Do patients with more education receive more subsidized dental care? Evidence from a natural experiment using the introduction of a school reform in Norway as an instrumental variable

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Abstract

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Background: In Norway, for several dental conditions, a substantial part of treatment costs are reimbursed by the National Insurance Scheme. Ideally, the probability of receiving subsidized dental care should be independent of social determinants of health, such as education, so that dental services are accessible to everyone independent of their social status.

Objective: To estimate the causal effect of education on the probability of receiving subsidized dental care in the adult Norwegian population.

Research design: During the period 1960-1972, all municipalities in Norway were required to increase the number of compulsory years of education from seven to nine years. This education reform was used to create exogenous variation in the education variable. Since municipalities implemented the reform at different times, we have both cross-sectional and time-series variation in the reform instrument. Thus we were able to estimate the effect of education on the probability of receiving subsidized dental care by controlling for municipality fixed effects and trend variables.

Subjects: All Norwegian adults, born 1947-58.

Measures: Information about education (number of years), whether the individuals had received subsidized dental care, place of residence (municipality).

Results: The probability of receiving subsidized dental care increased by 2 percentage points per additional year of education.

Conclusion: People with the most resources benefit the most from a universal welfare scheme that is addressed to reach everybody. We suggest providing information about the subsidy scheme in a way that is easily available and understandable to all individuals, independent of their level of education.

Key words: education, causal estimates, instrumental variable, subsidized dental care, observational data

INTRODUCTION

Equality in access to health services, dental services included, is an important part of Norwegian welfare policy.[1] This policy goal is an important justification for free dental care for children up to the age of 18.[2] It is undesirable that children's access to dental services should be limited by their parents' financial situation. The regular, out-reaching service provided by the public dental service helps to ensure that all children and young people have equal access to dental services, and that inequalities in dental health are minimized.[3]

In Norway, there is no national policy that dental services should be free of charge for adults. However, the National Insurance Scheme has introduced a public subsidy scheme for dental care. For several dental conditions, a substantial part of treatment costs are reimbursed by the National Insurance Scheme.[4] The payments from this scheme cover about 20% of the total costs for dental care for adults.[5] Similar, but slightly more extensive, subsidy schemes exist in the other Scandinavian countries.[6-8] The experience from these countries is that utilization of services has increased after the introduction of these schemes.[9-11]

To our knowledge, there has been no research into the distributional effects of subsidized dental care for adults. Does everybody who needs dental care have equal access to treatment that is covered by an insurance scheme? Ideally, the probability of receiving dental care that is subsidized by the state should be independent of social determinants of health, such as education, income and living conditions. If that is the case, the subsidy scheme has come a long way to redistribute resources so that dental services are accessible to everyone independent of their social status.[12]

The key hypothesis we tested was whether the probability of receiving subsidized dental care was independent of individual resources, such as education. The focus of our analyses was to estimate causal effects of education. To test the robustness of the causal estimates we carried out four supplementary analyses in which we tested: 1) **whether estimates at the individual level** were similar to estimates at the level of the municipality 2) whether the effect of education was influenced by the inclusion of the control variables 3) whether education had an effect on costs for dental treatment that were reimbursed by the Norwegian Health Economics Administration 4) whether the effect of education varied according to types of dental service.

METHODS

Data

Reimbursement payments for dental care for people 20 years and above are administered by the Norwegian Health Economics Administration.[13] All adults who receive subsidized dental treatment are registered with this body. For these adults, the dependent variable in Equation (2) equals 1. For adults who were not registered, the dependent variable equals 0. In our data, the proportion of individuals who had received subsidized dental care was 12.9%. We used the data for 2013, since that was the first year they were available electronically.

The subsidies do not vary across regions. The fees that are reimbursed by the National Insurance Scheme are the same independent of patient's place of residence. The fees are adjusted annually for inflation at the national level (on the first of January). There is no yearly

time series variation in fees. Our outcome variable is measured during a one-year period (2013); i.e. the fees were the same during that time period.

All persons who live in Norway have a unique personal identification number. This made it possible to merge the data from the Norwegian Health Economics Administration with two data registers in Statistics Norway. The first register, the Norwegian Standard Classification of Education, contains information about the highest education of all persons living in Norway from 1967. The second register, the Population and Housing Census, contains information about place of residence (municipality) of all persons living in Norway in 1960. By merging data from the Norwegian Health Economics Administration with data from Statistics Norway, our final data file encompasses the whole adult population excluding immigrants. Most immigrants to Norway have not been exposed to the school reform.[14] Therefore they were not included in our analyses.

School reform as an instrumental variable

We used random variation induced by the introduction of a compulsory school reform in Norway to estimate the causal effects of education on the probability of receiving subsidized dental care. The reform, which increased the minimum number of years of education from seven to nine years, was introduced during the period 1960-72. Such reforms are now commonly used within social sciences to estimate causal effects of education on different types of health outcome (for a review see: [15-17]). Comprehensive descriptions of the Norwegian reform are given by: Grytten et al.[18], Lie[19], Salvanes and co-workers[20-23], Telhaug.[24]

The municipalities implemented the reform at different times, as shown in Supplemental Digital Content 1, Fig. S1. We were then able to compare individuals in the same municipality who had nine years compulsory education with those who had seven years.

Let subscript mtj denote an individual j who has grown up in municipality m, and was born in year t. Then, the first stage regression for the individual's years of education (Y_{mtj}) can be written as:

$$Y_{mtj} = \gamma_0 R_{mt} + \gamma_1 T_{mt} R_{mt} + \gamma_2 T_{mt} + \text{Fixed effect for year of birth} + \text{Municipality fixed effects} + e_{mtj} \quad (1)$$

R_{mt} is a dummy variable that equals 1 if the individual was 14 years or younger when the reform was introduced in the municipality, (i.e. he/she was affected by the reform), and 0 otherwise. T_{mt} is a centred linear trend variable, i.e. defined as zero the first year of the reform. Since municipalities implemented the reform at different points in time, the trend variable is defined by the timing of the reform. The after-reform trend was flat when we restricted the sample to individuals with a maximum of nine years of education. We therefore allowed the model to have different time trends before and after the reform. This was captured by the interaction term between reform (R_{mt}) and time-trend (T_{mt}).

Let $\hat{Y}_{mtj}$ be the predicted value of the individual's years of education from the first stage regression, and let the probability of obtaining subsidized dental care ($SDC_{mtj} = 1$) be the dependent variable. The second stage regression is then:

$$SDC_{mtj} = \beta_0 \hat{Y}_{mtj} + \beta_1 T_{mt} + \text{Fixed effect for year of birth} + \text{Municipality fixed effects} + v_{mtj} \quad (2)$$

An instrumental variable should satisfy the criteria of exogenity.[25] Instrument exogenity requires that the implementation of the reform influenced the probability of receiving subsidized dental care only through the individuals' level of education (the exclusion restriction). There are several reasons why this is likely to be the case. First, when it was decided to implement the reform in a municipality, the reform encompassed all children or adolescents in the municipality, i.e. the reform affected everybody independent of their abilities, time preferences, health status and health behaviour. Second, there is no evidence of selective migration from or to municipalities in which the reform was implemented early in the 1960s.[19,24] Third, Salvanes and co-workers have shown that there was no relationship between the timing of the implementation of the reform and municipal characteristics such as the size of the municipality, the unemployment rate, or the proportion of employed people who work in manufacturing industries.[20,22] Further, they found no relationship between the timing of implementation and inhabitants' level of income or age. In Supplemental Digital Content 2, Table S1 we extended the analyses of Salvanes and co-workers to show that the mean values on some key variables were similar before and after the reform. Therefore, in sum, there is reason to believe that the use of the school reform as an instrument variable fulfills the criteria of exogeneity.

We used a linear probability model in the estimation.[26] Some of our results are also presented as reduced form estimates where the probability of obtaining subsidized dental care is regressed directly on the reform variable. Our main analyses were carried out on a sample including 4 years on each side of the reform (Table 1). Additional analyses were performed with different bandwidths to test the robustness of the findings.

RESULTS

OLS estimates

Education had a positive effect on the probability of receiving subsidized dental care. The sizes of the regression coefficients were in the range 0.009 to 0.013 (Fig. 1). This implies that the probability of obtaining treatment increased by 0.9 to 1.3 percentage points per additional year of education. The value 0 was not contained in any of the 95% confidence intervals. This indicates that the estimates were statistically significant at less than the 5% level.

Reduced form estimates

The school reform had a positive effect on the probability of receiving subsidized dental treatment. The size of the regression coefficient was 0.016 ($p<0.001$) (Table 1). This implies that the probability of receiving treatment increased by 1.6 percentage points for those who had nine years of compulsory education compared to those who had 7 years.

First stage estimates

The reform resulted in 0.82 additional years of education (Table 1 and Supplemental Digital Content 1, Fig. S2). The size of the coefficient for the effect of the reform on years of education is well within the range that Salvanes and co-workers report from their studies.[23]

The regression coefficient was highly significant, with a t-value of 31.4. This means that we have a high F-value for the instrumental variable, fulfilling all the criteria proposed in the literature for a strong instrument.[27]

The sign of the regression coefficient for the trend variable was positive (Table 1). This indicates that educational levels increased over time prior to the reform (Supplemental Digital Content 1, Fig. S2). The post-reform trend is given by the sum of the trend coefficient and the reform x trend coefficient. The sum was close to zero. This is because our sample was restricted to individuals who had a maximum of nine years education.

Second stage estimates

Education had a positive effect on the probability of receiving subsidized dental treatment. The size of the regression coefficient was 0.02 ($p<0.001$) (Table 1). This implies that the probability of receiving treatment increased by 2 percentage points per additional year of education. In the analyses with the broader bandwidths, the estimates are clearly more precise than the estimates with the narrower bandwidths (Fig. 2). However, the sizes of the estimates are fairly similar, i.e. our results are robust across samples.

The proportion of individuals who received subsidized dental care was nearly 13%. An implication of the second stage results is that one additional year of education increased the proportion of individuals who had received subsidized care by about 15%.

Supplementary analyses

Data aggregated to the municipal level

To test the robustness of our individual level results, we performed additional analyses at the municipal level. Our outcome variable was then measured as the proportion of individuals

who obtained subsidized dental care per municipality. Municipality fixed effects and time trends were included as control variables.

The reform resulted in 0.79 additional years of education, and a large F-value for the reform variable (Supplemental Digital Content 2, Table S2). For both the reduced and second stage estimates, the sign and size of the regression coefficients were similar to those from our analyses of the individual level data presented in Table 1. In the analyses using aggregated data, the standard errors were larger than in the analyses using individual data, leading to less precision. This is as expected, due to fewer observations in these analyses. However, the coefficients were still statistically significant at **p=0.06** (Supplemental Digital Content 2, Table S2).

Inclusion of control variables

We extended our main regression analysis by including the following control variables: household income before tax per member of the household, household income after tax per member of the household, gender, single person household, and disability pension (2013 figures). For each of the control variables the mean values for individuals who were exposed and individuals who were not exposed to the reform are given in Supplemental Digital Content 2, Table S1. The mean values were calculated for a 4-year period before the reform (non-exposed individuals), and for a 4-year period after the reform (exposed individuals). For all variables, with the exception of disability pension, the mean values were similar before and after the reform was introduced.

The regression estimates are reported in Supplemental Digital Content 2, Table S3. We show results with household income per family member before and after tax (columns I and II). For both measures of income, the sizes of the regression coefficients were similar, but small ($p<0.05$). For household income before tax, the regression coefficient was 0.000015. This implies that the probability of receiving subsidized dental care increased by 0.0015 percentage points with an increase in household income of EUR 10 000.

The regression coefficients for gender, single person household and disability pension had a statistically significant effect on the probability of receiving subsidized dental care ($p<0.05$). For gender, the regression coefficient was -0.041. This implies that the probability of receiving subsidized dental care was 4.1 percentage points higher for women than for men. However, the coefficient for our education variable was not influenced by the inclusion of the control variables. This gives support to our belief that the instrumental variable was not biased due to confounding variables.

Education and cost per patient for dental treatment that is reimbursed

We examined whether the cost per patient for dental treatment that is reimbursed by the Norwegian Health Economics Administration are higher for patients with more education than for patients with less education. If this is the case, it could indicate provider-induced demand, which would be an undesirable effect of the subsidy scheme.[28,29] This was tested by estimating Equation (2) with cost per patient for dental treatment that is reimbursed as the dependent variable.

The results are shown in Figure 3. For all samples, the regression estimates are small. The value 0 was contained in all the 95% confidence intervals. This indicates that the estimates were not statistically significant at less than the 5% level.

The effect of the reform on different types of dental service

The dependent variable in Equation (2) is a measure of all types of dental treatment that are subsidized. This is a broad measure of output. For adults, the National Insurance Scheme reimburses treatment costs according to ten different dental diagnoses[30] (Supplemental Digital Content 2, Table S4). In 2013, patients diagnosed with periodontal disease were the largest group that had the cost of treatment subsidized. Patients diagnosed with diseases and abnormalities in the mouth and jaw, excluding caries, were the second largest group. The effect of education on the probability of obtaining subsidized dental care may vary according to diagnosis. We applied a multinomial logit model to estimate reduced form effects of the school reform on the probability of obtaining subsidized dental care for each of the following 5 diagnostic groups:

1. Periodontal disease
2. Diseases and abnormalities in the mouth and jaw, excluding caries
3. Attrition/erosion leading to loss of tooth substance
4. Xerostomia (dry mouth)
5. All other diagnostic groups for which treatment costs are reimbursed by the National Insurance Scheme, merged into one category

The reference group was those who did not receive subsidized dental treatment. The specification is:

$$ln\frac{P(D_{jmt}=x)}{P(D_{jmt}=Reference\ group)} = \varphi_0^x R_{mt} + \varphi_1^x T_{mt} + \varphi_2^x T_{mt} R_{mt} + Fixed\ effect\ for\ year\ of\ birth + Municipality\ fixed\ effects + \vartheta_{jmt}^x \quad (3)$$

$x = 1,2,..5$. We present logit coefficients and marginal effects based on samples with 4 and 6 years on each side of the reform.[31]

The school reform had a positive effect on the probability of receiving treatment for periodontal disease, and for diseases and abnormalities in the mouth and jaw excluding caries ($p<0.05$) (Table 2). The probability of receiving treatment for periodontal disease increased by 1.1-0.7 percentage points for those who had nine years compulsory education compared to those who had seven years. For diseases and abnormalities in the mouth and jaw, excluding caries, the probability of receiving treatment increased by 0.5-0.4 percentage points.

For treatment of attrition/erosion, the logit coefficient for the reform variable was statistically significant at **p<0.10** in the sample with 6 years on each side of the reform. In this sample, the probability of receiving treatment for attrition/erosion increased by 0.2 percentage points for those who had nine years of compulsory education compared to those who had seven years. There was no statistically significant effect of the reform variable on the probability of receiving treatment for xerostomia.

DISCUSSION

In several Western European countries, particularly in the Scandinavian countries, subsidized dental care is an important part of welfare policy.[1] An aim of this policy is to make dental services equally accessible to everybody. Subsidized dental care may reduce inequalities in access between socio-economic groups. However, the results from our study indicate that subsidized dental care alone is not sufficient to eliminate inequalities. People with more education make more use of subsidized dental care than people with less education. The difference is large and causal. This is particularly so for the probability of receiving subsidized dental care for periodontal disease (Table 2).

In the OLS estimate, unobserved variables that were correlated with both education and the probability of receiving subsidized dental care were not controlled for. Unobserved variables that are frequently cited in the literature are ability, place of residence and time preferences.[32-34] These are variables that may be positively correlated with both education and the probability of receiving subsidized dental care. Therefore, omission of these variables could lead to an upward bias of the OLS estimate. Morbidity is likely to be positively correlated with the probability of receiving subsidized dental care, and negatively correlated with education. Therefore, unless morbidity is taken into account by the identification strategy, the estimate will be downward biased.[35] The **instrumental variable** estimate was not that different from the OLS estimate (Table 1 and Fig. 1). This indicates that the net effect of the unmeasured variables that give an upward bias (ability, place of residence, time preferences) and a downward bias (morbidity) of the OLS estimate, is approximately equal to null.

Numerous studies have reported that people with high education have better dental health than people with low education.[36-39] Subsidized dental care is not the sole determinant of dental health, but it plays a role.[40-43] Therefore, to some extent, differences in access to subsidized dental care according to education, may explain the reported differences in dental health according to education. We were not able to pursue this issue further, as we had no access to dental health outcome measures in our data.

Within the framework of our study, it is not possible to suggest which policy, if any, would be effective in reducing inequalities in access to subsidized dental care. One obvious alternative is to use measures that strengthen the demand side, for example, to provide information about the subsidy scheme in a way that is easily available and understandable to all individuals, independent of their level of education. Part of this strategy could be to inform people with lower education in particular that use of subsidized dental care can be an effective way to maintain good dental health. Dentists treat their patients equally, i.e. the amount of subsidized services provided is independent of patients' level of education (Fig. 3). Therefore, there is no need to introduce supply side measures aiming to change the way dentists respond to the treatment needs of patients who belong to different education groups.

A limitation of the present study is that the school reform variable only gives a local average treatment effect. This effect is at the bottom tail of the educational distribution. The relationship between years of education and the probability of receiving subsidized dental treatment may be non-linear. Therefore, we should be cautious in generalizing the findings to people with more than nine years education, that is to the middle and upper ranges of the education distribution.[44] This is basically a selected group of people who would be motivated to acquire knowledge, independently of the number of years they attended school. Therefore,

for people with more than nine years education, the effect of education could be weaker; i.e. non-linear.[44,45]

In conclusion, we have estimated the casual effect of education on the probability of receiving subsidized dental care in the adult Norwegian population. We found that people with the most resources, i.e. highly educated people, benefit the most from a universal welfare scheme that is addressed to reach everybody. In order to reduce inequalities in access, we suggest providing information about the subsidy scheme in a way that is easily available and understandable to all individuals, independent of their level of education.

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Figure legends:

FIGURE 1. Ordinary least square regression coefficients for the probability of receiving subsidized dental care according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.

FIGURE 2. Second stage regression coefficients for the probability of receiving subsidized dental care according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.

FIGURE 3. Second stage regression coefficients for reimbursments per patient (EUR) according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.

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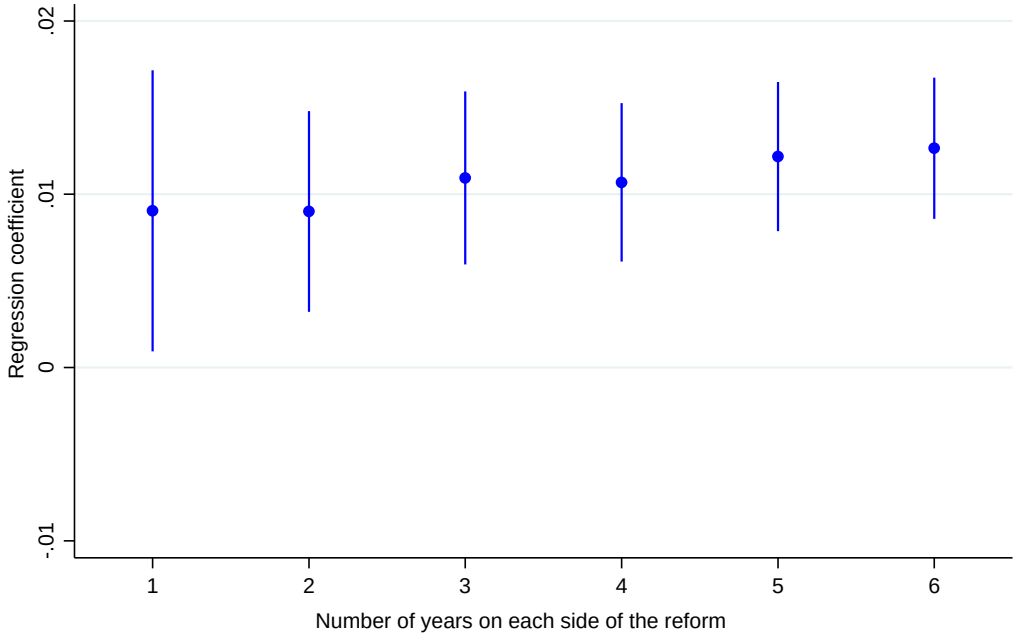
Supplemental Digital Content 1 Figures.pdf

Supplemental Digital Content 2 Tables.docx

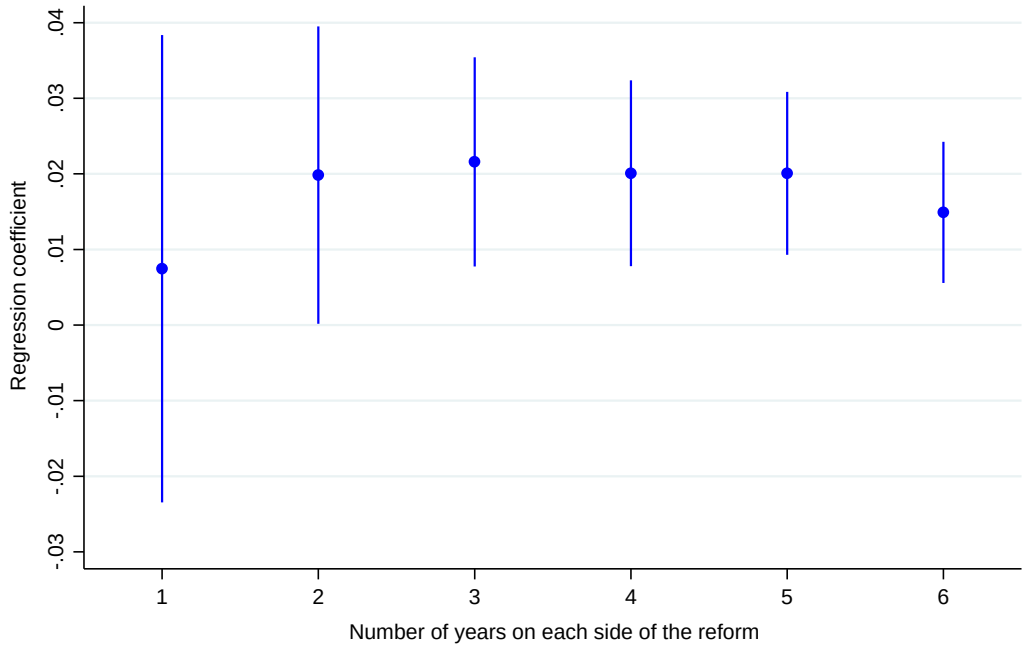
Figure

Figure 1. Ordinary least square regression coefficients for the probability of receiving subsidized dental care according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.



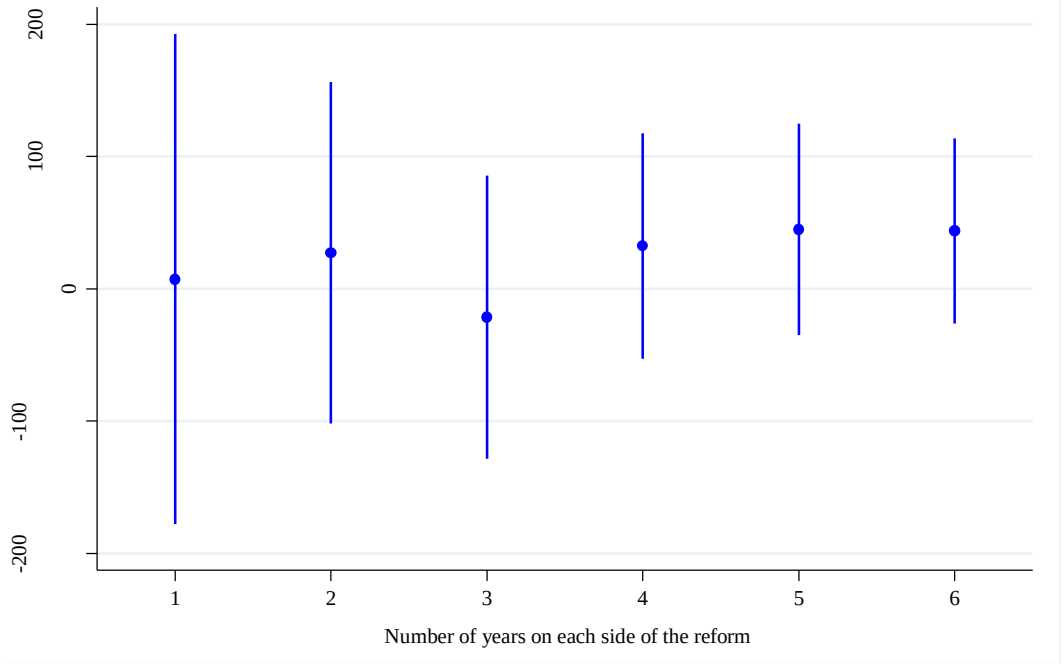
Figure

Figure 2. Second stage regression coefficients for the probability of receiving subsidized dental care according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.



Figure

Figure 3. Second stage regression coefficients for reimbursements per patient (EUR) according to years of education. Estimated on samples with different number of years on each side of the reform. 95% confidence intervals.



Table

TABLE 1. The effect of the Norwegian school reform on the number of years of education and on the probability of receiving subsidized dental care. First and second stage regressions estimated on a sample with 4 years on each side of the reform. Regression coefficients with standard errors clustered by municipality (in brackets)

Variables	Regression coefficients (standard errors)	
Reduced form		
School reform	0.016	*
	(0.005)	
First stage estimates		
School reform	0.826	*
	(0.026)	
Linear trend	0.100	*
	(0.009)	
School reform x linear trend	-0.093	*
	(0.007)	
F- value	961	
Second stage estimates		
Education (in years)	0.020	*
	(0.006)	
N	84 599	

* $p<0.001$

Control variables: Municipality fixed effect and time trends

Table

TABLE 2. The effect of the Norwegian school reform on the probability of receiving subsidized dental care according to diagnostic group. Logit coefficients and marginal effects with standard errors clustered by municipality (in brackets)

	Sample with 4 years on each side of the reform		Sample with 6 years on each side of the reform	
Diagnostic groups	Logit coefficient (standard errors)	Marginal effect	Logit coefficient (standard errors)	Marginal effect
Periodontal disease	0.136 **	0.011 **	0.101 **	0.007 **
	(0.049)	(0.004)	(0.041)	(0.003)
Diseases and abnormalities in the	0.227 **	0.005 **	0.204 **	0.004 **
the mouth and jaw, excluding caries	(0.094)	(0.002)	(0.077)	(0.001)
Attrition/erosion leading to loss of	0.248	0.002	0.229 *	0.002
tooth substance	(0.157)	(0.001)	(0.130)	(0.001)
Xerostomia (dry mouth)	0.051	0.0003	-0.002	-0.0002
	(0.143)	(0.001)	(0.117)	(0.001)
All other diagnostic groups for which	-0.010	-0.0002	-0.009	-0.0002
treatment costs are reimbursed	(0.158)	(0.001)	(0.131)	(0.001)
No subsidized care	Reference category	-0.018 **	Reference category	-0.014 **
		(0.005)		(0.004)
N	84 599	84 5499	127 915	127 915

** p<0.05
* p<0.10

Control variables: Municipality fixed effect and time trends

FIGURE S1. The number of municipalities that had implemented the reform according to year.

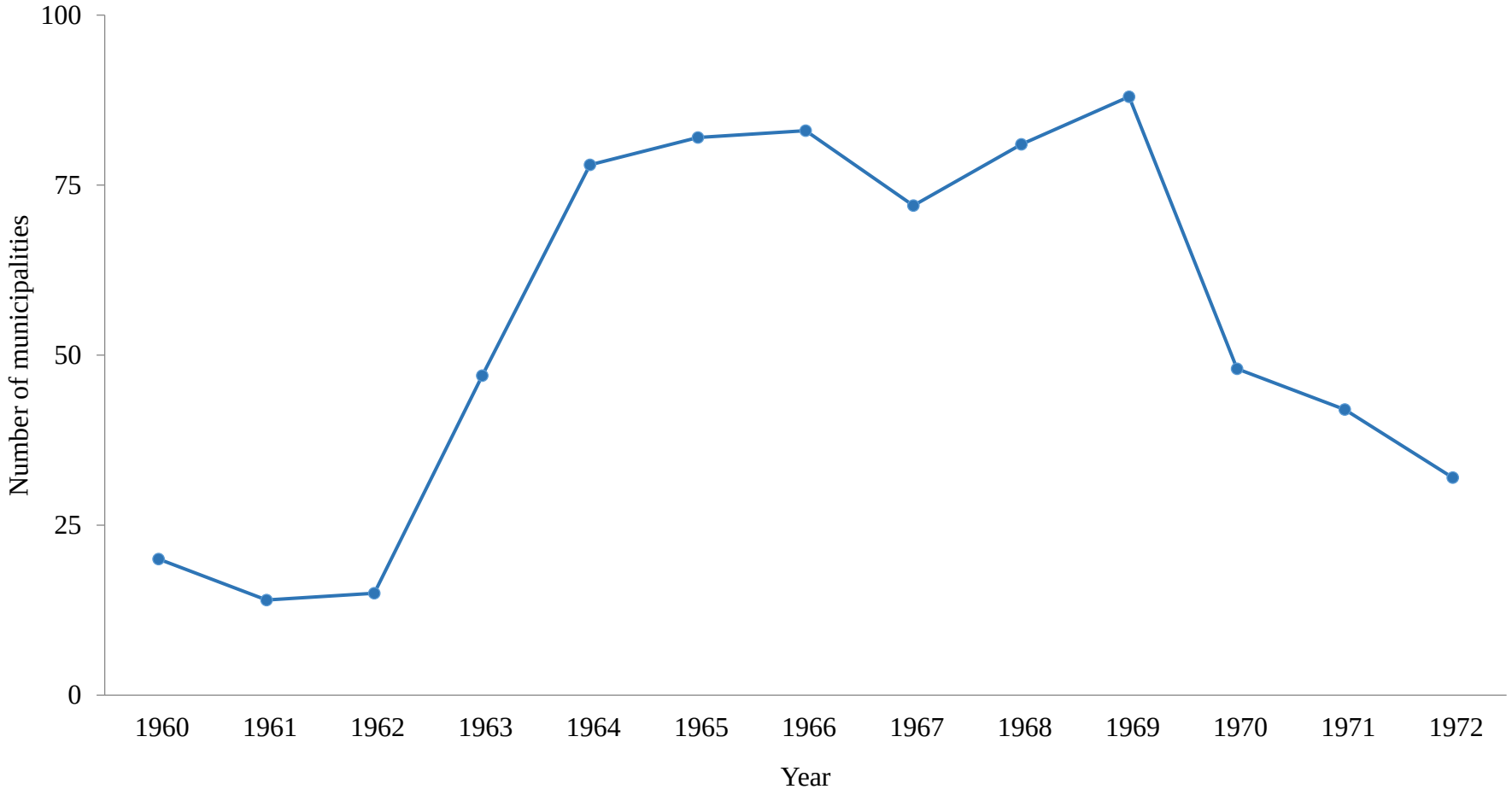


Figure S2. The effect of the Norwegian school reform on the mean number of years of education.

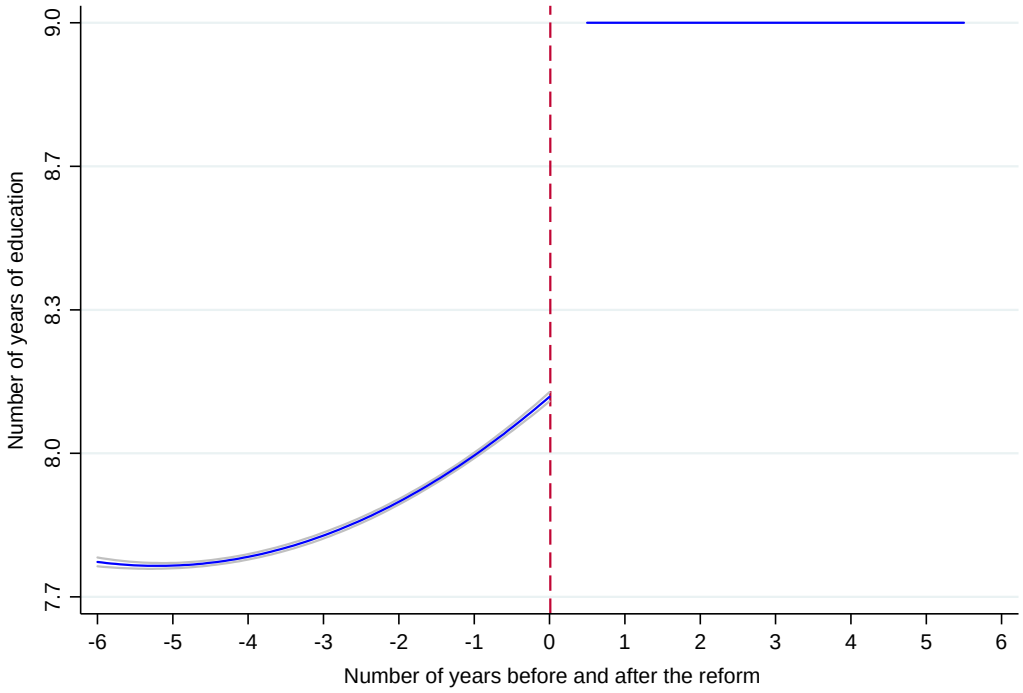


TABLE S1. Descriptive statistics of control variables. Sample with 4 years on each side of the reform

	Before the reform		Reform and after	
Variables	Mean/proportion	N	Mean/proportion	N
Household income before tax per member of the household (in EUR 100)	362	36 508	386	46 018
Household income after tax per member of the household (in EUR 100)	286	36 496	295	45 998
Male	0.47	37 633	0.47	46 966
Single person household	0.26	37 633	0.25	46 966
Disability pension	0.44	36 299	0.35	45 901

TABLE S2. Analyses at the level of the municipality. The effect of the Norwegian school reform on the number of years of education, and on the proportion of individuals who obtained subsidized dental care. First and second stage regressions estimated on a sample with 4 years on each side of the reform. Regression coefficients with standard errors clustered by municipality (in brackets)

Variables	Regression coefficients (standard errors)	
Reduced form		
School reform	0.016	*
	(0.008)	
First stage estimates		
School reform	0.794	**
	(0.015)	
Linear trend	0.114	**
	(0.005)	
School reform x linear trend	-0.110	**
	(0.005)	
F- value	2540	
Second stage estimates		
Education (in years)	0.021	*
	(0.011)	
N (municipality-years)	5 962	

** p<0.001
* p<0.10

Control variables: Municipality fixed effect and time trends

TABLE S3. Analyses with control variables included. The effect of the Norwegian school reform on the number of years of education and on the probability of receiving subsidized dental care. Second stage regressions estimated on a sample with 4 years on each side of the reform. Regression coefficients with standard errors clustered by municipality (in brackets)

Variables	Regression coefficients (standard errors) I	Regression coefficients (standard errors) II
Education (in years)	0.020 * (0.006)	0.020 * (0.006)
Control variables		
Household income before tax per member of the household (in EUR 100)	0.000015 * (0.000007)	
Household income after tax per member of the household (in EUR 100)		0.000028 * (0.00001)
Male	-0.041 ** (0.002)	-0.041 ** (0.002)
Single person household	-0.010 * (0.003)	-0.010 * (0.003)
Disability pension	0.006 * (0.003)	0.006 * (0.003)
N	82 093	82 061

** p<0.001
* p<0.05

Municipality fixed effect and time trends in the analyses

Table S4. Descriptive statistics. The distribution of individuals who received subsidized dental care according to diagnostic group. Sample with 4 years on each side of the reform

Diagnostic groups	Number	Per cent
Periodontal disease	8 167	64.1
Diseases and abnormalities in the mouth and jaw, excluding caries	2 188	17.2
Attrition/erosion leading to loss of tooth substance	721	5.7
Xerostomia (dry mouth)	934	7.3
All other diagnostic groups for which treatment costs are reimbursed [1]	731	5.7
Total	12 741	100.0

[1] Includes: Rare medical conditions, cleft lift, cleft palate, tumours in the oral cavity, adjacent tissue or in the head, treatment to prevent infection in connection with specific medical conditions (i.e. organ transplant, HIV/AIDS), congenital disorders of the teeth, allergic reaction to restorative materials in the oral cavity, occupational injury and accidents resulting in damage to the teeth, and inability to care for oneself due to permanent disability that has resulted in poor dental health.