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Educational inequalities in access to fixed prosthodontic treatment in Norway. Causal effects using the introduction of a school reform as an instrumental variable

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ABSTRACT

Objectives: Equality in access to dental services, independent of level of education, is an important aim of Scandinavian welfare policy. In Norway, this policy is the main justification for a dental public subsidy scheme for adults. An important part of the subsidy scheme is to cover the cost of fixed prosthodontic treatment, including implant-based crowns and bridges for premolars, canines and incisors. A stated policy goal is to secure equal access to everybody in need of these services, independent of their level of education. The aim of this study was to estimate the causal effect of education on the probability of receiving fixed prosthodontic treatment in the adult Norwegian population.

Methods: 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. We used this education reform as the instrumental variable to create exogenous variation in the individual's number of years of education. The education data were combined with data from the Norwegian Health Economics Administration, which contained our outcome variable. Our sample included individuals with 9 years education or less. Altogether 113 237 individuals were included in the study.

Results: For men, the first stage regression coefficient was 0.87 (confidence interval: 0.82–0.92). This means that the reform resulted in 0.87 additional years of education. For men, the probability of receiving fixed prosthodontic treatment increased by 0.67 percentage points per additional year of schooling. For women, the first stage regression coefficient was 0.84 (confidence interval: 0.79–0.88). The second stage regression coefficient was small (= 0.0022) and not statistically significant at a conventional level.

Conclusion: From a welfare policy point of view, for men, the subsidy scheme has not succeeded to redistribute resources so that dental services are accessible independent of their social status.

1. Introduction

Equality in access to dental services is an important aim of Scandinavian welfare policy (Ministry of Health, 2002). In Norway, this policy is the main justification for free dental care for children up to the age of 18, and for a dental public subsidy scheme for adults (Holst, 2007; Ministry of Social Affairs, 1983). An important part of the subsidy scheme is to cover the cost of fixed prosthodontic treatment, including implant-based crowns and bridges for premolars, canines and incisors (Ministry of Health and Care Services, 2019). A stated policy goal is to secure equal access to everybody in need of these services. Ideally, the probability of receiving fixed prosthodontic treatment should be independent of socio-economic characteristics such as education, income and living conditions.

Several studies have shown that utilization of dental services increases with the introduction of a subsidy scheme or dental insurance (Baicker et al., 2018; Bailit et al., 1985; Davidson et al., 2015; Eriksen and Håkansson, 1982; Grytten et al., 1996; Ikenwilo, 2013; Niert et al., 2005; Raittio et al., 2014; Schwarz, 1996; Wang, Norton and Rozier, 2004). To our knowledge, there are no studies in which the distributional effects of these schemes have been examined. 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 (Evans and Williamson,

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1978).

The key question is: Does everyone have equal access to treatment that is covered by a subsidy or an insurance scheme? We examined this research question by testing the hypothesis of whether the probability of receiving fixed prosthodontic treatment is independent of individual resources, such as education. These are high cost treatments that few patients would be able to afford unless they were covered by the subsidy scheme. In Norway, the mean cost of these treatments is EUR 1422. For most patients in need of such treatment, to actually receive the treatment improves their quality of life in terms of better chewing function and improved appearance (Brennan, Houston, O'Sullivan, & O'Connell, 2010; Feine et al., 1994; Pjetursson et al., 2005; Reissmann et al., 2017). In Norway, there are unmet dental treatment needs in the population. This evidence comes from national surveys where the respondents were asked to rate their oral health using the Oral Health Impact Profile (OHIP) questionnaire (Slade and Spencer, 1994). The questionnaire measures several dimensions of oral health, such as pain, discomfort, and psychological and social disability, that possibly have an impact on everyday well-being. The proportion of the population that report that they often or sometimes suffer from these dimensions of oral health is in the range 10–20 per cent (Dahl, Wang, Holst, & Öhrn, 2011a; Dahl, Wang, Skau, & Öhrn, 2011b; Åstrøm et al., 2006). For example, in one survey, 19 per cent of the adult population reported that they often or sometimes have discomfort when eating (Dahl et al., 2011b).

To study the relationship between education and the probability of receiving fixed prosthodontic treatment is not straightforward. An estimation of this using ordinary least squares regression, may give biased results (Grytten, 2017a; Listl et al., 2016). Mainly, this is because the estimation does not take account of unobservable variables that are correlated with both education and treatment. Unobserved variables that are frequently cited in the literature are cognitive ability, place of residence, time preferences and morbidity (Grossman, 2006; Grossman and Kaestnar, 1997; Oreopoulos and Salvanes, 2011). For example, highly educated and wealthy individuals tend to live in affluent areas. These are also areas where the supply and quality of both schools and dental services are likely to be high. As place of residence is positively correlated with both education and treatment, omission of this variable would lead to an upward bias of the ordinary least square estimate. Another example is morbidity, which is likely to be positively correlated with treatment and negatively correlated with schooling. Therefore, unless morbidity is taken into account by the identification strategy, the ordinary least square estimate will be downward biased.

One way to control for unobserved variables is to use instrumental variables. So far, within the social science literature, the most promising type of instrumental variable has been the introduction of compulsory schooling laws (Eide and Showalter, 2011; Mazumder, 2012). Such laws were introduced in several European and North American countries in different time periods during the last century (Gathman, Jürges and Reinhold, 2015). We used the introduction of a compulsory school reform that was introduced in Norway during the period 1960–1972 to estimate the causal effect of education on the probability of receiving fixed prosthodontic treatment. To our knowledge, there are only two studies within dentistry in which instrumental variable methodology has been used to estimate the casual effect of education on any dental outcome (Grytten and Skau, 2017; Matsuyama et al., 2019). In a study from Great Britain, exogenous variation in length of schooling due to a school reform was used to examine the causal effect of education on tooth loss in older age (Matsuyama et al., 2019). In a study from Norway, Grytten and Skau (2017) used a school reform to estimate the causal effect of education on use of periodontal services. In both studies, causal effects were identified.

Below we first describe the Norwegian school reform and the research design we used to identify causal effects. Then we describe the data. Finally, the results are presented and discussed.

2. The Norwegian school reform

We used random variation induced by the implementation of a compulsory school reform in Norway to estimate the causal effects of education on the probability of receiving fixed prosthodontic treatment. In 1960, Norway started to implement a nationwide school reform to increase the length of compulsory education from seven to nine years. Municipalities decided when to implement the reform, with a deadline at the end of 1972. The gradual implementation of the reform meant that Norway, during a 12-year period, had two parallel school systems. Whether a particular child underwent seven or nine years of compulsory education depended on the municipality where he/she grew up and his/her year of birth. The first birth cohort for which a nine-year compulsory education was possible was the cohort of 1947; the children in the last cohort to complete the old system were born in 1958. All children started school in the year they became seven, i.e. they were aged between six and a half and seven and a half when they started school. School entry occurs once a year in the middle of August and children are entitled to attend the nearest school in the municipality where they live.

Children finished compulsory education at the age of 14 in the old system, and 16 in the new system. The main effect of the reform was to increase the number of years of education (Lund, 1999). For further details about the reform, see Grytten et al. (2020), Lie (1973) and Telhaug (1969).

We used the 1960 census to identify the municipality in which the child grew up (Statistics Norway, 1987). We identified the timing of the reform in 706 of the 735 municipalities that existed in 1960 (Ness, 1971). The municipalities implemented the reform at different times as shown in Fig. 1. Many rural municipalities adopted the reform early. Municipalities with major cities implemented the reform later (Fig. 2).

3. Methods

3.1. Research design and assumptions of the IV analyses

The key advantage of this design is the time series variation across municipalities. We could compare individuals in the same municipality who were subjected to 9 years of compulsory schooling with those who were not. Since municipalities implemented the reform at different points in time, we were able to include municipality fixed effects in the regression model. Therefore, all unobserved cross-sectional variation between municipalities that could be correlated with individuals' level of education and the probability of receiving fixed prosthodontic treatment was cancelled out.

We estimated causal effects of education on the probability of receiving fixed prosthodontic treatment using instrumental variable analysis. This type of analysis is an effective tool to obtain causal estimates if certain assumptions are fulfilled. The assumptions are (Angrist and Pischke, 2009; Greenland, 2000; Martens et al., 2006):

First, the instrumental variable must have a clear effect on the treatment variable: in our case on educational level. This is usually termed "instrument relevance".

Second, the instrumental variable must not be correlated with the error term in the first stage regression, i.e. where the treatment variable is regressed on the instrumental variable. Alternatively, for a strong instrument this correlation can be small.

Third, the instrumental variable must not be correlated with the error term in the second stage regression equation. This is usually termed "the exclusion restriction". This means that the instrumental variable has an effect on the outcome *only* through the treatment variable. In that case, biases caused by reversed causality and omission of a third variable are eliminated.

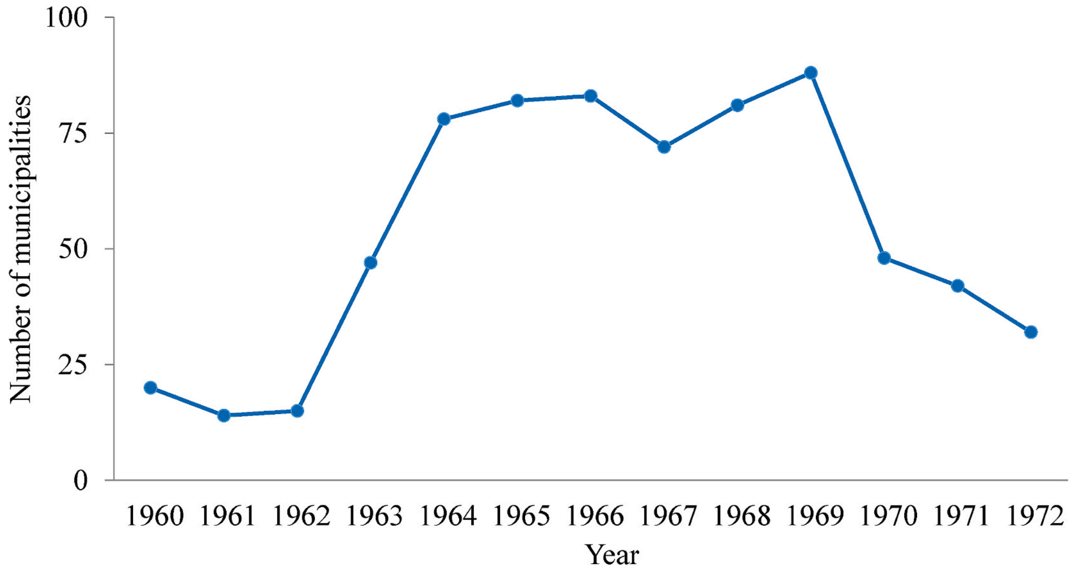

Fig. 1. The number of municipalities distributed according to the year the reform was implemented.

3.2. Empirical specification

We defined our sample to include individuals with 9 years education or less. The exposure variable was years of education for these individuals. For these individuals, the mean number of years of education was 8.52 (Appendix 1). Let subscript mtj denote an individual j who has grown up in municipality m, and was born in year t. R_{mtj} 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}). The first stage regression for the individual's number of years of education (E_{mtj}) can be written as:

$$E_{mtj} = \alpha_0 + \gamma_1 R_{mt} + \gamma_2 T_{mt} R_{mt} + \gamma_3 T_{mt} + \textit{Fixed effect for year of birth} + \textit{Municipality fixed effects} + e_{mtj} \quad (1)$$

Let $\hat{E}_{mtj}$ be the predicted value of the individual's number of years of education from the first stage regression, and the probability of receiving fixed prosthodontic treatment ($FPT_{mtj} = 1$) be the dependent variable. The second stage regression is then:

$$FPT_{mtj} = \alpha_0 + \beta_1 \hat{E}_{mtj} + \beta_2 T_{mt} + \textit{Fixed effect for year of birth} + \textit{Municipality fixed effects} + v_{mtj} \quad (2)$$

We used a linear probability model in the estimation (Angrist, 2001). Some of our results are also presented as reduced form estimates where the probability of receiving fixed prosthodontic treatment is regressed directly on the reform variable. Our main analyses were carried out on a sample including 5 years on each side of the reform. Peer group effects might generate within-municipality correlation. Therefore, we estimated the model with robust standard errors clustered at the municipality level (Cameron and Miller, 2015).

3.3. Data on outcome

Reimbursement payments for dental care for people 18 years and above are administered by the Norwegian Health Economics Administration (Norwegian Health Economics Administration, 2019). Patients 18 years and above who receive fixed prosthodontic treatment have most of their costs covered by the National Insurance Scheme (Ministry of Health and Care Services, 2019). For these adults, the outcome variable in Equation (2) equals 1. If not registered, the outcome variable equals 0. We used the data for 2013, since that was the first year the data were available electronically.

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 (Statistics Norway, 2000). The second register, the Population and Housing Census, contains information about place of residence (municipality) of all persons living in Norway in 1960 (Statistics Norway, 1987). By merging data from the Norwegian Health Economics Administration with data from Statistics Norway, our final data file encompassed the population of people with 9 years of education or less. Most immigrants to Norway have not been exposed to the school reform (Grytten et al., 2013). Therefore they were not included in our analyses. Altogether, our study encompassed 113 237 individuals.

3.4. Supplementary analyses

In order to test the robustness of our results, we carried out additional analyses.

First, we extended our main regression analysis by including the following control variables: household income after tax per member of the household, single person household, disability pension and unemployment benefits (for descriptive statistics see Appendix 1). This was done to test the exogenity assumption of our instrumental variable. That assumption requires that the school reform increases the probability of receiving fixed prosthodontic treatment only through more schooling. In that case the instrumental variable is not correlated with the error term in the regression equation; i.e. biases due to confounding are eliminated. In this supplementary analysis, we expect the coefficient for our education variable not to be influenced by the inclusion of the control variables.

Second, we tested the consistency of our results in different samples. Individuals who were close to the time when the reform was introduced may be more similar in terms of unobservable characteristics compared to individuals who were far from the time when the reform was

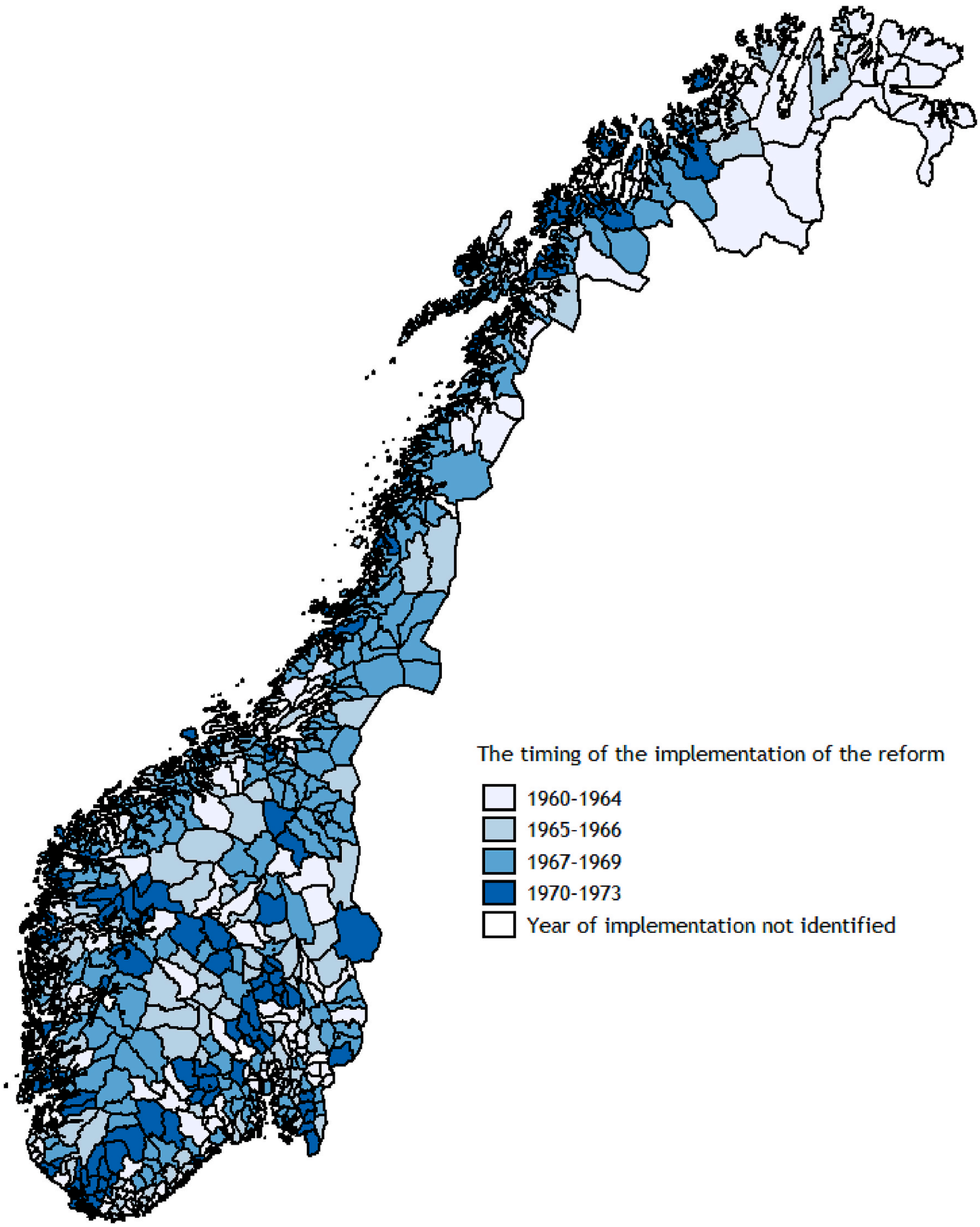

Fig. 2. The implementation of the Norwegian school reform.

introduced. We examined this by estimating reduced form models on different samples with different bandwidths.

Third, we did a placebo test. An advantage with our data is that it was possible to check whether our results for men were biased due to unobservable variables. This test was used to test the validity of the instrumental variable. This is an experiment in which we pretend that the increase in the number of years of schooling was introduced earlier than it actually was. In such an experiment, we do not expect the reform to have any effect on the probability of receiving fixed prosthodontic treatment. If there is an effect, the instrumental variable would be correlated with a third variable. Then we have a poor instrument.

We redefined the reduced form regression to capture pre- and post-reform effects. The pre-reform effects were measured using lead variables, and the post-reform effects were measured using lag variables.

We defined the following independent variables: The contemporaneous effect was defined as 1 in the year the reform was introduced, and 0 in all other years. The first lead dummy variable was equal to 1 in the two years preceding the implementation of the reform, and 0 otherwise. The second lead dummy variable was equal to 1 three and four years before the implementation of the reform, and 0 otherwise. The lagged dummy was equal to 1 two years after implementation of the reform, and 0 otherwise. Our outcome was the probability of receiving fixed prosthodontic treatment.

Fourth, we examined whether the cost per patient of fixed

Table 1
First and second stage regressions. The effect of the Norwegian school reform on the number of years of education, and on the probability of receiving subsidized fixed prosthodontic treatment. Estimated on a sample with 5 years on each side of the reform. Regression coefficients with standard errors clustered by municipality (in brackets).

Variables	Men	Women
Ordinary least square		
Education (in years)	0.0039 *	0.0019
		0.00144
	(0.0017)	
Reduced form		
Reform = 1	0.0059 *	0.0017
		(0.0027)
	(0.0029)	
First stage estimates		
Reform = 1	0.87 **	0.84 **
	(0.03)	(0.02)
Linear trend	0.0868 **	0.0610 **
	(0.0073)	(0.0050)
Reform x linear trend	−0.0794 **	−0.0651 **
	(0.0071)	(0.0054)
F- values	727	1145
Second stage estimates		
Education (in years)	0.0067 *	0.0021
		(0.0033)
	(0.0034)	
N	53 544	59 693

*p < 0.05 **p < 0.001.

prosthodontic treatment that is reimbursed by the Norwegian Health Economics Administration was higher for patients with more education than for patients with less education. If this was the case, it could indicate supplier-induced demand, which would be an undesirable effect of the subsidy scheme (Birch, 1988; Grytten, 2017b; Grytten et al., 1990). This was tested by estimating Equation (2) with *cost per patient of fixed prosthodontic treatment* as the dependent variable.

4. Results

4.1. Main results – regression estimates

4.1.1. OLS estimates

For men, education had a positive effect on the probability of receiving fixed prosthodontic treatment. The size of the regression coefficient was 0.0039 ($p < 0.05$) (Table 1). This implies that the probability of obtaining treatment increased by 0.39 percentage points per additional year of education. For women, the regression coefficient was not statistically significant at a conventional level.

4.1.2. Reduced form estimates

For men, the school reform had a positive effect on the probability of receiving fixed prosthodontic treatment. The size of the regression coefficient was 0.0059 ($p < 0.05$) (Table 1). This implies that the probability of receiving fixed prosthodontic treatment increased by 0.59 percentage points for those who had nine years of compulsory education compared to those who had seven years. For women, the regression coefficient was small and not statistically significant at a conventional level.

4.1.3. First stage estimates

The reform resulted in 0.87 additional years of education for men, and 0.84 additional years of education for women (Table 1). The T-values for the regression estimates were 29 for men and 42 for women, which means that the p-values are small (< 0.001). Thus, there is strong evidence against the null hypothesis of no correlation between the reform variable and the education variable. The F-value for men was 727 and for women 1145. These are high values, which means that all the criteria proposed in the literature for a strong instrument have been fulfilled (Stock et al., 2002). With such high F-values, the first

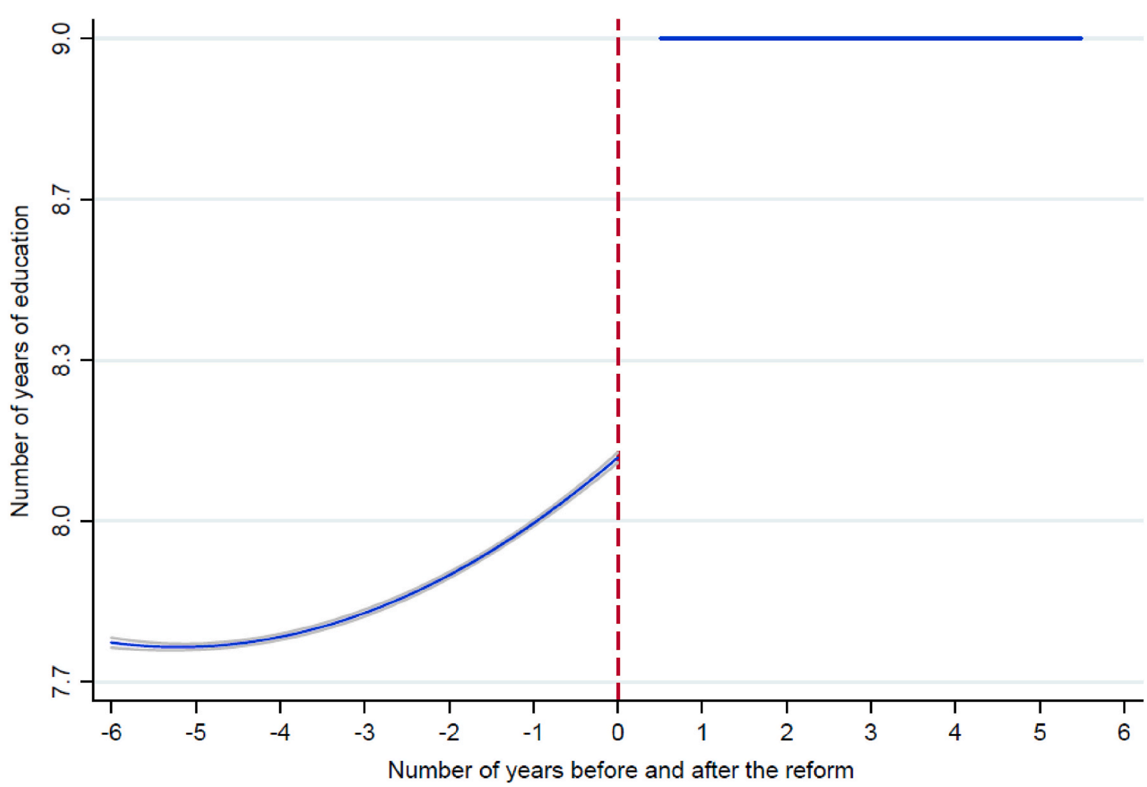

Fig. 3. The effect of the Norwegian school reform on the mean number of years of education.

assumption for our instrumental variable analyses has been fulfilled. This is the assumption about instrument relevance, which says that the instrumental variable must have a strong effect on educational level.

The sign of the regression coefficient for the trend variable was positive (Table 1). The coefficient was 0.086 for men, and 0.061 for women. This indicates that educational levels increased over time prior to the reform (Fig. 3). This is because those who finished compulsory schooling before the reform was introduced could have an optional extra year at school. This extra year was vocational. During our study period, about 8% of those who finished seven years of compulsory schooling had an extra year (Statistics Norway, 1964). 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.

4.1.4. Second stage estimates

For men, education had a positive effect on the probability of receiving fixed prosthodontic treatment. The size of the regression coefficient was 0.0067 ($p < 0.05$) (Table 1). This implies that the probability of receiving treatment increased by 0.67 percentage points per additional year of education. The proportion of men who received fixed prosthodontic treatment was 2.2% (Appendix 1). An implication of the second stage results is that one additional year of education increased the proportion of men who had received fixed prosthodontic treatment by about 30%. For women, the regression coefficient was small and not statistically significant at a conventional level.

4.2. Supplementary analyses

4.2.1. Inclusion of control variables

In this supplementary analysis, we tested the assumption that the school reform increased the probability of receiving fixed prosthodontic treatment only through more schooling. The first stage estimates with the control variables included are reported in Appendix 2. The regression coefficient was 0.87 for men, and 0.84 for women. The sizes of these coefficients were similar to the first stage coefficients from our main analyses in which no control variables were included (Table 1). This gives support to our result that the school reform increased the probability of receiving fixed prosthodontic treatment only through more schooling.

The regression coefficients for disability pension had a statistically significant effect on the probability of receiving fixed prosthodontic treatment ($p < 0.05$) (Appendix 2). For men, the regression coefficient was 0.0065. This implies that the probability of receiving fixed prosthodontic treatment was 0.65 percentage points higher for men with a disability pension than for men without. For the other control variables, there were no statistically significant effects at conventional levels of significance on the probability of receiving fixed prosthodontic treatment. The coefficient for our education variable was not influenced by the inclusion of the control variables (Appendix 2). This gives further support to our belief that the instrumental variable was not biased due to confounding variables.

4.2.2. Samples with different bandwidths

In the analyses with the larger samples, the estimates are slightly more precise than the estimates with the smaller samples (Fig. 4). However, the sizes of the estimates are similar, i.e. our results are robust across samples. Furthermore, the results support our findings shown in Table 1. The main effect of the school reform has been to increase the probability of receiving treatment for men. There was little or no effect for women.

4.2.3. Unobservable variables – a placebo test

We did not expect the lead variable to have any significant positive effect on the outcome. This is supported by the results. The regression coefficients were small (Fig. 5). These results were in clear contrast to the effects of the lag variable. For men the coefficients for the lag variables were of a reasonable size, they had the correct sign (positive), and the value 0 was not contained in the 95% confidence interval. The estimate for the variable measuring the contemporaneous effect was about the same as the reduced form estimate in Table 1. As expected, for women there were no effects.

4.2.4. Education and cost per patient of fixed prosthodontic treatment

The results are shown in Fig. 6. 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.

5. Discussion

5.1. Education and fixed prosthodontic treatment – interpretation of our results

In several Western European countries, particularly in the Scandinavian countries, subsidized dental care is an important part of welfare policy (Holst, 2007; Ministry of Health, 2002). An aim of this policy is to make dental services equally accessible to everyone. 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. This finding is consistent with the findings from a large European survey of socio-economic inequalities in the use of dental services (Palència et al., 2014). Altogether 11 countries were included in the survey. In countries with public coverage of dental care, access to services was better than in countries with no or only a small coverage. However, even in countries with coverage, highly educated people utilized more dental services than less educated people (Palència et al., 2014).

In our data, there was no information about oral health status. It is unlikely that the children's oral condition before finishing school would affect their educational attainment. Therefore, it is reasonable to assume that dental status is a mediator from education to dental treatment. It would have been interesting to estimate the causal effect of education on oral health. Most likely, this effect is positive, i.e. more education leads to better oral health. Further, those with good oral health will need less fixed prosthodontic treatment than those with poor oral health. In that way, those who have good oral health as a result of more education, may have benefitted the least from the subsidy scheme for fixed prosthodontic treatment.

Less treatment need, due to a more highly educated population, may lead to supplier-induced demand. The basis for the supplier-induced demand hypothesis is that if the dentist's income is threatened, then he/she can counteract the fall in income by increasing the quantity of services he/she offers (Grytten and Sørensen, 2000). One possibility is to do more fixed prosthodontic treatment for those with less treatment need; i.e. highly educated patients. Our results, reported in Fig. 6, showed that this was not the case.

Men with more education made more use of fixed prosthodontic treatment than men with less education. The difference was fairly large and causal (Table 1). There was no effect for women. Most likely, this is because women value the appearance of their teeth more than men, and women do so independently of the socio-economic group they belong to (Carlsson et al., 2008; Dahl et al., 2012; Fukai et al., 1999; Suominen-Taipale et al., 2000; Östberg et al., 1999). For example, in surveys a higher proportion of women than men have reported that it is important to have sound and healthy teeth. Typically, for women it is important to have "beautiful and perfect teeth" and they think that "tooth loss that is visible is something to be ashamed of" (Carlsson et al., 2008).

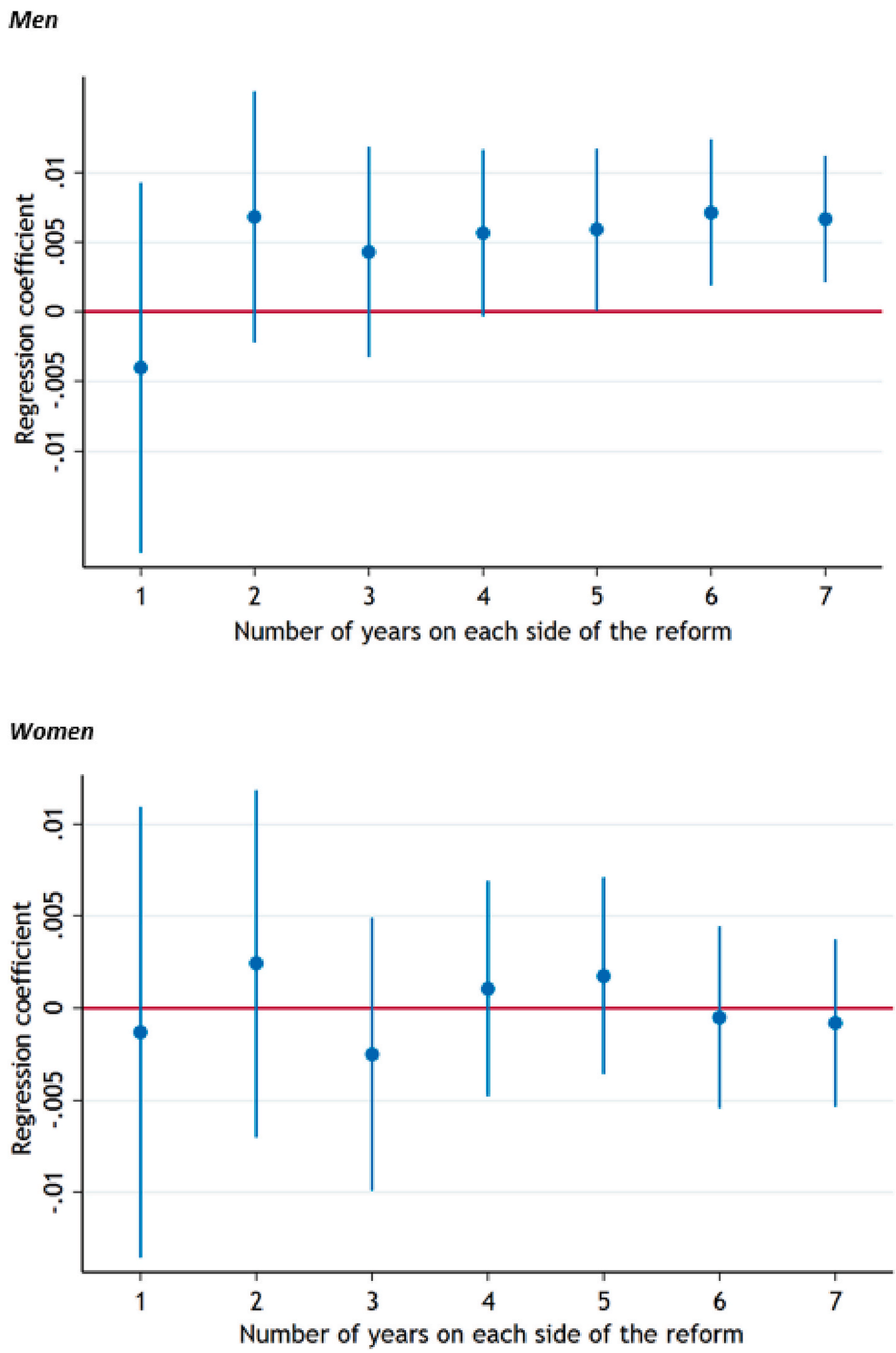

Fig. 4. The effect of the Norwegian school reform on the probability of receiving subsidized fixed prosthodontic treatment. Reduced form regression coefficients with 95% confidence intervals. Samples with different number of years on each side of the reform.

5.2. Education and fixed prosthodontic treatment – methodological considerations

For men, the causal estimate was twice as large as the estimate from the OLS regressions. This indicates that morbidity is an important confounding variable that has been omitted from the OLS estimation. Ability, place of residence and time preferences are not likely to be important confounders. This is because the OLS estimate is downward rather than upward biased. Our results from the instrumental variable estimation underscore the importance of using a statistical technique in which confounding variables have been controlled for. These variables are unobserved, hence their individual effects cannot be estimated (Angrist and Pischke, 2009; Rassen et al., 2009).

In our analyses we did not adjust for oral health status. A relevant question is how such an adjustment would have affected our causal estimates. This can be discussed with reference to the figure in Appendix 3. In the figure, oral health status is depicted as a mediating variable from education to subsidized fixed prosthodontic treatment. Thus, education affects access to treatment through two paths. One path is through a direct effect that goes from education to treatment, without passing through the mediating variable. Another path is through an indirect effect that passes from education to treatment, through oral health status.

One possible way to adjust for severe oral conditions would be to

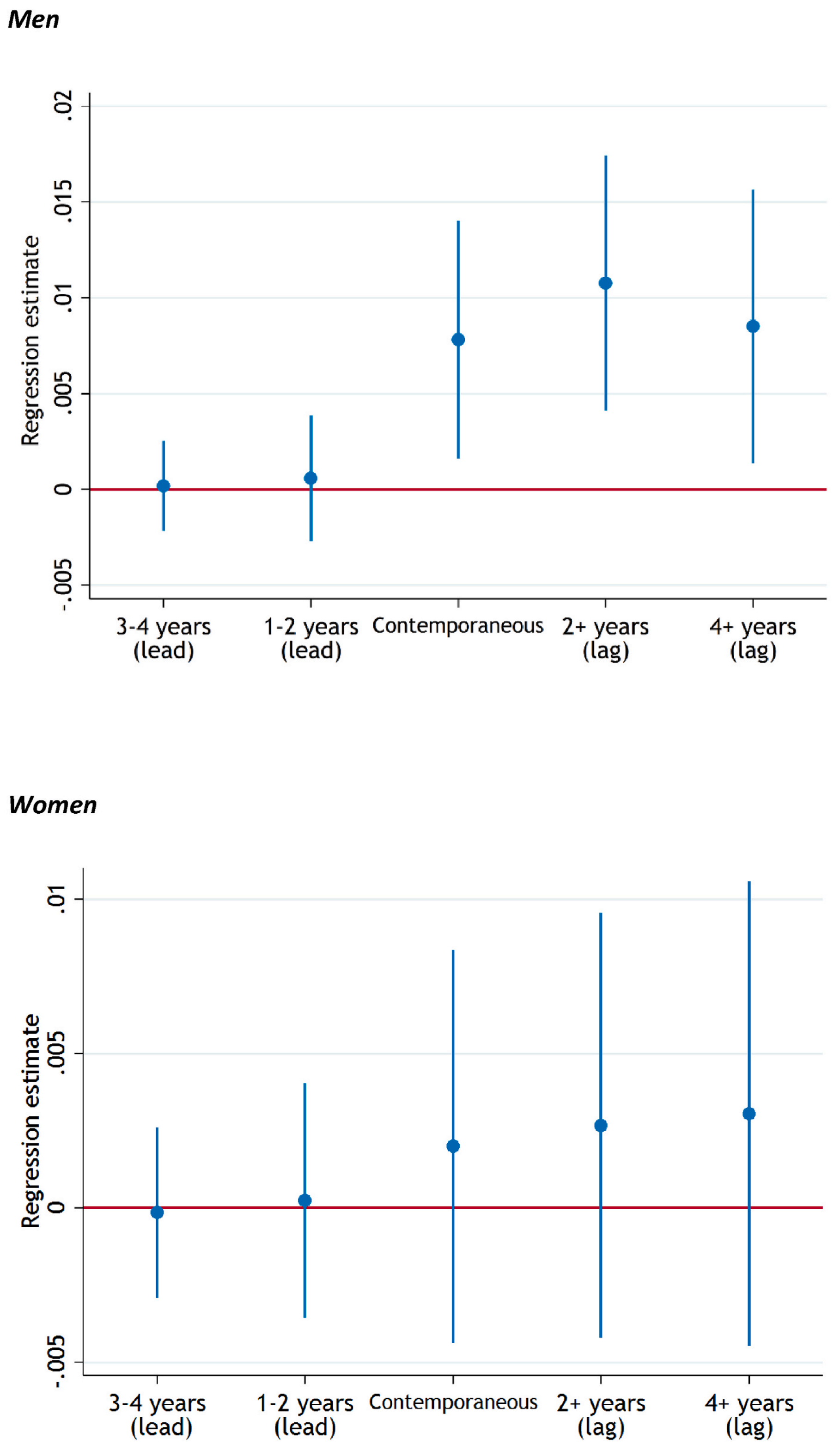

Fig. 5. Lead-lag effects of the Norwegian school reform on the probability of receiving subsidized fixed prosthodontic treatment. Reduced form estimates with 95% confidence intervals.

estimate the following regression (Gerber and Green, 2012; McKinnon, 2015):

$$FPT = \alpha_0 + \delta Education + \theta Oral\ health\ status + e_2 \quad (3)$$

In Equation (3), the parameter δ gives the direct effect of education on treatment. The indirect effect is the product of the path from education to oral health status (governed by the parmeter λ) and from oral health status to treatment (governed by the parameter θ).

Education is assigned randomly through the introduction of the school reform. This is not the case for the mediating variable. Most likely, oral health status is related to unmeasured causes of our treatment variable. Therefore, adding oral health status to the regression

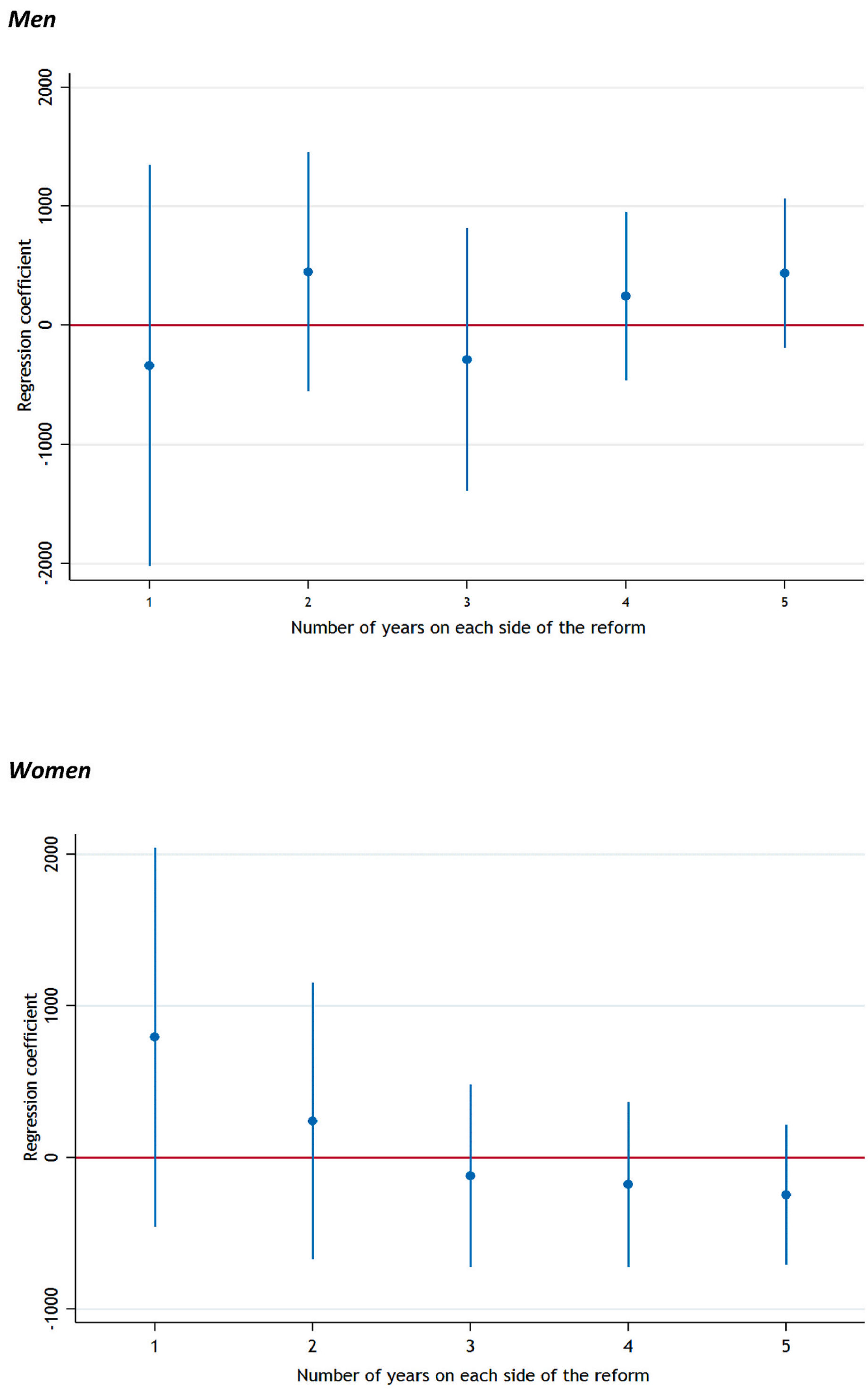

Fig. 6. Second stage regression coefficients for reimbursements for fixed prosthodontic treatment per patient (EUROs). Samples with different number of years on each side of the reform (95% confidence intervals).

model would lead to biased estimates of the parameter θ in Equation (3), and of any variable that oral health status is correlated with. In our case, oral health status is likely to be correlated with education. Therefore, in Equation (3) both the parameters δ and θ would be biased. According to Gerber and Green (2012) the bias will be in a direction that exaggerates the effect of the mediation variable on the outcome. In our data, there is no variable measuring oral health status. However, even if such a variable had been available, adjusting for it without introducing bias in our estimates would be difficult.

5.3. Education and fixed prosthodontic treatment – policy implications

It is not easy to suggest which policy, if any, would be effective in reducing inequalities in access to fixed prosthodontic treatment for men. 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 men with a low level of education. On the other hand, this approach would only be effective for people who do not know about the scheme. We do not have data about what proportion of the population do not know about the subsidy scheme. Therefore, it is difficult to assess how effective a policy of providing more information about the subsidy scheme would be. There are also barriers to access to fixed prosthodontic treatment, other than lack of information about the scheme. Examples are dental anxiety and travelling time to the dentist (Freeman, 1999; Grytten, 1991; Hill, Chadwick, Freeman, O'Sullivan and Murray, 2013). People with dental anxiety or long travelling time may not visit the dentist to receive fixed prosthodontic treatment, even though they have information about the subsidy scheme. Dentists treat their patients equally, i.e. the amount of subsidized services provided is independent of patients' level of education (Fig. 6). 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.

In conclusion, we have estimated the causal effect of education on the probability of receiving fixed prosthodontic treatment in the adult Norwegian population. We found that men benefitted most from a universal welfare scheme targeted at everybody. We suggest providing information about fixed prosthodontic treatment and the subsidy scheme, targeted at men with a low level of education.

Authors' contribution

JG conceived the study, planned its design and its coordination, and drafted the manuscript. IS organized and built the database and performed the statistical analyses. JG and IS contributed to data interpretation. Both authors approved of the final version of the manuscript.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2020.113105.

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