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DOES EDUCATION AFFECT IMMIGRATION ATTITUDES?

*Evidence from an education reform**

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Abstract

Empirical research consistently finds that people with high education have more liberal immigration attitudes. To what extent this relationship reflects a causal effect of education is, however, largely unknown. We rely on the staggered introduction of a major Norwegian education reform to get exogenous variation in respondents' level of education. The reform lifted the bottom of the education distribution by increasing the compulsory years of education by two years. We find no significant differences in immigration attitudes between those who were educated in the old and the new education system. Our results suggest that if education has a causal effect on immigration attitudes, it is likely to operate on other education margins.

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EDUCATION AND IMMIGRATION ATTITUDES

Does staying longer in school cause more liberal views on immigration? A number of studies find a positive association between education and liberal views of immigration and immigrants,¹ which has been interpreted in several ways. The *cognitive framework* argues that more education causes higher levels of tolerance towards cultural diversity (Quinley and Glock 1979; Bobo and Licari 1989; Hainmueller and Hiscox 2007; Van der Waal et al. 2010). In one version of this argument, Quinley and Glock (1979) claim that formal education provides people with cognitive skills to detect prejudice, understand its dangers, and reject it. Another version emphasizes how receiving more education might correct misperceptions and misinformation about the roots of majority-minority differences and about the actual levels of immigration (Strabac 2011).² The *labor market framework* contends that low-educated citizens are more likely to compete with immigrants on the labor market and to experience potentially negative externalities from immigration on their workplaces, neighborhoods, and schools. These conflicts might create less tolerant immigration attitudes (Scheve and Slaughter 2001; Mayda 2006; O’rourke and Sinnott 2006; Card, Dustmann, and Preston 2012). There are thus several mechanisms through which citizens’ level of education can influence their stance on immigration issues.

There is, however, scant evidence of to what degree the education gap reflects causal effects of education or mainly reflects selection into education. Recent empirical work on, for example, the relationship between education and voter turnout suggests that selection explains a large part of the correlation between education and the propensity to vote (Persson 2015). In a similar manner, childhood factors—such as parental political socialization and socio-economic resources, neighborhood socialization and opportunities, and personality traits—might explain both immigration attitudes and level of education. As Hainmueller and Hopkins (2014, 241) conclude in their review of the immigration attitudes literature, “quasi-experimental manipulation of education

¹See e.g Hainmueller and Hiscox (2007) and Sides and Citrin (2007).

²The gap might also reflect stronger social desirability pressure among the highly educated (Jackman and Muha 1984; Janus 2010).

levels in the study of immigration attitudes would be of substantial value in unpacking the causal mechanisms that underpin the education effect.”

In this short article, we provide such a causal assessment of the link between education and liberal immigration views. We do so by exploiting a comprehensive education reform in Norway that extended compulsory schooling from seven to nine years. The key to our approach is that the reform was implemented in a staggered fashion across the around 400 municipalities, which creates plausibly exogenous variation in levels of education across cohorts within municipalities. In this note we combine data on the municipal timing of the education reform with survey data that includes information on the respondents’ municipality of upbringing as well as their immigration attitudes when they were in their 50s. Using this data we estimate the effect of being educated under the new school system versus the old school system. Due to the ethnic and religious homogeneity of Norway in the 50s and 60s, all respondents grew up in homogenous contexts. Diversity might be more difficult to handle when you are less used to it (Hopkins 2010), implying that the type of reform we study might plausibly have effects on immigration attitudes in our setting.³ Our results helps understanding to what degree the education gap in views on immigration is causal or due to selection.

Importantly, the variation provided by the reform mainly identifies an education effect that is not mixed with the effect that education may have on immigration attitudes through labor market competition or neighborhood effects. The education reform lifted the bottom of the education distribution: those who previously completed only seven years of education were now forced to take at least nine years of education. The reform did not, however, move many people into higher education (Aakvik, Salvanes, and Vaage 2010, 492) and consequently into high-skill occupations, where immigrants are complements rather than competitors on the labour market (see Dustmann, Frattini, and Preston 2013). The reform therefore had limited effect on the exposure to labor market competition from immigrants. An effect of the reform

³Low poverty rates and high levels of education are contextual characteristics that might push in the other direction.

will thus mainly be due to an increase in years spent at school, which will reflect the cognitive and informational mechanisms between education and immigration attitudes.

To our knowledge, only two previous studies have studied the effect of educational reforms on immigration attitudes using a causal inference framework (d’Hombres and Nunziata 2016; Cavaillé and Marshall 2017). These papers exploit cross-country variation in the timing of national-level educational reforms in Europe, combined with survey data from the European Social Survey, to study attitudes to immigration. Both studies document a positive effect of education; yet, Cavaillé and Marshall (2017) find important variation in the effect. Most notably, their results show a small and insignificant effect in Sweden, which implemented the reform at about the same time as Norway. Our approach complements these studies but has several additional benefits. We compare municipalities within the same institutional and cultural environment and an, at the time, small immigrant population, which was quite evenly spread across the country. There is therefore little reason to believe that the timing of the educational reform across municipalities was in any way connected to trends in immigrant attitudes. It also has the benefit that by using subnational variation across municipalities in the timing of the same school reform, we can control for general cohort effects. This control alleviates the concern that we conflate the effect of the education reform with cohort trends in views of immigration. In contrast, Cavaillé and Marshall (2017) ignore that the Swedish education reform was implemented in staggered fashion across municipalities (Holmlund 2008).

In line with the selection effect story, we find no evidence of an effect of the education reform on immigration attitudes. Our coefficient estimates are not only statistically insignificant, they are also close to zero in substantive terms. Although our results suggest that expansions of compulsory education does not change immigration attitudes, they are not informative about causal effects from manipulations of other education margins. For instance, it is possible that reforms which expand access to higher education might causally affect immigration attitudes. Moreover, the results in

d’Hombres and Nunziata (2016) and Cavaillé and Marshall (2017) suggest that effects of education reforms on immigration attitudes are heterogeneous across countries. Before we return to these discussions in the concluding remarks, we present the education reform and the empirical strategy in the next section and the empirical results in the ensuing section.

RESEARCH DESIGN

We exploit a large-scale Norwegian reform passed in 1959 that required all Norwegian municipalities to increase compulsory schooling from seven to nine years. Crucially, however, the year of the implementation was left to the municipalities, leading the reform to be introduced in a staggered fashion between 1959 and 1972. In the Online Appendix A.1, we describe the reform in more detail and review the literature on the causal effects of the reform on citizen’s education and socio-economic outcomes in adulthood. We also show that the timing of the reform is plausibly exogenous.

Using the education reform, the crux of our empirical design is to compare the difference in attitudes between adjacent cohorts within the same municipality, where some cohorts were educated in the old and some in the new education system. We study the cohorts born between 1947-1958 (Black, Devereux, and Salvanes 2005), which ensures that we have within-municipality variation in what school system the respondents faced.

To capture the causal effect of education, induced by the education reform, we use two different versions of fixed-effects regression models. The first model takes the following form:

$$Y_i = \gamma_c + \sigma_t + \alpha \cdot \text{NEW SYSTEM}_{c,m} + X_i' \beta + \epsilon_i, \quad (1)$$

where i , c , m , and t indexes individuals, birth cohort, municipality, and survey year, respectively. `NEW SYSTEM` is a dummy variable equal to 1 if the reform was implemented in the year the respondent started eighth grade, i.e., the respondent were educated in the new school system. In this specification, we compare the attitudes between respondents who were and were not exposed to the reform *within the same cohort*. The birth cohort fixed effects, γ_c , remove general cohort effects in immigration

attitudes. In addition, the survey year fixed effects σ_s remove common time trends in attitudes. The exogenous vector of covariates X_i' contains a dummy for female, as well as a set of dummy variables for whether the individual grew up in a central, suburban, or rural neighborhood (see Online Appendix A.2 for details). The covariates are not necessary for identification but are included to improve precision.⁴ Standard errors are clustered on municipalities, m , since the reform was implemented at the municipal level.

Despite the results indicating that the timing of reform was exogenous to observable characteristics of the municipalities, one might still worry about average differences in immigration attitudes between early and late adopters of the reform. The second version of the model therefore additionally includes municipality fixed effects:

$$Y_i = \eta_m + \gamma_c + \sigma_t + \alpha \cdot \text{NEW SYSTEM}_{c,m} + X_i' \beta + \epsilon_i. \quad (2)$$

In this model we add municipality fixed effects, η_m , which implies that we identify the reform from the within-municipality variation, i.e., we identify the effect from variation in the year of implementation. With municipality and cohort fixed effect, this model is in essence a difference-in-differences model where α is the average treatment effect of the reform, assuming that there are no other events that simultaneously influenced the treated cohorts and immigration attitudes.

To estimate the regressions in Equations 1 and 2, we need survey data that include immigration attitudes, year of birth, and, crucially, municipality of upbringing. Few surveys include the latter variable, but the 2005-2013 surveys from the bi-annual survey program *Norsk Monitor* have this information. The Online Appendix A.2 describes *Norsk Monitor*, details the survey questions we rely on in the analysis, provide descriptive statistics, and discusses potential concerns regarding low response rates, the number of observations per municipality, and recall errors in respondents' reporting of municipality of upbringing. Reassuringly, our survey data supports the key assumption that the timing of the reform is uncorrelated with observable municipality and individual characteristics and reproduce downstream effects of the

⁴Table A-11 shows that our results and conclusions remain similar if we exclude the covariates.

reform which has been documented in previous research.⁵

We study two types of outcomes: immigration attitudes and vote choice. To capture individuals' underlying, latent *attitude toward immigration*, we use the survey's five items that asks about different aspects of immigration. The first item deals with receiving refugees, that is whether Norway should or should not strive to receive more refugees. The second concerns the cultural contribution of immigrants, in particular whether cultural diversity is an asset or threatens Norwegian culture. The third item asks about the economic contribution of immigrants; whether they are hardworking contributors or whether they exploit welfare arrangements. The fourth asks whether limiting immigration is a particularly important issue to solve. Finally, the fifth question asks whether the respondent like or do not like that Muslim denominations have been established in Norway. The five items thus include both cultural and economic aspects of immigration. We construct a factor score and an index of the proportion positive answers (see Online Appendix A.3).

The second type of outcome is *vote choice*; specifically, it is a dummy for whether the individual would vote for the Progress Party (Frp) if there was a national election next Monday. The Progress Party is the only party with representation in the Norwegian parliament which explicitly state in their party program that they want to reduce immigration. As with the immigration attitudes, we code those answering "would not vote" and "don't know" as zero.

⁵First, we do not find a relationship between the reform and self-reported economic situation in childhood (see Table A-6). Second, we find that respondents educated in the new system are more likely to have education beyond the lowest level of education, higher household income, and perceive their current economic situation as being "above average" (see Table A-7). The effect of the reform on level of education could potentially be the first stage in an IV estimation of the effect of years of education on immigration attitudes, by using the reform as an instrumental variable (Black, Devereux, and Salvanes 2008). Unfortunately, the education level categories in Norsk Monitor do not discriminate well between the lower education levels before and after the reform: The question on level of education has five answer categories. The lowest category pools together respondents with seven (primary education, "Folkeskole") and eight years of education (continuation school, "Framhaldskole"), while the second lowest level of education pools together respondents with nine (middle school, "ungdomsskole") and ten years of education (academic track, "realskole"). This classification problem weakens the relationship between the reform and years of education, and results in a first stage which is too weak to avoid serious weak instrument bias in the second stage (see columns 3 and 4 in Table A-7). We therefore continue with the reduced form estimate of the reform on immigration attitudes. In size, however, the estimated effect of the reform on years of education (.2 with and without the municipality fixed effects) is similar to the effects reported in the literature (.25 in Aakvik, Salvanes, and Vaage 2010).

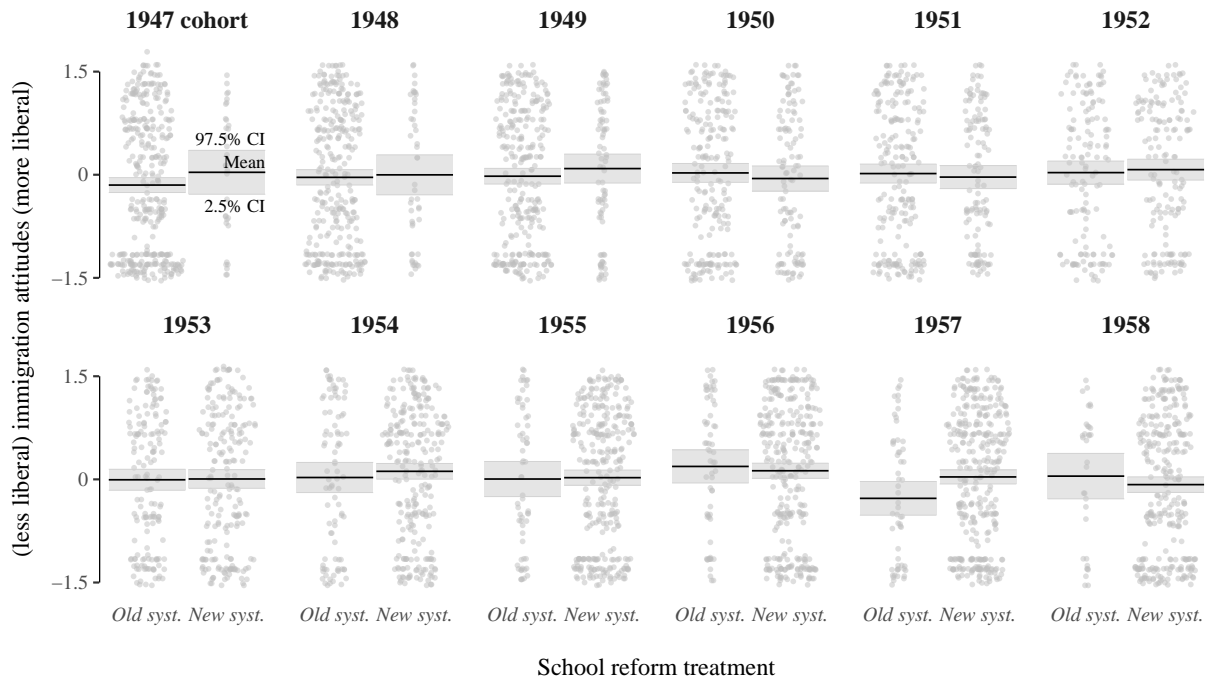


Figure 1: Distribution and means of the FA immigration index for respondents who where educated in the old and new education system within each cohort (index residualized on survey year and covariates).

As in previous research, we find a strong positive correlation between education and more liberal immigration attitudes (see Table A-9). Having more than primary education is associated with being half a standard deviation more liberal on the immigration attitude index, as well as having about 7 percentage points lower probability of intending to vote for the Progress Party. As pointed out above, however, these correlations cannot be interpreted as causal, since they may reflect characteristics of the individual that has both an impact on educational level and immigration attitudes. We therefore turn to the main analysis, where we evaluate the causal effect of the education reform on views of immigration.

A NULL EFFECT OF EXTENDING COMPULSORY EDUCATION

To provide graphical evidence of our empirical strategy and results, Figure 1 plots immigration attitudes separately for respondents who where educated in the old and new education system *within each cohort*. The figure shows the distributions of the individual observations (grey dots) as well as the means with 95 confidence interval (black lines and grey rectangles) for each of these categories. Since the immigration

attitude index has been residualized on survey year and the covariates before being graphed, the figure corresponds to the identification strategy in Equation 1 above. An empirical pattern to notice is that as move from the 1947 toward the 1958 cohort, more and more respondents are treated, that is, are educated in the new education system. This pattern mirrors the gradual implementation of the reform. More importantly, we can see that for each of the cohorts the difference between the means for respondents in old and new education system are either nonexistent or substantively negligible. A *t*-test of the difference in means between the groups also reveals that the differences are statistically insignificant in every cohort, with the partial exception of 1957.⁶ The graphical evidence consequently portrays that the additional years in school induced by the reform did not make individuals more liberal in their views of immigration.

This conclusion also holds when we more rigorously estimate the effect of education on immigration attitudes and vote choice using Equations 1 and 2 above. The coefficients on the education reform, along with 95 percent confidence intervals are plotted in Figure 2 (see Table A-10 for the full regression results). The results mirror Figure 1 and are thus easy to summarize. First, the coefficients are statistically insignificant and substantively small in size. Even if we were to disregard the confidence intervals, the point estimate in, for instance the top row, suggest an effect of one-twentyfifth of a standard deviation on immigration attitudes. Second, the point estimates do not show a consistent direction of effects. We find both positively and negatively signed coefficients for being educated in the new school system.⁷ Third, the standard errors are also small, which indicates that the null effect is precisely estimated. Together, the insignificant effects, the varying direction of the effects, and the in general small sizes of the coefficients, all point in the direction that there is no causal impact of the educational reform on immigration attitudes.

⁶*P* = .08 in 1957 (see Table A-8).

⁷The same conclusions hold when we separately analyze the individual items underlying the immigration views index (see Table A-12).

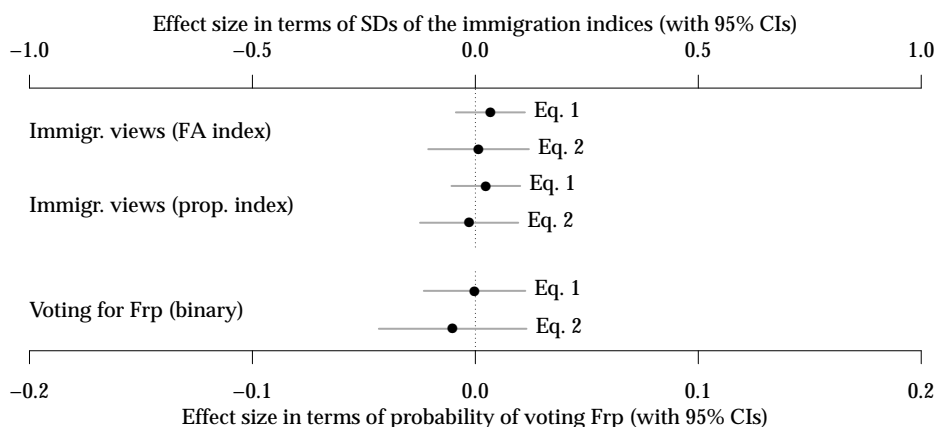


Figure 2: The effect size of the new education system on views of immigration (upper panel) and vote choice (lower panel). See Table A-10 for full regression results.

CONCLUDING REMARKS: EFFECTS ALONG OTHER EDUCATION MARGINS?

In this study, we find no causal effects of a large-scale Norwegian education reform on immigration attitudes and anti-immigration voting. The reform increased the level of formal education, in particular among the low educated. Our results consequently suggest caution when interpreting the commonly found positive association between level of education and liberal immigration attitudes. One interpretation of our null results is that the correlation between education and immigration attitudes is purely spurious and reflects selection into education. However, since the reform mainly affected those with low levels of education, we cannot rule out that an education reform which manipulates other education margins, such as the expansion of higher education, induces individuals to hold more liberal immigration attitudes. One reason might be that the cognitive effects of formal education on the ability to reject prejudice works for those with medium or high levels of education. This interpretation is consistent with research in the literature on democratic values, where Zellman and Sears (1971) find that U.S. children in the ages 9-14 are unable to internalize abstract ideals regarding free speech for out-groups. Another reason might be that anti-immigration attitudes and voting is more strongly linked to labor market and economic conflicts than previous research indicates,⁸ implying that a reform which

⁸See Finseraas, Røed, and Schøne (forthcoming), but see Naumann, Stoetzer, and Pietrantuono (forthcoming) and Hainmueller, Hiscox, and Margalit (2015) for the opposing view.

has strong downstream effects on individuals' position in the labor market will have an impact on attitudes. Finally, the results in d'Hombres and Nunziata (2016) and Cavaillé and Marshall (2017) indicate that comparable types of education reforms might have effects in other countries. Cavaillé and Marshall (2017) find, however, no effect of a similar reform in Sweden, a country which is similar to Norway along many relevant dimensions. Thus, we suspect that the institutional context is important for whether reforms of mandatory education affects immigration attitudes. In any case, we urge researchers to be more specific about the mechanism and education margin they have in mind when theorizing about the relationship between education and immigration attitudes.

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A ONLINE APPENDIX

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A.1 *The education reform*

The education reform had two main components (see Lie 1973*a,b*; Lindbekk 1993). The first and most important component was that the reform forced the municipalities to extend compulsory schooling from seven to nine years. The starting age was kept unchanged at age seven. Prior to the reform, the students had four choices after completing the compulsory seven years: an academic track which qualified the student for high school; a vocational track; a continuation school track; or leave school. After the reform, the continuation school track was closed down and all students completed six years of primary school and three years of middle school before they could leave school. The second component was a change from a selective to a more comprehensive type of schooling, and implied a standardization of the curriculum.

The aim of the reform, which was implemented by a Social Democratic majority government, was to reduce the disparities in primary education between urban and rural areas. The reform forced the municipalities to implement the changes in their primary education; nevertheless, the exact *timing* of the implementation was left to the municipalities. Even though the Norwegian Parliament passed the reform in 1959, it was implemented locally over the period from 1959 to 1972.⁹ To avoid implementation differences between municipalities with different levels of financial resources, the national government funded most of the reform.¹⁰

To be able to use the reform as a natural experiment, we need that the reform was plausibly exogenous to other changes in the municipalities that might influence immigration attitudes. Figure A-1 illustrates graphically the speed and geographic spread in the implementation of the reform. In panel (a), we show that the year of adoption varied considerably across municipalities, whereas in panel (b) we display that the timing of the implementation was spread out geographically. Moreover, previous research has found that the timing of the reform implementation was uncorrelated with observable characteristics of the municipalities, including average

⁹28 municipalities implemented the reform already in 1959, before its parliamentary adoption.

¹⁰To have their reform costs covered, such as for building new schools and hiring more teachers, the municipalities applied to the Ministry of Education (Black, Devereux, and Salvanes 2008, 1030).

earnings, taxable income and educational levels, average age of the population, region, or the municipality size (Lie 1973^{b,a}; Black, Devereux, and Salvanes 2005). The implementation was instead a “complex adoption process,” where particularly effective local administrators played an important role (Black, Devereux, and Salvanes 2008, 1031). This process led to a staggered implementation of the reform, which we exploit to construct a treatment group which were educated in the new system and a control group which were educated in the old system.

Besides knowing that the reform was uncorrelated with observable characteristics, we also know that the reform increased the educational level for around thirteen percent of the population (Aakvik, Salvanes, and Vaage 2010). According to Aakvik and co-authors’ (2010: Table 7) estimates, the reform reduced the proportion of the population with seven years of schooling as their highest level of education by nine percentage points. It additionally increased the proportion with vocational qualifications as their highest level of education by about three percentage points and the proportion with upper secondary schooling as their highest level by 1.5 percentage points. The reform had a negligible effect on the share having higher education. Notably, our results should therefore be interpreted as an effect of increasing education at the *bottom end* of the education distribution.

Concordant with these educational effects of the reform, previous research has found that the reform had positive and substantive effects on earnings in adulthood (Aakvik, Salvanes, and Vaage 2010), postponed treated mothers’ timing of their first birth (Monstad, Propper, and Salvanes 2008), and increased regional mobility (Machin, Salvanes, and Pelkonen 2012). The reform does not appear to have influenced the propensity to vote (Pelkonen 2012) and to have mixed effects on the intergenerational submission of human capital (Black, Devereux, and Salvanes 2005). No previous study has looked at the reform’s impact on attitudes.

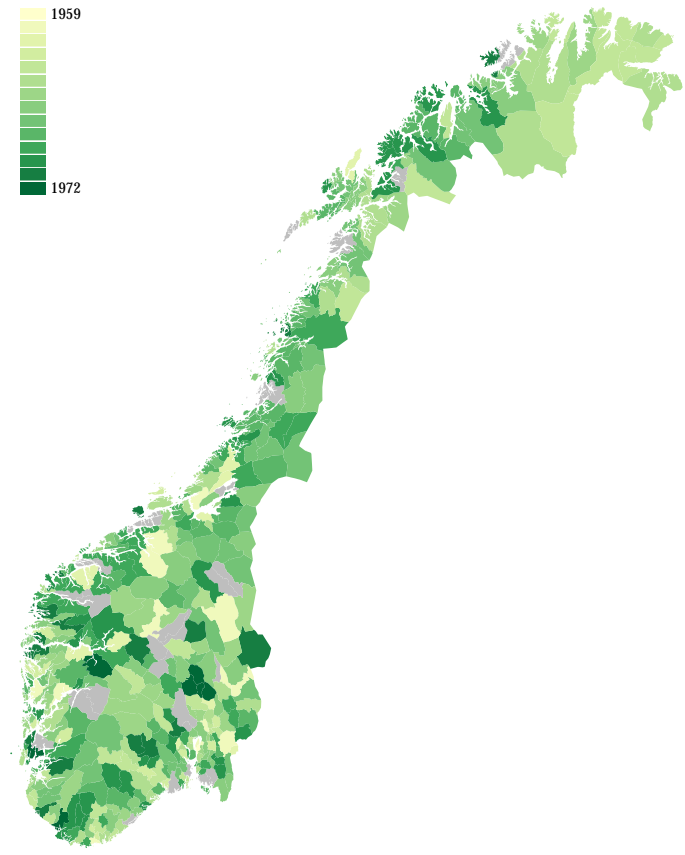
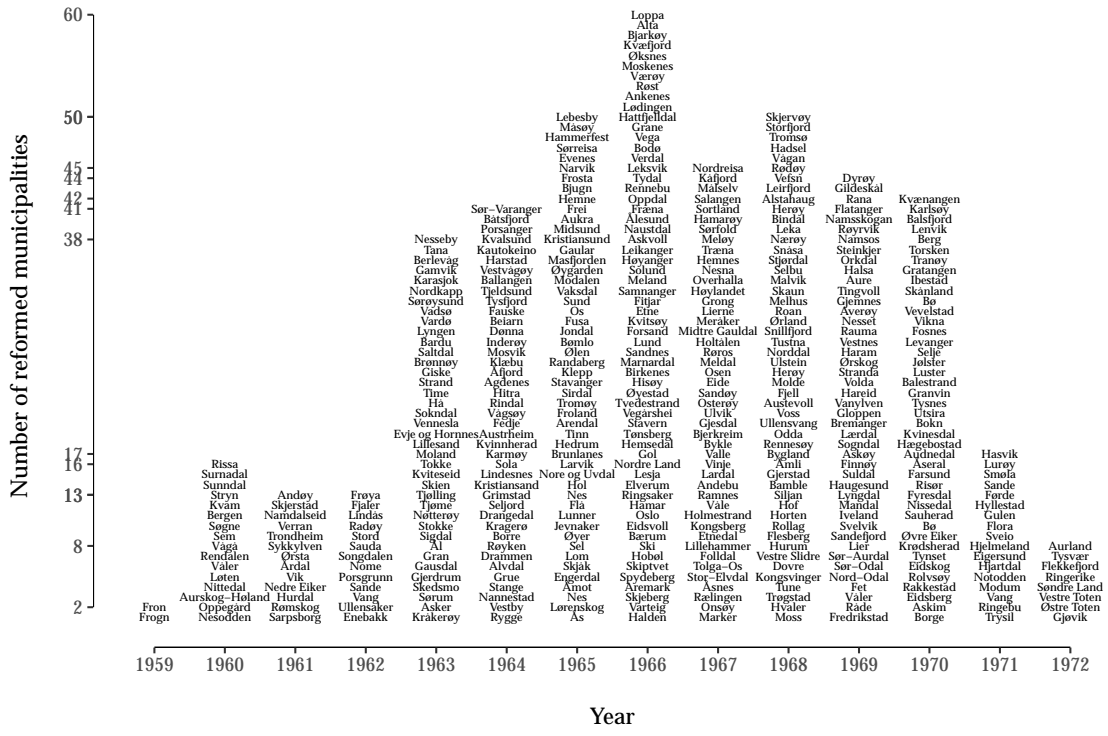


Figure A-1: Municipalities' year of adoption of the education reform.

Source: Norwegian Centre for Research Data's Regional Database

A.2 *The Norsk Monitor surveys and descriptive statistics*

A.2.1 **Norsk Monitor**

Norsk Monitor (NM) is a large sociocultural study which has been conducted biannually since 1985. The data is collected through a short telephone interview and a subsequent self-completed mail questionnaire. The sample is drawn from the population above 15 years old and consist of around 4,000 net respondents in each wave, with a new sample for each wave. The sampling is a simple random sample from telephone directories (landline and cell). We use the 2005-2013 biannual waves. The response rate by wave ranges from 7 to 4 percent, implying that one might think of the sample as a type of convenience sample that is often used in experimental research. The low response rates might be a cause of worry, at least for external validity. To examine the response rate bias in NM, Hellevik (2016) presents a range of analyses comparing estimates from NM to estimates from surveys with much higher response rates and to estimates from administrative data from Statistics Norway. He concludes that the bias for these variables are low. For our purpose, the main concern is whether the response rate is correlated with treatment status, which would be the case if the education reform had an effect on willingness to participate in surveys. We find, however, no significant correlation between treatment status and number of responses per municipality, as we detail in Section A.2.3 below.

A.2.2 **Municipality of upbringing and measurement error**

To identify the respondents that are educated in the old and the new system, we use information about municipality of upbringing and the year of birth. We use the following survey items:

Municipality of upbringing. Where did you live during the main share of your upbringing? State the municipality. (Hvor bodde du i hoveddelen av din oppvekst? Oppgi kommune.) (Survey item Q87)

Year born. What is your age? (Hva er din alder?) (Q310)

To identify year of birth, we subtract the stated age from the year the survey was conducted (Norwegian enrollment rules follow year of birth). Because municipality of upbringing is based on recall, there is potentially measurement error in this variable. Measurement error in the independent variable may bias our results towards zero. It is therefore reassuring that we, in line with previous research, find treatment effects on education, household income, and perceived economic situation (see Table A-7). We can, however, also compare the available information from the survey to information from official resources (Statistics Norway) to check whether measurement error in this variable seems to be an important problem. We do so in two ways.

First, we compare the response rate to the municipality of upbringing question to other questions about current and past place of living. Using our sample, Q87 (municipality of upbringing) has 4,370 non-missing answers (out of 4,920). There is a companion question Q88, clearly less likely to suffer from recall bias, where the respondent is asked to characterize the centrality of their place of upbringing (Q88A). 4,724 people characterize their place of upbringing (Q88A). Thus, the level of missingness is not vastly different on these two questions. Other information that should be more easy to remember in the same set of questions is Q89: “Have you lived in your current municipality all your life or have you moved here after having lived in a different municipality” and Q93: “Do you live on a farm today”. They have non-missing responses of 4,837 and 4,737 respectively. There is therefore no large differences between the response rate on the question of municipality of upbringing and other related questions about municipality of upbringing or residence.

Second, in the 1970 census, Statistics Norway include a binary measure of whether the census unit in which an individual resided was densely or sparsely populated, which we compare to the survey respondents’ characterization of their place of upbringing. We do so in the following way. First, using the 1970 census, we calculate the share of the population in each municipality that lives in densely populated areas.¹¹ Next, we merge this variable with our survey data based on the respondents’ stated

¹¹The Norwegian Center for Research Data provides a 8.33 percent sample of the 1970 census, see <http://www.nsd.uib.no/data/folketellingsdatabanken/>.

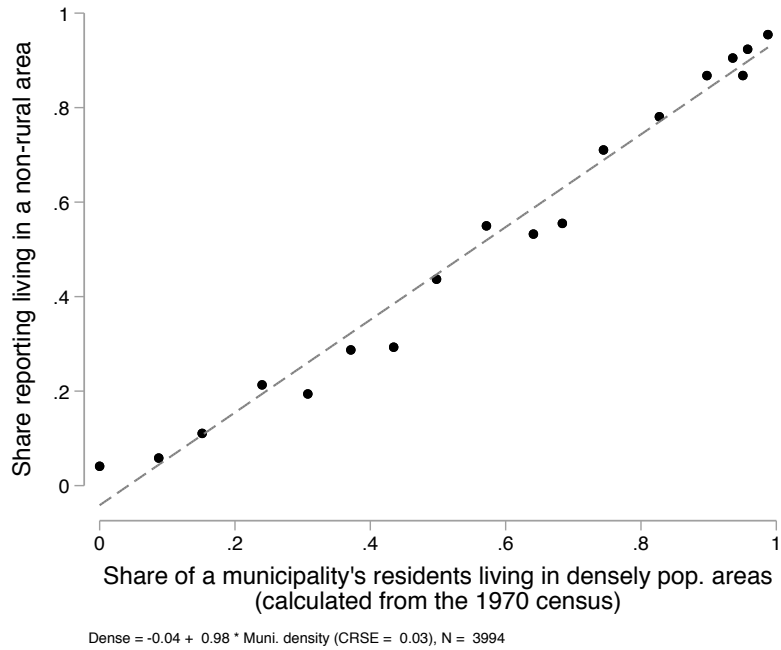


Figure A-2: Binned scatterplot of the relationship between a census-based measure of the share of the municipal population living in densely populated areas (x -axis) and the share of respondents reporting that they grew up in a non-rural area (y -axis).

municipality of upbringing. Finally, we use the survey item in which the respondents were asked to characterize their place of upbringing and create a variable that codes whether the respondents report growing up in a rural area (0) or not (1). An indication of measurement error is to see whether there is a close correspondence between this measure and the municipality measure from the 1970 census.

Figure A-2 displays a binned scatterplot with the results of this exercise. The scatterplot bins the observations into quantiles based on the x -axis variable and then plots the means of the x - and y -axis variables within each bin. There is an extremely tight relationship between respondents' reporting that they grew up in a non-rural area (y -axis) and the share of the population in a municipality living in dense areas. With a regression coefficient of .98, there is almost a perfect one-to-one correspondence between the two measures. Moreover, as Table A-1 displays, there is no difference in this relationship between respondents in the new and old education system.

Together, the evidence presented indicates that the problem with measurement error in the municipality of upbringing is not severe: the response rate to this question

Table A-1: OLS regression of the association between a census-based measure of the share of the municipal population living in densely populated areas and the share of respondents reporting that they grew up in a non-rural area (dependent variable)

	Reported upbringing.	
	(1)	(2)
Dense (census-based)	.97 (.040) <i>.000</i>	.98 (.041) <i>.000</i>
New edu. system	-.013 (.025) <i>.599</i>	-.046 (.028) <i>.099</i>
Dense × new edu.	.019 (.037) <i>.608</i>	.017 (.039) <i>.660</i>
Cohort FEs		✓
Survey year FEs		✓
Observations	3,994	3,994
Municipalities	390	390

Robust standard errors adjusted for clustering on municipalities in parentheses and *p*-values in italics.

is high compared to similar questions; moreover, the correspondence between how the respondents characterize their municipality of upbringing and how Statistics Norway characterize them is high.

A.2.3 Number of responses

We use the variation in the timing of municipalities' implementation of the reform to estimate the effect of the education reform (see Equation 1 and Equation 2 in the article). To see if there is any measurement error connected to a change in number of responses before and after the reform, we calculate the mean number of responses within each municipality before and after the reform and run a regression estimating an effect of the reform on these number of responses. Before the reform, each municipality had on average 5.6 observations; after the reform, each municipality had on average 7.1 observations. Specifically, if we regress the number of municipality-level responses on the reform, the coefficient on the reform is an insignificant increase of 1.5 (SE =

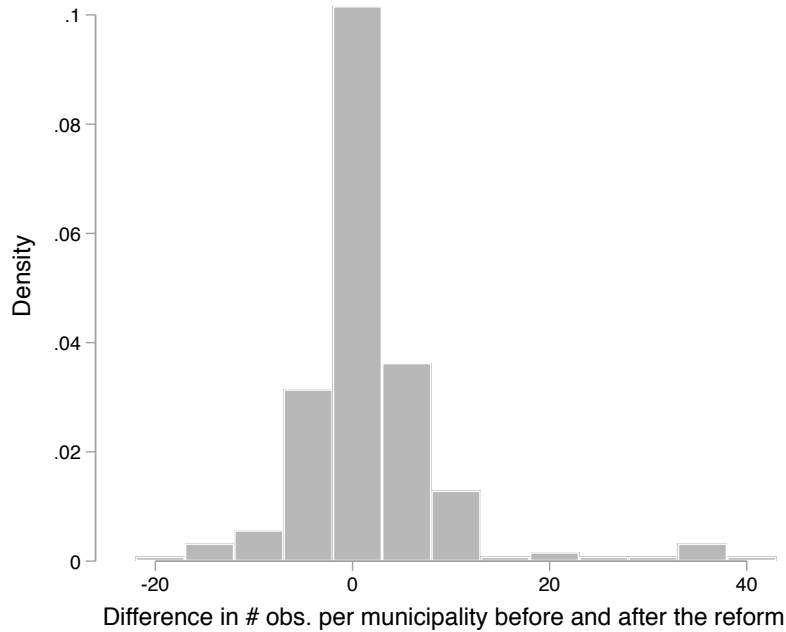


Figure A-3: Histogram of the change in the number of observations per municipality before and after the reform.

1.2, $p = .217$). Indeed, Figure A-3 displays that for a large share of municipalities, the change in responses per municipality before and after the reform is close to zero. If we additionally examine the number of responses that are missing in the main analysis in Table A-10 before and after the reform per municipality, the same picture emerges: the mean number of missing before the reform is .23, after the reform it is .26 and the difference of .03 is insignificant ($SE = .05$, $p = .542$). Together, there is consequently a negligible difference in response rates for respondents educated in the old and the new system. The low average number of observations per municipality before and after the reform might still be a concern since the variation we are using is somewhat limited. However, we do find significant effects on other outcomes (see e.g. A-7), which implies that we have sufficient variation to identify differences between the groups.

A.2.4 Dependent variables

To measure the dependent variables, we use the five different questions about immigrant attitudes referred below. The assigned value in the dummy variables used in the

analysis is indicated in parenthesis. Table A-2 below shows the descriptive statistics for the immigration attitudes items.

Refugees. Which of the following statements below are best in line with your view on how Norway should relate to refugees (survey item Q74)?

1. (1) We have to strive as much as possible to receive more refugees in Norway
2. (0) Instead of receiving refugees in Norway, we should use funds to help them in their own country or their neighbouring countries
3. (0) We cannot afford to use that much money to help refugees as long as we have many unsolved problems in our own country

Culture. Two persons discuss the consequences of immigrants from distant cultures coming to Norway. Who do you agree most with, A or B (Q75)?

A says: Immigrants contribute to a larger cultural diversity in Norway, with exciting new food, music, art, etc.

B says: Immigrants way of living does not fit in Norway. The foreign customs are a disadvantage to the surroundings and may be a threat to Norwegian culture

1. (1) I most agree with A
2. (0) I most agree with B
3. (0) Impossible to choose

Economic. Who do you most agree with on the perception of immigrants, A or B (Q76)?

A says: Immigrants wish to exploit our welfare arrangements and get benefits they have not contributed in creating.

B says: Immigrants are skilled and hardworking people who make a valuable contribution to the Norwegian economy and working life.

1. (0) I mostly agree with A

2. (1) I mostly agree with B
3. (0) Impossible to choose

Immigration. Below you see a list of different sociopolitical issues. Can you read slowly through the list and mark the issues you think are especially important to solve in Norway (Q80)?

Limit immigration

1. 1 (0)
2. 0 (1)

Muslim. Below is a list of things that have recently happened, or still happens in Norway. We would like to know what you think of these. Do you like them and think they are right? Or is there something you don't like and think is wrong? Or are there things you are not concerned about, and think is neither right nor wrong (Q102)?

That we have gotten Muslim denominations in Norway

1. (1) Like - is right
2. (0) Do not like - is wrong
3. (0) Not concerned about it

Table A-2: Descriptive statistics for the immigration attitudes items in the Norsk Monitor survey.

	Mean	Std. dev.
Refugees	0.27	(0.45)
Culture	0.59	(0.49)
Economic	0.41	(0.49)
Immigration	0.62	(0.49)
Muslim	0.27	(0.44)
N	4202	

A.2.5 Covariates and other variables used in the analyses

The covariates included in the regressions are a dummy for *female* (Q309) and a set of dummies for the *type of neighborhood in which the respondent grew up* (Q88A, see also Table ?? above). Specifically, the survey item asks: “How would you characterize the place where you grew up?” It has the following response categories: (1) city, central district; (2) city, outer district; (3) city suburb; (4) town, central district; (5) town, outer district; (6) town suburb; (7) small town; (8) village; (9) rural area. We collapse 2 and 3 into one category, 5 and 6 into one category, and create a no-response category before we create a set of dummy variables based on the survey item. Where noted in the regression tables, we include this set of dummy variables.

Additional variables that are used in the analysis include *education* (V32, “What is your highest level of completed education”), *household income* (Q291, “What would you estimate your household’s gross income to be per year?”)¹², *economic situation* (Q67, “When you compare yourself to other Norwegians, how is your economic situation?”)¹³, and *childhood economic situation* (Q69, “When you look back at your upbringing, how was your family’s economic situation?”).¹⁴

A.2.6 Descriptive statistics for all variables

The descriptive statistics for all variables are provided in Table A-3 on the next page.

We see in Table A-3 that 2.6 percent of the sample reports an education level below middle school and lower secondary academic track. After the reform, there should be no respondents in this category. In our sample 28 respondents (1.2 percent) in the treatment group report that they had primary or continuation school, which is a minor deviance. 96 respondents (3.9 percent) in the control group report the same level of schooling. This is lower than the share with only primary level schooling reported in

¹²100,000 brackets up to 1,000,000, then a category for “1 million or more”.

¹³The answer categories are: much worse than average, somewhat worse than average, as the average, somewhat better than average, much better than average.

¹⁴Answer categories: “The family had at times little money, with worries about how one should make ends meet”; “The family had sufficient funds if one were careful with how the money was spent”; “The family had a generous economy with opportunities to acquire what one needed and wanted”.

Aakvik, Salvanes, and Vaage (2010) (13 percent). The discrepancy might be due to: (a) classification differences in the education variable: Those in the control group who report middle school in the survey (the second lowest education category) have only primary schooling according to the Statistics Norway definition of schooling used in Aakvik et al (2010), (b) measurement error in the treatment indicator, or (c) sampling bias: respondents in the survey are generally higher educated than the population average.

Table A-3: Descriptive statistics for the sample: individual characteristics, characteristics of family of upbringing, and outcomes variables.

	Mean	Std. dev.
<i>Individual characteristics</i>		
Year born	1952.5	(3.49)
Female	0.53	(0.50)
Neighborhood of upbringing		
City center	0.066	(0.25)
City suburbs	0.14	(0.34)
Town center	0.064	(0.24)
Town suburbs	0.067	(0.25)
Small town	0.087	(0.28)
Village	0.15	(0.36)
Rural	0.42	(0.49)
Not specified	0.0088	(0.093)
Education		
Primary and continuation school	0.026	(0.16)
Middle school and lower sec. academic track	0.10	(0.30)
Upper secondary school	0.31	(0.46)
University deg., lower level	0.46	(0.50)
University deg., higher level	0.11	(0.31)
Log household income	13.3	(0.48)
Economic situation above average	0.38	(0.49)
Childhood economic situation		
Family economic situation good	0.10	(0.30)
Family economic situation difficult	0.22	(0.41)
<i>Outcomes</i>		
Immig. attitudes index (FA)	0.0060	(1.00)
Immig. attitudes index (prop.)	0.43	(0.35)
Would vote for the Progress Party	0.090	(0.29)
N	4202	

A.3 Factor analysis of immigration attitudes

In this section we describe the construction of the factor score index. The answer categories for these items have different scales: some between zero and two, some either zero or one. We have transformed the variables into dummy variables indicating more liberal answers. We code those answering “don’t know” and “impossible to choose” as zero to avoid conditioning the sample on an endogenous variable (for instance, education might affect the probability of choosing these categories). The exact wording and coding of the survey items are detailed in the section A.2. Table A-2 shows the proportion of the respondents who give a liberal answer to each of the items.

We use factor analysis (FA) to estimate where the respondents fall on the latent immigration attitude scale, ranging from restrictive to liberal. Since our variables are dichotomous, we use a correlation matrix with tetrachoric correlation coefficients to estimate the factor loadings. All the five items load heavily on one common factor, as shown in Table A-5.¹⁵ From the factor analysis we calculate the standardized factor score variable (with a mean of zero and a standard deviation of one). This immigration attitudes index will serve as our main dependent variable. Additionally, to make sure that our results are not driven by our factor analysis, we create an alternative index in which we take the proportion of positive answers to the five items for each respondent. For both of these indices, a higher value means that the respondent has more *liberal* views on immigration.

¹⁵The Eigenvalue for the first factor is 3.4 and for the second it is 0.07. We thus retain one factor.

Table A-4: Tetrachoric correlation between the five binary survey items used for the immigration attitudes factor analysis index ($N = 4,837$).

	Ref.	Cult.	Econ.	Immi.	Muslim
Refugees	1.00	0.71	0.65	0.80	0.58
Culture	0.71	1.00	0.77	0.76	0.65
Economic	0.65	0.77	1.00	0.71	0.54
Immigration	0.80	0.76	0.71	1.00	0.64
Muslim	0.58	0.65	0.54	0.64	1.00

Table A-5: Item loadings for factor analysis based on the tetrachoric correlation matrix in Table A-4.

	F1: Immigr.	F2	Uniqueness
Refugees	0.83	-0.14	0.29
Culture	0.88	0.12	0.22
Economic	0.81	0.15	0.33
Immigration	0.88	-0.11	0.21
Muslim	0.70	-0.01	0.51

The Eigenvalue of Factor 1 is 3.38 and Factor 2 0.07.

A.4 The reform and childhood economic situation

Table A-6: OLS regression results of the effect of the education reform on self-reported childhood economic situation

	Childhood economic situation			
	(1) Good	(2) Good	(3) Difficult	(4) Difficult
New edu. system	.007 (.010)	.010 (.01)	-.009 (.02)	-.02 (.02)
	<i>.444</i>	<i>.502</i>	<i>.610</i>	<i>.328</i>
Mean (SD) Y	.1 (.3)	.1 (.3)	.22 (.41)	.22 (.41)
Covariates	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓
Municipality FEs		✓		✓
Observations	4,250	4,250	4,250	4,250
Municipalities	375	375	375	375

The dependent variables are the based on the survey question “When you think back on your childhood, how was the financial situation of your family? Which one of the following descriptions best fit the situation when you were 10-15 years if age? 1) We had financial difficulties at times, with concerns about how to make it, 2) We had a decent financial situation, as long as we spend out money carefully, 3) We had a good financial situation and could buy what we needed and wanted” Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics. All models include controls for gender and type of neighborhood during upbringing.

A.5 Effects of the reform on education and income

Table A-7: OLS regression results of the effect of the education reform on level of education, household income, and perceived economic situation

	Lower 2nd. edu.		Educ. years		Log(hh. inc.)		Econ. sit.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New edu. system	.02 (.007)	.02 (.01)	.2 (.1)	.2 (.1)	.04 (.02)	.06 (.03)	.02 (.02)	.05 (.02)
	<i>.002</i>	<i>.024</i>	<i>.083</i>	<i>.208</i>	<i>.018</i>	<i>.019</i>	<i>.160</i>	<i>.022</i>
Mean Y	.97	.97	13.59	13.59	13.28	13.28	.38	.38
SD Y	.16	.16	2.66	2.66	.48	.48	.49	.49
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FEs		✓		✓		✓		✓
Observations	4,138	4,138	4,138	4,138	4,138	4,138	4,138	4,138
Municipalities	372	372	372	372	372	372	372	372

The education variable is a dummy equal to 1 if the respondent reports a higher level of education than completed primary school (Folkeskole) or continuation school (Framhaldskole). Log household income is log of self-reported household income. Economic situation is a dummy equal to 1 if the respondent answers "somewhat better than average" or "much better than average" on the question "When you compare yourself to other Norwegians, how is your economic situation?" Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics. All models include controls for gender and type of neighborhood during upbringing.

A.6 *T-test of differences in immigration attitudes*

Table A-8: *T-test of differences in immigration attitudes between respondents exposed and unexposed to the education reform (within cohorts). See also Figure 1 in the main text.*

Cohort	Mean (NS = 0)	Mean (NS = 1)	Abs. diff.	P-value	N (NS = 0)	N (NS = 1)
1947	-0.15	0.08	0.23	0.21	379	32
1948	-0.06	-0.02	0.04	0.80	347	46
1949	-0.04	0.09	0.12	0.30	322	84
1950	0.07	-0.05	0.12	0.29	253	116
1951	-0.00	-0.05	0.05	0.66	242	125
1952	-0.00	0.07	0.07	0.54	175	161
1953	-0.01	0.02	0.02	0.81	189	206
1954	0.01	0.13	0.13	0.24	127	262
1955	-0.00	0.03	0.04	0.76	100	306
1956	-0.02	0.14	0.16	0.17	98	315
1957	-0.17	0.03	0.20	0.08	91	342
1958	-0.05	-0.06	0.01	0.94	61	300

NS stands for “new system”, i.e., whether the respondents are unexposed (0) or exposed (1) to the education reform.

A.7 Correlation between education and immigration attitudes

Table A-9: OLS regression results of the association between education level and immigration attitudes and voting for Frp, respectively.

	Immigr. attitude index				Voting	
	(1) FA	(2) FA	(3) Prop.	(4) Prop.	(5) Frp	(6) Frp
More than primary edu	.49 (.07) <i><.001</i>	.50 (.08) <i><.001</i>	.18 (.02) <i><.001</i>	.18 (.03) <i><.001</i>	-.09 (.04) <i>.023</i>	-.07 (.04) <i>.062</i>
Mean (SD) Y	.01 (1)	.01 (1)	.43 (.35)	.43 (.35)	.09 (.29)	.09 (.29)
Covariates	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓	✓	✓
Municipality FEs		✓		✓		✓
Observations	4,202	4,202	4,202	4,202	4,202	4,202
Municipalities	378	378	378	378	378	378

Note: Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics. All models include controls for gender and type of neighborhood during upbringing.

A.8 OLS regression results of the effect of the reform on immigration attitudes

Table A-10: OLS regression results of the effect of the education reform on indexes of respondents' immigration attitudes and voting for immigration sceptical party

	Immigration attitudes (more liberal)				Voting	
	(1) FA	(2) FA	(3) Prop.	(4) Prop.	(5) Frp	(6) Frp
New edu. system	.034 (.039)	.007 (.057)	.008 (.014)	-.005 (.019)	-.000 (.012)	-.010 (.017)
	<i>.392</i>	<i>.901</i>	<i>.548</i>	<i>.803</i>	<i>.975</i>	<i>.546</i>
Mean (SD) Y	.01 (1)	.01 (1)	.43 (.35)	.43 (.35)	.09 (.29)	.09 (.29)
Covariates	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓	✓	✓
Municipality FEs		✓		✓		✓
Observations	4,207	4,207	4,207	4,207	4,207	4,207
Municipalities	378	378	378	378	378	378

Note: Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics. All models include controls for gender and type of neighborhood during upbringing. Higher values on the immigration attitude index means more liberal. "FA" refers to the factor analysis index, whereas "Prop." refers to the proportion index (see the text for details). "Frp" is Norwegian Progress Party.

A.9 The main regression results in Table A-10 but without covariates

Table A-11: OLS regression results of the effect of the education reform on respondents' immigration attitudes.

	Immigration attitude index				Voting	
	(1) FA	(2) FA	(3) Prop.	(4) Prop.	(5) Frp	(6) Frp
New edu. system	.038 (.039) <i>.328</i>	.006 (.059) <i>.912</i>	.010 (.013) <i>.420</i>	-.005 (.020) <i>.802</i>	.001 (.011) <i>.891</i>	-.010 (.017) <i>.551</i>
Mean (SD) Y	.01 (1)	.01 (1)	.43 (.35)	.43 (.35)	.09 (.29)	.09 (.29)
Covariates						
Cohort FEs	✓	✓	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓	✓	✓
Municipality FEs		✓		✓		✓
Observations	4,207	4,207	4,207	4,207	4,207	4,207
Municipalities	378	378	378	378	378	378

Note: Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics.

A.10 Regression results for individual survey items

Table A-12: OLS regression results of the effect of the education reform on respondents' immigration attitudes

	Refugees		Culture		Econ.		Immig.		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
New edu. system	-.01 (.02)	-.00 (.02)	-.00 (.02)	.00 (.03)	.00 (.02)	-.03 (.03)	.04 (.02)	.04 (.03)	-.00 (.02)	-.02 (.03)
	<i>.745</i>	<i>.853</i>	<i>.975</i>	<i>.992</i>	<i>.821</i>	<i>.259</i>	<i>.025</i>	<i>.246</i>	<i>.937</i>	<i>.370</i>
Mean (SD) Y	.27 (.45)	.27 (.45)	.59 (.49)	.59 (.49)	.41 (.49)	.41 (.49)	.62 (.49)	.62 (.49)	.27 (.44)	.27 (.44)
Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Survey year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FEs		✓		✓		✓		✓		✓
Observations	4,207	4,207	4,207	4,207	4,207	4,207	4,207	4,207	4,207	4,207
Municipalities	378	378	378	378	378	378	378	378	378	378

See the section "The Norsk Monitor surveys" for question wordings. Robust standard errors adjusted for clustering on municipalities are in parentheses and *P*-values in italics. All models include controls for gender and type of neighborhood during upbringing.

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