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DISCUSSION PAPER SERIES

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AUGUST 2018

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## ABSTRACT

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# The Effects of Education on Health: An Intergenerational Perspective\*

This paper presents evidence of substantial causal effects of parental education on children's health behaviours and long-term health. We study intergenerational effects of a compulsory schooling increase in Germany after World War II, which was implemented across federal states at different points in time. Maternal schooling reduces children's smoking and overweight in adolescence. The effects persist into children's adulthood, reducing chronic conditions that often result from unhealthy lifestyles. We find no effects of paternal education. Children's peer environment early in life and increased investments in their education are possible effect channels. The intergenerational effects exceed the direct effects on health.

**JEL Classification:** I12, I24, I26

**Keywords:** parental education, returns to education, smoking, overweight, compulsory schooling, health behaviour

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# 1 Introduction

While educational differences in health behaviours, general health status, chronic conditions, or longevity are well documented across countries, it is still an ongoing debate to what extent the link between education and health is causal. Many studies use plausibly exogenous variation from education policies, such as compulsory schooling regulations, to estimate causal effects. Despite the many theoretical arguments for a *causal* relationship (e.g. Grossman, 2006), this large literature produces mixed findings and is far from being conclusive (e.g. Lochner, 2011; Clark and Royer, 2013; Grossman, 2015; Black, Hsu, and Taylor, 2015; Galama, Lleras-Muney, and van Kippersluis, 2018).

This paper presents new evidence for substantial effects of compulsory schooling on health behaviours and health *on the children* of affected individuals. The intergenerational perspective can account for important margins that may substantially drive the strong correlations between education and health, but that are not captured by direct causal effect estimates: Health-related habits are key sources of the leading health risk factors in high-income countries (WHO, 2009), but they may already be shaped before schooling reforms - which are typically used to identify effects - affect individuals. For example, smoking is typically initiated during the teenage years, often while children are still in school (Jürges and Meyer, 2017). Also, dietary habits may already have been shaped substantially, and overweight problems in childhood and adolescence often persist into adulthood (e.g. Serdula et al., 1993; Guo et al., 2002; Rhee, 2008). Expansions of schooling within the same school and peer environment may only have a limited impact on *altering* health-related habits. Education effects on long-term health or mortality may then be small.

In contrast, increases in *parental* education can impact children's health already in the *formation* of health-related habits: Better educated parents may purchase healthier food and better health services (e.g. Soteriades and DiFranza, 2003; Case, Lubotsky, and Paxson, 2002; Currie and Stabile, 2003; Currie, Shields, and Price, 2007); they may serve as a better role model and transmit attitudes toward healthier behaviours (e.g. Powell and

Chaloupka, 2005; Göhlmann, Schmidt, and Tauchmann, 2010). Better educated parents may send their children to better schools improving their peer environment early in life. Better health in childhood and increased educational attainments of children improve children's earnings prospects and may even alter discount rates and risk aversion such that the initiation of unhealthy behaviours becomes more costly (Fuchs, 1982; Becker and Mulligan, 1997; Behrman and Rosenzweig, 2004; Black, Devereux, and Salvanes, 2007; Case, Fertig, and Paxson, 2005; Cutler and Lleras-Muney, 2010). Increases in parental education may also affect family stability, fertility choices, and mating behaviour, which can impact children's health behaviours and education outcomes.<sup>2</sup> The variety of possible channels illustrates that intergenerational effects of schooling on health behaviours and long-term health may be substantial even if the direct causal effects of education reforms on health outcomes are small.

In this paper, we study the impact of parental schooling on children's smoking behaviour and on being overweight in adolescence, and trace the effects on health-related behaviours, chronic conditions, and long-term health into children's adulthood. Smoking and being overweight are important causes of future health problems and chronic conditions (such as cardiovascular diseases, type 2 diabetes, and cancer) as well as premature death (e.g. Must, 1999). While smoking is itself a risky health behaviour, being overweight is closely related to other common risky health behaviours, such as physical inactivity or a poor diet (see, e.g., Hill, 1998, 2003; Janssen et al., 2005). For the identification of causal effects of parental schooling, we study a compulsory schooling reform in Germany: The reform increased the minimum number of school years from eight to nine, stepwise across federal states between 1949 and 1969. We first use the reform as an instrument for parental schooling, a standard approach in the literature on causal effects of schooling (e.g. Card, 1999; Holmlund, Lindahl, and Plug, 2011). In a second approach, we additionally exploit the fact that the German education system ability-tracks students into different

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<sup>2</sup>Studies documenting effects of family factors on child obesity and overweight include, e.g., Rhee (2008); Schmeer (2012); studies documenting effects of family factors on smoking include, e.g., Ermisch, Francesconi, and Pevalin (2004); Francesconi, Jenkins, and Siedler (2010).

schools: The compulsory schooling reform affected parents in the basic school track. As the reform did not impact the completed school track of parents, we focus on children of parents with basic track schooling and exploit the state-time variation of the compulsory schooling reform in difference-in-differences models. Children with parents from higher tracks serve as a natural placebo group and are additionally used as a control group in triple-differences models. The main analysis builds on data from the German Micro Census, an annual representative survey of one percent of all households in Germany. We focus on children aged 15 to 18 living with their parents. We then use supplementary data from the German Socio-Economic Panel Study (SOEP), tracking the effects of the compulsory schooling reform into children's adulthood after they moved out from home.

The main findings of the paper are as follows: One additional year of maternal schooling reduces children's probability to smoke (3.8 percentage points, or about 17 percent) and to be overweight (3.6 percentage points, or about 21 percent) in adolescence. The effects persist into children's adulthood, eventually lowering the risk of chronic conditions and improving the general self-reported health status by about 0.1 standard deviations. For smoking, the effects mainly arise because children never initiate smoking. Although paternal years of schooling also correlate highly with children's health-related outcomes, we find no evidence for a causal relationship. The findings pass a large set of specification and placebo checks.

The paper then attempts to investigate potential channels through which maternal education impacts children's health-related outcomes. We present evidence that children of treated mothers achieve higher levels of education, they attend better school tracks, and are exposed to a better school peer environment in adolescence - a critical period for the initiation of smoking and other risky behaviours (such as drinking and substance abuse). Increased levels of education also improve children's earnings prospects, which may increase the costs of engaging in unhealthy behaviours (Fuchs, 1982; Cutler and Lleras-Muney, 2010). We test other channels through which increases in maternal schooling could improve children's health-related outcomes, including changes in family in-

come and mating (which allows living in better neighbourhoods, to purchase sports club memberships, better health services, or better food, see, e.g., Currie and Stabile, 2003; Carneiro, Meghir, and Parey, 2013), family stability and fertility (which may change the time and material investments parents can invest in their children, see, e.g., Hanushek, 1992; Francesconi et al., 2010), and parental health behaviours (as parents may serve as a role model, see, e.g. Powell and Chaloupka, 2005). The empirical relevance of these channels appears small, although even small changes in these dimensions may contribute to substantial improvements in unhealthy behaviours and long-term human capital.

The study makes the following important contributions. First, we present the first causal evidence of parental schooling effects on *children's health behaviours*. A large literature on the causal effects of education on health exclusively focuses on the direct effects within the same generation (see, e.g., Grossman, 2015; Galama et al., 2018, for recent reviews).<sup>3</sup> Only a very small literature on intergenerational effects of education on health presents evidence for improved child health around birth and a more health-oriented behaviour of mothers during pregnancy (e.g. Currie and Moretti, 2003; Chou, Liu, Grossman, and Joyce, 2010; McCrary and Royer, 2011). Lindeboom, Llena-Nozal, and van der Klaauw (2009), Carneiro et al. (2013) and Lundborg, Nilsson, and Rooth (2014) examine effects on child outcomes in later childhood and adolescence, including children's overweight as an outcome. Despite some sizeable point estimates, these studies reject effects on children being overweight. We extend the literature by providing the first robust evidence for maternal schooling effects on children's smoking behaviour and being overweight in adolescence, and by demonstrating that these effects persist into children's adulthood. The findings help better understand the strong educational gradients in health behaviours and health status that are important for the intergenerational transmission of socio-economic status.

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<sup>3</sup>Some studies document correlations between parental education and children's health behaviour (Waldron and Lye, 1990; Lowry, Kann, and Collins, 1996) or try to estimate the causal effect using parental background information as instruments for parental schooling (e.g. Kemptner and Marcus, 2013). However, these instruments do not credibly overcome the endogeneity problem (Kenkel, Lillard, and Mathios, 2006).



Furthermore, the study improves our understanding of *non-market* benefits of education. Typically, the literature cannot disentangle effects of education *per se* from effects of higher incomes that are often associated with more schooling (see, e.g., Oreopoulos and Salvanes, 2011, for an overview). A major challenge in the literature is to disentangle the two because policy implications vary widely. Our analysis builds on a reform for which Pischke and von Wachter (2008) report zero-returns to compulsory schooling.<sup>4</sup> In our data, we also arrive at the zero-effects on earnings, but substantial effects on children’s health outcomes. The reform thereby provides a natural experiment to analyse non-market benefits of education when the income channel is mostly closed. Our findings suggest that these *non-market* benefits are still substantial. We complement findings by Lundborg et al. (2014), for whom it appears that income effects can explain most of the effects of maternal schooling on children’s cognitive skills and health.

Finally, we extend the scarce literature on *long-term* effects of educational interventions: Most studies on intergenerational effects of schooling focus on children’s schooling outcomes (e.g. Black, Devereux, and Salvanes, 2005; Oreopoulos, Page, and Stevens, 2006; Holmlund et al., 2011; Piopiunik, 2014; Dickson, Gregg, and Robinson, 2016). The novel evidence on intergenerational schooling effects on health behaviours and health status that last into children’s adulthood can further justify public investments in education as individuals would typically not consider these spill-over effects on their children in their educational investment decisions.

The remainder of the paper is organised as follows. Section 2 provides information about the German education system and compulsory schooling law changes. Section 3 introduces the data and outlines the empirical strategy. We present the main results in Section 4 and a large set of sensitivity checks in Section 5. Section 6 concludes.

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<sup>4</sup>In a paper draft, Cygan-Rehm (2018) suggests that significant changes to the sample selection and empirical strategy of Pischke and von Wachter (2008) can lead to positive earnings returns. However, the reduced-form reform effect on women’s earnings - the mothers in our setting - is not significant. Our main findings are robust to Cygan-Rehm’s suggested main specification adjustments (available upon request).

## 2 The compulsory schooling reform in West Germany

At the time of the compulsory schooling reform, children typically entered primary school at age six and attended school jointly for four years. Thereafter, students were ability-tracked into three different secondary school tracks: The basic school track (*Volksschule* or *Hauptschule*) with school completion after eight or nine years of schooling, the middle track (*Realschule*) with completion after ten years of schooling, and the high-ability track (*Gymnasium*) with completion after 13 years of schooling.<sup>5</sup> Students completing the high track earned the general university entrance qualification and could study at university. Students from the basic and middle tracks typically proceeded with vocational trainings. In the 1940s, the basic track was attended by about eighty percent of students. As the availability of places at higher tracks has expanded rapidly since the 1950s, the share gradually declined to below fifty percent in 1970.<sup>6</sup>

After World War II, all West German federal states increased the minimum number of school years in the *basic school track* from eight to nine. The reform was implemented at different points in time across federal states (see Appendix Table B.1). It is exploited in other studies analysing the impact of increases in schooling on labour market outcomes (Pischke and von Wachter, 2008; Kamhöfer and Schmitz, 2016), health behaviours and health status of the affected generation (Kemptner, Jürges, and Reinhold, 2011), civic engagement (Siedler, 2010), and fertility (Cygan-Rehm and Maeder, 2013). Piopiunik (2014) uses the reform to study the effects on children’s school track.<sup>7</sup> Cohorts affected by the reform had to stay in school one year longer. The first cohorts with a ninth grade

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<sup>5</sup>The mechanism for selecting students into the different school tracks varies across cohorts and federal states. Generally, it depends on grades in primary school, teacher recommendations, parental choice, or, for the high-ability track, formal admission exams. Mobility between tracks after initial assignment is generally very low (Dustmann, 2004).

<sup>6</sup>In the empirical analysis, the general increase in education levels in the population is accounted for by cohort-fixed effects and state-specific time trends. A large set of robustness checks is dedicated to ruling out that the empirical results are confounded by general trends, or state-cohort specific shocks, instead of increases in compulsory schooling of parents.

<sup>7</sup>Cygan-Rehm and Maeder (2013) and Piopiunik (2014) report different years of the reform in four small states. We adhere to Pischke and von Wachter (2008), but the main findings are robust to using the alternative dates.

in the basic track were born between 1934 (in Hamburg) and 1955 (in Bavaria).

The motives for introducing a ninth grade vary across states, probably because the goals of basic schooling shifted over time in post-war Germany. The first reforms in the early postwar period, such as in Hamburg, were motivated by high youth unemployment rates and limited vocational training places (Schneider, 1952). In the Hamburg Accord (*Hamburger Abkommen*) of 1964, all federal states agreed that the minimum number of school years should be nine, emphasising educational and developmental goals of an additional school year. The economy needed better-educated individuals, and transitioning 14-year old children into the labour market may be harmful for their development because they are in a vulnerable stage of their psychological development (Petzold, 1981).<sup>8</sup>

In the additional grade, continued general education was a focus. Students would typically be continuously taught in the main subjects (e.g. mathematics, language arts, sciences, and vocational preparation). The exact curricula differed partly between states. For example, the state of Bremen focused on general knowledge, while Niedersachsen focused more on consolidating basic skills and on teaching political responsibility (Pischke and von Wachter, 2008).

### 3 Data and empirical strategy

#### 3.1 Data

The main analysis is based on the German Micro Census, an annual representative survey of one percent of all households in Germany (RDC, 2017). Participation in the Micro Census is legally compulsory. The scientific use file of the data contains a 70 percent random subsample. Although the data is used in studies on the causal effects of education

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<sup>8</sup>In some states, the introduction of the ninth grade was preceded by local and temporary introductions of a ninth grade. For example, before the ninth grade became compulsory in the states of Bavaria (1969) and Niedersachsen (1962), these states allowed counties and towns to mandate a temporary ninth grade in the early 1950s to reduce youth labour market tensions. The findings are robust to controlling for potentially affected cohorts of the temporary introduction of a ninth grade (see Section 5).

on health behaviours and health status (e.g., Kemptner et al., 2011), and on intergenerational associations in education outcomes (e.g., Riphahn and Trübswetter, 2013), this is the first study using the data to estimate *intergenerational effects* of education.

The large data set contains rich socio-economic information, including the highest school degree, labour market outcomes, the state of residence, the birth year, and information on children in the household. We focus on adolescent children aged 15 to 18, as the share living with their parents is very high at 97.6 percent in 2009, and stable over time (see Appendix Table B.2). This is because most children live with their parents until they complete vocational training or until they graduate from the academic track school and because they need parental approval to move out before age 18.<sup>9</sup>

We determine parents' years of schooling based on their highest school degree and the typical number of years required to earn this degree. We assume that parents went to school in the state they currently live in. This assumption seems reasonable, as cross-state mobility is low in Germany: 85 percent of individuals aged 40 to 50 still live in the state they went to school in (based on data from the German Socio-Economic Panel Study, SOEP, see Wagner, Frick, and Schupp, 2007). We focus the analysis mainly on parents with basic track schooling, of which 91 percent still live in their schooling state.<sup>10</sup>

Health-related questions are asked in several waves of the Micro Census to a 45 percent

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<sup>9</sup>To rule out that our findings are confounded by changes in children's moving-out behaviour, we also restrict the sample to children aged 15-17, when they are almost exclusively still in school and living at home (see robustness Section 9). Furthermore, we estimate the effects in data from the German Socio-Economic Panel Study (SOEP), in which information on children is also available after moving out from home. The Micro Census data contain direct pointers from children to mothers and fathers in the household from 2005 onward. In earlier waves, identifying parents is possible based on children's relationship to the household head, information on household heads' partners, their marital status, and an age-range plausibility check on the potential parents (especially in multi-generational households). Using data from the 2005 Micro Census that include parent-pointers for cross-validation, we can identify about 98 percent of parents correctly. Any differences between survey waves that may arise from the improvement in reporting quality after 2005 should be unrelated to the compulsory schooling reforms, and are accounted for by survey year fixed-effects.

<sup>10</sup>As in other international data, direct measures on years of schooling are rare in German data. Assigning the usual length of schooling based on the highest educational degree is a typical procedure in the literature. For a subsample, we calculate the years of schooling based on the year of the final educational degree and find similar, though noisier, first stage coefficients.

random subsample of households. Information on smoking behaviour is available in the surveys from 1989, 1995, 1999, 2003, 2005, and 2009; information on self-reported body height and weight for the years 1999, 2003, 2005, and 2009. To make best use of the available information, we pool all waves constituting two random samples.<sup>11</sup>

We focus the analysis on two main outcomes: Smoking and being overweight. While smoking is an important risky health behaviour, the overweight measure serves as an indicator for individual's general health status that is also related to other common risky health behaviours, such as low levels of physical activity or a poor diet (see, e.g., Hill, 1998, 2003; Janssen et al., 2005). Both outcomes are strong predictors of future health problems and chronic conditions (such as heart diseases, diabetes and cancer), and they are key determinants of major health risks in high-income countries (WHO, 2009). In the analysis, the variable smoking is one if adolescents smoke regularly or occasionally. The overweight assessment is based on adolescents' body mass index (BMI, calculated from body height and weight) and overweight thresholds for Germany (Kromeyer-Hauschild et al., 2015). Children are classified as overweight if their BMI is above the age and gender-dependent 90th percentile-threshold. This definition is standard and employed in other analyses on child obesity in Germany (e.g., Cawley and Spiess, 2008; Reinhold and Jürges, 2012). International thresholds based on Cole et al. (2000) are used in a robustness check.

Answering health-related questions in the Micro Census is voluntary and self-reporting may be prone to misreporting. Unless potential misreporting or missing information are systematically related to parents' being affected by the compulsory schooling reform, the causal effect analysis is not biased. Whether misreporting is an issue cannot be tested directly. Still, we can compare the reported body height and the body weight information

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<sup>11</sup>The Micro Census follows a subsample of individuals for four consecutive waves, but the health-related questions appear only once during this period. As health information only became available after 1989 and as we consider children up to age 18, we over-represent younger parental cohorts. The results are robust to weighting the regressions with inverse probability weights to represent the original distribution of birth cohorts (see Section 5.2).

to a large representative health survey of children in Germany that is based on external assessments rather than self-reporting (KiGGS study, conducted between 2003 and 2006 by the Robert Koch-Institute, see Kurth, 2007). The average body height in the Micro Census is slightly larger, and the body weight slightly lower.<sup>12</sup> With respect to missing information, we run a multivariate regression of missings in children’s health-related information on their age, gender, parents’ years of schooling, and household income (Panel A of Appendix Table B.3). The probability for missing information in smoking behaviour decreases with age. Parental education correlates positively with missing information. We test for a causal relationship between missing information and parental schooling within the estimation framework outlined in Section 3.2 and cannot find any evidence (Panel B of Appendix Table B.3). Therefore, missing information is not biasing effect estimates.

The samples include children with parents based in West Germany (excluding Berlin) who were born between 1930 and 1960. Descriptive statistics on the main samples are reported in Appendix Table B.4. About 16 percent of children smoke, and about 9 percent are classified as overweight. The general trends in children’s outcome variables go in opposite directions: While adolescence smoking is downward-trending after 2000, overweight is slightly upward-trending (see Appendix Figure A.1).

### 3.2 Empirical strategy

We estimate the causal effects of parent  $p$ ’s schooling  $S$  on child  $i$ ’s health-related outcome  $H$  separately for mothers and fathers with the following model:

$$\begin{aligned}
 H_i = & \alpha_1 S_i^p + \alpha_2 (\text{birth year FE})^p + \alpha_3 (\text{state FE}) \\
 & + \alpha_4 (\text{state-time trend})^p + X_i' \alpha_5 + u_i
 \end{aligned}
 \tag{1}$$

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<sup>12</sup>For example, 15-year old females’ average height (weight) was 165 centimetres (59.9 kilograms) in the KiGGS study, and 166 centimetres (57.4 kilograms) in the 2005 Micro Census. 15-year old males’ average height (weight) was 175.1 centimetres (66.4 kilograms) in the KiGGS study, and 174.4 centimetres (65.1 kilograms) in the 2005 Micro Census (these values are within the 95 percent confidence interval). For details, see Stolzenberg, Kahl, and Bergmann (2007). Smoking is also self-reported in other surveys and cannot be validated by external assessments.

To overcome endogeneity in parental years of schooling, we first use the introduction of a ninth grade in the basic school track as an instrumental variable (IV)  $Z_i^p$  for parental schooling  $S_i^p$ :

$$S_i^p = \beta_1 Z_i^p + \beta_2 (\text{birth year FE})^p + \beta_3 (\text{state FE}) + \beta_4 (\text{state-time trend})^p + X_i' \beta_5 + \epsilon_i \quad (2)$$

The introduction of a ninth grade in the basic school track varied across cohorts and federal states. Conditional on parents' birth cohort ("birth year FE") and federal state ("state FE"), parental exposure to the compulsory schooling reform is exogenous. Stephens and Yang (2014) stress the importance of accounting for region-specific trends when using regional variation in compulsory schooling laws for identification, as differential, region-specific improvements in, e.g., economic conditions may falsely be assigned to changes in compulsory schooling and overestimate the true benefits. To rule out that differences in regional trends are driving the estimated effects, we include interaction terms of parental state of residence dummies with a linear trend in their year of birth ("state-time trend"). The vector  $X_i$  includes dummies for children's age, gender, and the survey year. The model accounts for policy changes at the federal level (such as cigarette tax increases) through birth year dummies, survey wave dummies, and children's age dummies. Their combination controls flexibly for children's year of birth and parents' age. The IV-strategy is similar to previous studies using education reforms to estimate the intergenerational effects of education, such as Holmlund et al. (2011) and Chou et al. (2010).

The resulting IV-estimator is the Wald estimator that rescales the reduced form effects by the first stage effects. For the identification of causal effects of increases in compulsory schooling, the German tracking systems provides an advantage over other school systems without tracking, such as the US: The reform was only binding for students in the basic track. Other school tracks already had more years of schooling. For treated basic track students, the required number of years in school increases by one, and the first stage coefficient from eq. 2 within this subsample equals one.

This feature is useful for two reasons: First, if the compulsory schooling reform had no

effect on the highest school degree of parents, we can estimate the reduced form effect for children with parents from the basic track. Children with parents from higher tracks constitute a natural placebo group to test whether the reform effect estimates capture effects related to parents' compulsory schooling increase or general trends unaccounted for by the model. Second, estimating the reduced form model in the complier sample can result in more precise estimates. We estimate the following difference-in-differences (DiD) model in subgroups based on parents' school track:

$$\begin{aligned} H_i = & \gamma_1 Z_i^p + \gamma_2 (\text{birth year FE})^p + \gamma_3 (\text{state FE}) \\ & + \gamma_4 (\text{state-time trend})^p + X_i' \gamma_5 + \xi_i \end{aligned} \quad (3)$$

The notation follows analogously, and the coefficient of main interest is  $\gamma_1$ .<sup>13</sup>

The identification strategy rests crucially on the common trend assumption. We perform several checks (beyond the checks in the placebo sample) on the plausibility of this assumption: We check whether the results are sensitive to the inclusion of state-time trends or control variables; we simulate placebo reforms assuming that the ninth grade was introduced earlier; we estimate the effects on a health-related placebo outcome (adult body height is less malleable than body weight); we also include children's cohort-federal state fixed effects, which is possible due to the variation in maternal age at birth; we use children with parents from higher school tracks as an additional control group in a difference-in-differences-in-differences model to account for potential nonlinear state-specific trends. We also forgo the comparison to other states and estimate effects based on the idea of a regression discontinuity model in which state trends in children's health behaviours are interrupted by the introduction of the ninth grade for some parents. Finally, we provide

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<sup>13</sup>Note that the IV-estimator collapses to the DiD estimator if the reform increases schooling only in the basic track by one year (first stage equals one for compliers, zero for always-takers, the reform has no never-takers). With  $\lambda$  being the share of parents in the low track, a simple Wald estimator (all other covariates netted out) of  $\alpha_1 = \frac{E(H_i|Z_i^p=1) - E(H_i|Z_i^p=0)}{E(S_i^p|Z_i^p=1) - E(S_i^p|Z_i^p=0)} = \frac{\lambda[E(H_i|Z_i^p=1, \text{basic track}) - E(H_i|Z_i^p=0, \text{basic track})] + (1-\lambda)[E(H_i|Z_i^p=1, \text{higher track}) - E(H_i|Z_i^p=0, \text{higher track})]}{\lambda[E(S_i^p|Z_i^p=1, \text{basic track}) - E(S_i^p|Z_i^p=0, \text{basic track})] + (1-\lambda)[E(S_i^p|Z_i^p=1, \text{higher track}) - E(S_i^p|Z_i^p=0, \text{higher track})]} = \frac{\lambda[E(H_i|Z_i^p=1, \text{basic track}) - E(H_i|Z_i^p=0, \text{basic track})] + (1-\lambda)[0]}{\lambda[E(S_i^p|Z_i^p=1, \text{basic track}) - E(S_i^p|Z_i^p=0, \text{basic track})] + (1-\lambda)[0]} = \frac{[E(H_i|Z_i^p=1, \text{basic track}) - E(H_i|Z_i^p=0, \text{basic track})]}{[E(S_i^p|Z_i^p=1, \text{basic track}) - E(S_i^p|Z_i^p=0, \text{basic track})]} = \frac{\gamma_1}{1}$ , the reduced form DiD estimator in the sample of parents from the basic track.



graphical evidence reassuring that unaccounted trends are not driving the results. All models are estimated with robust standard errors clustered at the state by parental year of birth level.<sup>14</sup>

## 4 Results

### *4.1 The compulsory schooling reform and maternal schooling*

We first analyse the effect of the changes in compulsory schooling on maternal schooling, the first stage in the IV-estimations (Panel A of Table 1). In both samples, with information on children's smoking behaviour and overweight, the introduction of a compulsory ninth grade increases mothers' years of schooling by 0.48 and 0.65 years, respectively. The effect is highly statistically significant with F-statistics above 20. The estimated effects on mothers' years of schooling differ between the samples (although not statistically) because children's overweight is observed in later waves of the German Micro Census than children's smoking behaviour. The data then contains fewer mothers born early in the observation period when the share of students in the basic track was higher. Due to educational expansions in this period, the share of women attending higher school tracks increased continuously. Such general, nationwide, trends in educational attainments are independent of the compulsory schooling reform and accounted for by the empirical model.

Increases in the years of schooling may result from the basic track requirement to stay in school for one more year, or from upgrades of students to higher school tracks that require more years of schooling. If we estimate the reform effect on the probability of completing school with a higher track degree instead of the basic track degree, we find no evidence for reform effects on the highest school degree (Panel B of Table 1). The introduction of a ninth grade increases schooling only through increases in basic track schooling and the

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<sup>14</sup>We draw the same conclusions if we cluster standard errors at the federal state level, accounting for the small number of ten clusters with Wild Cluster Bootstrap procedures (see Cameron, Gelbach, and Miller, 2008, for details and Appendix Table B.5 for results.).

reform effect on mothers' years of schooling reflects the mean share of mothers enrolled in the basic track when the reform was implemented in the federal states.<sup>15</sup>

#### *4.2 Maternal schooling and children's health-related outcomes in adolescence*

We now turn to effects of parental schooling on children's health behaviours and health status. In Panel A of Table 2, we first estimate the relationship between children's smoking behaviour and being overweight in adolescence, and mothers' years of schooling using ordinary least squares regressions, as outlined in eq. 1, finding a strong correlation. Children of mothers with one more year of schooling are about 2.2 percentage points less likely to smoke and 1.9 percentage points less likely to be overweight in adolescence.

To circumvent the potential endogeneity problem in maternal education, we instrument mother's years of schooling using a dummy for the introduction of a ninth grade in the basic track. The IV-estimates in Panel B of Table 2 show that one additional year of mothers' schooling significantly reduces the probability that children smoke (3.8 percentage points) and are overweight (3.6 percentage points). The IV-estimates are larger than the OLS estimates though the OLS estimates fall in the confidence band of the IV-estimates. One reason IV-estimates exceed OLS estimates is that IV-estimates identify local average treatment effects. Increases in maternal education may have particularly large effects at lower levels of parental education (e.g. Imbens and Angrist, 1994).<sup>16</sup>

We may be worried that the IV-estimates capture general trends in children's outcomes

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<sup>15</sup>As we do not observe years of schooling directly in the data, we assign the typical number of school years for different school degrees based on parents' year of birth and the federal state they live in. From 2005 onward, the Micro Census contains information on the year in which individuals completed their latest professional degree. We use this information in the much smaller sample to estimate the effect of the compulsory schooling change on mothers' years of schooling (see Appendix Table B.6). The point estimates on the observed years of schooling are similar to estimates on the imputed number of years of education, but due to the much smaller sample size, it is less precisely estimated.

<sup>16</sup>Bingley and Martinello (2017) suggest that measurement error in years of schooling can upward bias estimates on the returns to education, because years of schooling is a bounded variable and any measurement error is non-classical. In Danish data, he suggests a bias of 38 percent in the IV-estimates on returns to education. Taking the magnitude of the bias at face value in our findings still leads to point estimates that are larger than the OLS estimates (smoking 2.4 percentage points, being overweight 2.2 percentage points).

that are falsely attributed to changes in compulsory schooling of parents. Note that cohort-specific trends are taken into account by a set of dummies for mothers' birth year, children's age and the survey year. They capture changes in child outcomes that relate to general changes in public health expenditures, changes in cigarette prices and taxes, or general smoking bans, for instance. These factors do not vary at the federal state level. Further, the empirical model allows for state-specific trends in mothers' birth year to additionally account for unobserved state-specific trends.

To test whether unaccounted or federal-state specific trends may still drive the results, we make use of the German tracking system. We estimate the reduced form (DiD) effects of changes in compulsory schooling in subsamples stratified by the maternal school track completed (Panel B of Table 2). In the sample with mothers from the basic track, where the compulsory schooling law was binding (and where the first stage equals one), the reduced form estimates are very similar to the IV-estimates, and more precisely estimated for overweight. Mothers from higher school tracks – born in the same birth years and residing in the same states – were not affected by the reform. We use their children as a placebo group and find that the estimated effects are small and insignificant.<sup>17</sup>

To get an idea of relative effect sizes, it is important to determine an appropriate baseline level. Relating the point estimates to the sample mean would be misleading, because of trending outcome variables and substantial differences in health-related outcomes by parental education, i.e. between children of mothers from the basic track and mothers from higher school tracks. Therefore, we calculate the baseline level from counterfactual outcomes of children with mothers from the basic track. One more year of maternal education reduces children's smoking rates by about 17 percent, and the incidence of overweight by about 21 percent.<sup>18</sup> How do these effect sizes relate to the literature?

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<sup>17</sup>We also calculate IV-weights based on the formulas provided in Løken, Mogstad, and Wiswall (2012) to identify the part of the maternal education distribution that is contributing to the linear IV-estimates. The IV-estimators assign 96-99 percent of the marginal effects of mothers' education to mothers with 8 to 9 years of schooling, i.e. to mothers with basic track schooling (see Appendix Table B.7).

<sup>18</sup>The counterfactual mean is obtained from the post-treatment mean for the treatment group minus the treatment effect, i.e.  $E(H_i^0 | Z_i^p = 1, \text{mother from low track}) = E(H_i^1 | Z_i^p = 1, \text{mother from low track}) - \hat{\gamma}_1$

Lundborg et al. (2014) study intergenerational effects of a set of education reforms in Sweden on measures of cognitive and non-cognitive skills of military, male draftees. Even though the absolute magnitude of the effects appears small, the point estimates propose reductions of 33 to 50 percent. However, the estimates are imprecise and incidence of obese individuals in their sample is low at two percent.<sup>19</sup>

In Table 3, we investigate the effects on children’s health behaviours and health status in more detail. We provide the IV-estimates (Panel A) followed by the reduced form effects for children of mothers from basic tracks (Panel B). To immediately check whether the common trend assumption is plausible for this set of outcomes, we report the estimates for children of unaffected mothers below. The reductions in smoking are mainly caused by children who never start smoking, but there are also small effects on quitting rates (only significant in reduced form, see columns 1-2). Moreover, increased maternal schooling mainly reduces regular smoking, but there are also small reductions in occasional smoking (only significant in IV-estimates, see columns 3-4). Conditional on smoking, we find no effects on the age at smoking initiation or on the number of cigarettes smoked (columns 5-6).

With respect to children’s weight problems, the overweight assessment is based on age and gender dependent thresholds for German children (Kromeyer-Hauschild et al., 2015). The context-specific thresholds are important, as regional or gender differences in children’s growth process can alter the overweight assessment. International reference values, as reported in Cole et al. (2000), could be used, but as they are not context-specific, they

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such that  $\delta H_i$  in % =  $\hat{\gamma}_1/[E(H_i^0|Z_i^p = 1, \text{mother from low track})]$ . For smoking,  $-17\% = -0.0412/[0.20 - (-0.0412)]$ ; for overweight,  $-21\% = -0.0359/[0.13 - (-0.0359)]$ .

<sup>19</sup>Other studies on the subject by Lindeboom et al. (2009) and Carneiro et al. (2013) also consider children’s weight problems but cannot identify effects. One reason may be limitations in the identification strategy: Lindeboom et al. (2009) exploit the UK’s 1947 cohort increase in the minimum school leaving age with a regression discontinuity design. This approach needs to rely on assumptions on general cohort trends, with RD-estimates likely underestimating the effects of the minimum schooling policy (Lochner, 2011). Carneiro et al. (2013) estimate effects of maternal education on a large number of child outcomes at ages 7-8 and 12-14. They instrument maternal schooling with regional and family characteristics that alter individuals’ costs of schooling. The instruments are rather weak and result in partially imprecise estimates, such that some sizeable effects on being overweight (e.g. white children aged 7-8) only turn significant in robustness checks.

may be more noisy. Indeed, using international reference values results in effects similar in magnitude, but they are less precisely estimated (column 7). If we estimate effects on children’s obesity (BMI above the 97th percentile, based on Kromeyer-Hauschild et al., 2015, see column 8), the reduced form point estimate is also substantial if compared to the sample mean, but insignificant. The data may not provide sufficient power to identify effects in the extreme parts of the distribution. To make best use of the (context-*independent*) BMI information, we estimate effects along the distribution of children’s BMI (see Figure A.2).<sup>20</sup> Maternal schooling mainly affects the upper quartile of the BMI distribution. There are no effects on lower parts.

Who benefits the most from increases in maternal schooling? In Table 4, we report estimates of treatment effect heterogeneity, interacting the treatment dummy with heterogeneity dimensions.<sup>21</sup> Boys typically react more strongly to changes in early childhood conditions (e.g. Waldfogel, 2006), which may also relate to changes in maternal schooling. This is particularly true for children from lower socio-economic backgrounds (e.g. Autor, Figlio, Karbownik, Roth, and Wasserman, 2016). The increase in maternal compulsory schooling reduces girls’ and boys’ smoking probability, but the reduction is stronger for boys. With respect to being overweight, the effect on girls is slightly larger but not statistically different from the effect on boys. We report further effect heterogeneities by family income, for children from single mothers and both parents, from smoking and non-smoking mothers, as well as from overweight and non-overweight mothers, but there are no significant differences between these groups. If at all, the effects appear larger for children from single mothers and mothers who do not smoke.

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<sup>20</sup>This analysis estimates the model from eq. 3 within a recentered-influence-function approach (RIF-DiD) at each BMI quantile. The method was suggested by Firpo, Fortin, and Lemieux (2009), and applied in, e.g., Havnes and Mogstad (2015).

<sup>21</sup>The estimations also include the respective group dummy. Family income, single motherhood, mothers’ smoking behaviour and overweight must not be affected by the compulsory schooling reform for unbiased effect estimates on child outcomes. We provide evidence for this in Section 4.5.

#### *4.3 Paternal schooling and children's health-related outcomes in adolescence*

Does paternal schooling also improve offspring's health outcomes? We turn to this question in Table 5, analysing children whose fathers were born between 1930 and 1960.

Column 1 reports the reform effect estimate of the introduction of a ninth grade on paternal years of schooling. In both samples on children's outcomes, the estimated coefficients suggest an increase of 0.53-0.56 years. As for mothers, there is no evidence for reform effects on the highest completed school track of fathers (column 2). While OLS-estimates suggest that paternal schooling is associated with about 1.8 percentage points reduced smoking and being overweight (column 3), IV-estimates are much smaller, swap the sign of the relationship and turn insignificant (column 4). The reduced form effect of the introduction of a ninth grade in the subsample of children with fathers from the (treated) basic school track produces similar result (column 5). Effects in the subsample of children with untreated fathers from higher tracks are expectedly close to zero and insignificant (column 6).<sup>22</sup>

Within compulsory schooling changes, the limited impact of paternal schooling on children's outcomes is documented in other contexts as well. For example, Holmlund et al. (2011) and Lundborg et al. (2014) do not find evidence of effects on children's schooling and general health in adolescence, respectively, if fathers are affected by schooling reforms. However, they also find effects of maternal schooling. Our finding may be rationalised by the observation that mothers are the primary caregiver of children.

#### *4.4 Parental schooling and children's health-related outcomes in adulthood*

Both smoking and being overweight in adolescence are highly predictive of related unhealthy behaviours and chronic conditions later in life (e.g. Serdula et al., 1993; Guo et al., 2002; Jürges and Meyer, 2017). Therefore, improvements in adolescent health behaviours

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<sup>22</sup>We also check for heterogeneous effects of paternal schooling on girls and boys, but cannot find any evidence.

and health status should persist into adulthood, eventually resulting in better long-term health. In order to examine the long-term effects of parental education, we draw a sample of individuals aged 30 to 50 from the German SOEP data.<sup>23</sup> We employ the same difference-in-differences strategy as outlined in eq. 3 and again restrict the sample to children of parents born between 1930 and 1960. We report the estimated reduced form effects on treated children, and contrast them with the placebo estimates on children of untreated parents from higher tracks. Individuals providing the necessary information at different ages are included repeatedly to increase the sample size and the precision of the estimates. Inference is based on robust standard errors clustered at the state by parental year of birth level.

The results are reported in Table 6. Panel A shows that adult children are significantly less likely to smoke if their mother was affected by an increase in compulsory schooling (column 1), mostly because children never initiated smoking (columns 2-3, based on a smaller sample as information on quitting and never-smoking is only available in fewer waves). Children also have a lower BMI and a lower probability of being overweight (columns 4-5). As smoking and being overweight can cause severe chronic conditions, we test whether children of treated mothers are eventually less likely to suffer from chronic conditions later in life. We find evidence of a significant reduction (column 6). Children also report an improved general health status (column 7).<sup>24</sup> There are no such effects in the placebo sample of children with mothers from higher school tracks. As with adolescents, there are also no effects of paternal increases in compulsory schooling on children’s health-related outcomes (see Panel B of Table 6). The results show that health-related behaviours are already determined early in life by maternal schooling. It also shows that increased

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<sup>23</sup>To maximise the sample size, we use children-reported information on parental birth year and highest level of education if it is not reported by parents themselves. We restrict the sample to individuals below age 50 in order to reduce the risk of endogenous sample selection that may result from maternal schooling effects on children’s longevity.

<sup>24</sup>The outcome “chronic condition” is based on the survey question “Have you been suffering from any conditions or illnesses for at least one year or chronically?”; the outcome “general health” is based on “How would you describe your current health?” (measured in five categories ranging from very good to bad, rescaled such that higher values indicate better outcomes). “General health” is standardised to a mean of zero and a standard deviation of one.

maternal schooling reduces the socio-economic gap in health conditions.

#### *4.5 Potential channels*

What may explain the substantial effects of increases in maternal schooling on children's health behaviours and health status? We aim at shedding some light on potential channels by analysing compulsory schooling effects on changes in children's human capital, their peer environment and family characteristics (family income, mating, family stability, fertility, as well as parental health status and health behaviours).

**Children's human capital.** We build on the theoretical framework by Cunha and Heckman (2007) and Heckman (2007) on the dynamic formation of human capital. In this framework, human capital is multidimensional and results from a multistage production technology. Improvements in earlier human capital, e.g. in the form of better health at birth, foster the development of later human capital (self-productivity). Moreover, increased human capital in early stages of life make investments in later periods more productive (dynamic complementarities). Currie and Moretti (2003) show that increases in maternal education in the US (caused by opening new colleges) improve prenatal care, lower smoking during pregnancy, increase gestational age, and reduce the risk of low birth weights. Further, Chou et al. (2010) show that increases in maternal education lower the incidence of low birth weights and infant mortality in Taiwan.<sup>25</sup> This may immediately reduce the inherent disadvantage that children are born with. Behrman and Rosenzweig (2004) and Black et al. (2007) show that low birth weights affect outcomes in adulthood, resulting in lower educational attainments and lower earnings. Case et al. (2005) show that poor health during childhood is associated with lower educational attainment, poorer health, and lower social status in adulthood.

Consequently, improvements in early health may improve other human capital dimensions.

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<sup>25</sup>McCrary and Royer (2011) cannot find such effects using school entry policies as an instrument for schooling. However, the authors cannot rule out that maternal schooling effects on infant health are heterogeneous and not captured by their instrument.



With dynamic complementarities, healthier children may benefit more from schooling. Indeed, numerous studies provide evidence that increases in maternal education increase the educational attainments of their children (e.g. Oreopoulos et al., 2006; Maurin and McNally, 2008; de Haan, 2011; Holmlund et al., 2011; Chevalier, Harmon, O' Sullivan, and Walker, 2013; Piopiunik, 2014). Increases in children's educational attainments, in turn, improve cognition (helping them process health-related information) and earnings prospects. These factors increase the costs of engaging in unhealthy behaviours, whose reductions ultimately improve long-term health (e.g. Grossman, 2006; Cutler and Lleras-Muney, 2010).

To test whether maternal schooling affects children's human capital in our data, we analyse children's educational attainment as one important dimension of it. We observe whether adolescents attend the basic, middle or high school track.<sup>26</sup> The school track is highly correlated with children's cognitive capabilities and earnings prospects: Dustmann and Schönberg (2011) report that the PISA scores in reading and mathematics of middle (high) track students are about 0.6 (1.5) standard deviations higher than those of children in the low track. They earn 24 (49) percent higher wages.

Indeed, the probability that children attend a higher track increases significantly if mothers are affected by the increase in compulsory schooling, mainly because children are more likely to attend the middle track rather than the low track (Panel A of Table 7, about 11 percent). These results are in line with Piopiunik (2014), who finds in data from the German SOEP that the increase in mothers' compulsory schooling in Germany improves children's school track.

**Children's peer environment.** Improvements in children's school track also improve the peer environment. Children in Germany are ability-tracked into physically separated schools as early as age ten. The differences in health behaviours and human capital of the

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<sup>26</sup>Due to data limitations in early waves of the German Micro Census, we only reliably observe the (completed) school track at age 17 and 18, when children either completed schooling or are still enrolled to the final grades of academic track schools. We restrict the sample accordingly.

peer environment are substantial: Compared to children in the basic track, children from higher tracks are 46 percent less likely to smoke, 50 percent less likely to be overweight, and 2.6 times more likely to have parents with a university entrance qualification (based on the German Micro Census). To estimate the effects of maternal schooling on the peer environment, we define peers to be children born in the same year attending the same school track in the federal state. We find that maternal schooling reduces the probability that peers smoke and are overweight; parents of the peers are also better educated (Panel B of Table 7).

These improvements in adolescents' peer environment may play an important role in developing health-related behaviours. With respect to smoking, the large majority of individuals who smoke initiate smoking while they are still in school. Most of the educational gradient in smoking already exists before compulsory education is completed (Jürges and Meyer, 2017). Powell, Tauras, and Ross (2005) show that school-level smoking rates play an important role for smoking initiation of adolescents. Moving students from a non-smoking school to a school where 25 percent of students smoke increases their smoking probability by about 14.5 percentage points, *ceteris paribus*. Lundborg (2006) also provides evidence for substantial school-peer effects in smoking, binge drinking, and drug use. Peer effects are also identified for weight problems (Trogon, Nonnemaker, and Pais, 2008; Carrell, Hoekstra, and West, 2011) and related behaviours such as sports, exercise, and unhealthy diets (Ali, Amialchuk, and Heiland, 2011). Consequently, the tracking system may amplify improvements in early human capital and schooling resulting from increases in maternal schooling.

**Family income and mating.**<sup>27</sup> Numerous studies document a strong family income gradient in child health-related behaviours (e.g. Soteriades and DiFranza, 2003) and health (e.g. Case et al., 2002; Currie and Stabile, 2003). Higher family income allows parents to purchase better health services for their children as well as to invest in healthier lifestyles

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<sup>27</sup>The following analyses include mothers with basic track schooling of all children aged 18 or younger to increase the sample size and the precision of the estimates.

or safer and healthier environments. However, increased income may be less important in countries with almost universal health care that is of low private cost. Reinhold and Jürges (2012) find only weak evidence for a causal effect of parental income on child health in Germany, Kuehnle (2014) finds only small effects for the UK.

To investigate the role of the family income channel in our setting, we first estimate reduced form effects on maternal labour market outcomes (Panel C of Table 7). Affected mothers have a 1.5 percentage points higher probability to work and wages increase by 0.5 percent.<sup>28</sup> These small effects are in line with the zero-returns to compulsory schooling in Pischke and von Wachter (2008) and Kamhöfer and Schmitz (2016). The authors argue that basic track students already learned labour market-relevant skills earlier.<sup>29</sup>

In Panel D, we examine effects on maternal mating. Treated mothers mate slightly older men with 0.23 more years of schooling. The increase in partners' years of schooling is largely related to partners also being affected by changes in compulsory schooling. The reform has no impact on the highest school degree of the partner, on the employment probability, or on earnings. The absence of labour market returns is not surprising given that the increase in partners' schooling stems from increases in compulsory schooling that did not generate significant labour market returns. Note, however, that the estimates are only informative on changes in family income, but not on the allocation of income. Affected mothers could still allocate more family income towards health-related inputs (Grossman, 1972). For example, mothers may provide healthier food for their children, invest in physical activity, or move to better neighbourhoods.

**Family stability and fertility.** Family disruptions can cause stress and make chil-

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<sup>28</sup>For the calculation of maternal log hourly wages, we divide the net monthly income by the weekly working hours times 4.3, as in Pischke and von Wachter (2008). The log wage of non-working mothers is set to zero.

<sup>29</sup>Cygan-Rehm (2018) suggests several sample restrictions that lead to positive and significant estimates, mainly for men, on the returns to compulsory schooling in Germany in other data. Cygan-Rehm excludes parents of pivotal reform cohorts, and cohorts born before 1945, as well as controls for the short school years as analysed in Pischke (2007). Applying these restrictions to the German Micro Census data does not lead to robust estimates on earnings in the Micro Census data. The main findings of this paper on children's health-related outcomes of children are robust to these suggested sample restrictions.

dren more likely to initiate smoking (Francesconi et al., 2010) or to become overweight (Schmeer, 2012). We find no evidence for a reform effect on the probability of living with a single mother or of being married (Panel E). Another potential explanation relates to fertility effects of education. If more schooling reduces the number of children or increases the age at birth, more resources may be allocated to a child (e.g., Hanushek, 1992). We find some evidence for effects on the number of children living in the household, pointing to fertility effects of the compulsory schooling reform. The effect estimate on maternal age when giving birth is slightly positive, but insignificant. The fertility effects are consistent with McCrary and Royer (2011), Cygan-Rehm and Maeder (2013), and Lundborg et al. (2014). The effects are also consistent with the quantity-quality trade-off theory. According to this theory, better educated parents invest more resources in the human capital of fewer children.<sup>30</sup>

**Parental health status and health behaviours.** Another explanation for children's health is seen in the health status and health-related behaviours of parents who may serve as role models (e.g. Powell and Chaloupka, 2005; Loureiro, Sanz-De-Galdeano, and Vuri, 2010; Göhlmann et al., 2010). In Panel F, we analyse the effects on the mother's own smoking behaviour and that of her partner, and cannot find any evidence. Potentially, this is because the introduction of a ninth school year affected children at a time when they had already initiated smoking. Moreover, most cohorts may have started smoking before the dangers of smoking became publicly recognized following the 1964 reports of the US Surgeon General on the harms of smoking (Lochner, 2011). The findings correspond to Kemptner et al. (2011), who evaluate the direct health effects of the reform. With respect to parental weight problems, we do not find evidence for effects on mothers' BMI and being overweight, but there is a reduction in the probability that the partner is

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<sup>30</sup>Substantial effects of education on fertility may induce an endogeneity problem, as selection into motherhood may confound the sample. This feature is shared by nearly all studies on intergenerational effects of schooling that also document effects on fertility as in, e.g., Carneiro et al. (2013) and Lundborg et al. (2014). Reviewing the literature on the causal effects of education and fertility suggests that education affects, if at all, mainly the intensive fertility margin (e.g. McCrary and Royer, 2011; Fort, Schneeweis, and Winter-Ebmer, 2016) preventing our analysis from biases due to endogenous sample selection in parental characteristics.

overweight (Panel G).

In sum, the effects of increased maternal schooling on children’s smoking and being overweight are best explained by taking a dynamic perspective on human capital formation, including self-productivity of human capital and dynamic complementarity. Parents invest more in children’s human capital (likely already improving early health outcomes), which improves children’s schooling attainments. Resulting better cognition and earnings prospects may increase the costs of unhealthy behaviours. The effects are amplified by improvements in the peer environment at a sensitive developmental period of children. Improvements in the family environment (i.e. in terms of family income, mating, family stability, household size, and parental health behaviours) appear less important empirically. One explanation may be that the employed measures are not differentiated enough. For example, despite only small effects on parental health behaviours, parents may still be more aware of the consequences of unhealthy behaviours, thus encouraging children’s physical activity, improving children’s diet, or imposing smoking rules at home (Powell and Chaloupka, 2005). Additionally, the parent-child relationship may have changed, which can also impact the formation of health behaviours (Powell and Chaloupka, 2005).

## 5 Robustness checks

### *5.1 Identification assumptions*

At the heart of the identification strategies is the common trend assumption: States that introduced the compulsory schooling reform would have developed similarly over time with respect to children’s outcomes as states that did not (yet) increase compulsory schooling of the parents.

Throughout the analysis, we demonstrate that children’s smoking behaviour and overweight only improve if mothers indeed attended the affected basic school track, assuring that the model is not just capturing general trends that affect other children as well. In

alternative checks on the common trend assumption, we simulate placebo reforms (Panel A of Table 8). We drop children with treated mothers from the sample and assume that the compulsory schooling reform was implemented two to five years before the actual reform.<sup>31</sup> The coefficient estimates vary around zero with changes in their sign and are statistically insignificant. The smaller sample size, however, increases the noise in the estimates.

Alternatively, we check for effects on a health-related placebo outcome (Panel B of Table 8): Body height in adulthood is largely determined by genetic factors in high-income countries (Silventoinen, 2003).<sup>32</sup> We estimate the effect on adult children in the SOEP data (as in Section 4.4) for children with mothers from the basic track. The estimate is very small and insignificant, suggesting that overweight effects relate arise from changes in more malleable body weight rather than mostly genetically determined body height.

As the common trend assumption is conditional on covariates, we assess the sensitivity of the estimates to varying sets of covariates (columns 2-7 of Table 9): We first drop the X-variables (gender, children's age, survey wave); we add controls for states' GDP and unemployment rate when children are aged 18 to account for differential state trends; we include two dummies in the main specification to indicate cohorts that were exposed to short/long school years when the national school calendar was harmonized around 1966 (for reform details, see Pischke, 2007); we include two dummies for cohorts potentially affected by region-specific and temporary school year increases before the general increase in compulsory schooling was mandated (see Section 2 for details); we drop the linear state-specific trend in mothers' year of birth; and finally we flexibly account for cohort - state specific trends by including fixed effects at the children's birth year - federal state level instead of parametric trends. The last specification is very flexible and possible due to variations in maternal age at birth. It can account for any cohort-state-specific change

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<sup>31</sup>A placebo reform in the year preceding the actual reform may be confounded by grade repeaters and late school entry.

<sup>32</sup>Smoking may stunt growth in adolescence (e.g. Stice and Martinez, 2005).

that may arise from state-specific smoking regulations, education policies, overweight campaigns, or regionally varying cohort specific labour market conditions. Across all specifications, the IV-estimates and reduced form estimates are very stable, and none of the estimated coefficients are statistically different from the main estimates.

In another set of tests on the common trend assumption, we employ models that rest on alternative identification assumptions. First, we use the idea of a regression discontinuity design (column 8): We centre the sample 15 years before and after the respective reforms and allow for a linear trend in the maternal year of birth for children's outcomes in each federal state. This trend is allowed to jump with the increase in mothers' compulsory schooling. The resulting IV-estimates are similar to the main effects. Reduced form estimates are smaller, but in the confidence band of the main estimates. Note, however, that regression discontinuity designs likely underestimate the long-term benefits of education if outcomes are impacted by spill-overs from other cohorts (which is likely the case for health-related outcomes, see Lochner, 2011).

Alternatively, we use children with mothers from higher tracks as an additional control group in a difference-in-differences-in-differences model. This model accounts flexibly for any state-specific shock that is related to maternal birth year but unrelated to the school track of the mother. The estimates are very similar to the main results (column 9).<sup>33</sup>

Finally, we check on omitted trends graphically. We calculate residuals from the difference-in-differences models with the treatment dummy added back in. Figure A.3 plots average residuals by the distance of mothers' cohorts to the compulsory schooling reform in their federal state. Systematic time trends that were not captured by the difference-in-differences model would be revealed in the residuals. This is not the case: Before the introduction of the reform, the residuals vary around zero. After the reform, they are constantly below zero for children of mothers from the basic track. In the placebo sample

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<sup>33</sup>IV-estimates are identical to the reduced form estimates because maternal years of schooling are generated based on their school degree, such that the first stage in the sample of basic-track mothers is one, and zero in the sample of mothers from higher tracks.

of children with mothers from higher tracks, the residuals continuously vary around zero.

### *5.2 Sample choices and weighting*

We now perform checks on the sensitivity of the main results to changes in sample choice (see Appendix Table B.8). While we restrict the main sample to children of mothers born between 1930 and 1960, we could also centre the sample around the reforms, as employed by, e.g., Brunello, Fort, and Weber (2009). In columns 2-5 of Appendix Table B.8, we include children of mothers born 15 years before and after the increase in compulsory schooling, and gradually narrow the window down to 9 years. We obtain similar estimates.

In the analysis, we need to assume that parents went to school in the state that they currently live in. Cross-state mobility is generally low in Germany: 85 percent of adults in their 40s are still living in the same state where they went to school. This number is smaller in city-states (Hamburg, 63 percent, and Bremen, 70 percent, for details, see Section 3.1). Removing observations from these small states yields expectedly similar results (column 6). Furthermore, a lagged roll-out of the programme within certain regions of the federal states, as well as early school entry of parents may introduce some fuzziness in the treatment assignment around the reform introduction. Dropping maternal cohorts that should have been treated first results in the same conclusions (column 7). Finally, we want to rule-out that the results on adolescents are not biased by children moving out from home at age 18. If we drop children aged 18 from the analysis (about 25 percent of the sample), the main findings are confirmed, although the precision of estimates is expectedly lower.

The data on children's smoking behaviour (overweight) was only collected in the German Micro Census from 1989 (1999) onward. Therefore, it is more likely to observe more recent cohorts of mothers than older cohorts. In Panel A of Appendix Figure A.4, we plot the frequency with which mothers' birth cohorts appear in the main sample, and the frequency of all female births contained in the Micro Census 1989. Similarly, the overweight-sample over-represents mothers giving birth at older ages (Panel B of Appendix Figure A.4,



based on the full sample from the German Micro Census of children aged 0-18 living with their parents). We run the main analysis using inverse probability weights to match the frequencies on mothers' year of birth and age at birth as plotted in Appendix Figure A.4 (see columns 9-10 of Appendix Table B.8). Assigning higher weights to cohorts with few observations (and less informational content) and lower weights to cohorts with more observations increases the noise in some estimates. Still, the results are statistically equivalent to the main findings.

## 6 Conclusion

This paper traces the effects of an increase in compulsory schooling in Germany on children's health behaviours and long-term health. Mothers' increase in schooling substantially reduces children's probability to smoke and to be overweight in adolescence and adulthood. The findings pass a large set of robustness and placebo checks. For fathers, we do not find such effects. The effects on children's health-related outcomes are likely a product of a multiplicity of factors that improve throughout childhood, such as early childhood health, better schooling attainment, and a better peer environment in adolescence. Improvements in the family environment, including family income, mating, family stability, household size, and parental health behaviours, appear empirically less relevant overall. In a dynamic framework of human capital formation, even small changes in these dimensions may contribute to substantial improvements in health behaviours and long-term human capital of children.

While this study establishes the causal link between parental education on children's health outcomes, the analysis also has some limitations. The underlying mechanisms deserve further research attention. An exciting, though challenging, avenue for future research is to better understand *how* parental education impacts children's health. A major challenge will be to acquire detailed information on, e.g., parental inputs around birth and throughout childhood, or on child-parent relationships. One should also bear

in mind that the results apply to children of parents with rather low levels of education (although more than half of the parents attended this track). If we expect the benefits of schooling to be larger for lower levels of education (Imbens and Angrist, 1994), the estimates should be interpreted as an upper-bound. Future research should try to identify effects in higher parts of the parental education distribution.

The paper contributes to our understanding of the link between education and health, suggesting that a substantial portion of the causal relationship is overlooked if inter-generational effects are not considered. The results show that the impact of maternal education on health behaviours and health is an important mechanism through which economic status is transmitted. The paper significantly contributes to our understanding of non-market benefits of education. While the literature cannot typically disentangle the effects of education from effects of higher incomes, we provide evidence from a setting in which the income channel is mostly closed. The paper also improves our understanding of long-term effects of educational interventions that strengthen the case for public investments in general education. Finally, the long-term impact of educational investments may eventually lead to reductions in public health expenditures.

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Table 1: Reform effects on mothers' schooling

Independent variable	Sample	
	Currently smoking (1)	Overweight (2)
<i>Dep. variable: Mother's years of schooling (imp.)</i>		
Cohort with 9th grade in basic track	0.6454*** (0.0424)	0.4833*** (0.1051)
F-test: instrument=0	231.21	21.16
<i>Dep. variable: Mother with middle/high track schooling instead of basic track schooling</i>		
Cohort with 9th grade in basic track	0.0064 (0.0117)	-0.0000 (0.0222)
Sample mean	0.46	0.57
Number of observations	27,339	12,853

*Notes:* All OLS regressions are based on the DiD model of eq. 3 and also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 2: Effects of mothers' schooling on children's health-related outcomes

Independent variable	Dependent variable (age 15-18)	
	Child smokes (1)	Child is overweight (2)
<i>Panel A: Full sample</i>		
<i>OLS estimates</i>		
Mothers' years of schooling (OLS)	-0.0216*** (0.0018)	-0.0187*** (0.0015)
Number of observations	27,339	12,853
<i>Instrumental variable estimates (IV)</i>		
Mothers' years of schooling (IV)	-0.0384*** (0.0127)	-0.0360* (0.0211)
Number of observations	27,339	12,853
<i>Panel B: Reduced form estimates (DiD)</i>		
<i>Subsample: Mothers from basic track</i>		
Mother's cohort with 9th grade in basic track	-0.0412*** (0.0137)	-0.0359** (0.0172)
Sample mean	0.18	0.13
Number of observations	14,799	5,497
<i>Subsample: Mothers from middle/high tracks</i>		
Mother's cohort with 9th grade in basic track	0.0016 (0.0136)	-0.0084 (0.0121)
Sample mean	0.12	0.07
Number of observations	12,540	7,356

*Notes:* All regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 3: Further reform effect estimates on children's health-related outcomes

Independent variable	Dependent variable related to							
	Smoking behaviour				Weight problems			
	Never smoked (1)	Quitted smoking (2)	Smokes regularly (3)	Smokes occasionally (4)	Smoking starting age (5)	No. of cigarettes (6)	Overweight (international thresholds) (7)	Child is obese (8)
Mothers' years of schooling (IV)	0.0384*** (0.0127)	0.0057 (0.0037)	-0.0236* (0.0132)	-0.0148* (0.0085)	0.0847 (0.1266)	0.2761 (0.5694)	-0.0406 (0.0322)	-0.0048 (0.0112)
Number of observations	27,339	27,339	27,339	27,339	4,223	4,293	12,853	12,853
<i>Instrumental variable estimates (IV)</i>								
Mothers' cohort with 9th grade	0.0412*** (0.0137)	0.0086*** (0.0032)	-0.0283*** (0.0119)	-0.0129 (0.0080)	0.0006 (0.1301)	0.4219 (0.5560)	-0.0361 (0.0236)	-0.0092 (0.0105)
Sample mean	0.82	0.01	0.14	0.04	15.58	10.69	0.17	0.05
Number of observations	14,799	14,799	14,799	14,799	2,673	2,727	5,497	5,497
<i>Reduced form estimates (DiD)</i>								
<i>Subsample: Mothers from basic track</i>								
Mothers' cohort with 9th grade	-0.0016 (0.0136)	-0.0034 (0.0049)	0.0058 (0.0129)	-0.0042 (0.0061)	0.1910 (0.1390)	0.1857 (0.9154)	-0.0108 (0.0174)	0.0032 (0.0062)
Sample mean	0.88	0.01	0.09	0.04	15.56	9.18	0.10	0.02
Number of observations	12,540	12,540	12,540	12,540	1,550	1,566	7,356	7,356
<i>Subsample: Mothers from middle/high tracks</i>								

*Notes:* All regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .  
*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 4: Heterogeneity analysis of mothers' schooling effects

Independent variable	Dependent variable:			
	Currently smoking		Overweight	
	Coefficient	s.e.	Coefficient	s.e.
	By gender			
Mother's cohort with 9th grade · female	-0.0259*	(0.0148)	-0.0444**	(0.0192)
Mother's cohort with 9th grade · male	-0.0554***	(0.0156)	-0.0276	(0.0195)
P-value for group difference	0.03		0.34	
	By household income			
Mother's cohort with 9th grade	-0.0375***	(0.0137)	-0.0429**	(0.0176)
Mother's cohort with 9th grade · household income (in TEUR)	0.0016	(0.0057)	-0.0102	(0.0081)
	By single mother status			
Mother's cohort with 9th grade · single mother	-0.0493**	(0.0236)	-0.0623*	(0.0353)
Mother's cohort with 9th grade · both parents	-0.0386***	(0.0139)	-0.0320*	(0.0180)
P-value for group difference	0.63		0.40	
	By mother's smoking behaviour			
Mother's cohort with 9th grade · smoking	-0.0343*	(0.0186)	-0.0219	(0.0250)
Mother's cohort with 9th grade · non-smoking	-0.0406***	(0.0138)	-0.0363*	(0.0184)
P-value for group difference	0.70		0.53	
	By mother's overweight			
Mother's cohort with 9th grade · overweight	-0.0395***	(0.0143)	-0.0552**	(0.0229)
Mother's cohort with 9th grade · not overweight	-0.0496**	(0.0217)	-0.0283	(0.0186)
P-value for group difference	0.62		0.29	

*Notes:* The table reports reduced form estimates based on the DiD model of eq. 3 and the sample of children with mothers from the basic track. Effect heterogeneity of household income is evaluated at the mean income. The OLS regressions also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year, a quartic in mothers' age, and the interaction variable. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. Mothers' smoking behaviour is missing for 29 observations. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 5: Effects of fathers' schooling on children's health-related outcomes

Independent variable	Dependent variable:					
	Father with middle/high track schooling		Children's health behaviour (sample-dependent)			
	All fathers (1)	All fathers (2)	All fathers (3)	All fathers (4)	Only basic track (5)	Only high tracks (6)
	<i>Subsample by fathers' education:</i>					
	<i>Sample: Child is smoking</i>					
Father's cohort with 9th grade (DiD)	0.5636*** (0.0466)	-0.0035 (0.0127)			0.0137 (0.0154)	0.0045 (0.0099)
Fathers' years of schooling (OLS)			-0.0181*** (0.0014)			
Fathers' years of schooling (IV)				0.0126 (0.0173)		
F-test: instrument=0	146.14			146.14		
Number of observations	26,991	26,991	26,991	26,991	15,000	11,991
	<i>Sample: Child is overweight</i>					
Father's cohort with 9th grade (DiD)	0.5271*** (0.0731)	0.0061 (0.0187)			0.0121 (0.0164)	-0.0045 (0.0109)
Fathers' years of schooling (OLS)			-0.0177*** (0.0014)			
Fathers' years of schooling (IV)				0.0065 (0.0204)		
F-test: instrument=0	51.97			51.97		
Number of observations	14,137	14,137	14,137	14,137	6,892	7,245

*Notes:* All regressions also include the full set of fathers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in fathers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  fathers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table 6: Effects of parental schooling on children's health-related outcomes at age 30-50

Independent variable	Dependent variable measured at age 30-50:						
	Currently smoking (1)	Quitted smoking (2)	Never smoked (3)	BMI (4)	Overweight (BMI>25) (5)	Chronic condition (6)	General health (z-score) (7)
<i>Panel A: Effects of mothers' schooling</i>							
<i>Sample: Mothers from basic track</i>							
Mother's cohort with 9th grade	-0.0905** (0.0383)	0.0436 (0.0347)	0.0802* (0.0442)	-0.8893** (0.3803)	-0.0606* (0.0335)	-0.0677*** (0.0231)	0.0960* (0.0528)
Sample mean	0.35	0.19	0.39	25.92	0.52	0.31	-0.05
Number of person-year obs.	27,901	10,991	10,991	21,320	21,320	21,180	65,845
Number of individuals	8,035	5,238	5,238	7,421	7,421	6,826	9,572
<i>Sample: Mothers from middle/high tracks</i>							
Mother's cohort with 9th grade	0.0279 (0.0361)	-0.0285 (0.0525)	0.0046 (0.0662)	0.4826 (0.4462)	0.0199 (0.0435)	0.0214 (0.0291)	0.0197 (0.0573)
Sample mean	0.27	0.20	0.46	24.92	0.42	0.29	0.11
Number of person-year obs.	9,014	3,024	3,024	7,553	7,553	8,624	21,992
Number of individuals	3,136	1,675	1,675	2,997	2,997	2,841	3,766
<i>Panel B: Effects of fathers' schooling</i>							
<i>Sample: Fathers from basic track</i>							
Father's cohort with 9th grade	-0.0047 (0.0410)	0.0001 (0.0504)	0.0321 (0.0564)	0.0629 (0.4265)	0.0266 (0.0381)	0.0139 (0.0280)	-0.0273 (0.0591)
Sample mean	0.36	0.18	0.40	26.00	0.52	0.31	-0.06
Number of person-year obs.	22,709	8,704	8,704	17,603	17,603	17,592	52,757
Number of individuals	6,650	4,211	4,211	6,175	6,175	5,733	7,777
<i>Sample: Fathers from middle/high tracks</i>							
Father's cohort with 9th grade	0.0265 (0.0475)	0.0273 (0.0525)	-0.0163 (0.0822)	0.1218 (0.4705)	-0.0519 (0.0457)	0.0183 (0.0361)	0.0573 (0.0617)
Sample mean	0.28	0.21	0.44	24.95	0.42	0.29	0.13
Number of person-year obs.	8,824	2,942	2,942	7,448	7,448	8,495	20,925
Number of individuals	3,059	1,643	1,643	2,933	2,933	2,794	3,575

*Notes:* The table reports reduced form estimates based on the DiD model of eq. 3. All OLS regressions include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, and dummies for the survey year. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* SOEP v32long, own calculations.

Table 7: Effects of mothers' schooling on children's human capital, peer environment, family characteristics and parents' health-related outcomes

Dependent variable	Sample mean (1)	Reduced form effect (2)	s.e. (3)	Number of observations (4)
<i>Panel A: Children's educational attainment</i>				
Child attends middle/high track	0.56	0.0376***	(0.0144)	16,081
Child attends middle track	0.29	0.0312*	(0.0163)	16,081
Child attends high track	0.27	0.0064	(0.0163)	16,081
<i>Panel B: Children's peer environment</i>				
Peer students smoke	0.25	-0.0129***	(0.0049)	16,081
Peer students are overweight	0.10	-0.0116***	(0.0036)	16,081
Peer students' average maternal schooling	9.25	0.0719***	(0.0161)	16,081
Peer students' average paternal schooling	9.32	0.0479**	(0.0216)	16,081
<i>Panel C: Mothers' labour market outcomes</i>				
Mother works	0.48	0.0158	(0.0107)	70,477
Mother's log hourly wage	1.49	0.0058	(0.0124)	70,477
<i>Panel C: Assortative mating</i>				
Father's age in years	43.96	0.1555*	(0.0828)	62,811
Father's years of schooling	8.99	0.2324***	(0.0346)	62,811
Father's from middle/high track	0.22	0.0081	(0.0095)	62,811
Father works	0.91	-0.0039	(0.0075)	62,811
Father's log hourly wage	2.36	0.0112	(0.0156)	62,811
<i>Panel E: Family characteristics</i>				
Single mother	0.13	-0.0065	(0.0063)	70,477
Mother is married	0.88	0.0040	(0.0063)	70,477
Number of children in HH	2.14	-0.0467*	(0.0273)	70,477
Mother's age at child birth	29.93	0.0639	(0.0670)	70,477
<i>Panel F: Parents' smoking behaviour</i>				
Mother smokes	0.35	0.0061	(0.0139)	36,462
Mother has never smoked	0.49	-0.0080	(0.0150)	36,462
Father smokes	0.40	0.0080	(0.0160)	32,597
Father has never smoked	0.35	0.0172	(0.0174)	32,597
At least one parent smokes	0.51	0.0084	(0.0172)	38,117
Parents have never smoked	0.28	0.0064	(0.0151)	36,462
<i>Panel G: Parents' overweight</i>				
Mother's BMI	25.08	0.1178	(0.3227)	10,723
Mother is overweight	0.43	0.0162	(0.0386)	10,723
Father's BMI	27.16	-0.2069	(0.2362)	9,280
Father is overweight	0.70	-0.0773***	(0.0251)	9,280

*Notes:* The table reports the reduced form (DiD) coefficients based on eq. 3 and focuses on (children of) mothers' from the basic track. Each coefficient is estimated in a separate regression. Analyses of Panels A and B are based on a sample children aged between 17 and 18 with mothers from the basic track (the school track of younger children cannot be observed due to data restrictions); the dependent variables in Panel B refer to school track-state-birth year specific characteristics obtained from the German Micro Census. Analyses in Panels C-G are based on parents of children aged 18 and younger. Fathers' outcomes refer to effect estimates of maternal education on fathers' health behaviour (assortative mating and mothers' spill over effects). Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.



Table 8: Placebo reforms and placebo outcome

Independent variable	<i>Panel A: Placebo reforms</i>				
	Actual	Placebo reforms in			
	reform	t-2	t-3	t-4	t-5
	(1)	(2)	(3)	(4)	(5)
	<i>Dependent variable: Currently smoking</i>				
Mother's cohort with 9th grade	-0.0412*** (0.014)	0.0308 (0.021)	-0.0181 (0.022)	-0.0031 (0.023)	0.0196 (0.020)
Number of observations	14,799	7,087	7,087	7,087	7,087
	<i>Dependent variable: Overweight</i>				
Mother's cohort with 9th grade	-0.0359** (0.017)	0.0128 (0.045)	-0.0384 (0.028)	-0.0062 (0.044)	0.0539 (0.044)
Number of observations	5,497	1,175	1,175	1,175	1,175
	<i>Panel B: Placebo outcome</i>				
	<i>Dependent variable: Body height in cm (age 30-50) SOEP sample, children with mothers from basic track</i>				
Mother's cohort with 9th grade	-0.0629 (0.5393)				
Sample mean	172.88				
Number of person-year observations	21,320				

*Notes:* All OLS regressions are based on the DiD model of eq. 3 and include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth, a dummy for female, dummies for children's age, dummies for the survey year and a quartic in mothers' age. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009 and SOEP v32long, own calculations.

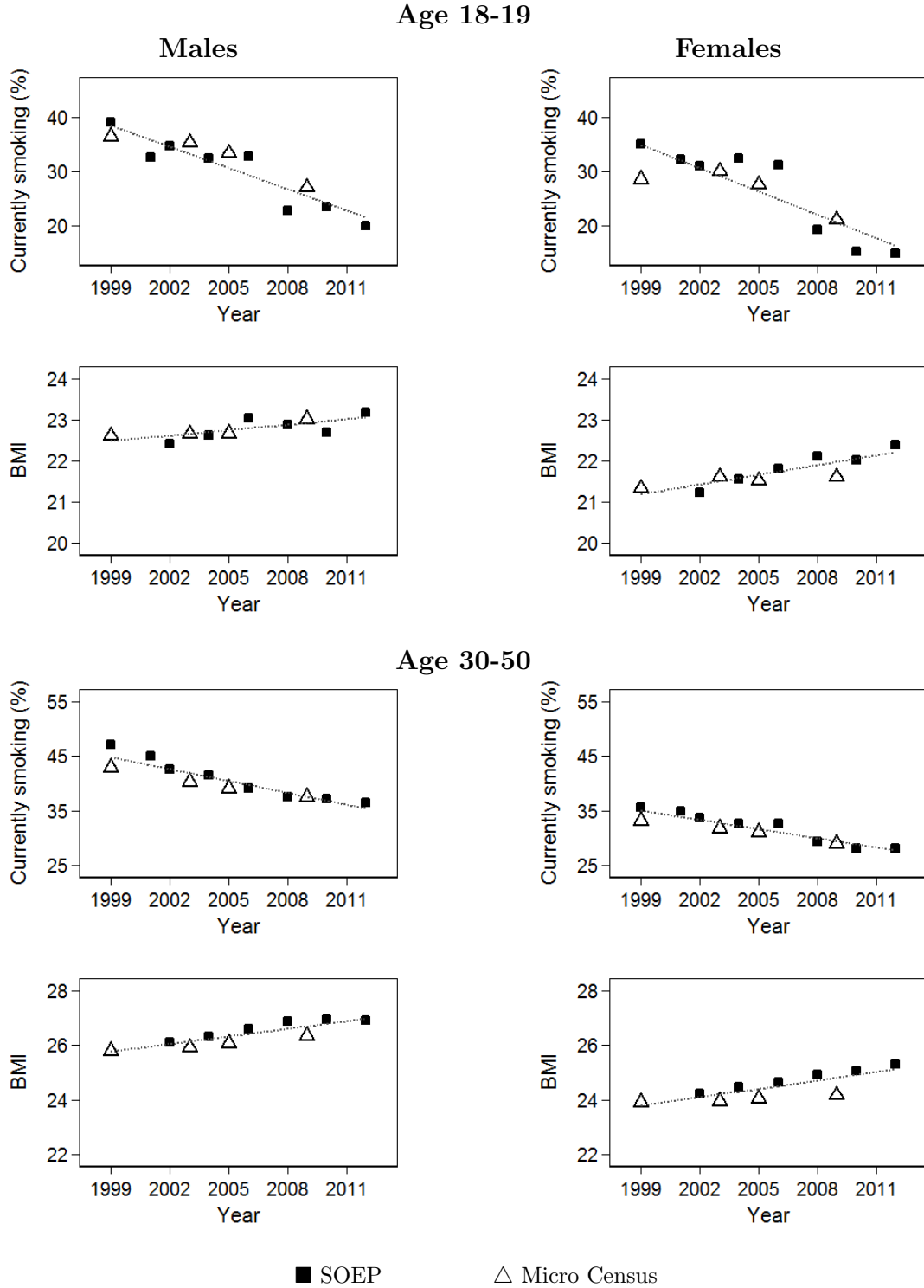
Table 9: Robustness checks: Control variables, time trends and models with alternative identification assumptions

Indep. variable	Control variables			State-specific time trends			Regression Discontinuity	Triple-Difference	
	Main (1)	Without X-vector (2)	With state controls (3)	Short school years (4)	Early introduction (5)	Without state-trends (6)	Child birth year $\times$ state FE (7)	In mothers' birth year (8)	Between mothers' school tracks (9)
<i>Panel A: Dependent variable: Currently smoking</i>									
<i>IV-Sample: All mothers</i>									
Mother's years of schooling (IV)	-0.0384*** (0.013)	-0.0324** (0.013)	-0.0391*** (0.013)	-0.0287* (0.015)	-0.0368*** (0.013)	-0.0334*** (0.012)	-0.0396*** (0.014)	-0.0346*** (0.013)	
Number of observations	27,339	27,339	27,339	27,339	27,339	27,339	27,339	25,591	
<i>Reduced form samples:</i>									
<i>Mothers with basic track schooling</i>									
Mother's cohort with 9th grade	-0.0412*** (0.014)	-0.0388*** (0.014)	-0.0416*** (0.014)	-0.0338** (0.014)	-0.0408*** (0.014)	-0.0403*** (0.013)	-0.0396*** (0.015)	-0.0237* (0.012)	
Number of observations	14,799	14,799	14,799	14,799	14,799	14,799	14,799	13,739	All mothers -0.0388*** (0.014) 27,339
<i>Panel B: Dependent variable: Overweight</i>									
<i>IV-Sample: All mothers</i>									
Mother's years of schooling (IV)	-0.0360* (0.021)	-0.0350** (0.015)	-0.0360* (0.021)	-0.0479* (0.026)	-0.0323 (0.021)	-0.0345** (0.014)	-0.0301** (0.015)	-0.0400** (0.020)	
Number of observations	12,853	12,853	12,853	12,853	12,853	12,853	12,853	12,270	
<i>Reduced form samples:</i>									
<i>Mothers with basic track schooling</i>									
Mother's cohort with 9th grade	-0.0359** (0.017)	-0.0293** (0.014)	-0.0363** (0.017)	-0.0370** (0.019)	-0.0344** (0.017)	-0.0301** (0.014)	-0.0256* (0.015)	-0.0274* (0.017)	
Number of observations	5,497	5,497	5,497	5,497	5,497	5,497	5,497	5,301	All mothers -0.0293** (0.014) 12,853

*Notes:* All regressions include the treatment dummy, the full set of year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in year of birth. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. State-controls are unemployment rates and GDP per capita at age 18. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .  
*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

## Appendix

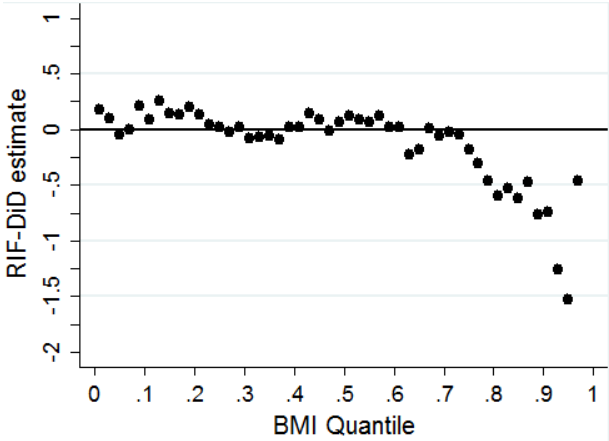
Figure A.1: Smoking rates and BMI for individuals aged 18-19 in the German Socio-Economic Panel Study and the German Micro Census



*Notes:* The figures plot mean values of smoking rates and BMI of individuals aged 18-19 in the German Socio-Economic Panel Study (SOEP) and the German Micro Census (West Germany, excluding Berlin). Information on outcomes in the SOEP is available from age 18 onward.

*Source:* SOEP v32long, German Micro Census (RDC, 2017), own illustration.

Figure A.2: Quantile treatment effects of maternal schooling on children’s BMI distribution (at age 15-18)

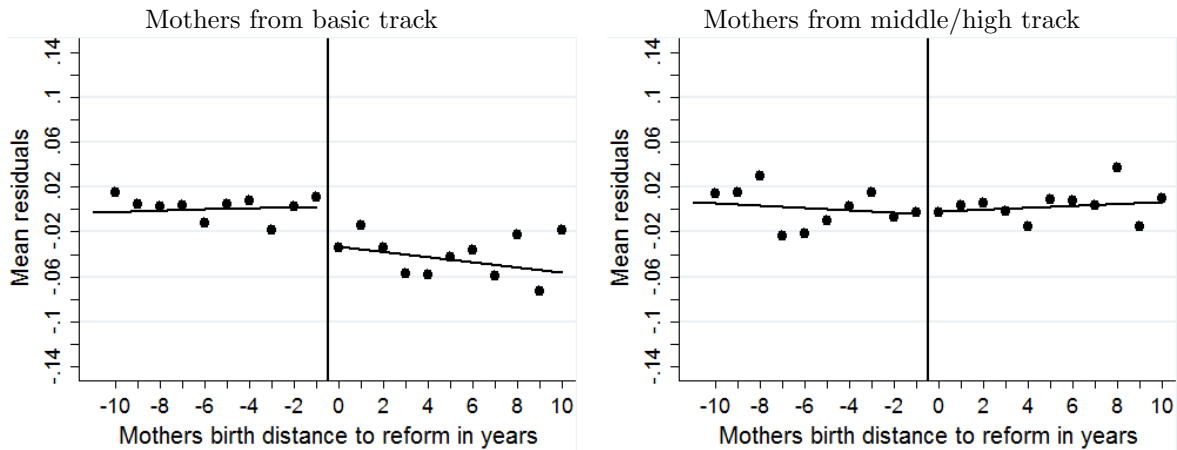


*Notes:* The figure plots quantile treatment effect estimates of maternal exposure to the compulsory schooling reform on children’s BMI at age 15-18. Estimations are based on recentered influence function estimations of the difference-in-differences model from eq. 3 (“RiF-DiD”, see Firpo et al., 2009, for details).

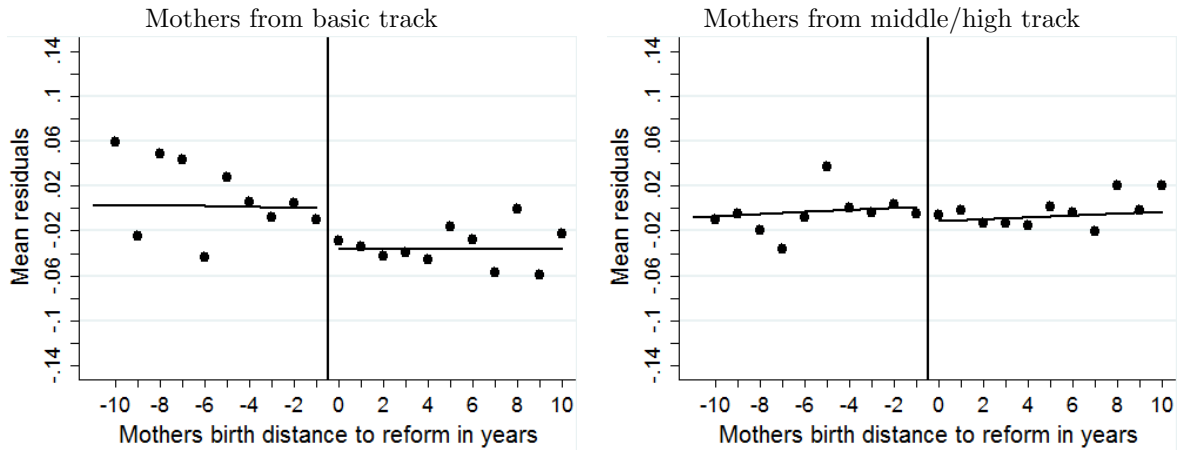
*Source:* RDC (2017), German Micro Census 1999, 2003, 2005, 2009, own illustration.

Figure A.3: Residuals from the difference-in-differences regression models of children’s health-related outcomes on mothers’ compulsory schooling exposure

Panel A: Outcome: Child is smoking (age 15-18)



Panel B: Outcome: Child is overweight (age 15-18)

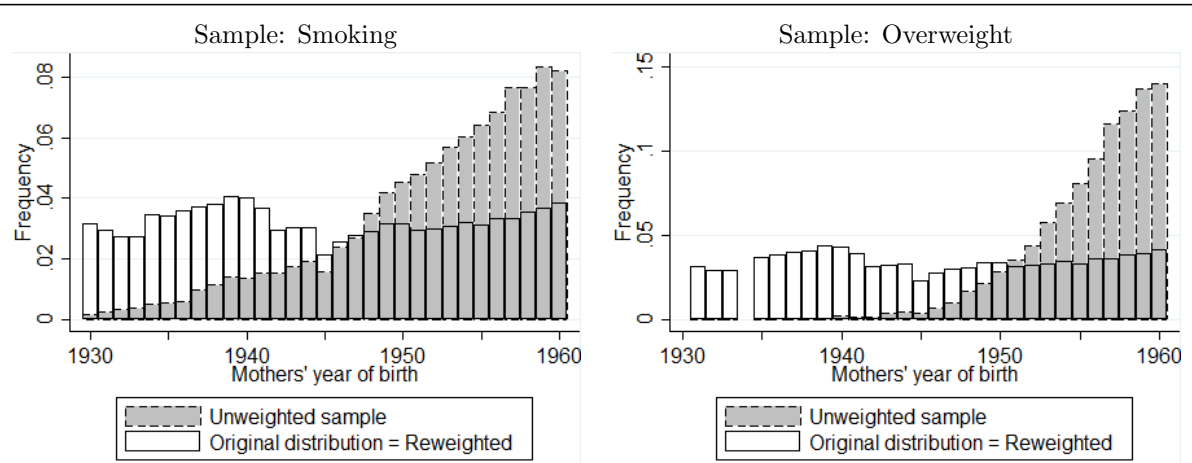


Notes: The graphs plot residuals and linear fits before and after the reform from the main difference-in-differences regression model (treatment dummy added back in) separately for children of mothers from basic track schools (affected group) and middle/high track schools (unaffected group).

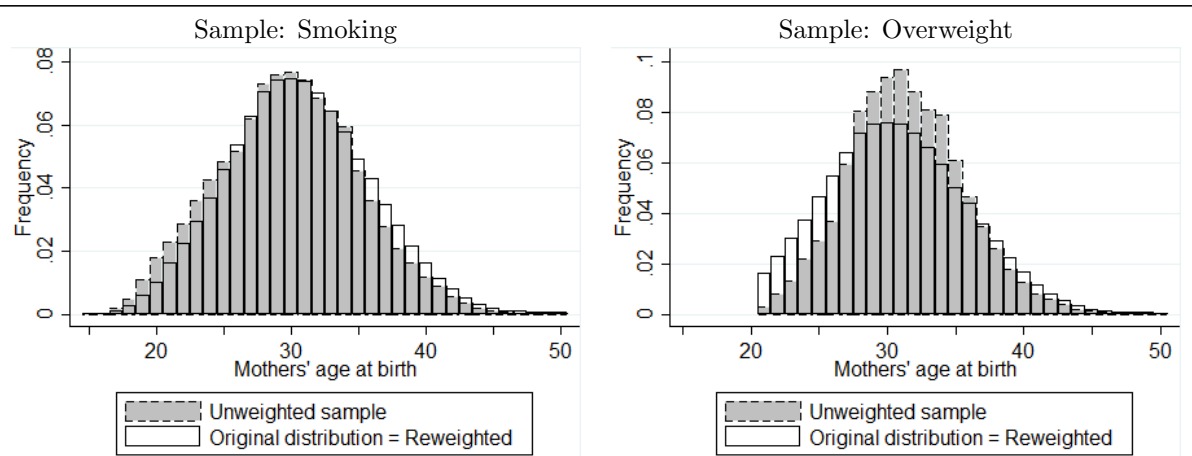
Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own illustration.

Figure A.4: Distributions of mothers' year of birth and mothers' age at birth

Panel A: Distribution of mothers' year of birth



Panel B: Distribution of mothers' age at birth



Notes: The histograms plot the distributions of mothers' year of birth and mothers' age at childbirth in the population and in the two main samples. The population distributions are used to construct inverse probability weights that are employed in weighted regressions reported in Table ??.

Source: RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own illustration.

Table B.1: Introduction of 9th grade in basic track of secondary school.

State (Bundesland)	First year when all students are supposed to graduate after 9 years	Birth cohorts with 9 years of school
Hamburg	1949	1934
Schleswig-Holstein	1956	1941
Bremen	1958	1943
Niedersachsen	1962	1947
Saarland	1964	1949
Nordrhein-Westfalen	1967	1953
Hessen	1967	1953
Rheinland-Pfalz	1967	1953
Baden-Württemberg	1967	1953
Bayern	1969	1955

*Source:* Pischke and von Wachter (2005).

Table B.2: Share of children aged 15-18 living with at least one parent

All years	Year					
	1989	1995	1999	2003	2005	2009
96.50 (18.39)	96.75 (17.74)	96.21 (19.09)	95.78 (20.11)	95.89 (19.86)	96.72 (17.81)	97.63 (15.20)

*Notes:* The table reports descriptive statistics on the share of individuals in private households aged 15-18 living with at least one parent. Standard deviations are reported in parentheses.

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table B.3: Statistical relationship for missing information on child outcomes

Independent variable	Dependent variable (child outcome): Missing information for	
	Currently smoking (1)	Overweight (2)
<i>Panel A: Multivariate regressions</i>		
Child age	-0.0056*** (0.0019)	0.0000 (0.0030)
Female	-0.0045 (0.0044)	0.0117 (0.0072)
Mothers' years of schooling	0.0043** (0.0020)	0.0077*** (0.0029)
Fathers' years of schooling	0.0058*** (0.0016)	0.0057** (0.0024)
Household net income (in 1000 EUR)	-0.0021 (0.0017)	-0.0058** (0.0026)
Number of observations	31,353	18,268
<i>Panel B: Instrumental variable estimations</i>		
<i>Sample: Mothers</i>		
Mother's years of schooling (IV)	0.0022 (0.0157)	-0.0191 (0.0344)
Sample mean	0.16	0.30
Number of observations	31,353	18,268
<i>Sample: Fathers</i>		
Father's years of schooling (IV)	0.0215 (0.0231)	0.0253 (0.0371)
Sample mean	0.16	0.30
Number of observations	25,417	14,525

*Notes:* OLS regressions in Panel A also include the full set of mothers' year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in mothers' year of birth and dummies for the survey year. The regressions include dummy variables for missing information for socioeconomic characteristics. In Panel B, IV-estimations are based on the main estimation model outlined in Section 3.2. Each coefficient is estimated in a separate regression. Standard errors are clustered at the federal state  $\times$  mothers' birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.



Table B.4: Descriptive statistics for the main samples

	Sample by child outcome (age 15-18)			
	Smoking <sup>1</sup>		Overweight <sup>2</sup>	
	Sample mean	s.d.	Sample mean	s.d.
<i>Child characteristics</i>				
Currently smoking (D)	0.16	(0.36)		
Overweight (D)			0.09	(0.29)
Female (D)	0.49	(0.50)	0.48	(0.50)
Age in years	16.58	(1.11)	16.61	(1.11)
<i>Mother characteristics</i>				
Years of schooling	9.69	(1.62)	10.15	(1.61)
Married (D)	0.87	(0.33)	0.86	(0.34)
Working (D)	0.66	(0.47)	0.73	(0.44)
Work hours/week (if working)	26.94	(14.15)	25.12	(13.15)
Log hourly wage in EUR	2.23	(0.58)	2.30	(0.59)
Age at birth in years	29.82	(5.24)	31.45	(4.31)
Number of children in household	2.07	(0.96)	2.01	(0.91)
Currently smoking (D)	0.27	(0.44)	0.27	(0.44)
BMI	24.33	(4.20)	24.36	(4.21)
BMI>25 (D)	0.35	(0.48)	0.35	(0.48)
<i>Father characteristics</i>				
Years of schooling	9.81	(1.87)	10.29	(1.89)
Working (D)	0.91	(0.28)	0.91	(0.29)
Log hourly wage in EUR	2.65	(0.47)	2.73	(0.49)
Currently smoking (D)	0.31	(0.46)	0.28	(0.45)
BMI	26.57	(3.66)	26.57	(3.64)
BMI>25 (D)	0.64	(0.48)	0.64	(0.48)
<i>Household characteristics</i>				
Household size	4.00	(1.09)	3.91	(1.05)
Household net income in EUR	3039.29	(1849.74)	3498.15	(2096.89)
Both parents in household	0.86	(0.35)	0.84	(0.36)
Number of observations	27,339		12,853	

*Notes:* The table provides descriptive statistics for the different samples depending on the child outcomes. Standard deviations are reported in parentheses.

*Source:* RDC (2017). <sup>1</sup> Based on German Micro Census 1989, 1995, 1999, 2003, 2005, 2009; <sup>2</sup> based on German Micro Census 1999, 2003, 2005, 2009.

Table B.5: Robustness check: Clustering of standard errors

Independent variable	Coefficient estimate (1)	<i>p</i> -value for clustering at	
		Federal state - mothers' birth year level (2)	Federal state level (3)
<i>Dependent variable: Child is smoking</i>			
Mother's cohort with 9th grade	-0.0412	[0.0029]***	[0.0200]**
<i>Dependent variable: Child is overweight</i>			
Mother's cohort with 9th grade	-0.0359	[0.0390]**	[0.0922]*

*Notes:* Column (1) reports OLS coefficient estimates of the main DiD model of eq. 3 in the sample of children with mothers from the basic track. Column (2) reports *p*-values based on robust standard errors clustered at the federal state  $\times$  mothers' birth year level (303 clusters for smoking, 202 clusters for overweight). Column 3 reports *p*-values based on clustering at the federal state level using wild cluster bootstrap procedures to account for the small number of clusters (10 clusters, 999 replications, Mammen weights, testing under H0, for details see Cameron et al., 2008). \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table B.6: Comparing first-stage coefficients on mothers' schooling of imputed and observed information

Independent variable	Sample					
	Smoking		Overweight		All	
	Imputed (1)	Observed (2)	Imputed (3)	Observed (4)	Imputed (5)	Observed (6)
<i>Dependent variable: Mother's years of education:</i>						
Mother's cohort with 9th grade	0.6454*** (0.0424)	0.8044* (0.4208)	0.4821*** (0.1005)	0.5313 (0.4473)	0.6485*** (0.0297)	0.7114* (0.4004)
Number of observations	27,339	7,736	12,853	6,721	55,217	9,074

*Notes:* The table reports reduced form estimates of mothers' years of education on an indicator of a 9th grade in the basic track. "Imputed" years of education are assigned based on mothers' school degree and the typical length of schooling in the federal state. "Observed" years of education are calculated from information on the year in which the highest vocational degree was completed. This information is only available for a subsample of 90% of individuals from 2005 onward. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table B.7: IV-weights

Years of schooling margins	Sample	
	Child is smoking (1)	Child is overweight (2)
8 to 9 years	0.961	0.988
9 to 10 years	0.004	0.001
10 to 12 years	0.018	0.010
12 to 13 years	0.017	0.000

*Notes:* The table reports weights that the IV estimator assigns to the marginal effects of maternal education across the years of schooling distribution. The weights are reported for the two main samples employed in the analyses. The weights were obtained based on the formulas provided in Løken et al. (2012).  
*Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.

Table B.8: Robustness checks with respect to sample choices

Independent variable	Symmetric time window around reform implementation (in years)					Sample restrictions			Regressions reweighted to match distribution of	
	+/- 15 (2)	+/- 13 (3)	+/- 11 (4)	+/- 9 (5)	W/o Hamburg & Bremen (6)	W/o first treated cohort (7)	Only children below age 18 (8)	Female births 1930-1960 (9)	Mothers' age at birth (10)	
Main (1)										
Mother's years of schooling (IV)	-0.0384*** (0.013)	-0.0322*** (0.011)	-0.0339*** (0.012)	-0.0330*** (0.013)	-0.0303** (0.014)	-0.0365*** (0.013)	-0.0417*** (0.013)	-0.0352* (0.019)	-0.0414*** (0.013)	
Number of observations	27,339	35,554	32,713	28,941	24,395	26,641	25,898	27,339	27,339	
Mother's cohort with 9th grade	-0.0412*** (0.014)	-0.0386*** (0.014)	-0.0404*** (0.015)	-0.0388** (0.015)	-0.0316** (0.016)	-0.0400*** (0.014)	-0.0460*** (0.016)	-0.0324* (0.018)	-0.0447*** (0.014)	
Number of observations	14,799	18,081	16,706	14,931	12,782	14,500	14,008	14,799	14,799	
<i>Panel A: Dependent variable: Currently smoking</i>										
<i>IV-sample: All mothers</i>										
<i>Reduced form sample: Mothers from basic track</i>										
Mother's years of schooling (IV)	-0.0360* (0.021)	-0.0383* (0.020)	-0.0403** (0.020)	-0.0448** (0.022)	-0.0348 (0.024)	-0.0368* (0.022)	-0.0384 (0.024)	-0.0402 (0.024)	-0.0389* (0.023)	
Number of observations	12,853	20,786	18,932	16,380	13,242	12,540	12,163	12,853	12,853	
Mother's cohort with 9th grade	-0.0359** (0.017)	-0.0364** (0.017)	-0.0356** (0.016)	-0.0397** (0.017)	-0.0328** (0.017)	-0.0386** (0.017)	-0.0350* (0.020)	-0.0712* (0.018)	-0.0457** (0.012)	
Number of observations	5,497	8,959	8,175	7,116	5,791	5,399	5,178	5,497	5,497	
<i>Panel B: Dependent variable: Overweight</i>										
<i>IV-sample: All mothers</i>										
<i>Reduced form sample: Mothers from basic track</i>										

*Notes:* All regressions include the treatment dummy, the full set of year of birth dummies, federal state dummies, interactions of federal state dummies with a linear trend in year of birth. Weighted regressions in columns 8-9 reweight the observations to match the birth distributions based on mothers' birth year (information based on Micro Census 1989 for all females born between 1930 & 1960) and mothers' age at birth (based on the full sample of children aged 0-18 living with their parents). Standard errors are clustered at the federal state  $\times$  birth year level and reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . *Source:* RDC (2017), German Micro Census 1989, 1995, 1999, 2003, 2005, 2009, own calculations.