0.06,0.25] 0.07	[0.06,0.30] −0.08	[−0.04,0.19] −0.00	[−0.04,0.31] 0.03
Male	[−0.03,0.10] 0.06*	[−0.14,0.10] 0.06*	[−0.02,0.16]	[−0.24,0.08]	[−0.10,0.10]	[−0.15,0.20]
Reform X Low parental education	[0.03,0.09]	[0.03,0.09] 0.07		0.16*		−0.03
Reform X High parental education		[−0.04,0.18] −0.11		[0.01,0.31] −0.08		[−0.18,0.13] −0.11
		[−0.26,0.04]		[−0.27,0.11]		[−0.34,0.12]
N	3673	3673	1973	1973	1700	1700

* indicates the 5% significance level.

low educated parents more than of children with medium and highly educated parents. These improvements were of considerable size and have consequences for monetary (Psacharopoulos and Patrinos, 2018) and non-monetary outcomes (e.g., Schneeweiss et al., 2014). Hence, the reforms contributed to reducing educational inequalities.

There are, however, heterogeneities in the results across the three outcome variables that have been analyzed to assess the reforms' impact on educational mobility; namely, years of education, upper-secondary education completion, and post-secondary education completion. With respect to years of education there is an overall reduction in educational inequalities. The results for the other two dependent variables, upper-secondary education completion, and even more so higher education completion reveal fewer overall impacts of the reforms. This shows that the effects of the reforms are found rather at the lower than at the higher end of educational attainment. The lower ends of educational attainment are the levels directly affected by the reforms. Across countries, the reforms in age at tracking affected the completion of upper secondary but not the completion of post-secondary education. The latter was only the case in Belgium. This finding implies that there are no positive spillover effects of changing the age at tracking for inequality in educational attainment at the level of tertiary education.

In addition, our results with respect to gender differences in the effects of the reforms on educational mobility are not in line with our theoretical expectations either. Based on the arguments and the empirical evidence put forward by previous research (Scheeren, 2022), we expected reforms in the age at tracking to affect educational mobility more among women than among men and that this would be specifically pronounced in more gender egalitarian countries. However, this hypothesis is not confirmed. Heterogeneous effects of the reforms on educational mobility for women and men varied across contexts and outcome variables. Regarding years of education, the reforms increased educational mobility more among women than among men in France and Italy. In Finland, the reform increased educational mobility more among men than among women and in Belgium and Denmark no gender differences in the effects of the reforms on educational mobility were observed. Overall, these findings are not in line with our expectation that the reforms in the age at tracking increased educational mobility more among women than among men in countries that were, at the time of the reform, more gender-egalitarian. They are rather more in line with that the most disadvantaged population profited most from the reforms, these were the female offspring from lower educated families in Italy and France as well as the male offspring from parents with lower levels of education in Finland. Whilst we consider the variation of the effects of reforms in the age at tracking on educational mobility by gender to be an interesting hypothesis, the empirical evidence does not support it.

Our study focused on educational attainment and thereby took a different perspective than previous studies which focused on the overall dispersion of the outcome (e.g., Montt, 2011) or average achievement test scores (e.g., Hanushek and Wößmann, 2006). Unfortunately, we do not have information on achievement test scores in our data. There could also be other positive implications of tracking, for instance a more specialized labor force, smoother school-to-work transitions, or higher lifetime earnings, that we cannot test for with our data. At the same time, there could also be negative consequences of tracking with respect to outcomes that we do not observe in our data, for instance, average achievement test scores or the educational ambitions among low-track students. In addition, we observe a negative effect of the reform in Belgium on average educational attainment. This is a puzzling finding, also because it does not occur in the other countries. Yet, given the confidence intervals, negative effects can also not be completely ruled out in the other countries. However, given the available data, the negative effect of a reform in the age at tracking on average educational attainment does not seem to be a robust, general consequence of reforms in the age at tracking. Because of these limitations of our study, it would be relevant for future studies to assess alternative outcomes.

Unaffected by these limitations of our analysis, the main finding of this study remains clear and is very much in line with the results from previous research: Reforms in the age at tracking increase educational mobility. This finding is very robust across countries. It implies that the effects of these reforms may not be moderated by the context in which they occur.

CRedit authorship contribution statement

Michael Grätz: Writing – original draft, Formal analysis, Conceptualization, Writing – Review & Editing. **Marieke Heers:** Writing – original draft, Conceptualization, Writing – Review & Editing.

Table 5
Linear probability models predicting post-secondary education.

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
<i>Panel A: Belgium</i>						
Low parental education	−0.24* [−0.28,−0.20]	−0.28* [−0.33,−0.23]	−0.22* [−0.27,−0.17]	−0.29* [−0.36,−0.22]	−0.26* [−0.32,−0.21]	−0.27* [−0.35,−0.19]
High parental education	0.19* [0.15,0.24]	0.18* [0.12,0.24]	0.20* [0.14,0.26]	0.17* [0.08,0.26]	0.18* [0.11,0.25]	0.20* [0.10,0.29]
Reform	−0.07* [−0.13,−0.00]	−0.12* [−0.20,−0.03]	−0.06 [−0.15,0.02]	−0.16* [−0.28,−0.05]	−0.06 [−0.15,0.02]	−0.07 [−0.19,0.06]
Male	−0.01 [−0.04,0.02]	−0.01 [−0.04,0.02]				
Reform X Low parental education		0.09* [0.01,0.16]		0.16* [0.05,0.26]		0.02 [−0.10,0.13]
Reform X High parental education		0.02 [−0.07,0.11]		0.06 [−0.06,0.18]		−0.03 [−0.16,0.10]
<i>N</i>	4010	4010	2148	2148	1862	1862
<i>Panel B: Denmark</i>						
Low parental education	−0.16* [−0.19,−0.12]	−0.17* [−0.22,−0.12]	−0.12* [−0.17,−0.07]	−0.14* [−0.21,−0.07]	−0.20* [−0.24,−0.15]	−0.20* [−0.27,−0.13]
High parental education	0.30* [0.26,0.34]	0.30* [0.24,0.37]	0.31* [0.25,0.37]	0.31* [0.22,0.41]	0.29* [0.23,0.36]	0.30* [0.20,0.39]
Reform	0.00 [−0.06,0.07]	−0.00 [−0.08,0.08]	0.04 [−0.06,0.14]	0.03 [−0.09,0.14]	−0.03 [−0.13,0.06]	−0.03 [−0.14,0.08]
Male	−0.05* [−0.08,−0.02]	−0.05* [−0.08,−0.02]				
Reform X Low parental education		0.02 [−0.05,0.09]		0.03 [−0.07,0.13]		0.00 [−0.09,0.10]
Reform X High parental education		−0.01 [−0.09,0.08]		−0.01 [−0.13,0.12]		−0.01 [−0.13,0.12]
<i>N</i>	3494	3494	1720	1720	1774	1774
<i>Panel C: Finland</i>						
Low parental education	−0.11* [−0.15,−0.06]	−0.15* [−0.22,−0.09]	−0.10* [−0.17,−0.04]	−0.15* [−0.24,−0.06]	−0.11* [−0.18,−0.04]	−0.16* [−0.26,−0.06]
High parental education	0.27* [0.22,0.32]	0.23* [0.15,0.30]	0.22* [0.15,0.28]	0.21* [0.12,0.31]	0.32* [0.24,0.39]	0.23* [0.12,0.35]
Reform	0.03 [−0.05,0.11]	−0.04 [−0.14,0.07]	0.04 [−0.07,0.16]	−0.01 [−0.16,0.13]	0.02 [−0.10,0.13]	−0.07 [−0.22,0.08]
Male	−0.15* [−0.19,−0.12]	−0.15* [−0.19,−0.12]				
Reform X Low parental education		0.10* [0.00,0.19]		0.11 [−0.02,0.24]		0.10 [−0.04,0.24]
Reform X High parental education		0.08 [−0.02,0.18]		0.01 [−0.12,0.14]		0.15* [0.00,0.30]
<i>N</i>	2522	2522	1258	1258	1264	1264
<i>Panel D: France</i>						
Low parental education	−0.22* [−0.25,−0.18]	−0.21* [−0.26,−0.16]	−0.25* [−0.30,−0.19]	−0.25* [−0.32,−0.18]	−0.18* [−0.24,−0.13]	−0.16* [−0.24,−0.09]
High parental education	0.30* [0.25,0.35]	0.29* [0.22,0.37]	0.30* [0.23,0.36]	0.27* [0.17,0.37]	0.31* [0.24,0.39]	0.32* [0.21,0.43]
Reform	−0.01 [−0.08,0.07]	0.00 [−0.09,0.09]	−0.06 [−0.16,0.04]	−0.06 [−0.19,0.06]	0.05 [−0.06,0.15]	0.07 [−0.06,0.20]
Male	−0.08* [−0.11,−0.05]	−0.08* [−0.11,−0.05]				
Reform X Low parental education		−0.02 [−0.10,0.06]		−0.00 [−0.11,0.11]		−0.04 [−0.15,0.07]
Reform X High parental education		0.02 [−0.08,0.12]		0.05 [−0.08,0.19]		−0.01 [−0.16,0.13]
<i>N</i>	2936	2936	1549	1549	1387	1387
<i>Panel E: Italy</i>						
Low parental education	−0.35* [−0.41,−0.28]	−0.33* [−0.43,−0.24]	−0.35* [−0.44,−0.26]	−0.37* [−0.51,−0.24]	−0.34* [−0.44,−0.25]	−0.30* [−0.43,−0.16]
High parental education	0.14* [0.08,0.20]	0.23* [0.15,0.31]	0.20* [0.12,0.28]	0.21 [0.12,0.31]	0.08 [−0.02,0.18]	0.25* [0.15,0.35]

(continued on next page)

Table 5 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Women	Women	Men	Men
Reform	[0.03,0.25] −0.00 [−0.04,0.04]	[0.08,0.39] 0.03 [−0.11,0.17]	[0.04,0.35] −0.01 [−0.07,0.05]	[−0.00,0.42] −0.04 [−0.23,0.15]	[−0.08,0.24] 0.01 [−0.06,0.08]	[0.02,0.48] 0.10 [−0.10,0.30]
Male	0.02 [−0.00,0.04]	0.02 [−0.00,0.04]				
Reform X Low parental education		−0.03 [−0.16,0.10]		0.04 [−0.15,0.22]		−0.09 [−0.28,0.10]
Reform X High parental education		−0.19 [−0.41,0.04]		−0.04 [−0.35,0.27]		−0.31 [−0.62,0.1]
N	3673	3673	1973	1973	1700	1700

* indicates the 5% significance level.

Data availability

The data used in the publication is publicly available. Please consult the websites of ESS (<https://www.europeansocialsurvey.org/>) and SHARE (<https://www.share-eric.eu>).

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.ssresearch.2025.103188>.

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