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Does Education Affect Attitudes towards Immigration? Evidence from Germany

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Using data from the German Socio-Economic Panel and exploiting the staggered implementation of a compulsory schooling reform in West Germany, this article finds that an additional year of schooling lowers the probability of being very concerned about immigration to Germany by around six percentage points (20 percent). Furthermore, our findings imply significant spillovers from maternal education to immigration attitudes of her offspring. While we find no evidence for returns to education within a range of labour market outcomes, higher social trust appears to be an important mechanism behind our findings.

Keywords: attitudes towards immigration; intergenerational effects; schooling; externalities; instrumental variables estimation

JEL Classification: I26; J15; J62

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1 Introduction

Public concerns over immigration are increasingly used by populist political forces to mobilise voters. Framing immigrants as a threat to national-cultural identity, to the welfare system and to employment is an important element of the populist-right's political agenda: the European Union membership referendum in the United Kingdom was dominated by immigration issues and the message "take back control of borders" (Goodwin and Heath, 2016, p. 328); US president Donald Trump based his campaign largely on promises to restrict immigration; the populist party Alternative für Deutschland (AfD) explains in its programme why Islam does not belong in Germany (Alternative für Deutschland, 2016). Moreover, the strong prevalence of anti-immigration politics in the programmes of far right-wing parties has led some scholars to view anti-immigration and racism as the main reasons why these parties have established themselves in many Western European countries (Rydgren and Ruth, 2011).

The increasing spread of populist political ideas raises the question whether education is an important factor that explains the demarcation of societies by their attitudes towards immigration. A positive correlation between education and pro-immigration attitudes is observed in voting polls, discussed excessively in the media, and documented in the scientific research (Scheve and Slaughter, 2001, Mayda, 2006, Hainmueller and Hiscox, 2007, Card et al., 2012). However, a causal relationship between education and pro-immigration attitudes is difficult to establish because of potential omitted variables. For instance, if individuals with highly educated parents have higher levels of education, and also inherit positive parental attitudes towards immigration, the OLS estimate of education is likely to be upwardly biased.

To estimate the effect of schooling on attitudes towards immigration, the present study uses the staggered implementation of a compulsory schooling reform in West Germany as a source of exogenous variation. Our instrumental variable (IV) estimates indicate that an additional year of schooling reduces the probability of having high concerns about immigration to Germany by around six percentage points (20 percent). We further analyse potential intergenerational effects of maternal education on the offspring's immigration attitudes. The findings suggest that returns to education are not limited to the person directly affected, but also extend to the next generation. The probability of adult children being very concerned about immigration decreases by almost seven percentage points (27 percent) with an extra year of maternal schooling. This suggests an important composite effect of both educational and attitudinal spillovers.

Our analysis suggests that labour market outcomes are not the central transmission channel, since we confirm the previous findings that an additional year of schooling does not generate significant positive effects on outcomes such as labour force participation and labour income in Germany (Pischke and von Wachter, 2008, Kamhöfer and Schmitz, 2016). The analysis further reveals a positive impact of additional schooling on social capital, measured by general trust. We discuss the latter as a significant transmission channel. Due to data limitations, our ability to examine potential transmission channels of maternal schooling on offspring's attitudes is restricted. Nevertheless, in the light of the previous literature we argue that increased educational attainment of the offspring due to a higher level of maternal schooling and intergenerational correlations in immigration attitudes and trust are potentially relevant mechanisms.

The paper makes three main contributions to the literature. First, we examine the effect of schooling on individual's own *and* her offspring's attitudes towards immigration. The analysis of intergenerational effects, which to the best of our knowledge has not been done in other studies to date, is of particular interest because it has potentially important implications for the non-monetary returns to education: in presence of a positive effect of parents' education on children's attitudes, the net non-monetary returns are greater than individual estimates would suggest. As such, we are able to account for a more complete picture in terms of individuals' tolerance than previous studies. Second, given that the existing literature on mechanisms of education is mixed (Scheve and Slaughter, 2001, Dustmann and Preston, 2007, Hainmueller and Hiscox, 2007), we provide further evidence on relevant mechanisms, in particular labour market outcomes and the social capital of the individual. Third, we add to the relatively small literature on civic returns to education in general, and on the impact of schooling on attitudes towards immigration in particular.[1]

The remainder of the paper is organised as follows: Section 2 reviews the related literature, Section 3 provides the institutional background for Germany, and Section 4 presents the data. In Section 5, we discuss the empirical strategy and we present the results in Section 6. Section 7 discusses the potential transmission channels, and Section 8 concludes.

[1]While there exists a large literature on the monetary returns to education (see, for example, Angrist and Krueger, 1991, Card, 1999, Pischke and von Wachter, 2008, Kamhöfer and Schmitz, 2016, Oreopoulos, 2006), there is considerably less and mixed evidence on the civic returns to education (Dee, 2004, Milligan et al., 2004, Siedler, 2010, Lochner, 2011, Oreopoulos and Salvanes, 2011, Pelkonen, 2012). Furthermore, we are only aware of a single study (d'Hombres and Nunziata, 2016) that investigates the causal impact of education on attitudes towards immigration, without studying intergenerational effects however.

2 Related Literature

A large and growing body of literature examines the determinants of attitudes towards immigration. The studies most closely related to the present paper are those that explore the relationship between education and preferences over immigration. While the literature unequivocally agrees on a positive association between education and pro-immigration attitudes, the interpretation of factors behind this association differ.

One strand of studies attributes the association between education and immigration attitudes mainly to economic factors. Scheve and Slaughter (2001) find that less skilled individuals are more likely to prefer to restrict immigration. They use data from the American National Election Studies (NES) in the US for the years 1992, 1994 and 1996 and measure skills both by educational attainment and average wage in the individual's occupation. To determine whether labour market competition between natives and immigrants explains their findings, the authors split the estimation sample between labour force participants and non-participants. If education predominantly measures labour market skills and immigrants are more likely to be low-skilled, the negative correlation between education and anti-immigration attitudes should only hold among labour force participants. By and large, this is what Scheve and Slaughter (2001) find. These results are corroborated by cross-country studies by Mayda (2006) and O'Rourke and Sinnott (2006) using data from the International Social Survey Programme (ISSP). Both studies reveal that more educated individuals are less likely to have anti-immigrant sentiments, and the association is stronger in richer countries where natives are more skilled than the immigrants.

Another strand of studies attributes the positive association between education and pro-immigration attitudes to non-economic factors. Arguably, education is related to other factors, such as openness and beliefs, that correlate with immigration attitudes (Hainmueller and Hopkins, 2014). Educational systems may convey tolerant and egalitarian values (Lancee and Sarrasin, 2015), enhance cognitive and analytical thinking to circumvent oversimplifications of social reality (Coenders and Scheepers, 2003), or even reduce the cost of analysing political allegories to be less susceptible to hate-creating stories (Glaeser, 2005, Mocan and Raschke, 2016).

Related to this, Hainmueller and Hiscox (2007) argue that the conventional belief about labour market competition between natives and immigrants and anti-immigration sentiments is based on a misreading of the available evidence. The authors use data from the European Social Survey (ESS) with questions on attitudes towards immigration in four groups of country of origin: poor European, rich European, poor non-European and rich non-European. The reasoning is if labour market competition between natives and immigrants by skill level is the critical determinant of immigration preferences, education should be strongly and positively linked to support for immigration from poorer countries and more weakly, perhaps even negatively, correlated with the support for immigration from richer countries. The authors find that the highly educated are more likely to favour immigration irrespective of the immigrants' country of origin. Note, however, that the argument assumes immigrants to be a random sample from the source country, while there is strong evidence that immigrants are a self-selected group along a range of characteristics, including skills (Borjas, 1987). In contrast to Scheve and Slaughter (2001), Hainmueller and Hiscox (2007) do not find a significant difference in the association between education and attitudes towards immigration between the sub-samples of labour force participants and non-participants. Instead, they report that the more educated are less racist, place greater value on cultural diversity, and are more likely to believe that immigration brings benefits for the host economy as a whole.

Dustmann and Preston (2007) argue that across different levels of education, the aversion towards immigration is driven by various motives. The authors focus on three channels: welfare concerns, labour market concerns, and racial and cultural prejudices. Their findings suggest that welfare considerations are the most relevant factor in attitude formation of anti-immigration sentiments for the more educated, while racial and cultural prejudices play a dominant role for the less educated. At odds with the common expectation, the authors also find that for the less educated respondents labour market concerns play a minor role. This last finding should be interpreted and generalised with caution, since, unlike in the US and many European countries, UK immigrants, on average, have more schooling than the native-born whites (Dustmann and Preston, 2001).

Although extensive empirical research documents the positive association between education and pro-immigration attitudes, selection issues of individuals into higher education remain mostly unresolved. This point is raised by Lancee and Sarrasin (2015) in their recent analysis. The authors estimate the effect of education on attitudes towards immigration as adolescents pass through education in Switzerland, using a hybrid model that estimates a within-individual effect and a between-individual effect. The results confirm that education is the strongest between-individual predictor of attitudes towards immigration, but, when only within-person variance is used, the effect of education becomes statistically insignificant. This suggests that at least part of the positive relationship between education and pro-immigration attitudes is driven by selection into education.

The present study uses a compulsory schooling reform in West Germany to examine the effect of an additional year of schooling on immigration attitudes of native Germans.[2] In addressing the endogeneity issue of education, the present study is most closely related to recent work by d'Hombres and Nunziata (2016). The authors use data from the ESS and focus on a pool of 12 European countries.[3] For the whole sample of countries, they find a positive effect of education on pro-immigration attitudes in the order of 6-11 percentage points, on average. Considering that European countries differ greatly with respect to their institutional setting and historical development, the average effect for the joint sample of countries might hide important cross-country heterogeneity. Yet, single-country analysis remains an open dimension. Additionally, the authors use reforms across Europe, where schooling years differ in the base level of schooling to which the reform applies and the duration of additional schooling. For example, while schooling was increased from eight to 12 years in Belgium, it was increased from four to seven years in Italy. This asymmetry in the schooling reforms across countries generates distinct country-specific complier sub-populations for which an averaged local average treatment effect (LATE) is identified.

3 Institutional Background

3.1 Immigration to Germany

Germany is the largest migrant host country in Europe. By 2015, 21 percent of the total population had a migration background. The three most frequently represented migrant nationalities are Turkish, Polish and Italian (Bundesamt für Migration und Flüchtlinge, 2016).

The significant influx of foreign-born population began after World War II and has been continuous ever since. An important group of immigrants after the war were ethnic Germans. According to the Basic Constitutional Law of the Federal Republic of Germany, this group can obtain German citizenship based on their ethnicity (Fassmann and Munz, 1994). Around 3.9 million ethnic Germans immigrated to Germany between 1950 and 1998.

[2]Starting with the seminal work by Angrist and Krueger (1991), there is a comprehensive body of literature that uses compulsory schooling laws as a source of exogenous variation. Lochner (2011) and Oreopoulos and Salvanes (2011) review the literature on non-pecuniary returns to education, and Holmlund et al. (2011) survey the intergenerational literature on the effects of parent's schooling on children's schooling. For a survey on pecuniary returns to schooling, see Card (1999). In the German context, studies analyse the effect of compulsory schooling on labour market outcomes (Pischke and von Wachter, 2008, Kamhöfer and Schmitz, 2016), political behaviour (Siedler, 2010), health (Kemptner et al., 2011), fertility (Cygan-Rehm and Maeder, 2013), and the intergenerational transmission of education (Piopiunik, 2014). Moreover, exploiting exogenous variation in years of schooling allows us to address potential issues of reverse causality resulting from effects of immigration on the schooling of natives (McHenry, 2015, Hunt, 2017).

[3]d'Hombres and Nunziata (2016) include the following countries: Belgium, Denmark, Germany, Finland, France, Greece, Italy, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom (UK).

In the 1960s, driven by the labour shortage, Germany initiated multiple bilateral agreements with southern European countries, primarily with Turkey and Italy. Worbs (2003) mentions that the number of Turkish guest workers in Germany rose from 2,495 in 1960 to 617,531 in 1974. By the end of 1973, migration policies in Germany were restrained due to oil price shocks and fears of economic recession. Nevertheless, the influx of immigrants continued at moderate rates mainly as a result of family reunification.

In the early 1990s, the influx of foreigners to Germany peaked due to a large number of asylum seekers and refugees, most of them fleeing from the war in former Yugoslavia. From 1989 to 1996, the annual total influx of foreign-born immigrants to Germany was consistently over one million, reaching roughly 1.6 million in 1992.

At present, Germany remains a major host country for economic migrants and refugees. A large proportion of economic migration is from other EU countries, mainly from recent EU member states, and as a result of the recession in the southern states. In 2015, the total number of immigrants reached 2.14 million, representing the largest number of immigrants since the start of the statistical recordings in 1950 (Bundesamt für Migration und Flüchtlinge, 2016). EU citizens account for 40 percent of the new arrivals, while by single country origin, Syrians and Romanians represent the largest group.

For further information about immigration to Germany since World War II see, for example, Riphahn (2003), D'Amuri et al. (2010), Glitz (2012), Braun and Kvasnicka (2014) and Braun and Mahmoud (2014).

3.2 The School System in Germany

Germany has a tripartite school system regulated at the federal state level. Nevertheless, the main features of the education system are almost identical across federal states (Dustmann, 2004). Compulsory school attendance begins at the age of six. Students spend the first four years at the primary school, after which they are tracked into three types of secondary schools: lower (*Hauptschule*, formerly called *Volksschule*), intermediate (*Realschule*) and academic (*Gymnasium*).[4] Currently, the *Hauptschule* requires attendance of nine years. It is the least demanding track and provides general education as a basis for apprenticeship training. The *Realschule* ends after the tenth class. This intermediate school track is more academically integrated and prepares students for both blue and white-collar jobs (Dustmann, 2004). The academic track—the *Gymnasium*—leads to a tech-

[4] In Berlin and Brandenburg, the tracking takes place after the sixth class.

nical college entrance diploma (*Fachhochschulreife*) or to a university entrance diploma (*Abitur*). Obtaining an *Abitur* requires 12 to 13 years of schooling in all states in western Germany.[5]

Historically, governance of school education was under state authority in the Weimar Republic, until the Nazi Regime declared it to be under central organization in the beginning of 1934. As a result syllabi, teaching material and other school regulations fell largely under national control (Nicholls, 1978, Tent, 1982). With the emphasis on education and extracurricular activities loyal to the regime, the government promoted standardization of the school system and at least eight years of schooling. After World War II and the division of Germany into four occupation zones, state authorities regained decision power over schooling. The main focus was on the re-establishment of continuous teaching and the denazification of educational content.

This study exploits a schooling reform in West Germany that extended compulsory schooling from eight to nine years. It was implemented over a span of 20 years—from 1949 to 1969—in different federal states at different points of time. This generates exogenous variation in years of schooling across states and over cohorts of students. Table 1 shows each federal state with the relevant reform date and year of birth of the first cohort to be affected by the education reform, taken from Pischke and von Wachter (2005).

In 1949, the first federal state, Hamburg, prolonged mandatory schooling from eight to nine years. Schleswig-Holstein, Bremen, Lower Saxony and Saarland followed within a time frame of fifteen years. Several debates and initiatives to further reform the school system accompanied. In 1964, in an effort to uniformly reform the system, the states passed the *Hamburg Accord*, the key agreements of which included the introduction of nine-year compulsory schooling nationwide and the implementation of two short school years under an unchanged curriculum. As late reformers Baden-Wuerttemberg, Hesse, North Rhine-Westphalia, and Rhineland-Palatinate extended compulsory schooling by one year in 1967 and, at the same time, postponed the beginning of the school year from the time around Easter to fall for two consecutive years.[6] Bavaria was the last state to introduce longer compulsory schooling in 1969.

[5]Before 2001, students obtained their *Abitur* after 13 years at school. Since 2001, federal states introduced a school reform to shorten the length of high school by one year, resulting in overall schooling of 12 years instead of 13 (Huebener and Marcus, 2017). However, this reform did not affect compulsory schooling.

[6]Moreover, three other states, namely Schleswig-Holstein, Bremen, and Saarland, temporarily shortened the school years. We control for state-specific cohort trends in the main analysis and further discuss the influence of these short school years on our estimates in robustness section 6.3.

4 Data and Descriptive Statistics

We use data from the Socio-Economic Panel study (SOEP v30)—a large representative longitudinal household survey that has interviewed around 11,000 households and 30,000 individuals annually since 1984 (Goebel et al., 2018). The SOEP contains a comprehensive set of socio-economic and demographic background variables as well as measures on personality traits, personal attitudes, and values.

We draw two samples. We use *Sample I* to study the direct effect of education on individuals' attitudes towards immigration. *Sample II* is drawn such that it allows us to analyse potential spillover effects of maternal education on their adult children's concerns about immigration.

In *Sample I*, we follow Pischke and von Wachter (2008) and Siedler (2010) and restrict our first estimation sample to cohorts born in Germany between 1930 and 1960, excluding individuals who either currently reside or attended school in the area of the former German Democratic Republic. Because the information on the federal state of school attendance is not available for all individuals in our sample, we use the first federal state of residence that appears in the SOEP as a proxy for state of school attendance if this information is not available.[7] Following Pischke and von Wachter (2008), we use the school leaving qualification, the information on year of birth and federal state to construct years of schooling as follows: those who have completed the academic track are assigned 12 or 13 years of schooling depending on whether they have a *Fachhochschulreife* or an *Abitur*, respectively. Those who followed the intermediate track are assigned ten years of schooling. Those who completed the basic track and individuals with no school leaving qualification are assigned eight years of schooling if they were not exposed to the compulsory school reform and nine if they were. Accordingly, the compulsory school reform does not change the overall number of years of schooling for those who followed the intermediate or academic school track, while former students from the lowest school track have one additional year of schooling, depending on their year of birth and the state where they attended school.

Our main outcome describes individuals' attitudes towards immigration. We proxy this variable using the following question in the SOEP, which has been included every year since 1999: "Are you concerned about immigration to Germany?". The response categories are "very concerned", "somewhat concerned" and "not concerned at all". This measure of attitudes towards immigration has been previously used by Poutvaara and Steinhardt (2015) and Avdeenko and Siedler

[7] In the robustness section, we relax this assumption and find similar results.

(2017).[8] We recode the original three-category variable into a dichotomous variable that equals one for individuals who report that they are "very concerned". Our outcome takes the value of zero for individuals who report that they are "somewhat concerned" or "not concerned at all".

The validity of our dependent variable depends on whether it in fact captures salience related to concerns people have about immigration. There are two good reasons for this to be the case. First, the question is clearly formulated as a concern. Lancee and Pardos-Prado (2013) argue that survey items that intend to measure public concern, capture the importance of the issue and the degree to which it is a problem, meaning that the respondents for whom concerns about immigration are salient are the ones who perceive it as a problem, or are unsatisfied with the current policy on the issue. Second, Avdeenko and Siedler (2017) compare the proportion of people who report being very concerned about immigration with the official share of votes for far right-wing parties at the federal state level in Germany. They find a positive correlation coefficient of 0.40 between subjective measures on attitudes towards immigration as reported in the SOEP and the number of far right-wing votes in general elections.[9] Consequently, concerns about immigration are likely to translate into voting behavior that reflects individual intentions to reduce the concern.

In our main analysis, we use the first available observation for each individual. In the robustness section, we present an alternative specification exploiting the panel structure of the data: as an additional outcome, we use the proportion of years respondents are very concerned about immigration.

Sample II relies on retrospective information reported by adult children on the school leaving qualification and year of birth of their mother. We focus on mothers since the information reported about fathers can both refer to the biological father or to the mother's partner. Consistent with *Sample I*, *Sample II* comprises mothers born between 1930 and 1960 with valid information on their level of schooling.[10] Mothers' years of schooling and exposure to the reform are defined as in *Sample I*. The outcome captures adult children's concerns about immigration. The variable is dichotomous and is equal to one for adult children who report that they are "very concerned" about immigration and zero for children who report that they are "somewhat concerned" or "not concerned at all". In line with *Sample I*, we use the first available observation of attitudes towards

[8]Note that Avdeenko and Siedler (2017) study intergenerational correlations in attitudes towards immigration, whereas the present paper aims at estimating the causal intergenerational spillovers of education on attitudes towards immigration.

[9]The far right-wing parties are Deutsche Volksunion (DVU), Republikaner (REP) and Nationaldemokratische Partei Deutschlands (NPD).

[10]We proxy the state where the mothers attended school by the state of residence of their children.

immigration for each adult child.[11] Table A.1 in the appendix reports the number of observations in both samples by exposure to the reform and by the federal state.

Table 2 presents descriptive statistics by exposure to the reform. The upper panel reports the summary statistics for *Sample I*, and the lower panel for *Sample II*. Individuals affected by the increase in compulsory schooling are four percentage points less likely to report very concerned attitudes about immigration compared to individuals not affected by the reform. Furthermore, they are, on average, 13 to 14 years younger and have around one more year of schooling (see *Sample I*). The corresponding difference in concerns about immigration between adult children of mothers exposed to and not exposed to the reform is roughly four percentage points (see *Sample II*).

Figures 1 and 2 show the effect of the formal extension on school attainment. The figures depict average years of schooling against the distance in years to compulsory schooling reform in each state. Since the introduction of the ninth year was staggered across federal states, the first birth cohort affected differs between the states. Both figures illustrate a distinct jump in average years of schooling around the cut-off point.[12] The figures provide initial descriptive evidence that the compulsory years of schooling reform was effective in increasing average years of schooling for all cohorts affected and for treated mothers.

5 Research Design

5.1 Empirical Strategy

We start by estimating ordinary least squares (OLS) regression with a dichotomous dependent variable for our *Sample I*. The linear probability model linking immigration attitudes to years of schooling takes the form:

$$C = \tau S + \kappa X + State + Cohort + Syear + Trend + \epsilon \quad (1)$$

The outcome C equals one if the individual is very concerned about immigration, and zero otherwise (somewhat concerned, or not concerned at all). The variable S is the years of schooling and is assigned based on the school leaving qualification. $State$, $Cohort$, $Syear$, are federal state, cohort, and survey year fixed effects, respectively. Federal state fixed effects control for time-invariant differences in concerns about immigration between the states. They also capture the

[11] All adult children in the estimation sample were cohorts subject to nine years of compulsory schooling.

[12] Figure 2 also reveals an upward sloping trend in average maternal schooling even before the reform, which indicates an increasing educational attainment over the cohorts.

differential impact of the war (for instance, destroyed housing stock (Braun and Kvasnicka, 2014)) and time-invariant educational differences between states. Cohort fixed effects capture nationwide secular changes in educational attainment over birth years. Since we pool 15 interview years from 1999 to 2013, survey year fixed effects are important for accounting for systematic discrepancies that are borne by political and economic developments in a specific interview year. For instance, events such as 9/11 in 2001, a high unemployment rate in Germany in 2005, or the financial crisis in 2008 may affect individual attitudes. $Trend$ is a state-specific linear cohort trend and the vector X includes a male dummy and a quadratic in age as additional controls.[13]

A simple OLS regression is likely to estimate a biased effect of schooling on immigration attitudes. To overcome endogeneity, we instrument years of schooling of an individual with the compulsory schooling reform in West Germany in an IV framework. The first- and second-stages are specified as follows:

$$S = \delta R + \lambda X + State + Cohort + Syear + Trend + \upsilon \tag{2}$$

$$C = \theta \hat{S} + \gamma X + State + Cohort + Syear + Trend + \eta \tag{3}$$

where $\hat{S}$ captures the predicted years of schooling, and R is the dichotomous instrumental variable, which equals one if the individual is affected by the reform, and zero otherwise. The remaining variables are defined as in equation (1). To obtain accurate statistical inference, we cluster the standard errors over the federal state-year of birth cell, which results in 296 clusters in our main specification.[14]

5.2 Identifying Assumptions

The extension of compulsory schooling from eight to nine years is likely to be binding for students who otherwise would have completed their education after eight years of school attendance, meaning students in the lower school track. In our IV framework, we therefore estimate a local average treatment effect for the lower end of the educational distribution. Compliers in the sample are students who attend school for an additional year due to the compulsory schooling reform. Four

[13]The state-specific cohort trend is an interaction term between the state dummy and the year of birth. Note that age is the linear combination of year of birth and survey year, hence it is implicitly controlled for.

[14]To test the robustness of the inference, we also conducted the analysis with two-way clustering on the dimension of federal state and year of birth. The resulting standard errors are smaller. However, it should be noted that we only have ten clusters at the federal state level, which is susceptible to a "too few" clusters problem (e.g., Cameron and Miller, 2015).

assumptions need to be fulfilled to estimate the causal effect of one additional year of schooling on compliers' attitudes towards immigration—namely, the assumption of instrument independence, exclusion restriction, instrument relevance, and monotonicity (Angrist and Pischke, 2009, p. 114).

Instrument independence. The independence assumption requires the instrument—the compulsory schooling reform—to be independent of the vector of potential outcomes and potential assignments to one additional year of schooling. Similar to the previous literature using increases in compulsory years of schooling as a source of exogenous variation, one possible threat to our identification strategy is the potential bias due to policy preferences. For instance, the independence assumption would fail if the compulsory schooling reform was implemented to meet the political agenda of increasing social cohesion, or if the timing of the reform was correlated with immigration attitudes in a given state.

To assure that the independence assumption holds, in addition to federal state and cohort fixed effects, we include state-specific cohort trends. Moreover, we provide additional evidence. First, Table A.3 presents a short overview of the curriculum of the 9th year of schooling and reasons for introducing this additional year in the various federal states. For instance, the reasons for and aims of introducing the 9th year in North Rhine-Westphalia were (1) providing a more in-depth general education; (2) reinforcing political education, and (3) practising, acquiring and expanding basic skills and knowledge. Overall, across all federal states in Germany, the main reason for the compulsory schooling reform was to improve students' occupational maturity (Petzold, 1981). None of the laws and of the relevant literature mention aspects related to immigration, immigration policies, or civic attitudes as relevant aspects. Second, in our robustness section, we show that potential educational drivers such as the share of female teachers and teacher's age composition remained largely stable around the reform period in seven out of ten West German states for which we have reliable data. We also find no evidence of significant changes in immigration flows around the time of reform enactment in each of the federal states. Last, in Table A.4, we present the findings of regressing the timing of the reform on pre-reform state characteristics. Neither the student-teacher ratio in the basic track, nor general political orientation of the state, nor a group of socio-economic state characteristics, such as the share of migrants, seem to predict the timing of the reform.

Exclusion restriction. The exclusion assumption means that the compulsory schooling reform has no direct effect on concerns about immigration and influences attitudes towards immigration only through the additional year of education. While it is unlikely that perceived concerns about immigration change as a result of mere reform eligibility, the exclusion restriction would fail if compulsory schooling laws systematically altered the inflow or outflow of immigrants to a specific federal state. A higher or lower presence of immigrants due to the reform may affect locals' attitudes towards immigration *per se* or through a change in the political environment. In the robustness section, we address this issue by controlling for the proportion of foreigners during childhood at the federal state level.

Instrument relevance. With respect to the relevance assumption, the compulsory schooling reform must be sufficiently partially correlated with years of schooling, after controlling for the set of exogenous covariates. Otherwise, a weak instruments problem may cause our IV estimation to yield an even larger bias of the estimates than under OLS. The relevance requirement is empirically testable. For a single endogenous variable, the conventional rule suggests that the first-stage F-statistic of a significance test on the excluded instrument takes at least the value of ten (Staiger and Stock, 1997).

Monotonicity. Finally, the monotonicity assumption implies that while the instrument might have no effect for a certain proportion of the population, all those who are affected are affected in the same direction (Angrist and Pischke, 2009, p. 114). In the context of compulsory schooling, this implies that no student reduces his or her educational attainment because of the introduction of one additional year of schooling, i.e. there are no defiers. Although the monotonicity assumption is not directly verifiable, Angrist and Imbens (1995) suggest that it has a testable implication. They suggest plotting the cumulative distribution functions (CDF) of the treatment variable by the binary instrument. The monotonity assumption is likely to hold if the two CDFs do not cross.

In Figure 3, we plot the CDFs of individual schooling years by exposure to the compulsory schooling reform (affected or not affected). The figure shows that the CDFs for individuals affected by the reform lies below the CDF for individuals not affected by the reform (stochastic dominance). The two CDFs do not cross. To further assure that the reform did not affect the track choice of individuals, we estimate reduced form regressions with the track choice as an outcome, and find that the introduction of the ninth class does not have a significant effect on the choice of the school track (see Table A.2 in the appendix).

6 Empirical Evidence

6.1 Own Schooling and Immigration Attitudes

Table 3 shows the effect one additional year of schooling has on concerns about immigration. The focus is on *Sample I*, i.e., the relationship between an individual's own education and attitudes towards immigration. Columns (1) and (2) display OLS estimates, while columns (3) and (4) describe the results for the IV framework. Moreover, models in columns (1) to (4) include controls for sex, a quadratic in age, as well as a full set of federal state, birth cohort, and survey year fixed effects. To mitigate concerns that the introduction of the reform within states may be correlated with trends in education and immigration attitudes, in columns (2) and (4), we also control for linear state-specific cohort trends.

What unifies all findings in Table 3 is the clear-cut negative correlation between schooling and high concerns about immigration. OLS estimates in columns (1) and (2) suggest that one additional year of schooling is associated with a decrease of 4.4 percentage points (14.6 percent) in the probability of having high concerns about immigration. However, OLS estimates in the present setting may not convey the unbiased effect of schooling. Hence, we turn to the IV estimates.

Panel B of Table 3 reports the first-stage results. The first-stage estimates in columns (3) and (4) suggest that the compulsory schooling reform increases years of schooling by around 0.54 years. The point estimates are consistent with the findings of Pischke and von Wachter (2005) and Siedler (2010), who study the same reform in different contexts with different data-sets.[15] In both specifications, the first-stage F-statistics of the instrument are much larger than ten. Hence, the null hypothesis that the coefficient on the reform—δ in equation (2)—is equal to zero can easily be rejected (Staiger and Stock, 1997).

IV estimates in Panel A of Table 3 confirm that schooling has a negative effect on the probability of reporting high concerns about immigration. The inclusion of state-specific cohort trends only slightly alters the magnitude of the effect. In our preferred specification in column (4), an additional year of schooling decreases the probability of being very concerned about immigration by 6.3 percentage points. The effect is statistically significant at the five percent level. Against the base level of 30 percent of the full estimation sample, this translates into a decrease of around

[15]To be precise, Pischke and von Wachter (2005) find that the reform increased years of schooling by 0.59 years when using data from the Qualification and Career Survey and 0.55 when using data from the Micro Census. Siedler (2010) finds that the effect of the reform ranges from 0.39 to 0.54 when using ALLBUS data and from 0.43 to 0.52 when using ForsaBus data.

20 percent, which is a considerable effect.[16] When estimating our main regression separately by gender, the effect of schooling is larger for women (-0.08 (0.047)), however the estimates are not statistically different across sexes.

Are the estimated IV returns to schooling plausible? In an attempt to answer this question, we compare our findings with a related study. Closest to our study is d'Hombres and Nunziata (2016) who estimate education effects on pro-immigrations attitudes averaged over a pool of 12 European countries. The magnitude of our IV estimates is comparable to the findings of d'Hombres and Nunziata (2016). They find point estimates that are in the range of 6 to 11 percentage points.[17]

We also consult publications and historical documents to shed light on the curriculum of the ninth compulsory year of schooling. First, it should be noted that the same content was not merely spread over more schooling years (Leschinsky and Roeder, 1980). Rather, the main focus of the additional school year was on civic and occupational education. An official document published by the state parliament of North-Rhine Westphalia reads: "There is also complete agreement that political education, the foundations of which can only be partially laid during the first eight years of school, must be a key component of overall educational work in the ninth school year." (Nordrhein-Westfalen, 1962, p. 62, own translation). Similarly, with respect to the additional ninth year, Leschinsky and Roeder (1980) write: "The focus, however, was on expanding certain areas of learning, where the focus was on introducing students to their role as citizens, and on career and labour market orientation." (Leschinsky and Roeder, 1980, p. 334, own translation). Table A.3 in the appendix summarises the key goals of the additional ninth school year for the different federal states. It documents that the ninth year is mainly aimed at general education, rather than teaching specific skills.

Overall, the discussion indicates that the negative instrumental variable estimate of an additional year of schooling on high concerns about immigration shown in Table 3 appears to be quite plausible.

6.2 Maternal Schooling and Offspring's Immigration Attitudes

Immigration attitudes may be shaped not only by an individual's own educational attainment but also by parental education. The relationship between parents' education and the immigration at-

[16]In unreported regressions, we also estimated the reduced form—the direct effect of compulsory schooling reforms on attitudes towards immigration. The direct effect is three percentage points and is statistically significant at the ten percent level.

[17]Although the authors do not report the magnitudes of the effect in terms of percent, we attempted to recover these on the basis of the summary statistics reported on page 205 in their study. The effects range from 15 to 23 percent.

titudes of their offspring is likely to be multi-fold. On the one hand, the economic literature has repeatedly confirmed the intergenerational transmission of education (Plug, 2004, Black et al., 2005, Holmlund et al., 2011, Piopiunik, 2014, Dickson et al., 2016). On the other hand, young adults, whose parents were very concerned about immigration to Germany during their childhood years, are more likely to express strong concerns about immigration themselves (Avdeenko and Siedler, 2017).

Little is known about the effect of parental education on non-educational outcomes of their children.[18] In particular, to the best of our knowledge, intergenerational effects of schooling on adult children's attitudes and values have not been studied to date. We now examine whether a mother's education affects her offspring's attitudes towards immigration. The relevant first- and second-stage equations are:

$$S^m = \beta R^m + \delta X^o + State^m + Cohort^m + Syear^o + Trend^m + \zeta \tag{4}$$

$$C^o = \alpha \hat{S}^m + \omega X^o + State^m + Cohort^m + Syear^o + Trend^m + \nu. \tag{5}$$

In these equations, the superscript o denotes the offspring and m the mother. C^o is the dichotomous measure of immigration attitude of the adult child and is defined as in the main analysis. The variable $\hat{S}^m$ refers to predicted years of maternal schooling, R^m is a dichotomous variable indicating whether the mother was exposed to the reform. $State^m$, $Cohort^m$, and $Syear^o$ are state, cohort and survey year fixed effects, respectively, and $Trend^m$ is a vector of state-specific cohort trends. X^o controls for the sex of the child. The sample includes all mothers born between 1930 to 1960 (*Sample II*).

Table 4 reports the results. In all specifications maternal education has the expected sign. The higher the mother's schooling, the less likely the adult children are to report very concerned attitudes towards immigration. The OLS estimates point to a negative association of roughly three percentage points. The second stage IV estimates suggest that an additional year of maternal schooling reduces the probability of the mother's offspring reporting very concerned attitudes about immigration by 8.3 to 6.8 percentage points. As previously, we prefer the specification with state-specific cohort trends (column 4).[19]

The effect of a mother's schooling on her offspring's attitude is comparable to the main effect

[18] As an example, exceptions are Currie and Moretti (2003) for birth outcomes of children, Kemptner and Marcus (2013) for health outcomes, and Lundborg et al. (2014) for cognitive and non-cognitive skills.

[19] The coefficient on mothers' education remains precisely estimated and of comparable magnitude also after controlling for offspring's own education.

of an individual's own schooling on his or her own attitudes towards immigration (-6.8 and -6.3 percentage points).[20] A likely explanation for the comparable magnitudes is that exposure to the compulsory schooling reform has both educational and attitudinal spillovers: while the offspring of these mothers are likely to obtain a higher level of education themselves (Piopiunik, 2014), they are also likely to partially inherit the attitudes of their mothers (Avdeenko and Siedler, 2017).[21] It should be noted that in the first-stage estimations (Panel B, Table 4), we find a larger effect of the compulsory schooling reform on maternal schooling compared to the first-stage estimates of the instrument on own education in Table 3. This finding is anticipated, as the average number of years of schooling among women in these cohorts is considerably lower than that of men. As a result, the average increase in years of schooling due to the reform is greater.

Overall, our findings suggest a strong effect of schooling on immigration attitudes, directly and through maternal education. The next subsection tests the robustness of the findings. Whenever possible, we report the robustness checks for both *Sample I* and *Sample II*.

6.3 Robustness Checks

Teachers' age and gender composition. An alternative explanation for the effects may be environmental changes in schools that coincided with extended schooling. For instance, consider that the extension coincided with (or led to) hiring more teachers, in particular young ones, who may have more liberal attitudes, and these teachers may pass on their positive attitudes to their students. To address this possibility, we collected data on teachers' age in basic track schools. Figure A.1 in the appendix plots the number of teachers in the basic school track in different age categories. The vertical lines denote the reform date in each state. We are only able to construct these figures for seven federal states, because the reports by the Federal Statistical Office are inconsistent prior to 1958. The plots do not reveal a discontinuous change in the number of young teachers around the cut-off point. Furthermore, drawing on the accumulating evidence on the gender gap in political preferences (Edlund and Pande, 2002, Box-Steffensmeier et al., 2004), Figure A.2 plots the share of female teachers in basic track over 1950-1970. The figures show that the share of female

[20]If we also control for father's schooling, the estimate on mother's schooling is virtually the same (with standard errors slightly larger), while the coefficient on father's schooling itself is positive, but insignificant. Including father's schooling enables us to obtain the effect of the mother's schooling on her offspring's attitudes, net of assortative mating. Note, however, that schooling of spouses is highly collinear, thus inflating the standard errors and complicating the *ceteris paribus* interpretation of the coefficient.

[21]In unreported regressions, we use the self-reported information by the mothers themselves and merge them with their children. The resulting sample is much smaller in size. The OLS point estimates are highly comparable to those in Table 4. IV point estimates continue to be negative but are imprecisely estimated.

teachers has been rising from the mid 1950s onwards, however, there are no discontinuous changes around the reform dates. This suggests that the impact of schooling on high concerns about immigration is unlikely to be driven by a sudden increase in young and more female teachers.

Geographic mobility. In *Sample I*, the information on the federal state where the individual attended school, is available for half of our estimation sample. Similar to Pischke and von Wachter (2008) and Siedler (2010), we use the federal state of residence to proxy the school attendance state if the state of school attendance is unknown. If geographic mobility is high between federal states with different dates of reform introduction, this raises concerns about potentially imprecise assignment of the instrument. Descriptive evidence suggests that for 81 percent of individuals, the federal state of residence and the federal state of school attendance coincide. When we also consider that four federal states introduced the reform simultaneously, potential misspecification of the relevant reform date drops to 13.6 percent. Note that these figures are likely to be an upper bound, as the question on federal state of school attendance is included for younger cohorts in the SOEP, and older cohorts were geographically less mobile.

To further examine the issue of geographic mobility, we restrict the analysis to different subsamples. In Table 5, we present results for individuals with valid information on the state of school attendance (specification 1) and current state of residence (specification 2), respectively.[22] Thus, we only estimate the regressions for individuals for whom we have information on their state of school attendance and their current state of residence. Assignment of the instrument based on state of school attendance generates a stronger first-stage, as a pairwise comparison of the first-stage F-statistics in specification 1 and 2 of Table 5 reveals (130.8 compared to 79.9). However, IV estimates are highly comparable to each other. In line with Pischke and von Wachter (2008) and Siedler (2010), this suggests that proxying the state of school attendance with the current state of residence is unlikely to be problematic.

Leaving out the first affected cohort. The four states, which introduced the ninth class in 1966-67, also introduced two short school years due to a change in the start of the school year.[23] One short school year began on 1 April, 1966 and ended on 30 November, 1966, and a second short school year started on 1 December, 1966 and ended on 31 July, 1967 (Pischke, 2007). The nominal curriculum did not change for the transition cohorts, and Pischke (2007) further argues that these

[22] We are only able to carry this analysis for *Sample I* due to data limitations.

[23] The four federal states are Hesse, North-Rhine Westphalia, Rhineland-Palatinate and Baden-Wuerttemberg.

short school years did not lead to a reduction in human capital accumulation.[24] The determination of the first birth cohorts affected by the increase in compulsory years of schooling reflects the presence of short school years. Nevertheless, in some states, the actual implementation of the compulsory schooling reform might not have perfectly coincided with the official date. For these reasons, we repeat our analysis by excluding the first affected birth cohorts.[25] Specification 1 of Table 6 shows the results. The estimate on the effect of own schooling is largely unaffected by this modification, while the corresponding one for maternal schooling is 4 percentage points larger. This difference may indicate potential measurement error in the assignment of reform exposure for cohorts within one year of the cut-off, if adult children retrieve the birth year of their mothers with a small measurement error.

Region-specific year of birth effects. Stephens and Yang (2014) examine the common trend assumption in studies which use compulsory schooling laws as instruments in their analysis. The authors argue that the assumption of common trends in variables affecting different cohorts across states is unlikely to hold. In our baseline specification, we control for state-specific cohort trends, that should capture within-state developments correlated with the changes in law, educational improvements, and the outcome. Similar developments, however, may also occur at a regional level. Hence, to allow birth cohort effects to vary across regions, we divide West Germany into three broad regions—northern, central, and southern. The regional split is as follows: the northern part includes the states of Hamburg, Bremen, Schleswig-Holstein and Lower Saxony, the central part includes North Rhine-Westphalia, Hesse, Rhineland-Palatinate and Saarland, and the southern part consists of Baden-Wuerttemberg and Bavaria.[26] Including these regions-by-year-of-birth fixed effects results in second-stage IV coefficients on schooling that are roughly three percentage points larger than the coefficients from the baseline specification for both samples (specification 2).[27]

Social desirability bias. Questions on immigration attitudes may be sensitive to social desirability

[24]According to the Agreement on the Unification of the School System (*Hamburger Abkommen*), the start of the school year should have been moved to the end of the summer in all federal states by 1967 (Pischke, 2007). The implementation varied across federal states. West Berlin and Hamburg opted for one long school year. Bavaria already started the academic term in summer. Lower Saxony added an additional school period along with shorter school years in subsequent years for some types of schools. For further details see Pischke (2007).

[25]For instance, Siedler (2010) mentions that some schools in Bremen may have increased compulsory schooling in 1959 rather than in 1958 (the official introduction date).

[26]Stephens and Yang (2014) use US census regions—North-East, Midwest, South and West. The findings show that for the majority of studies examined, IV estimates become either close to zero or have the "wrong" sign.

[27]In unreported regressions we also added state-specific quadratic trends. The coefficient on education in *Sample I* remains identical. For *Sample II* the coefficient on mothers education is somewhat smaller in magnitude and imprecisely estimated. However, the quadratic trends are neither jointly nor individually statistically significant, suggesting that the specification with quadratic trends is the wrong specification.

and, consequently, might trigger reporting bias. If the well educated are more likely to identify the "socially desirable" answer in the presence of the interviewer and report it, we risk overestimating the true effect of schooling, unless these respondents also act in a "socially desirable" manner. We are able to partially address this problem since the SOEP follows a "mixed mode approach" in interview methods. In specification 3 of Table 6 we control for the interview mode by constructing a dichotomous variable that reflects whether the questionnaire is self-administered. This has virtually no effect on the main estimates, suggesting that social desirability bias is unlikely to be a concern.[28]

Bandwidths around the reform. We restrict the sample to cohorts (mothers) born between 1930 and 1960. While this selection is data efficient, it has its limitations: we create an asymmetric window around the first affected birth cohort in each state. This means that early implementer states contribute more to the treatment group, while late implementers contribute more to the control group. To assure that this sample restriction does not affect the main results, and to further account for potential omitted time-varying changes in attitudes and education, we re-estimate equations (2)-(5) in a regression discontinuity framework. To this end, for each state, we construct a pre-treatment and post-treatment sample composed of cohorts born six years before and after the reform. We repeat the exercise by providing a slightly broader bandwidth—eight years before and after the reform. This mode of sample restriction creates a symmetric window around the cut-off point in each federal state, with similar sample sizes for treatment and control groups by states. Moreover, it may also reduce concerns about unobserved developments that state-specific linear trends may not capture. However, these restrictions also result in smaller sample sizes compared to the main samples.

Table 6 presents the results in specifications 4 and 5. The point estimates on own schooling are larger compared to the baseline effect in the order of one to two percentage points. The opposite, however, applies to maternal schooling. The coefficients are smaller in magnitude and imprecisely estimated. The restriction by bandwidth may increase measurement error problems in mothers' year of birth: if retrospective retrieval of maternal year of birth occurs with an error of one year, it would result in distorted assignment of reform exposure immediately before and after the cut-off.

Childhood political and economic environment controls. A common concern in IV estimations is the possibility of omitted variable bias. Changes in the political and economic environment

[28]For the descriptive statistics of the interview methods used, see Table A.5 in the appendix.

that coincide with the timing of compulsory schooling reform may be a threat to the exclusion restriction. To alleviate these concerns, we re-estimate our main specification by controlling for a range of proxies for political climate of the state when the individual was 15 years old (specification 6). In particular, we add voter turnout in latest federal elections and the share of votes that two major parties–Christian Democratic Union (CDU) and Social Democratic Party (SPD)–receive, as well as number of seats each of these parties acquire in state parliaments.[29] In specifications 7 and 8 we control for GDP per capita, share of unemployed individuals in the population, and share of foreigners at the federal state level.[30] Specification 7 shows the results when we include GDP per capita, unemployment rate and migration inflow over ages 12 to 18, while specification 8 controls for the above mentioned variables for the interview year. The point estimates on schooling for both samples are precisely estimated and highly comparable to our main IV estimates.[31]

Ordered IV probit. In ordered IV probit regressions, we maintain the original three-category coding of the outcome. The IV probit coefficients on schooling are reported in specification 9 of Table 6. The estimates are negative and statistically significant at the five percent level, confirming that schooling negatively affects immigration concerns. The average marginal effects suggest that an additional year of schooling reduces the probability of reporting "very concerned" attitudes by 4.9 percentage points, with a standard error of 0.024 for individual's own schooling, and by 5.1 percentage points with a standard error of 0.021 for the maternal schooling.[32]

Average concerns over all available panel years. Despite the appealing panel structure of the SOEP, fixed effect estimation is not feasible in our analysis. The reason is that the main explanatory variable of interest—years of schooling—is time-invariant within individuals. Pooling all available panel years and treating it as a pseudo cross-section gives more weight to individuals who stay in the panel survey longer, and this pattern may be correlated with the schooling reform. Additionally, Lancee and Sarrasin (2015) show that there is little variation in immigration attitudes

[29]The data on state elections is available under $http://wahl.tagesschau.de/$. The data on voter turnout and vote share of CDU and SPD in federal elections comes from Statistical Yearbooks of German Federal Republic, various years.

[30]The data on GDP is provided by the official working group "Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder", the number of foreigners and the population data comes from the Federal Statistical Office of Germany. GDP per capita is measured in Deutschmark for specification 7, and in euros for specification 8. Data on the unemployment rate at the state level is only available as of 1960, so we use unemployment figures from the Federal Employment Agency and adjust these according to the population size.

[31]In unreported regressions, we also control for the residence community type of the respondent. Although choice of residence (urban or rural) may be endogenous, we reassuringly find that our point estimates on schooling are virtually unaltered.

[32]The marginal effects on reporting "somewhat concerned" or "not concerned at all" are 0.006 (0.003) and 0.044 (0.021), respectively, for *Sample I* and -0.006 (0.002) and 0.057 (0.222) for *Sample II*.

within individuals. Therefore, in our main analysis, we use the first available observation for each individual. To reassure that this sample selection criteria does not drive the results, we report the estimates with an alternative definition of the dependent variable in specification 10 of Table 6. We create a single measure for each individual by calculating the proportion of times that a person reported being very concerned about immigration over all available panel years. The dependent variable is now continuous and ranges from zero to one.[33] The resulting coefficient on schooling is comparable with the findings in the main section. An additional year of schooling reduces the probability of being very concerned about immigration by 4.3 and 5.4 percentage points in *Sample I* and *Sample II*, respectively, which is a reduction of 15 percent and 23 percent against the base level of average concerns.

Placebo tests. In specification 11 of Table 6 we randomly generate reform implementation dates from a uniform distribution for each state over the timespan 1949-1969, and hence "fake" reform exposure. The first-stage F-statistics for both *Sample I* and *Sample II* are very small. Accordingly, the estimates in the second stage are imprecise and implausible in magnitudes.[34] As an additional placebo test (not reported), we run our main specification with placebo outcomes, namely parental schooling of the individuals (*Sample I*). Reassuringly, the coefficient on schooling is statistically insignificant.

7 Channels

7.1 Channels of Own Schooling

From a policy perspective, distinguishing the channels is necessary to properly address concerns about immigration. If attitudes are driven by a more secure labour market position, then policy measures should target labour market institutions, while if attitudes stem from deeper knowledge or rectified reasoning, then policy measures should target quality of education. We start by considering potential labour market outcomes through which education might affect individual attitudes towards immigration and extend our analysis to generalized trust as a proxy for social capital.

Labour market outcomes. According to the canonical model, if a country experiences low-skilled migration influx (hence an increase in low-skilled labour supply), the wages for the native

[33]Note that this specification includes fixed effects for the number of times the respondent replied to the question, and average age along panel years for the individual's own schooling sample.

[34]In unreported regressions we also use the fake reform in a reduced form, and find that the fake reform has no significant effect on immigration attitudes in either of the samples.

population in a comparable skill group will decrease, whereas high-skilled natives might benefit (Dustmann et al., 2016). Hence, if attitudes towards immigration are motivated through fear of labour market competition, in countries with low-skilled immigration the highly skilled natives should be less likely to have negative attitudes towards immigration. However, the empirical literature is not univocal on the direction and magnitude of the impact immigration has on labour market outcomes of the natives.[35] The findings from the literature on immigration attitudes do not provide unambiguous empirical support for the labour-market-competition hypothesis either (e.g. Scheve and Slaughter, 2001, Mayda, 2006, Hainmueller and Hiscox, 2007, Card et al., 2012, Hainmueller et al., 2015).

To test whether labour market outcomes are behind our findings we examine a range of variables that should capture job market competition fears at different margins. We use our main identification strategy to test whether the compulsory schooling has a significant effect on the hypothesized channel. We start at the extensive margin by analysing whether compulsory schooling affects labour force participation and unemployment status at the time of the survey. To encompass larger timespan we also examine the probability of ever being unemployed until the time of the survey (row 1-3, Table 7). The point estimates suggest that an additional year of education increases the chances of participating in the labour force, and decreases the probability of being either contemporaneously or ever unemployed. However, none of the estimates are statistically different from zero.

Our focus is next on occupational outcomes, in particular the likelihood of being in a white-collar occupation, the likelihood of being employed as a civil servant and the likelihood of having an unlimited work contract.[36] The interest in the white- and blue-collar binary is motivated by the idea that the ones in blue-collar occupations are more likely to directly compete with immigrants in the job market, given that historically the immigration to Germany has been low-skilled. On the other hand having an unlimited contract or being a civil servant provides the highest level of job security. Hence, these subgroups should be least worried as far as job market competition is concerned. However, again we find no evidence that education significantly changes the likelihood of these outcomes.

[35]Dustmann et al. (2016) show that the parameters from different models are often not comparable and differ in their interpretation. In particular the national skill-cell approach identifies an effect for different experience levels within education groups, while the pure spatial approach estimates the total wage effect on a particular skill group. Furthermore, the authors argue that the national skill-cell approach implicitly assumes that natives and immigrants in the same education-experience cell are perfect substitutes, which introduces bias in the parameters.

[36]White-collar occupation is defined as occupational codes 1-5 based on the ISCO-88 occupational classification. This definition approximately translates into non-manual and manual classification as well.

Moreover, we consider the effect of additional education on individual's net monthly labour earnings and monthly household income hypothesizing that higher financial security may decrease the probability of perceiving immigration as an economic threat. Again, the point estimates indicate the expected direction, however they are statistically insignificant. Using the same compulsory schooling reform, Pischke and von Wachter (2008) find zero returns to compulsory schooling in terms of wages and speculate that this is because in Germany students generally learn skills relevant for the labour market earlier than in other countries. These findings are confirmed by Kamhöfer and Schmitz (2016) using SOEP data. Furthermore, labour market outcomes do not appear to be significantly correlated with immigration attitudes, after controlling for schooling (see Panel A of Table A.6).

Worries about immigration can be motivated through real as well as perceived threat. And even though the findings in this section suggest no statistically detectable schooling effect on realized labour market outcomes, it might still have an impact on how individuals perceive their own economic situation. To capture this dimension we additionally examine concerns about job security and own economic situation in Panel B of Table 7.[37] Again, although education reduces the probability of being very concerned along these two dimensions, the effects are not statistically different from zero.

Social capital. Putnam defines social capital as "features of social organization such as networks, norms, and social trust that facilitate coordination and cooperation" (Putnam, 1995, p. 67). While social capital has long been linked to economic growth and to the quality of institutions (Knack and Keefer, 1997, Zak and Knack, 2001), a recent literature proposes that it is also strongly linked to attitudes towards immigration. Herreros and Criado (2009) argue that immigration concerns are often a consequence of fears of the out-group. Social trusters avoid racial and cultural stereotyping when forming their beliefs about others' trustworthiness, hence they extend their trust also to foreigners. In a cross-country analysis of 19 European countries, the authors find that higher levels of social trust are associated with positive attitudes towards immigration. Economidou et al. (2017) and Halapuu et al. (2013) also confirm that across Europe individuals with high social capital exhibit a more positive attitude toward immigration.

Here we focus on one, yet crucial dimension of social capital—social trust. Delhey and Newton (2005) define it as "the belief that others will not deliberately or knowingly do us harm, if they can

[37]Concerns about job security and own economic situation have three response-categories: "very concerned", "somewhat concerned", and "not concerned at all". We recode the original three-category variables into a dichotomous variable that equals one for individuals who report that they are "very concerned", and zero otherwise.

avoid it, and will look after our interests, if this is possible" [p. 311]. The determinants of trust are not well understood. Nevertheless, education is posited to be one of the primary determinants (Helliwell and Putnam, 1999, Alesina and La Ferrara, 2002). In a meta analysis of 28 studies Huang et al. (2009) conclude that an additional year of schooling increases social trust by 4.6 percent of its standard deviation.[38]

We examine whether compulsory schooling affects general trust. The SOEP has measures on trust in 2005, 2008 and 2013. Respondents are asked to evaluate the following three statements on a four-point scale: (1) on the whole one can trust other people, (2) nowadays one cannot rely on anyone, (3) if one is dealing with strangers, it is better to be careful before one can trust them.[39] The validity of SOEP trust measures is reported in Fehr et al. (2003).

We first generate the mean of each item for each person over all available years. We subsequently collapse the respective items into a single standardised measure of trust with a mean of zero and standard deviation of one, and then link our estimation sample to trust outcomes.[40] Panel B of Table 7 shows the impact of an additional year of schooling on social (general) trust. An additional year of schooling increases trust by 17.6 percent of its standard deviation. This effect is precisely estimated at the five percent significance level. Furthermore, as Table A.6 shows, higher trust is strongly correlated with less concerns about immigration, after controlling for schooling.

Our analysis in this section suggests that labour market outcomes are unlikely to be behind the finding that education reduces the likelihood of high concerns about immigration. It indicates, however, that returns to education in terms of social trust are potentially mediating the effect of education on attitudes towards immigration.

7.2 Channels of Maternal Schooling

The channels through which a mother's schooling affects her children's attitudes are likely to be multifaceted. Below we discuss the potential mechanisms in the light of the previous literature. Due to data limitations, however, we are unable to disentangle the role and the importance of each mechanism.

In theories on the origins of preference formation, parents transmit their attitudes and prefer-

[38] Note, however, that the majority of the studies examined do not take into account the endogeneity issue of education.

[39] The scale has the following items: "strongly agree", "agree", "disagree", and "strongly disagree".

[40] Note that alternatively we can either restrict our main estimation sample to the outcomes from the years 2005, 2008 and 2013 for trust, or take one of the measures of the individual as constant and link it to the main estimation sample. The results are virtually unaffected by either approach.

ences to their children, actively or passively, leading to persistences across generations (Bisin and Verdier, 2001). Related to this, Avdeenko and Siedler (2017) find that young adults, whose parents were very concerned about immigration to Germany during their childhood years, have a much higher likelihood of also expressing strong concerns about immigration. The marginal effects for the intergenerational transmission in attitudes towards immigration vary between 23 to 30 percentage points, depending on the specification and parent-child gender pairs. As we show, cohorts exposed to the compulsory schooling reform are less likely to be worried about immigration. Consequently, building on the findings of Avdeenko and Siedler (2017), their offspring should also be less likely to be worried about immigration.

Another relevant channel through which a mother's schooling may have an impact on her children's immigration attitudes is through an induced increase in her children's educational attainment. For our estimation sample, we find a positive, yet statistically insignificant effect of maternal schooling on the educational attainment of the mother's offspring. However, exploiting the same compulsory schooling reform, with somewhat different sample restrictions, Piopiunik (2014) finds that an additional year of schooling for the mother significantly increases the likelihood of her sons completing intermediate or higher track schooling (10 years or more). The effect does not hold for daughters.

We also analyse whether mother's schooling affects her offspring's social trust. Unreported findings suggest a positive and insignificant effect on social trust. Although we do not find any evidence that maternal schooling increases the trust of adult children, the previous section documents a positive significant effect of an individual's own schooling on social trust. Consequently, the higher level of maternal trust due to the mother's additional year of schooling may have spillover effects on the trust level of her offspring. Related to that, Dohmen et al. (2012) analyse the intergenerational correlation in trust using the SOEP and find that increasing the mother's trust by one standard deviation increases the child's trust by around 0.24.

Overall, the previous literature provides suggestive evidence that a reduction of adult children's strong concerns about immigration due to higher maternal education might be driven by one or a combination of multiple mechanisms: a positive effect of maternal education on a child's educational attainment, which in turn changes adult child's attitudes towards immigration; an effect of the mother's education on the child's attitudes through modifying her own attitudes; a positive effect of maternal trust on child's trust, which in turn changes the adult child's attitudes towards immigration. We should reiterate that, due to data limitations, we are not able to disentangle the role and importance of each mechanism.

8 Conclusion

The present study examines the role of education in shaping immigration attitudes. Although previous literature establishes a strong positive empirical correlation between education and pro-immigration attitudes, this association cannot be interpreted as causal. Simple OLS regressions may produce inconsistent estimates of education due to selection on unobservables. We use compulsory schooling reforms in West Germany as a source of exogenous variation to estimate the consistent effect of schooling on immigration attitudes. Our results show that schooling has a sizeable impact on attitudes towards immigration. An additional year of schooling reduces the likelihood of being very concerned about immigration by around 20 percent. We extend the analysis to potential intergenerational transmission of a mother's education to her offspring's immigration attitudes. Our results imply that returns to compulsory schooling extend to the next generation. Adult children's high concerns about immigration decrease by almost seven percentage points with an extra year of maternal compulsory schooling, suggesting a composite effect of both educational and attitudinal spillovers. We show that these findings are robust to alternative specifications and sample selection criteria.

To shed some light on the mechanisms behind our estimates, we analyse potential channels through which an individual's own schooling may affect attitudes towards immigration. The findings suggest that labour market outcomes are unlikely to be behind our results, since there is no evidence on the effect of schooling on considered labour market outcomes. However, the evidence indicates that schooling significantly increases an individual's social capital.

Our empirical findings indicate that education may be an important tool to address concerns about immigration in a host country. Intergenerational transmission of educational attainment and attitudes from parents to their offspring implies that educational policies may address social cohesion through (at least) two generations. The role of education appears to expand over providing financial and labour market welfare, by seeding and moulding tolerant beliefs and attitudes.

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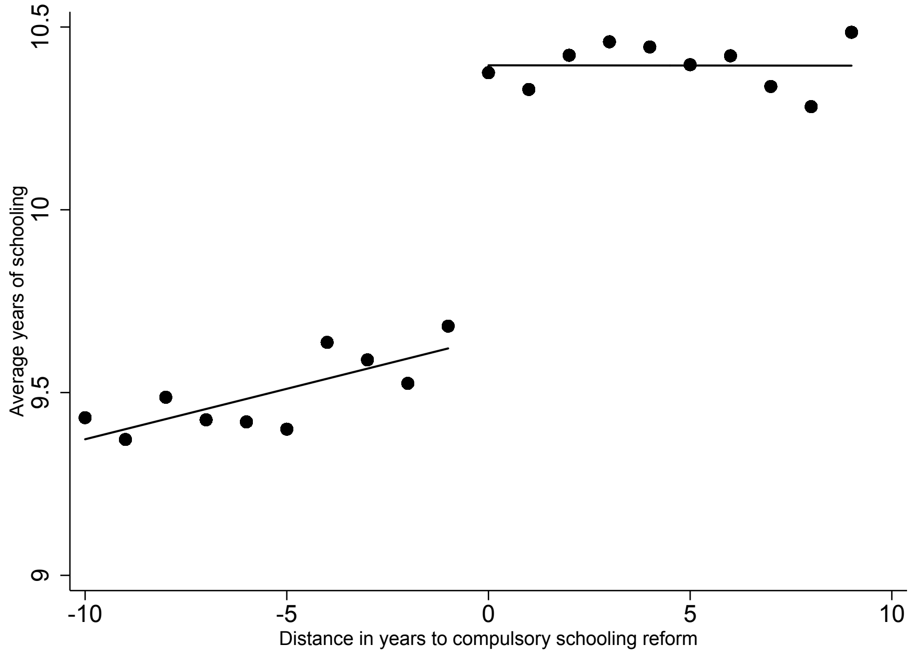
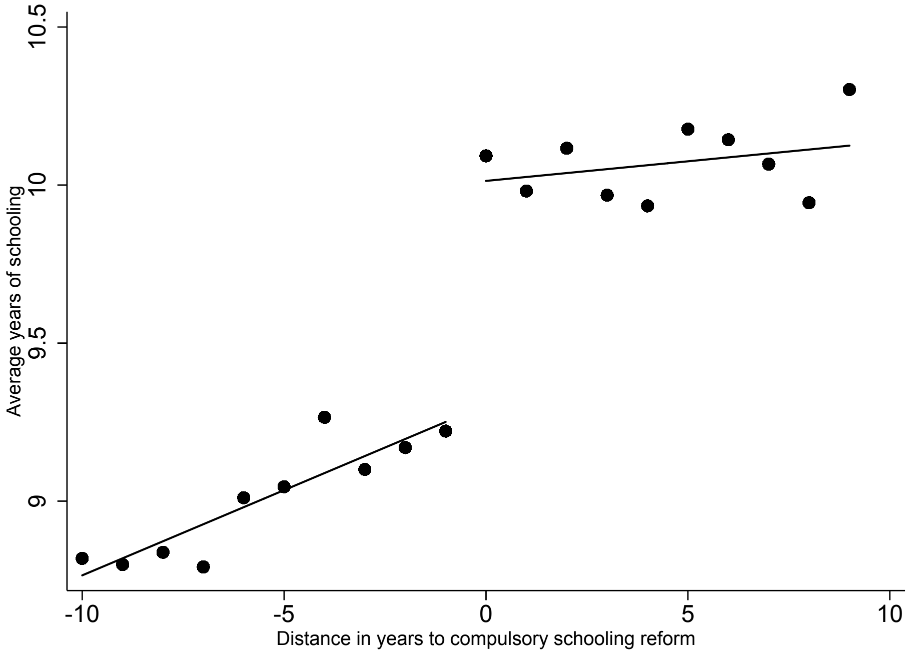
Figure 1: The effect of the reform on average years of schooling; *Sample I*



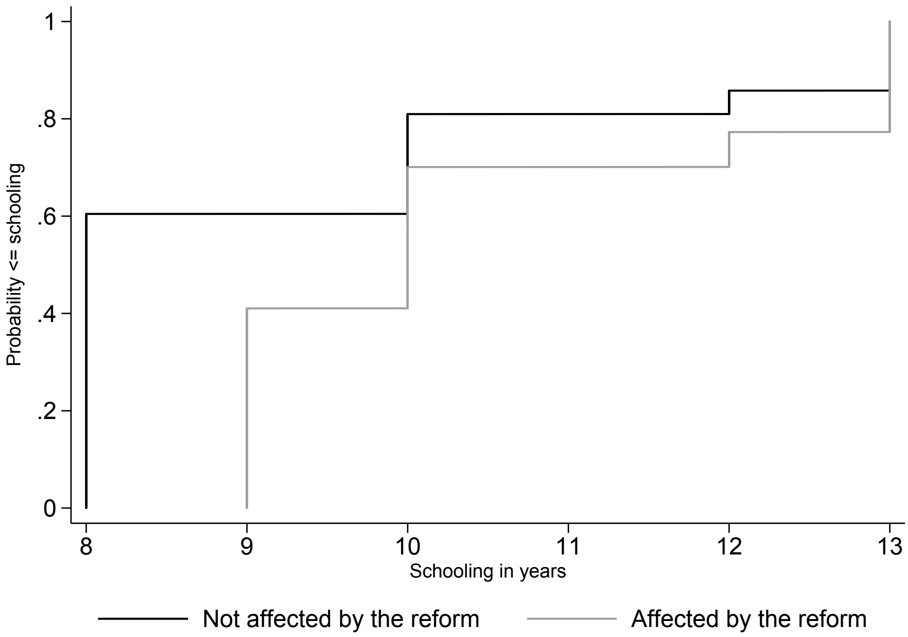
Source: Socio-Economic Panel (SOEP), own calculations.

Figure 2: The effect of the reform on average years of maternal schooling; *Sample II*



Source: Socio-Economic Panel (SOEP), own calculations.

Figure 3: Cumulative distribution function of schooling by reform exposure



Source: Socio-Economic Panel (SOEP), own calculations.

Table 1: Introduction of the ninth class in the basic track of secondary schooling

State	Year of reform	First affected birth cohort
Hamburg	1949	1934
Schleswig-Holstein	1956	1941
Bremen	1958	1943
Lower Saxony	1962	1947
Saarland	1964	1949
Baden-Wuerttemberg	1967	1953
Hesse	1967	1953
North Rhine-Westphalia	1967	1953
Rhineland-Palatinate	1967	1953
Bavaria	1969	1955

Source: Pischke and von Wachter (2005). Federal states are sorted in ascending order by year of reform.

Table 2: Descriptive statistics

	Not affected by reform		Affected by reform	
	Mean	Std. dev.	Mean	Std. dev.
Sample I:				
Very concerned about immigration	0.317	0.465	0.275	0.447
Years of schooling	9.314	1.849	10.415	1.604
Male	0.495	0.5	0.487	0.5
Year of birth	1941.8	6.456	1955.1	4.577
Age	61.59	7.975	48.07	6.781
Number of observations	8,388		4,810	
Sample II:				
Child very concerned about immigration	0.264	0.44	0.217	0.412
Mother's years of schooling	8.756	1.405	10.097	1.422
Male child	0.483	0.5	0.485	0.5
Mother's year of birth	1940.4	6.651	1954.8	4.704
Child's year of birth	1967.0	8.090	1981.5	6.819
Child's age	36.07	9.356	23.05	6.824
Number of observations	8,628		3,542	

Notes: Concerns about immigration are coded as dichotomous variables, taking on the value one if the individual is very concerned and zero if somewhat concerned or not concerned at all.

Table 3: Schooling and immigration attitudes

Sample I	OLS		IV	
	(1)	(2)	(3)	(4)
	Panel A: OLS and second-stage			
Schooling	–0.044***	–0.044***	–0.060*	–0.063**
	(0.002)	(0.002)	(0.031)	(0.031)
	Panel B: First-stage			
Reform			0.545***	0.539***
			(0.050)	(0.051)
F-statistic			116.8	110.3
Observations	13,198	13,198	13,198	13,198
Federal state fixed effects	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
State-specific cohort trend	No	Yes	No	Yes

Notes: In Panel A, the outcome is a dichotomous variable which takes on the value one if the individual is very concerned about immigration and zero otherwise. In Panel B, the outcome is years of schooling. All regressions also include a male dummy and quadratic in age as controls. Each column represents the coefficient from a different regression. Robust standard errors are clustered at the state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Maternal schooling and offspring's immigration attitudes

Sample II	OLS		IV	
	(1)	(2)	(3)	(4)
	Panel A: OLS and second-stage			
Maternal schooling	–0.029***	–0.028***	–0.083**	–0.068**
	(0.002)	(0.002)	(0.034)	(0.032)
	Panel B: First-stage			
Reform			0.681***	0.684***
			(0.075)	(0.078)
F-statistic			82.4	77.0
Observations	12,170	12,170	12,170	12,170
Federal state fixed effects	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
State-specific cohort trend	No	Yes	No	Yes

Notes: In Panel A, the outcome is a dichotomous variable which takes on the value one if the adult child is very concerned about immigration and zero otherwise. In Panel B, the outcome is maternal years of schooling. All regressions also include a male dummy for the offspring. Each column represents the coefficient from a different regression. Robust standard errors are clustered at the state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Robustness check: Geographic mobility

Sample I	Schooling	F-stat
1. Last state of school attendance	−0.083**	130.8
	(0.032)	
	[7,440]	
2. Current state of residence	−0.089**	79.9
	(0.039)	
	[7,440]	

Notes: The outcome is a dichotomous variable which takes on the value one if the individual is very concerned about immigration and zero otherwise. Each row represents a coefficient from a different regression. All regressions are instrumental variable estimations. Regressions also include full set of federal state, birth cohort and survey year fixed effects, state specific cohort trends, as well as male dummy and quadratic in age. The number of observations is in square brackets. Robust standard errors are clustered at the state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Robustness checks

	Sample I		*Sample II*	
	Schooling	F-stat.	Maternal schooling	F-stat.
Main IV estimates from Tables 3 and 4	−0.063**	110.3	−0.068**	77.0
	(0.031)		(0.032)	
	[13,198]		[12,170]	
Panel A: Specification and sample restriction issues				
1. Leaving out the first affected cohort	−0.068*	83.0	−0.112***	52.8
	(0.038)		(0.034)	
	[12,718]		[11,773]	
2. Region-specific year of birth effects†	−0.092**	34.3	−0.101***	62.6
	(0.044)		(0.033)	
	[13,198]		[12,170]	
3. Interview mode	−0.064**	107.8	−0.070**	76.7
	(0.032)		(0.032)	
	[13,198]		[12,170]	
Bandwidth around the reform date:				
4. Six years	−0.087**	75.5	−0.028	31.2
	(0.038)		(0.044)	
	[5,711]		[4,930]	
5. Eight years	−0.085***	92.7	−0.034	69.9
	(0.031)		(0.036)	
	[7,562]		[6,260]	
Additional controls				
6. Political environment when 15	−0.067**	170.7	−0.054*	69.1
	(0.028)		(0.031)	
	[10,356]		[8,984]	
7. Economic environment, ages 12-18	−0.066**	96.0	−0.066**	39.9
	(0.029)		(0.032)	
	[8,063]		[6,979]	
8. Economic environment, survey year	−0.064**	108.2	−0.066**	77.2
	(0.031)		(0.032)	
	[13,198]		[12,170]	
Panel B: Alternative outcomes				
9. Ordered IV probit	−0.152**	110.9	−0.168**	76.5
	(0.077)		(0.068)	
	[13,198]		[12,137]	
10. Proportion of years being very concerned††	−0.043*	87.7	−0.054**	76.2
	(0.023)		(0.022)	
	[13,198]		[12,170]	
Panel C: Placebo test				
11. Placebo reform	0.202	2.5	0.230	0.5
	(0.205)		(0.509)	
	[13.198]		[12.170]	

Notes: The outcome is a dichotomous variable which takes on the value one if the individual is very concerned about immigration and zero otherwise. All regressions are instrumental variable estimations. Regressions also include a full set of federal state, birth cohort and survey year fixed effects, state specific cohort trends and male dummy. *Sample I* specifications also include a quadratic in age. †Regional split is as follows: Northern Germany: Hamburg, Bremen, Schleswig-Holstein and Lower Saxony; Central Gemany: North Rhine-Westphalia, Hesse, Rhineland-Palatinate and Saarland; Southern Germany: Baden-Wuerttemberg and Bavaria. ††Includes full set of dummies of how many times the respondent answered to the outcome question and average age over the available panel years as a control for *Sample I*. The number of observations is in square brackets. Robust standard errors are clustered at the state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

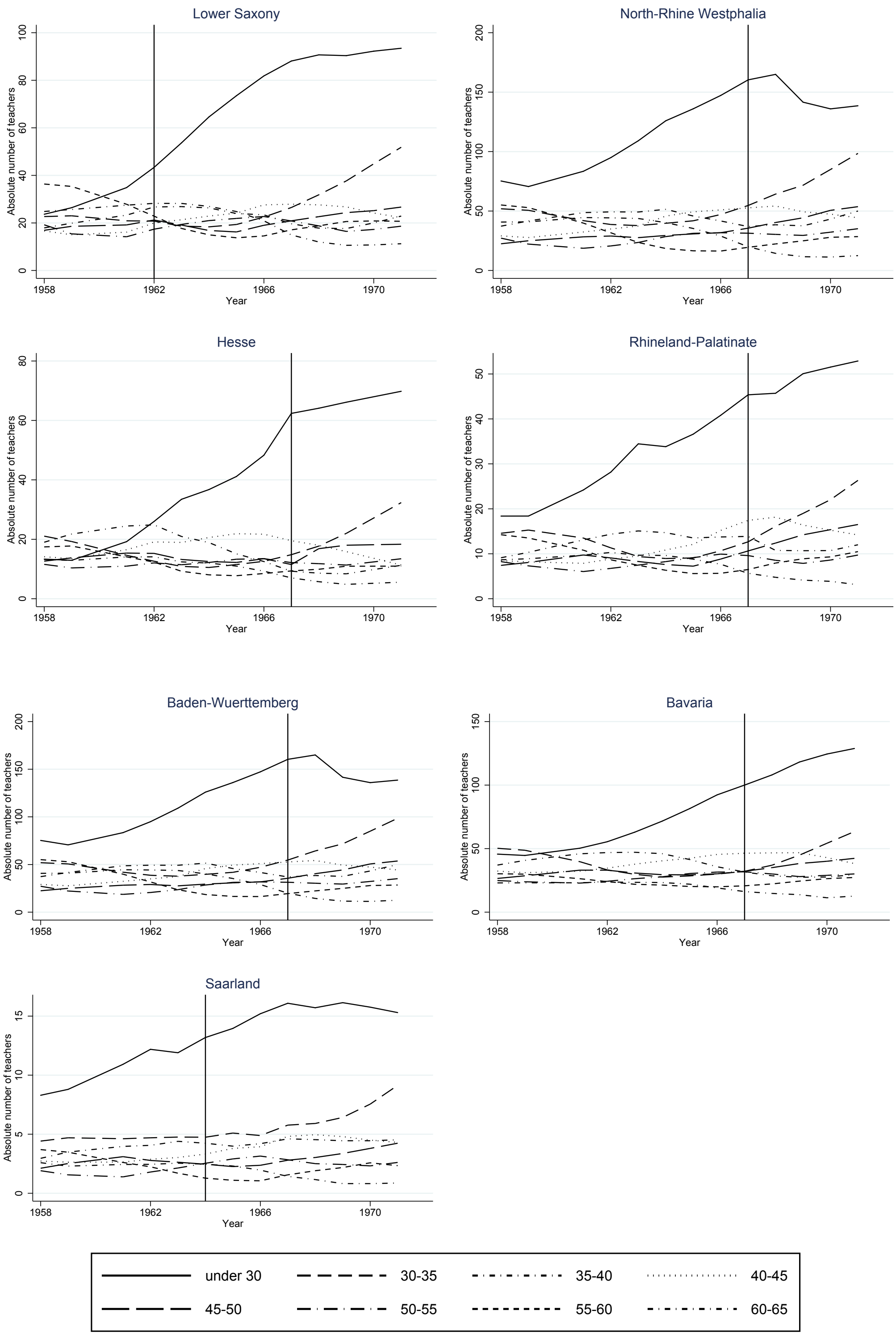
Table 7: The effect of schooling on potential channels

Sample I	Schooling	F-stat	Obs.
Panel A: Labour market outcomes			
1. Labour force status	0.017	110.2	13,198
	(0.028)		
2. Unemployed	–0.021	81.7	8,000
	(0.025)		
3. Ever unemployed	–0.036	89.9	12,088
	(0.029)		
4. White collar occupation	0.058	40.5	6,857
	(0.039)		
5. Civil servant	0.015	41.8	6,732
	(0.039)		
6. Unlimited labour contract	–0.053	39.4	7,003
	(0.036)		
7. Monthly labour earnings†	0.030	59.0	6,159
	(0.052)		
8. Household income††	0.044	97.4	11,926
	(0.037)		
Panel B: Concerns			
9. Very concerned about own job security	–0.036	49.9	6,978
	(0.025)		
10. Very concerned about own economic situation	–0.020	88.3	13,161
	(0.021)		
Panel C: Social capital			
11. Trust‡	0.176**	49.6	9,526
	(0.081)		

Notes: The outcomes are in row headings. Each row represents a coefficient from a different regression. All regressions are instrumental variable estimations. The regressions additionally control for a full set of federal state, birth cohort and survey year fixed effects, state specific cohort trends, as well as male dummy and quadratic in age. †Labour earnings refer to net salary in the previous month and are measured in logs. ††Monthly HH income is measured in logs. ‡Measures on trust are standardised with mean zero and standard deviation of one. Robust standard errors are clustered at the state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

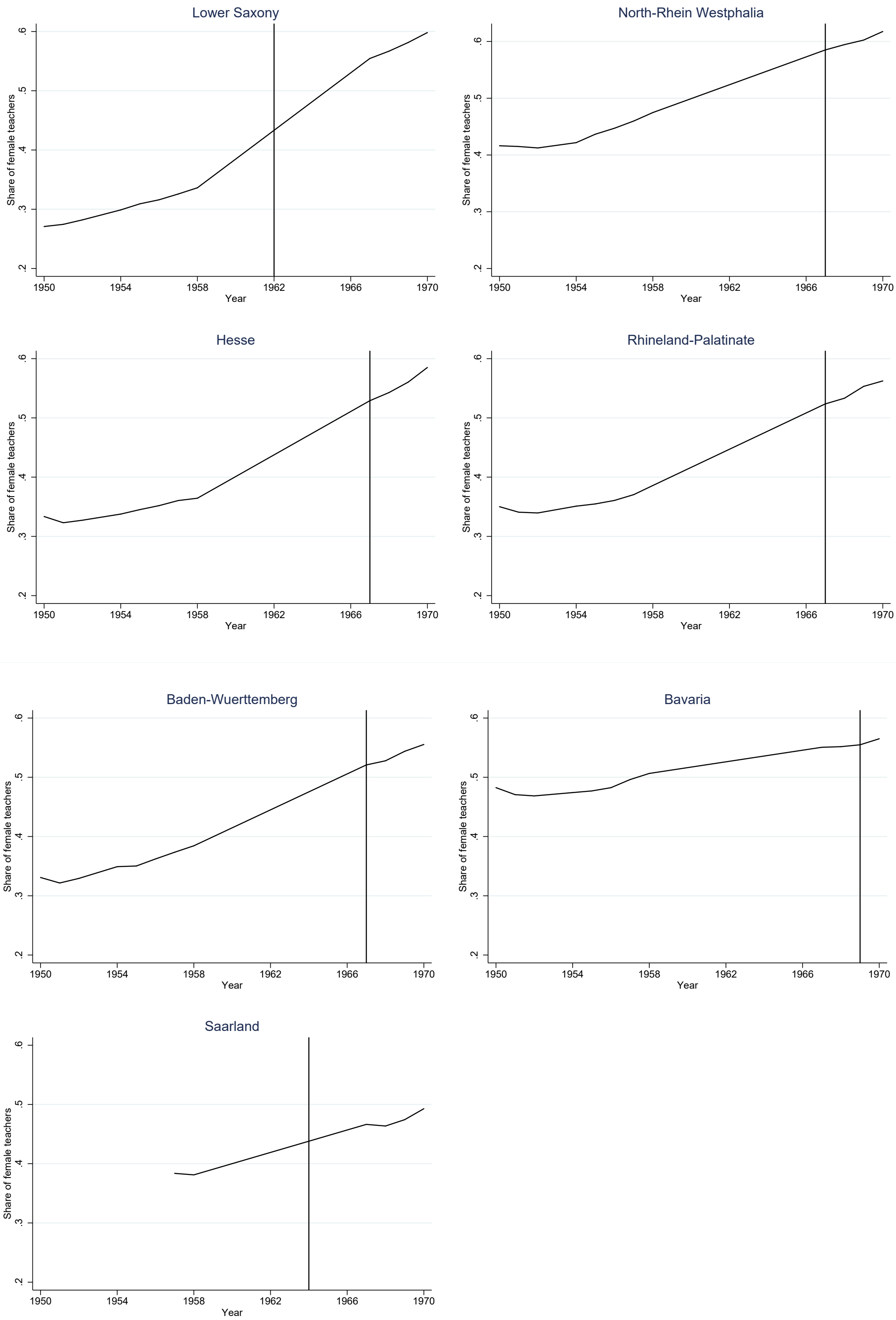
A Appendix

Figure A.1: Absolute number of teachers in basic track schools, in hundreds, by age groups and state



Source: Statistisches Bundesamt, Allgemeinbildende Schulen, Bildungswesen, Fachserie A/10/1.

Figure A.2: Share of female teachers in basic track schools by state



Source: Statistisches Jahrbuch für die Bundesrepublik Deutschland, 1952-1972

Table A.1: Number of observations in each state by reform exposure

State	*Sample I*		*Sample II*	
	Not Affected	Affected	Not Affected	Affected
Hamburg	22	282	32	205
Schleswig-Holstein	176	422	183	351
Bremen	54	86	45	65
Lower Saxony	823	852	883	654
Saarland	26	7	3	30
Baden-Wuerttemberg	1,217	606	1,461	508
Hesse	840	408	883	258
North Rhine-Westphalia	2,598	1,182	2,468	801
Rhineland-Palatinate	678	331	677	211
Bavaria	1,954	634	1,993	459

Federal states are sorted in ascending order by year of reform.

Table A.2: The effect of the reform on the track choice

Dependent variable	Reform	St. error	Obs.
Basic track	−0.011	0.017	13,198
Intermediate track	0.008	0.016	13,198
Academic track	0.003	0.013	13,198

Notes: The outcome is a dichotomous variable which takes on the value one if the individual attended the track mentioned, and zero otherwise. Each row represents a coefficient from a different regression. Regressions also include a full set of federal state, birth cohort and survey year fixed effects, state specific cohort trends, as well as a male dummy and quadratic in age. Robust standard errors are clustered at the federal state-year of birth level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.3: The curriculum for the 9th year of schooling in the various federal states

State	Summary of the content and goals	References
Hamburg	1. Competence in cultural technology 2. Overview of the political life 3. Vocational guidance	Nordrhein-Westfalen (1962), page 59
Schleswig-Holstein	**Historical-political topics for the 9th year** “The purpose of teaching history and politics education in the 9th year is directly related to current political, social, economic and cultural life, and to tasks which require a sense of responsibility and active participation.” **Outline** 1. Help the workers: The social question 2. Our village (our city) as a political-economic community 3. Our home–Schleswig-Holstein–as a federal state 4. Democratic state under rule of law - dictatorship 5. Divided Germany - indivisible Germany 6. Towards European unity 7. Efforts to secure world peace in our time 8. Billions of people inhabit the globe: are they all brothers?	Engelhardt and Jahn (1964), page 188 Nordrhein-Westfalen (1962)
Bremen	“The 9th year of schooling in Bremen is dedicated in particular to a more in-depth general education of people, and primarily supports all measures serving this purpose.”	Nordrhein-Westfalen (1962), page 60
Lower Saxony	“Political upbringing and education will help to fulfil the mandate of the legislature for schools to prepare the young people for life and work, and to make them committed citizens of a democratic and social legal state on the basis of Christianity, Western culture and German education.	Engelhardt and Jahn (1964), page 174

North Rhine-Westphalia	**Four educational tasks:** 1. Providing a more in-depth general education 2. Providing and reinforcing political education 3. Practising, acquiring and expanding basic skills and knowledge 4. Practical application	Nordrhein-Westfalen (1962), page 42
Hesse	**“The meaning and role of the 9th year** “To give young people a more in-depth general education and strengthen their character. Since the introduction of the 9th year, school can now provide a better political education than previously. Young people are becoming mature enough to formulate basic criticism, for “coexistence” and for fundamental insights: co-responsibility for the common good, preservation of human dignity, social justice and observance of the rules in the political struggle. The school environment and classroom style must correspond to these basic democratic values.	Engelhardt and Jahn (1964), page 7, 12
Baden-Wuerttemberg	**“From the education plan for the 9th school year”** “Even for a political consideration of political problems, young people have become more mature, so they can now gain a deeper historical awareness.” ...“Bearing this in mind, recent history is to be examined once again in the context of political education.” ...“Once treatment of the historical material has been completed to a certain extent by the end of the 8th school year, an integrated overview of history and social studies is now provided, aiming at democratic political education.”	Engelhardt and Jahn (1964), page 165-166
Bavaria	**“The 9th year of schooling”** “This provides an in-depth insight into professional, economic and cultural life, strengthens young people’s character, promotes active participation in the community and seeks to reinforce their values at a development stage when they are particularly impressionable.”	Engelhardt and Jahn (1964), page 168

Notes: We were unable to find comparable information for the federal states Rhineland-Palatinate and Saarland.

Table A.4: Timing of the reform and pre-reform state characteristics

	Reform			
Student-teacher ratio	–0.004			0.005
basic track	(0.012)			(0.014)
Share migrants		–0.143		–0.140
		(0.122)		(0.119)
Share males		–0.007		–0.010
		(0.031)		(0.030)
GDP per capita		0.000		0.000
		(0.000)		(0.000)
Share unemployed		–0.049		–0.076
		(0.051)		(0.054)
Share CDU politicians			–0.005	–0.012
			(0.006)	(0.008)
Constant	0.080	0.494	0.136	0.870
	(0.403)	(1.582)	(0.205)	(1.447)
Observations	136	136	136	136
R-squared	0.541	0.570	0.544	0.582

Notes: The time period covers the year 1950 up to the reform year in each state. Due to unavailability of reliable data before 1950, all federal states, but Hamburg, are included. The dependent variable is a binary reform indicator that takes on the value zero throughout the pre-treatment period for each states, and switches to one once the compulsory schooling is introduced. Further treatment periods are discarded. The regressors are pre-reform state characteristics from the Federal Statistical Office and the Federal Employment Agency. All shares are measured in percent and missing values are replaced by the closest available value. The share of CDU politicians refers to the Landtag (state parliament). OLS regression with federal state and year fixed effects. Robust standard errors are clustered at the state-year level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Interview modes by share of respondents

Interview mode	*Sample I*	*Sample II*
With interviewer's assistance	0.45	0.4
Oral interview	25.44	23.56
Self completed, with interviewer's help	16.28	22.36
Self completed, without interviewer's help	2.76	3.49
Oral and written	2.73	3.22
CAPI	47.60	38.96
Snail-mail	4.73	8.02
Phone interview	0.01	0.00
Proxy	0.01	0.00
Obs.	13,198	12,170

Source: SOEP, v30. Share of respondents is measured in percent.

Table A.6: Dependent variable: immigration concerns

Explanatory variable		F-stat	Obs.
Panel A: Labour market outcomes			
1. Labour force status	0.009 (0.018)	107.1	13,198
2. Unemployment	–0.017 (0.029)	73.4	8,000
3. Ever unemployed	–0.015 (0.015)	85.8	12,088
4. White collar occupation	–0.123 (0.096)	27.7	6,857
5. Civil servant	–0.019 (0.046)	42.2	6,732
6. Unlimited labour contract	0.037* (0.019)	38.8	7,003
7. Monthly labour earnings †	–0.055 (0.044)	64.7	6,159
9. Household income††	–0.010 (0.040)	85.1	11,926
Panel B: Concerns			
9. Very concerned about own job security	0.194*** (0.038)	46.1	6,978
10. Very concerned about own economic situation	0.183*** (0.026)	87.0	13,161
Panel B: Social capital			
11. Trust‡	–0.082*** (0.015)	58.1	9,526

Notes: The outcome is a dichotomous variable which takes on the value one if the individual is very concerned about immigration and zero otherwise. All regressions are instrumental variable estimations. The regressions also control for full set of federal state, birth cohort and survey year fixed effects, state specific cohort trends, as well as male dummy and quadratic in age. †Monthly labour earnings refer to net salary in the previous month and are measured in logs. ††Monthly HH income is measured in logs. ‡Measures of trust are standardised with mean zero and standard deviation of one. Robust standard errors are clustered at the federal state-year of birth level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.