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ORIGINAL RESEARCH

Can We Trust Education for Fostering Trust? Quasi-experimental Evidence on the Effect of Education and Tracking on Social Trust

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Abstract

Education is one of the most commonly proposed determinants of social trust (generalized trust). Nevertheless, the empirical evidence of a causal relationship between education and social trust is inconclusive. This study contributes to this discussion in two ways. First, its design provides strong grounds for causal inference across multiple countries by exploiting numerous European compulsory schooling reforms. Second, it considers how the structure of education, specifically between-school tracking, impacts the relationship between education and social trust. The article argues that less tracking is positive for social trust because it entails intergroup contacts between children with different social backgrounds. The results do not give support for a general positive effect of education on social trust as the effect of reforms that extend compulsory education is positive but small and not statistically significant. However, reforms that reduce tracking have a somewhat larger, but still modest, positive and statistically significant effect on social trust. The effect is more pronounced for individuals with poorly educated parents. The positive effect of detracking reforms goes hand-in-hand with more understanding attitudes towards persons with a different background than one's own. The lack of a clear effect of reforms that extend compulsory schooling on social trust reinforces the findings of recent single-country studies that have been unable to confirm a causal effect of education on social trust. However, the effect of detracking reforms, albeit modest, shows that education can have a positive effect on social trust but that the institutional character of education may be a conditioning factor.

Keywords Education · Social trust · Tracking · Education reform · Causal inference

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1 Introduction

Social trust relates to a plethora of positive outcomes ranging from economic growth to political participation (Bjørnskov 2012; Uslaner and Brown 2005). Because social trust is such a valuable quality, the research community has exerted substantial effort to explaining differences in social trust. While many different explanations have been proposed, hardly any single factor has been stressed more than education (e.g., Helliwell and Putnam 2007; Easterbrook et al. 2016; Rothstein and Uslaner 2005).

Hundreds of studies have asserted that more highly educated people express higher levels of social trust (Huang et al. 2009). However, almost all of these studies rely on cross-sectional evidence and assume that the established correlations describe a causal relationship. This interpretation has come into question as more recent studies using research designs that allow stronger causal claims have been unable to unanimously reproduce a clear relationship between education and social trust (Sønderskov and Dinesen 2014; Glanville et al. 2013; Oskarsson 2017; Yang 2019; but see, Huang et al. 2011). However, these studies are scarce, and the results are mixed, meaning that the question of the causal status of the relationship remains undecided.

Despite the existence of a substantial body of literature on education and social trust, earlier studies have nearly entirely focused on educational attainment in terms of years of education or educational degrees. How the design of educational institutions may condition the effect of education on social trust remains largely unexplored.

This study makes two essential contributions to the discussion of whether education has a causal effect on social trust. First, it uses a research design based on twenty-seven educational reforms in sixteen European countries that allows for a causal test of the effect of education on social trust that is not limited to a specific country or time. Second, the article moves beyond studying education only in terms of its length by considering how between-school tracking affects social trust.

Between-school tracking is one of the institutions that most strongly distinguishes educational systems (e.g., Pfeffer 2008; Busemeyer and Trampusch 2011). It refers to students being separated into different tracks according to ability or future plans for further education (Shavit and Müller 2000). These tracks are institutionally differentiated in terms of curricula, access to further education and diplomas. Within-school tracking (ability-grouping) is a less rigorous form of tracking as grouping then is done on a course-by-course basis and all students attend the same school with a shared institutional framework (Chmielewski 2014). Track selection in the case of between-school tracking implies following distinct educational programmes, typically located in physically different schools, that result in different qualifications after graduation. Mobility between tracks tends to be limited, if possible at all (Shavit and Müller 2000). This article focuses on between-school tracking as it is a more far-reaching practice in terms of separation and rigidity than within-school tracking. Between-school tracking (henceforth, *tracking*) also implies more severe socio-economic stratification between tracks (Chmielewski 2014). Because school performance and educational aspirations are strongly socially stratified (Breen and Jonsson 2005), separating students based on these factors results in more socially homogeneous schools in tracked systems (Pfeffer 2008). Conversely, reducing the level of tracking opens up for more contacts between children of different social backgrounds. I argue that such intergroup contact facilitates the formation of bridging social capital (Putnam 2000), which in turn promotes trust in the generalized other.

Whereas the results give no clear support for a general effect of education on social trust, reducing tracking has a modest positive effect, particularly for individuals who have poorly educated parents. This positive effect is robust to several alternative empirical specifications and is coupled with other effects that support the intergroup contact mechanism.

2 Previous Literature and Theory

The body of literature supporting a relationship between educational attainment and social trust is immense and includes a meta-analysis of 154 studies (Huang et al. 2009). Multiple mechanisms have been proposed to explain the relationship between education and social trust. Education may enhance an individual's ability to handle risk, interpret information, and understand other people's actions (e.g., Huang et al. 2011). This ability could accompany a capacity to assess others' trustworthiness, which may foster higher levels of trust as it becomes less hazardous to trust people in general. Furthermore, scholars have argued that we may learn more about people who are different from ourselves through education and thereby become less anxious about unknown people and more willing to trust them (Uslaner 2002). Studies have also suggested that education 'may cultivate a more benign view of the world' (Smith 1997) and make people embrace a normative stance that values social trust (Uslaner 2002). Another suggested mechanism is that education fosters a higher level of confidence in societal institutions that are important for enforcing trustworthiness and fairness, such as the legal system and the welfare state, and confidence in these institutions is in turn positive for social trust (Huang et al. 2011; Rothstein and Uslaner 2005).

Another line of reasoning has focused on mechanisms that emphasize the indirect effects of education on social trust. The social environment of education, particularly higher education, has been argued to nurture social trust, as students often must meet and socialize with unknown fellow students, including students from other social groups (Uslaner 2002; Huang et al. 2011). Education could also lead to higher levels of trust because the well-educated have greater economic success, which make them less exposed to crime and social issues that may impair social trust (Huang et al. 2011, 2009).

While most scholars have asserted that education causally precedes social trust, some have also argued for the reverse relationship. These researchers have stressed that high levels of trust improve one's chances of being successful in education because trust makes it easier to cooperate with other students (e.g., Putnam 2000). There are also some studies arguing that the effect of education on social trust is conditional upon other factors, including country-level variables such as quality of government (Charron and Rothstein 2016) or individual-level variables such as life satisfaction (Zanin 2017).

2.1 Correlation or a Causal Effect?

The causal status of the effects of education on civic outcomes has recently come into question, as education may act as a proxy for other factors (e.g., Kam and Palmer 2008; Persson 2015). Many scholars have also acknowledged that social trust correlates with several background variables, that it is difficult to isolate the effect of education and that the causal order is far from straightforward (e.g., Putnam 2000; Rothstein and Uslaner 2005; Nannestad 2008). Nevertheless, only a handful of studies have tried to address this problem of endogeneity in the study of education and social trust.

One method has been to use panel data with individual fixed effects. The results of these studies range from strong effects in Britain (Sturgis et al. 2009) to more marginal effects in the US (Glanville et al. 2013) and Denmark (Sønderskov and Dinesen 2014). Another method is to exploit twin designs, which effectively separates the effect of education from the influence of family background variables. A recent such study tested the relationship between education and social trust with Swedish data (Oskarsson 2017). The study finds no effect of education on social trust.

An alternative way of addressing the endogeneity problem of education is to find a source of exogenous variation in education. Huang et al. (2011) use incidence of illness during secondary education as an instrumental variable and find substantial positive effects of higher education on social trust in the UK. Exploiting compulsory schooling reforms is another approach to find exogenous variation in education that have been used extensively in studies of economic outcomes (e.g., Oreopoulos 2006), but have increasingly also been used to determine effects of schooling on attitudes (d'Hombres and Nunziata 2016; Cavaille and Marshall 2019). Milligan et al. (2004) exploit changes in compulsory schooling laws in Britain and the US to explore the effect of schooling on civic engagement and attitudes. They find an imprecise but substantial positive effect of education on social trust in the US. In a recent contribution, Yang (2019) uses the 1972 British compulsory schooling reform to analyse how education affects different forms of trust. The study finds positive but statistically insignificant effects of education on social trust.

The above studies represent valuable contributions addressing the causal status of the relationship between education and social trust. Nonetheless, considering the mixed results, they are unable to offer a conclusive answer to the causal question. Since they are all one-country studies, together covering a limited number of countries, the results could be dependent upon country-level factors such as the design of the educational system. Furthermore, the studies relying on panel data are unable to account for unobserved individual factors that change over time. For instance, some norms could be related to both educational choices and the development of trust, but the effect on trust may not be revealed until later in life. If such unobserved norms are common it would lead to an overestimation of the effect of education on social trust. Twin studies have clear advantages for isolating a causal relationship from genetic factors and early childhood circumstances. However, the within-twin pair variation in education is endogenous and of a specific character that does not necessarily allow for generalization. Instrumental variable approaches are highly sensitive to the construction of the instrument and whether the assumptions hold.

This article builds on a similar approach as Milligan et al. (2004) and Yang (2019) but includes a much larger number of reforms and countries to improve on generalizability. In addition, by separating different types of reforms the present study also makes it possible to explore how the effects of education differ with the degree of tracking.

2.2 Tracking, Intergroup Contact, and Bridging Social Capital

Nearly all of the studies on the relationship between education and social trust examine education in terms of either years of educational attainment or educational degrees (Huang et al. 2009). The circumstances under which we spend our time in education have received little attention. Stating that educational attainment would have the same effect regardless of the context of education is undoubtedly a drastic simplification.

One of the most central institutions in the comparative study of educational systems is the practice of tracking (Pfeffer 2008; Busemeyer and Trampusch 2011). The composition of schools and classes is directly affected by tracking, which also determines the peers whom pupils meet in school (Shavit and Müller 2000). More tracking implies earlier separation of children and a larger number of separate tracks. As track selection tends to be heavily associated with socioeconomic factors, tracking reinforces educational stratification and results in a stronger separation of children along socioeconomic boundaries (Van de Werfhorst and Mijs 2010; Pfeffer 2008). While many countries reduced tracking during the decades after the Second World War, others retained strongly tracked educational systems (Österman 2017). Austria and Germany are typical examples of the latter pathway, where tracking is introduced by the age of ten. Other countries such as the Nordic countries introduced educational reforms that have postponed tracking until the age of sixteen.

A new strand of research has started to explore the impact of tracking on civic and political attitudes (e.g. Witschge and van de Werfhorst 2019; Van de Werfhorst 2017; Janmaat and Mons 2011), but there have been few studies of the effects on social trust. Witschge et al. (2019) approached the topic by studying educational transitions in the Netherlands and found that transitions to general tracks are associated with positive effects on social trust compared to transitions to vocational tracks.

I argue that the increased social diversity of schools and classes that follows from reducing tracking (henceforth *detracking*) should be understood in the light of *intergroup contact theory* (Allport 1954; Brown and Hewstone 2005). This literature presents robust support for that intergroup contact reduces outgroup prejudice (Pettigrew and Tropp 2006). Allport emphasized that contact entails better knowledge of other groups and thus increases understanding, whereas others have stressed that contact reduces anxiety and fosters the development of empathy for other groups (Pettigrew and Tropp 2011). Intergroup contact theory has primarily been applied to ethnic groups but has also been shown to hold for other 'outgroups' (Pettigrew and Tropp 2006). While social psychologists mainly refer to outgroup prejudice, there is a strong connection to social trust and in particular to Putnam 's (2000) distinction between *bridging* and *bonding social capital*. Putnam claimed that bridging social capital refers to inclusive networks 'that encompass people across diverse social cleavages', whereas bonding social capital 'reinforce exclusive identities and homogeneous groups' (Putnam 2000, p. 22). In line with the role of intergroup contacts, Putnam argued that positive effects on social trust are mainly expected from bridging social capital.

The result of detracking is that children will spend many hours each day, typically over several years, with a more diverse set of classmates. Arguably, this allows intense intergroup contacts across socioeconomic strata, effectively creating bridging networks among pupils. These contacts occur during a formative period for the development of social trust (Flanagan and Stout 2010). In addition, intergroup contacts in detracked schools approach the optimal conditions for positive effects according to Allport (1954): groups meet in a setting of equal status, they have common goals, they need to cooperate and there is institutional support (Pettigrew and Tropp 2011; Dinesen 2011).

The implications of detracking can be illustrated by the 1977 reform in France that instituted a comprehensive lower secondary school. During previous decades, measures had been adopted in France to make secondary education more accessible to a broader group of children. For example, new types of secondary schools had been created. However, while the 1960s saw a rapid increase in the number of children continuing to secondary education, students were divided between three types of institutions (*Collèges d'Enseignement Général*, *Collèges d'Enseignement Secondaire*, and *Petits Lycées*). The 1977 reform unified these different schools into one four-year common lower secondary school, *Collège unique*. Thus, instead of being separated into different schools at age ten after five years of primary school, children would spend an additional four years together in a school with peers of all backgrounds.

My argument on the effects of detracking might be questioned in light of the extensive literature on ethnic diversity and social trust (e.g., Dinesen and Sønderskov 2018; Putnam 2007). The findings from this literature vary, but a recent review concludes that the effects of ethnic diversity on social trust in general are negative (Dinesen et al. 2020). However, I would argue that these results are not generally comparable to the effects of detracking. First, as Dinesen (2011) also argues, the school context is different and offers opportunity for intergroup contacts in a setting that closely resembles Allport's optimal conditions. Second, detracking will mainly result in increased social diversity, rather than ethnic diversity, since the large detracking reforms were mostly implemented during the 1960s and 1970s, before the large immigration flows to Europe. Many of the mechanisms proposed to explain the negative effects of ethnic diversity (Dinesen et al. 2020) do not apply to social diversity, at least not to the same extent, for example linguistic and cultural differences, senses of group threat and dissimilarity in appearance. Third, most of the studies on ethnic diversity consider it as a geographical factor and do not consider whether groups actually meet and interact with each other. Indeed, when intergroup contacts have been considered, they have been shown to counteract the negative effect of geographical diversity (e.g., Stolle 2013; Schmid et al. 2014). In a non-tracked school, intergroup contact is inevitable, as children have to interact in the class room across socioeconomic strata. Put differently, in this school setting it is impossible to 'hunker down' and 'pull in like a turtle', as Putnam (2007) famously describes the effects of ethnic diversity.

Another objection to my argument could be that the extent to which detracking results in more contacts across social strata is dependent upon the level of school segregation. While segregation may affect the magnitude of the detracking effect, segregation and tracking are distinct phenomena. Even with a high level of school segregation, tracking adds a dimension of separation by also selecting children to institutionally different schools. The institutional aspects of tracking, such as differences in curricula and diplomas, renders the social hierarchy between different tracks very explicit, reinforcing educational stratification (Van de Werfhorst and Mijs 2010). In addition, school segregation would only bias the effect of detracking downwards and thus does not represent a risk of creating spurious effects.

2.3 Hypotheses

To conclude the theoretical discussion, I turn to the hypotheses. Considering the abundance of studies that have demonstrated a strong relationship between education and social trust, I hypothesize a positive effect of prolonged education on social trust. Reduced tracking is also expected to have a positive effect through increased intergroup contacts and the formation of bridging social networks.

Hypothesis 1 Education has a positive effect on social trust.

Hypothesis 2 Detracking education has a positive effect on social trust.

3 Empirical Approach

This article seeks to overcome the drawbacks of earlier studies by exploiting European educational reforms for which affected and unaffected birth cohorts can be identified (for similar approaches, see Borgonovi et al. 2010; Cavaille and Marshall 2019; d'Hombres and Nunziata 2016). This approach assumes that the reforms create an exogenous variation in education, thereby approximating a natural experiment. My data include twenty-seven reforms implemented in sixteen European countries over six decades, where for each reform I can compare earlier reform-unaffected cohorts with later reform-affected cohorts. This approach has several advantages. Exogenous variation makes it possible to isolate the effect of education from potential confounders and excludes issues of reversed causality. The considerable time-window and the number of reforms allow for conclusions of a more general nature that are not dependent upon country- or time-specific factors.

The studied countries are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, the Netherlands, Poland, Portugal, Spain, Sweden, and the UK.

3.1 Educational Reforms as Quasi-experiments

Exogenous variation in education makes educational reforms empirically attractive. However, these reforms usually include several simultaneous changes to the educational system, such as raising the minimum school leaving age, reducing tracking or changing the curriculum. This presents a dilemma for the empirical researcher attempting to determine how these changes affect outcomes.

Earlier studies using educational reforms as a source of exogenous variation have only considered these reforms in terms of their effect on years of education by using the reforms as an instrument for education (e.g., d'Hombres and Nunziata 2016; Brunello et al. 2009; Yang 2019). There has been some discussion of the validity of this approach (Holmlund et al. 2011), as the reforms are likely to influence outcomes in other ways than through educational attainment, thereby violating the *exclusion restriction* (Angrist and Pischke 2009). That is, if a reform implements several changes to the educational system—apart from extending compulsory education—and these other changes affect outcomes, the estimates of the effect of the length of education will be biased. Although Brunello et al. (2013) have found compulsory schooling reforms to be a valid instrument for educational attainment in the case of cognitive outcomes, this finding does not necessarily extend to social or political outcomes, including social trust. It could, for instance, be that the different composition of classes following reduced tracking could affect social and cognitive outcomes differently. However, rather than considering the institutional differences between reforms as an empirical dilemma, these differences may be exploited to study how educational institutions condition outcomes.

Therefore, this article adopts a new approach. I have isolated (1) reforms that only extended compulsory schooling, referred to as *extension reforms*; (2) detracking reforms that did not extend compulsory schooling, referred to as *pure detracking reforms*; and (3) reforms that implied both extended compulsory schooling and detracking. Categories (2) and (3) together are referred to as *detracking reforms*. With few exceptions, all larger institutional reforms targeting the length of compulsory education and tracking during the studied period can be sorted into these three broad categories. Table 1 presents an overview of

Table 1 The number of reforms and their characteristics (the number of countries in parentheses)

		Extended schooling		
		No	Yes	Total
Detracking	No	No reform	14 (11) Extension ref.	**14 (11)**
	Yes	4 (3) Pure detracking ref.	9 (9)	**13 (11)** Detracking ref.
	Total	**4 (3)**	**23 (16)**	**27 (16)**

the number and types of reforms. A more detailed account, including sources, is available in the Supplementary material (see Tables A.19–A.20).

I chose to collapse reforms of categories (2) and (3) to the common category of detracking reforms due to that there has been relatively few detracking reforms compared to extension reforms. The number of pure detracking reforms has been particularly few. Creating a joint category thus alleviates issues of precision related to a small number of observations. A joint category of detracking reforms also means that I can focus on the difference between reforms that did reduce tracking and reforms that did not alter tracking (the vertical dimension in Table 1). Pure detracking reforms are also analysed separately to pinpoint the effect of detracking, although the small sample calls for some caution in my interpretation of the results. Alternative specifications are tested in the Supplementary material.

The studied reforms were implemented between the 1940s and the 1990s, affecting birth cohorts from 1933 onwards. For the selection of reforms, I draw on earlier contributions by Borgonovi et al. (2010) and Brunello et al. (2009), but I also expand the number of reforms. For details about the reforms, I rely on numerous country-specific papers. The included reforms do not represent an exhaustive list of educational reforms. Among other constraints, I am dependent upon available sources and data limitations for the selection of reforms. Although this implies that the results may not extend to the universe of educational reforms, the selection of reforms is wider than previous contributions using a similar design (cf. Cavaille and Marshall 2019; d'Hombres and Nunziata 2016).

Various aspects have been considered in the selection of reforms. First, it must be possible to identify the affected birth cohorts. However, few reforms are implemented in such a way that a birth cohort born one year is completely unaffected, whereas the entire cohort born the next year is affected. Reforms implemented gradually have been included if it is possible to identify a clear shift in the degree to which cohorts were affected. Second, reforms may not coincide with other extensive societal changes. For instance, the reforms carried out in the former socialist republics in Eastern Europe immediately following their democratization are not included. Third, only reforms implemented such that they start affecting cohorts born between 1930 and 1986 have been included due to individual-level data limitations. Fourth, I focus on reforms affecting compulsory schooling. This implies that the reforms generally target primary or lower secondary education.

For some countries, the data include several reforms. Moreover, reforms in a few countries were implemented in a step-wise manner on a regional level, meaning that there is some within-country variation regarding which birth cohorts that were affected by the reforms. I may exploit regional variation in Finland, Germany, and the UK, assuming that individuals have gone to school in the same region in which they lived when responding to the survey.

3.2 Describing and Distinguishing the Different Types of Reforms

Classifying the different reforms is challenging, as there is some ambiguity about how to draw the line between detracking and the extension of compulsory schooling. This section will detail the method by which this classification has been performed and illustrate the different types of reforms.

The first dilemma involves the differentiation between a reform that only extends compulsory schooling and a reform that also reduces tracking. A clear case of the former would be an extension of compulsory schooling for the period of education that takes place after students are selected for different tracks. Most extension reforms are of this variety. One example is the 1975 reform in the Netherlands that extended compulsory schooling from nine to ten years. Before the reform, after six years of primary education students could enter several different programmes of three to six years. The reform did not change the timing of the selection but ensured that even the lower vocational programmes would be four years long. Another example is the reforms implemented in the West German states after the Second World War in which the minimum length of the basic track (*Hauptschule*) was increased from eight to nine years.

When an extension of compulsory schooling is implemented by prolonging the non-tracked part of education, the case is more ambiguous since the age of selection is raised. However, I have only coded this type of reform as detracking if the extension involves the secondary level as tracking typically is seen as related to secondary education (Shavit and Müller 2000; Witschge and van de Werfhorst 2019).

Pure detracking reforms usually imply reducing the degree of tracking at the lower secondary level without extending compulsory schooling. One example is the aforementioned 1977 reform in France. Another illustration is the 1971 Belgian reform. Before the reform, children were separated after six years of primary school into four types of secondary schools—academic, technical, artistic or vocational—for at least two additional years of schooling. The reform did away with the division between different secondary schools and instead implemented a single school entity with one academic and one vocational study stream. The eight-year duration of compulsory schooling remained unchanged. Although the reform did not entail a fully comprehensive system, tracking was clearly reduced and children spent more time with a more diverse set of classmates.

Reforms that combine extended compulsory schooling and detracking are typically represented by comprehensive school reforms. In these reforms, an early selection system is replaced with comprehensive schooling for both the primary and lower secondary levels. The reforms also prolong compulsory schooling by one to three years; obliging everyone to complete lower secondary education. The comprehensive school reforms in the Nordic countries are typical examples. In pre-reform Sweden, for instance, there was a two-track system with a joint four- or six-year common primary school (*folkskola*) and a three- to five-year junior secondary school (*realskola*) that children could enter after finishing the common school. The 1962 reform abolished the different tracks and introduced a nine-year compulsory comprehensive school. The 1963 reform in Italy also represents this category. The two different types of junior high schools (*ginnasio* and *scuola di avviamento professionale*) were unified into a single junior high school (*scuola media*). The reform made the new junior high school compulsory, increasing compulsory schooling from five to eight years.

I do not differentiate the reforms based on their perceived intensity. Arguably, some reforms were more extensive than others. Some increased compulsory schooling by one year, whereas others increased it by up to four years. However, the actual reform effect may not be discerned from the legal changes. Reforms that legally imply large changes might not have much impact if, prior to the reforms, most children continued beyond compulsory schooling anyway (cf. Oreopoulos 2006).

3.3 Individual Level Data: ESS1–9

For the individual level, I am using cumulative *European Social Survey* (ESS) data, consisting of the nine rounds from 2002 to 2018. To ensure that the individuals have gone to school in the national education system, only respondents born in the country of residence are included in the sample. The data are weighted using ESS design weights.

To study generalized social trust, I am following the established approach of using a validated three-item scale (Reeskens and Hooghe 2008; Zmerli and Newton 2008). This scale consists of the classic trust question, an item on whether people try to be fair, and an item on whether people are helpful:

- 'Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?'
- 'Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair?'
- 'Would you say that most of the time people try to be helpful or that they are mostly looking out for themselves?'

All of the items may be answered on a scale from 0 to 10 (where 10 represents the highest level of trust) and the scale is calculated as the mean of the three items. The three-item scale clearly improves measurement reliability and cross-country validity compared to using a single item, such as the classic trust question. Internal consistency for the three items is reasonably high (Cronbach's alpha: 0.77). The scale ranges between 0 and 10 with a mean of 5.24 for my sample. See the Supplementary material for additional information on the construction of the social trust scale (Section A.1), as well as for models using the classic single-item measure of trust (Section A.9). Descriptive statistics on the individual-level variables are presented in Table 2.

3.4 Empirical Model

Consistent with the idea of a natural experiment, I compare the seven 'untreated' birth year cohorts before a reform with the seven 'treated' birth year cohorts after a reform (similar to Brunello et al. 2009; Borgonovi et al. 2010). Each set of such cohorts is referred to as a 'reform-window'. The size of the reform-window is chosen to attempt to balance obtaining large enough samples with avoiding other time-dependent changes that could affect the estimates (narrower windows are tested in the robustness section). Individuals outside of the reform windows are not included in the models. I am also excluding the first potentially affected birth year cohort. This is done because in many cases, the first cohort is only partly affected by a reform either because of implementation deficiencies or because a reform comes into effect in the middle of a year. Since I lack data on birth dates, excluding the first potentially affected cohort allows for a clearer differentiation between affected and unaffected cohorts.

Table 2 Descriptive statistics

	Count	Mean	SD	Min	Max
Birth year	68796	1959.2	11.64	1926	1993
Age	68796	50.76	11.96	25	80
Years of full-time education	68211	12.66	4.195	0	25
Most people can be trusted	68733	5.091	2.391	0	10
Most of the time people try to be helpful	68665	4.904	2.279	0	10
Most people try to be fair	68548	5.727	2.208	0	10
Social trust scale	68796	5.239	1.901	0	10
High parental education	64960	0.342	0.474	0	1
Female	68796	0.528	0.499	0	1
Belongs to ethnic minority	68796	0.0151	0.122	0	1
Foreign-born father, born within Europe	68796	0.0282	0.166	0	1
Foreign-born mother, born within Europe	68796	0.0261	0.160	0	1
Father born in country outside of Europe	68796	0.0103	0.101	0	1
Mother born in country outside of Europe	68796	0.00926	0.0958	0	1
General reform indicator	68796	0.491	0.500	0	1
Reform indicator: extension reforms	40632	0.514	0.500	0	1
Reform indicator: all detracking reforms	29931	0.447	0.497	0	1
Reform indicator: pure detracking reforms	8258	0.428	0.495	0	1
Understand different people	66903	7.317	2.056	0	10
Institutional trust index	59255	4.491	1.993	0	10

The main model can formally be described as follows:

$$Y_{ij} = \alpha + \beta_1 R_{ij} + \Gamma\mathbf{X_i} + \delta_j + \lambda_t + \varepsilon_{ij}$$

where Y_{ij} signifies social trust for individual i in reform-window j. R_{ij} is a dummy that identifies whether an individual belongs to the reform-affected or unaffected cohorts (*reform dummy*). $\mathbf{X_i}$ is a vector of individual covariates. δ_j denotes a full set of reform-window dummies—'reform-fixed effects'. λ_t is a set of ESS round dummies. The coefficient of interest is β_1, as it identifies the effect of the reforms on social trust.

In countries with only one reform, the reform-fixed effects are equivalent to country-fixed effects, whereas in countries with several reforms, the average differences between the reforms are also absorbed. One dilemma for the design is that there has been a trend of increasing educational attainment throughout the studied time period, which means that the reform-windows of treated and non-treated cohorts will also pick up the effects of this trend. To counter this, $\mathbf{X_i}$ includes a general quadratic birth year trend, reform-specific linear controls for birth year and reform-specific quadratic age trends. The quadratic terms are included to allow sufficient flexibility in absorbing possible non-linear trends of increasing education within the reform-window of seven treated and seven untreated birth year cohorts (for a similar specification, see Brunello et al. 2009)

This design might be questioned on the ground that educational reforms are not implemented randomly. When and where educational reforms are carried through may depend on many factors, including social trust levels. However, the point of the design is not that educational reforms as such are exogenous, only that the reform effect for the affected birth cohorts is exogenous compared to adjacent unaffected cohorts. As long as differences in possible confounding factors do not exactly coincide with the cohorts within a country that are affected and unaffected by a reform, these factors may not bias the estimates (see the Supplementary material, Section A.2, for a further discussion on this assumption). The reform-fixed effects are also essential because they imply that only the within-reform-window variation is used to estimate the effects and between-reform differences are factored out, such as pre-reform differences in social trust.

The reforms are expected to have different effects depending on which educational trajectory the affected children would have followed in the absence of the reforms. The reforms that extend compulsory schooling can only affect children that if it were not for the reforms would quit school at the earliest possible time. The size of this group differs between the reforms but will be less than half of the sample in most cases (see Oreopoulos 2006). In the case of detracking reforms, there is a clear difference between attending a new comprehensive school instead of a lower basic track or instead of a higher academic track. These two counterfactual cases would, for instance, imply different compositions of classmates. To study these differences and gain some precision in the estimates, I must identify the likely educational path that would have been followed in the absence of the reforms. The intergenerational correlation in educational choices is high (Breen and Jonsson 2005), and one way of identifying this counterfactual path is thus through parental education. Therefore, I define a dummy variable signifying low or high parental education. This variable is based on the five-step items for parental education in the ESS. High parental education entails that the average education of the parents belongs to the upper half of the distribution of average parental education relative to country and birth year. This relative definition has the advantage of having a similar meaning across birth cohorts and countries.

All models are estimated with OLS, using reform-fixed effects, and apply country-by-birth year clustered robust standard errors.[1] As the reforms are assumed to generate exogenous variation in education, in principle there is no need to include other determinants of social trust as control variables. Furthermore, it would be a case of 'bad controls' to include any variables that may be affected by the reforms, such as income (Angrist and Pischke 2009). For reason of precision I include dummies for gender, belonging to an ethnic minority, foreign born mother, and foreign born father with additional dummies for whether born outside Europe. The age span is limited to individuals aged 25 to 80 to ensure that most respondents have finished their education and to avoid selection effects regarding the oldest cohorts.

I have refrained from using an instrumental variable (IV) approach, which is the standard in similar designs (d'Hombres and Nunziata 2016; Borgonovi et al. 2010; Cavaille and Marshall 2019). The main reason is that using the educational reforms as an instrument for educational attainment assumes that the whole effect of the reforms on social trust would be mediated by educational attainment (*the exclusion restriction*, Angrist and Pischke 2009). Since a crucial part of my argument is that the institutional context of education may be of equal importance to educational attainment, such an approach would be clearly unsuitable. The presented reform effects are instead equivalent to the reduced form estimates in an IV-framework. However, IV-estimates on the extensions reforms—where the exclusion restriction is more likely to hold—are presented in the Supplementary material.

[1] An alternative would be to cluster the standard errors on the country level but such an approach would risk to lead to biased standard errors because of too few clusters.

I use three main model specifications and estimate these for the joint set of all reforms, as well as separately for the different types of reforms. I start with the average reform effect. Thereafter, I examine how the effect differs with parental education by adding parental education and its interaction with the reform dummy. In the third main model I allow the effect of parental education to vary with all other independent variables, including the reform-fixed effects.[2] This flexible model explores whether there exists any other conditional relationship between parental education and the covariates that could potentially bias the interaction estimate between parental education and reform exposure.

4 Results

The main results for how the reforms affect social trust are presented in Table 3.[3] Starting with the joint effect of all types of reforms, Model (1) shows that the average reform effect is positive and amounts to 0.063 on the 0–10 social trust scale, being statistically significant at the 95 per cent confidence level. Females have a somewhat higher level of social trust, whereas respondents belonging to an ethnic minority report a lower degree of trust. Model (2) explores how the effect differs depending on parental education. The *Reform* coefficient in this model demonstrates the effect of the reforms among individuals with poorly educated parents, whereas the effect for those with highly educated parents is equivalent to the sum of the reform and the parental education interaction coefficients. The reform effect is slightly larger for respondents with poorly educated parents, whereas the negative interaction coefficient demonstrates that the point estimate of the effect is smaller for those with highly educated parents. The estimates are similar in the flexible interaction specification of Model (3).

I continue by exploring how the reform effect differs across the different types of reforms in Models (4) to (12). The effects for the reforms that only extended education without altering tracking are presented in Models (4) to (6). The effects on social trust are smaller than for the earlier models on all of the reforms and the average effect in Model (4) is not significant ($p = 0.21$). The same applies to respondents with a weak educational background in Model (5) for whom we would expect a stronger reform effect ($p = 0.15$). This is the case even though these reforms give rise to a clear increase in educational attainment of about four months for respondents with poorly educated parents (significant at the 99 per cent level). The effect becomes smaller in the flexible interaction model.

However, the results look different when we turn to the detracking reforms. Models (7) to (9) present the results for the broad set of detracking reforms that includes reforms that only detracked education and reforms that both detracked and extended compulsory schooling. The average effect of these reforms across the whole sample is positive and equals 0.082, significant on the 95 per cent level. For respondents with poorly educated parents the effect is larger and amounts to 0.11. These effects should be interpreted taking into account that the detracking reforms have rather small effects on educational attainment, almost zero on average and about two months for the respondents with a weak educational background. The negative coefficient for the interaction with parental education in Model (8) tells us that the effect is about a third smaller for those with highly educated parents,

[2] 'Fully dummy-interactive' model (Franzese and Kam 2007).

[3] The effects on educational attainment are presented in the Supplementary material, see Section A.3.

Table 3 Reform effect on social trust index[a]

	All reforms			Extension reforms		
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.063**	0.083**	0.077*	0.053	0.067	0.043
	(0.030)	(0.034)	(0.041)	(0.042)	(0.047)	(0.055)
High parental edu		0.340***	–		0.336***	–
		(0.022)			(0.031)	
Ref × High par edu		– 0.043	– 0.030		– 0.045	0.039
		(0.033)	(0.059)		(0.045)	(0.081)
Female	0.061***	0.063***	0.039**	0.050**	0.058***	0.029
	(0.016)	(0.016)	(0.019)	(0.021)	(0.022)	(0.027)
Ethnic minority	– 0.261***	– 0.226***	– 0.200**	– 0.283***	– 0.231**	– 0.191*
	(0.067)	(0.069)	(0.080)	(0.087)	(0.090)	(0.105)
Adj. R^2	0.20	0.21	0.21	0.17	0.18	0.18
Countries	16	16	16	11	11	11
Observations	68796	64960	64960	40632	38520	38520
Flexible interaction[b]	–	No	Yes	–	No	Yes
	Detracking reforms			Pure detracking reforms		
	(7)	(8)	(9)	(10)	(11)	(12)
Reform	0.082**	0.113**	0.124**	0.030	0.086	0.130*
	(0.040)	(0.046)	(0.059)	(0.055)	(0.058)	(0.073)
High parental edu		0.346***	–		0.397***	–
		(0.030)			(0.052)	
Ref × High par edu		– 0.040	– 0.091		– 0.020	– 0.135
		(0.046)	(0.086)		(0.079)	(0.101)
Female	0.059***	0.056***	0.042	– 0.004	0.015	– 0.011
	(0.022)	(0.021)	(0.027)	(0.035)	(0.035)	(0.046)
Ethnic minority	– 0.243**	– 0.226**	– 0.237**	– 0.394**	– 0.404**	– 0.396*
	(0.099)	(0.103)	(0.119)	(0.172)	(0.192)	(0.204)
Adj. R^2	0.25	0.26	0.26	0.064	0.079	0.079
Countries	11	11	11	3	3	3
Observations	29931	28153	28153	8258	7808	7808
Flexible interaction[b]	–	No	Yes	–	No	Yes

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Country-by-birth cohort clustered standard errors in parentheses

[a]All models include reform FEs, a general quadratic birth year trend, and reform-specific trends for birth year (linear) and age (quadratic). Additional controls: foreign-born parents and ESS round dummies.

[b]The flexible interaction models interact all explanatory variables with parental education (the coefficient for parental education is not interpretable in these models)

and the conditional effect for this group falls short of statistical significance ($p = 0.16$). The effect becomes slightly larger for those with a weak educational background in the flexible interaction specification, Model (9).

Models (10) to (12) reveal the effect of the reforms that only detracked education without altering compulsory schooling. The average reform effect is small, but the point estimate is somewhat larger for respondents with poorly educated parents. The effect for this group in the flexible interaction specification is comparable to the corresponding effect for the broader set of detracking reforms and reaches statistical significance at the 90 per cent level.

To summarize, the results from Table 3 show generally positive but small to modest effects of the reforms on social trust. However, the size of the effects differs depending on the type of reform. Extension reforms result in smaller effects that are not statistically significant, in line with what Yang (2019) finds. Thus, these results do not lend support to Hypothesis 1. A concern may be that the impact of the extension reforms on educational attainment—two to four months of additional education—is too small to have a substantial effect on social trust, but similar reform effects on education have been shown to significantly affect other attitudes (d'Hombres and Nunziata 2016). Nevertheless, the effects of the reforms appear to not only be a matter of how they affect educational attainment. Detracking reforms result in somewhat larger positive effects on social trust that are statistically significant, even though these reforms have considerably smaller effects on educational attainment than the extension reforms. The effect of detracking reforms on social trust is also stronger among individuals with poorly educated parents. In addition, similar but less precise effects are found for the same group in the small sample of pure detracking reforms. While the imprecise estimates of the pure detracking reforms have to be interpreted cautiously, they are in line with the effects of the broader set of detracking reforms. Altogether these results show that reducing tracking in compulsory education has a positive effect on social trust, lending support to Hypothesis 2. Thus, the results stress that the institutional character of education may be as important for outcomes as the length of education.

However, the effect of detracking reforms on social trust is modest and just somewhat larger than the reform effect of extension reforms, even though the difference is large enough to make the former effect statistically significant. A valid question is whether effect sizes between 0.082 and 0.12 on a 0–10 scale are substantially interesting? Comparisons with some important determinants of social trust on the individual-level can put these effects into perspective. The average effect of 0.082 is equivalent to 25 per cent of the mean difference in social trust between ethnic minorities and the majority population in the ESS (0.327). The fact that the reform effect for individuals with poorly educated parents, 0.11–0.12, is equivalent to 35 per cent of the pre-reform difference between individuals with poorly and highly educated parents (0.346) could suggest that the effect actually has some substantial relevance, considering that family background is one of the most important determinants of social trust (Oskarsson 2017; Huang et al. 2011).

4.1 Exploring the Mechanism Behind the Effect of the Detracking Reforms

What drives the effect of detracking reforms? Can the mechanism of intergroup contacts be supported? The ESS offers some options for testing the mechanism, if not an exact test. First, separating the effect of detracking from the effect of educational attainment is important and can partly be done by controlling for education in a flexible way. I focus on the broad set of detracking reforms that also include elements of prolonged education. I specify these models in the same way as the previous models but add 'education fixed effects' by adding a full set of dummies for years of education that absorbs all of the differences in social trust that may be attributed to years of education. The results are presented in Table 4, where Model (1) shows the average effect and Model (2) includes the parental

Table 4 Testing the mechanism behind the effect of detracking reforms[a]

	Social trust: education controls		Social trust: restricted samples		Understand different people		Inst. trust
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform	0.071*	0.089**	0.154	0.255**	0.081*	0.116**	– 0.016
	(0.039)	(0.043)	(0.101)	(0.123)	(0.047)	(0.053)	(0.045)
High parental edu		0.130***				0.323***	
		(0.029)				(0.034)	
Ref × High par edu		– 0.029				– 0.059	
		(0.045)				(0.052)	
Female	0.068***	0.063***	– 0.018	0.100	0.290***	0.280***	– 0.075***
	(0.021)	(0.021)	(0.039)	(0.065)	(0.027)	(0.027)	(0.024)
Ethnic minority	– 0.214**	– 0.219**	– 0.486***		– 0.059	– 0.042	– 0.261**
	(0.090)	(0.092)	(0.179)		(0.111)	(0.114)	(0.102)
Adj. R^2	0.28	0.28	0.22	0.15	0.036	0.038	0.27
Countries	10	10	7	10	10	10	10
Observations	29663	27926	8337	3459	28982	27321	25759
Education FEs	Yes	Yes	No	No	No	No	No
Sample	Full	Full	≤40 yrs	Minority	Full	Full	Full

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Country-by-birth cohort clustered standard errors in parentheses.

[a]Sample and controls mimic Table 3 with the following alterations. Models (1) and (2) add a full set of dummies for years of education. Model (3) restricts the sample to respondents up to 40 years old, whereas Model (4) only includes respondents belonging to an ethnic minority, respondents being members of a discriminated group or respondents who have at least one foreign-born parent. Models (5) and (6) exchange the dependent variable to a measure of whether the respondent agrees that it is important to understand people who are different from oneself. In Model (7) the dependent variable is instead an index for institutional trust. See the Supplementary material, Section A.1, for further details on the dependent variables in Models (5), (6) and (7)

education interaction. The reform effect is remarkably robust and only a small part of the effect is lost compared to the corresponding models in Table 3, although the confidence level for the average effect falls to 90 per cent.

Second, if the effect on social trust stems from experiences in school, I would expect the results to be stronger among younger persons who spent time in school more recently. Model (3) in Table 4 tests this proposition by limiting the sample to those aged 40 or younger. The point estimate turns out to be about twice as large compared to Table 3 but is imprecise and does not reach conventional confidence levels ($p = 0.13$). Third, it could be hypothesized that the positive effect of meeting peers with different backgrounds than one's own should have a larger impact on social trust among individuals with a further social distance to the generalized other in a community, such as immigrants and ethnic minorities. Model (4) tests this suggestion by limiting the sample to those who identify as belonging to an ethnic minority or a discriminated group in the country, as well as those who have at least one foreign-born parent. The point estimate for the effect is more than three times larger than for the full sample and statistically significant at the 95 per cent level.

Fourth, if the mechanism of the positive effects of detracking on social trust depends on intergroup contacts and bridging networks I would expect to also find effects on attitudes more specifically about one's view of people with a different background than one's own. The ESS incorporates an item on whether the respondent thinks it is important to listen and understand people who are different from oneself, even if you disagree with these people. Models (5) and (6) in Table 4 use this item as the dependent variable, otherwise following the specification in Table 2. The effects are significant and resemble the effects on social trust. Finally, social and institutional trust are usually found to be quite strongly related and the more well-educated tend to express higher levels of institutional trust (Newton et al. 2018; Zmerli and Newton 2008; Sønderskov and Dinesen 2016). However, the mechanism of intergroup contacts is not expected to have substantial effects on institutional trust. In Model (7), I thus explore the reform effect of detracking reforms on institutional trust as a placebo test. The effect is insignificant and negative.

The models in Table 4 have been rerun on the extension reforms to verify the difference between detracking and extension reforms. The results are in line with what would be theoretically expected, none of the effects of extension reforms become significant and all point estimates are smaller than in Table 4, except for institutional trust (see the Supplementary material, Section A.6).

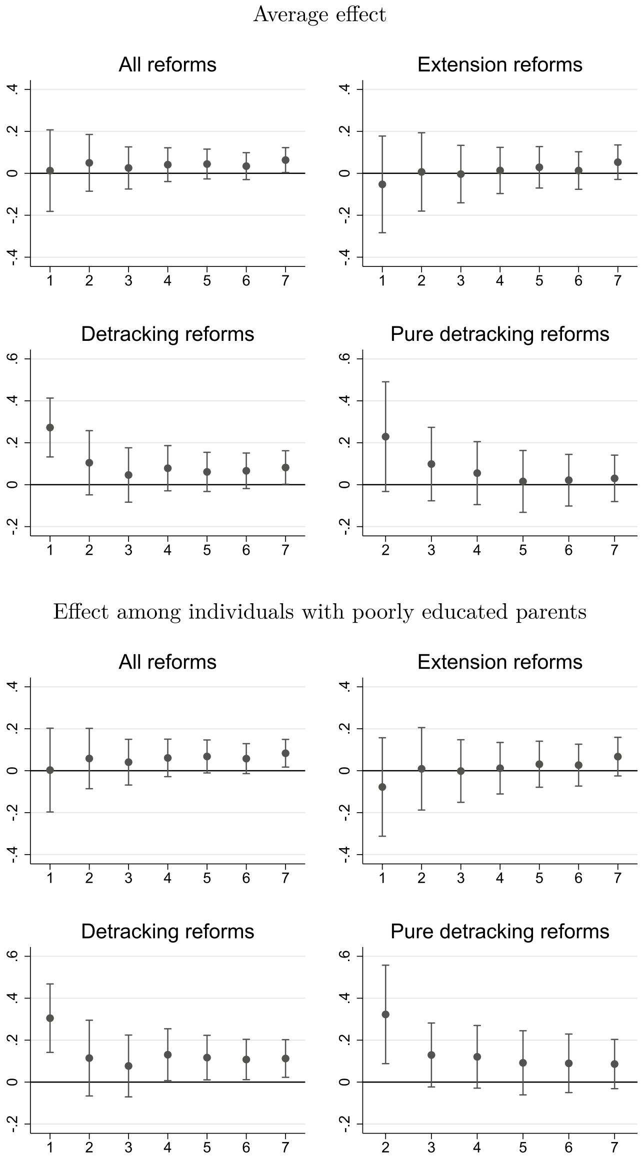
5 Robustness Checks

One method to assess the validity of this type of empirical approach is to carry out a placebo test. The idea is to see what happens if the timing of the exogenous variation is manipulated to a time point at which no exogenous variation would be expected. If an effect is still identified, that would bring the exogenous character of the variation into question. For this test, I am following an approach similar to that of d'Hombres and Nunziata (2016), generating randomly timed placebo reforms by a random draw from a uniform distribution. The same number and types of reforms are generated as in the actual data. Ten thousand Monte Carlo simulations are performed by repeatedly generating a set of placebo reforms and estimating the central regression models of Table 3.

The results from the simulations are reported in the Supplementary material (together with the results from the other robustness checks). The average effects of all types of placebo reforms on social trust are effectively zero (see Section A.7). Thus, the placebo test offers support for the argument that the reforms are a source of exogenous variation and therefore strengthens the causal interpretation of the results for detracking reforms.

The main model specification used a reform-window with a bandwidth of plus/minus seven birth year cohorts. However, there is a risk that this reform-window captures other cohort-dependent differences. Figure 1 shows the robustness of the results when narrowing the bandwidth stepwise down to plus/minus one cohort, with plots for the average effect and the effect among individuals with poorly educated parents. The effect of extension reforms on social trust approaches zero for narrower bandwidths, whereas the positive effect of detracking reforms is relatively stable. However, significance is lost for the average detracking effect with narrower bandwidths while the larger effect among individuals from weakly educated backgrounds is more robust. In sum, these plots corroborate the positive effect of detracking reforms but bring further uncertainty to the effect of extension reforms, indicating a null effect rather than a small insignificant effect.

Scholars have argued that the effect of education on institutional and social trust is dependent upon the quality of government (Charron and Rothstein 2016; Hakhverdian and Mayne 2012). My results could thus be questioned by arguing that the difference in the



95 per cent confidence intervals. The x-axis denotes the width of the reform-window; from +/− 1 year to +/− 7 years. The plots are based on the models in Table 3. The upper panel portrays the average reform effect and the lower panel portrays the reform effect in the standard parental education interaction models.

Fig. 1 Changing reform-window bandwidth: reform effect on social trust. 95 per cent confidence intervals

effect of extension and detracking reforms reflects differences in institutional quality of the countries that have implemented each type of reform. I test this proposition by letting the reform effect vary with an aggregate indicator for quality of government. If anything, there is a stronger positive reform effect on social trust in countries with low institutional quality (see Section A.8). This pattern is the same across extension and detracking reforms. My results thus contradict previous research, which could possibly be explained by that these earlier studies do not consider the endogenous nature of education.

Rerunning the models on the classic single-item social trust measure further corroborates the main results with stronger effects of detracking reforms but equal or weaker effects of extension reforms (see Section A.9). The results are also robust to several other specifications, such as excluding the Eastern European reforms, applying birth year fixed effects, not making pure detracking reforms part of the broader category of detracking reforms, and allowing for country-clustered standard errors with the wild cluster bootstrap procedure (see Sections A.10–13). Furthermore, using an instrumental variable approach when estimating the effects of the extension reforms also renders non-significant effects of education on social trust (see Section A.5).

6 Conclusion

This paper finds that compulsory schooling reforms have small to modest positive effects on social trust. However, the effect differs with the type of reform. For reforms that extend compulsory education without reducing the degree of tracking there is an indication of a positive effect, but it is not statistically significant and it approaches null in more rigid specifications. Thus, Hypothesis 1 cannot be verified. This finding implies that the results from recent single-country studies (Yang 2019; Oskarsson 2017) hold for a broad group of European countries and across time; the causal effect of the length of education on social trust is most likely small or even non-existent. However, I find statistically significant effects of educational reforms that reduce the level of tracking, lending support to Hypothesis 2. The effect is stronger for individuals with poorly educated parents but the effect may in general be considered to be of modest size. These results suggest that education at least to some extent can have a causal effect on social trust but that it is conditioned by the institutional context of education—particularly tracking. When assessing the size of the effect of detracking reforms it is also worth considering that the empirical approach in this paper may be seen as a difficult test of the effect of detracking on social trust. Most of the individuals in the data experienced these reforms decades before the data were collected.

Tests of the mechanisms at hand show that the effect of detracking reforms is not a matter of differences in educational attainment. Instead, I find more substantial effects for groups that should be more strongly affected by intergroup contacts in school. Additionally, I identify effects on related outcomes that accompany such a mechanism. The fact that the effect of detracking reforms is stronger among individuals with poorly educated parents implies that children from such homes benefit more from spending time with a more diverse set of peers—mainly children of more well-educated parents—than vice versa. Children from stronger backgrounds might be able to partly acquire the positive effects of intergroup contacts by other means. For example, well-educated parents may transfer knowledge about other groups, which in turn fosters understanding and out-group trust.

The positive effect of detracking reforms has important policy implications. It gives policy makers reasons to promote more integrated schools to foster higher levels of social trust, while prolonging compulsory education is of more questionable relevance. The fact that the detracking effect is stronger among individuals from less resourceful backgrounds, including individuals with a minority background, is encouraging, as it implies opportunities for increasing social cohesion through school reform. Whereas this study focused on tracking, the results could also have a bearing on other forms of separation of school children, particularly school segregation.

From a methodological standpoint, the importance of the institutional context causes me to question the common practice of combining different educational reforms that extend compulsory education as an instrument for education. This approach assumes that the full effect of the reforms is exerted via educational attainment (e.g., Cavaille and Marshall 2019). However, if the effect is partly conditioned by the institutional setting, this design would lead to biased estimates of the effect of education, confusing institutional effects with the effects of educational attainment. This conclusion is at odds with Brunello et al. (2013), but the difference could be explained by cognitive outcomes differing from attitudes and the institutional context playing a larger role for attitudes.

The findings of this study call for several future studies to advance our understanding of the determinants of social trust. The mechanism by which detracking affects social trust should be further explored. We would need to learn more about the factors that influence the size of the effect and a more in-depth study of intergroup contacts is warranted to confirm the argument set forth in this article. It would also be intriguing to compare the effects of socio-economic and ethnic diversity in the school setting, as well as to study how they interact, considering previous studies showing varying effects of ethnic diversity in schools on social trust (Dinesen 2011; Janmaat 2015). To further scrutinize how the context of education, such as type of education and curriculum, affects social trust would also be of great interest. Furthermore, the focus of this piece has been on primary and secondary education; additional research is needed on higher levels of education to explore whether the effects are comparable across educational levels.

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Compliance with Ethical Standards

Conflict of interest The author declares that he has no conflict of interest.

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