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RESEARCH ARTICLE

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Did adolescents in Norway respond to the elimination of copayments for general practitioner services?

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Abstract

Copayments for primary care services may lead to decreased access to and underconsumption of necessary health care for vulnerable patient groups, such as adolescents. In Norway, in 2010, adolescents aged 12 to 15 years were exempted from copayments for general practitioner (GP) services, and the aim of this study is to estimate whether being exempted from copayments led to increases in GP visits.

We apply the synthetic control method using the elastic net regression as a data-driven approach to construct a relevant counterfactual from our pool of age groups not affected by the reform. Data on the number of GP consultations for males and females from 2006 to 2013 is obtained from the Norwegian Health Economics Administration.

Our findings suggest that exempting adolescents from copayments increased the number of per person GP consultations by 22.1% among females and 13.8% among males. This indicates that adolescents between the ages of 12 and 15 were sensitive to copayments before the reform and females more so than males.

KEYWORDS

adolescents, copayments, general practitioner services, health care utilisation, synthetic control method

1 | INTRODUCTION

Copayments for primary care services involve a trade-off between efficient use of health care resources and access to health care services. The absence of copayments may function as a moral hazard because patients do not internalise the cost of these services and this drives up consumption. On the other hand, their presence may create a barrier to access and underconsumption of necessary health care for vulnerable patient groups.

Due to this trade-off, it is important for policy-makers to have evidence of to what extent different patient groups are sensitive to copayments. The effect of changes in copayments on use of health care services has been studied extensively in the literature (see Kiil & Houlberg, 2014; for a systematic review). However, most of these studies have focused on adult responses to increases in copayments, and few have addressed the issue of copayments on adolescent use of health care services. Several studies report that copayments are a barrier to access for adolescents, coupled with other factors such as fear of lack of confidentiality or parents finding out, waiting times and limited opening hours (Gleeson, Robinson, & Neal, 2002; Tylee, Haller, Graham, Churchill, & Sanci, 2007). Moreover, from a societal point

of view, addressing access to health care services for this age group is important because health care access at this age may have significant effects on future health and labour market outcomes (Case, Fertig, & Paxson, 2005).

In Norway, starting in 2010, a copayment reform was introduced that exempted all adolescents between the ages of 12 and 15 years from paying a fee of 17.5 EUR to see their primary care physician. The reform resulted in a natural experiment, where we have a considerable change in copayments for a treated age group and can use the other age groups as relevant control groups. This gives us the opportunity to assess the impact of copayments on demand for primary care services for adolescents and specifically the aim is to estimate whether being exempted from copayments led to an increased use of general practitioner (GP) visits for the treated age groups. Exempting adolescents from copayments might also reduce other barriers to access, such as the confidentiality concerns; by reducing their financial dependency on parents, it diminishes the need for parental involvement. Coupled with the size of the price change (from 17.5 to 0 EUR), the reform could thus have a considerable impact on adolescent use of GP consultations. In addition, since boys and girls start to develop different patterns in GP visits from the age of 12 (see Figure 1), the effect of the reform is estimated separately by gender.

In contrast to a previous study, which applied a difference-in-difference approach to estimate the causal effect of copayments on adolescents' use of primary care services (Olsen & Melberg, 2016), this paper applies a data-driven approach to select and weight the pool of control groups appropriately. We apply the synthetic control method (SCM) proposed and developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010, 2015). Advantages of the SCM, compared with the Difference-in-Difference method, are that it relaxes the parallel trends assumption, by allowing the effect of observed and unobserved factors to change over time, and it uses a combination of units and not just a single unit as a control group. However, the SCM places some constraints on the weighting of the control groups that appear to be too restrictive for our data. Therefore, we use the elastic net regression as a method for weighting the control groups, because this allows for relaxing the restrictions.

1.1 | Previous empirical studies on copayments and use of health care services

There have been three recent studies on adolescents and copayments within the European health care system context, and they have found mixed evidence. In the Czech Republic, children under the age of 18 were exempted from a copayment fee of 1.2 EUR in 2009. Two studies (Votapkova & Zilova, 2015; Zápal, 2010) analysed the effect of this policy change on use of outpatient care and prescription drugs. Both used difference-in-difference analysis and the adult population as a control group. Neither study found any effect of the exemption of copayments on the use of these services. The authors argue that this was because the size of the copayments was low to begin with, thus suggesting that the fee did not prevent children and their parents from using primary care services in the first place. However, they found that there was a small decrease in use of services the month before the reform was introduced, indicating a timing effect, where the patients waited to see to their GP until it was free. A similar policy change occurred in the Swedish county of Skåne in 2002, for adolescents aged 7 to 19, where they were exempted from a copayment fee of 10 EUR. Using

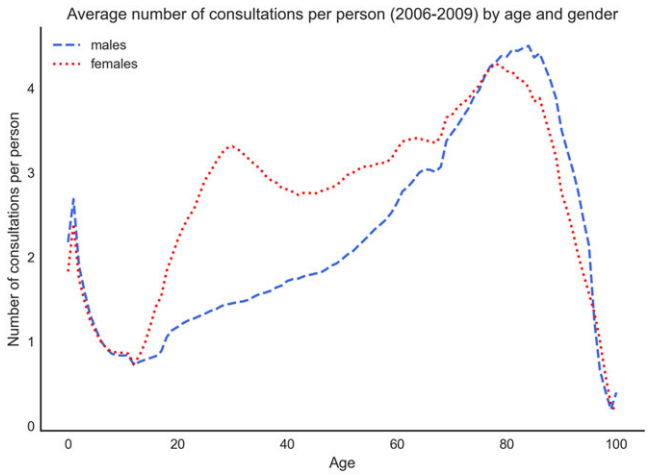

FIGURE 1 Number of consultations per person, averaged over the prereform period (2006 to 2009) by age and gender [Colour figure can be viewed at wileyonlinelibrary.com]

difference-in-difference analysis, here as well, with children (ages 3 to 6) and young adults (ages 20 to 24) as a control group, Paul and Nilsson (2014) found that the use of GP services increased by 10%. Additionally, the response differed by health and income but not by education.

The studies differ in terms of institutional settings and size of copayments that they evaluate. Patients in the Czech Republic have direct access to secondary care services (Alexa, Rečka, Votápková, et al., 2015), whereas Norwegian patients have limited access due to gatekeeping. Other substitutes for primary care services that are important to consider are, for example, emergency services, and specifically for adolescents, access to a school nurse. In Norway, patients may use emergency primary care services, but this also incurs a copayment fee. Adolescents and children may visit a school or public health nurse for a consultation for free, but the availability and services provided are limited compared with a GP. For example, a school nurse is on average available only 3.5 hr a week (Kjelvik, 2007). The next section provides a more detailed overview of the primary care setting in Norway.

2 | PRIMARY CARE IN NORWAY

Primary care in Norway is organised at the local municipal level, where municipalities contract with individual self-employed GPs to provide primary health care services. GPs are reimbursed through a mix of capitation (35%), fee-for-service (35%) and patient copayment (30%; Lindahl, 2017). Since 2001, every resident has a right to register with a GP of their choice, within their municipality, and per 2016, 99.6% of the population was registered with a GP (Norwegian Directorate of Health, 2017). The GP scheme in Norway functions as a gatekeeping system, where referrals from a GP are required for access to secondary care services and use of prescription drugs.

For a standard consultation with a GP in Norway, the copayment was approximately 17.5 EUR in 2010. Additionally, Norway has a copayment cap where, during a calendar year, patients pay a maximum of 230 EUR (in 2010)[1] for most of the health care services where a copayment is required. (An additional copayment cap exists for physiotherapy and certain dental services). According to data from the Norwegian Health Economics Administration (HELFO), 29% of the population reached this limit in 2012 and, thus, received a copayment exemption card. The limit on copayments means that the effect of copayments on number of consultations may be smaller than in countries without a similar cap.

From the age of 12, adolescents are allowed to contact the health care services without parental consent or parents being informed about the contact, although health care personnel are required to inform parents in cases where information is required to fulfil parental responsibility (Norwegian Patient's Rights Act, 1999). Prior to 2010, copayments for GP consultations were required for patients aged 12 and older. As of January 1, 2010, this age threshold was increased to age 16, thus exempting adolescents between the ages of 12 and 15 from paying a copayment fee. According to the budget proposal where the reform was introduced, the aim of the reform was to "Make it easier for young people in this age group to consult a doctor regarding mental conditions, suspicion of sexually transmitted diseases, questions about contraception, unwanted pregnancy, conditions associated with substance abuse, and so on" (Ministry of Health and Care Services, 2009).

3 | DATA

The analysis is based on data obtained from the primary care reimbursement administrative systems (KUHR), which is administered by the Health Economics Administration (HELFO). Visits to publically contracted GPs have been registered electronically in this system since 2006. There is a small number of GPs (2%) who are not registered in KUHR because they are in private practice without municipal contracts. The data set consisted of aggregated numbers of GP consultations for each age group and gender in the time period 2006 to 2013. By including data for 4 years in the postreform period, this ensures a sufficient time to assess an impact of the reform. The number of visits is based on consultations that generated a reimbursement tariff for standard consultations, emergency contacts with the GP office or electronic consultations.

The data were combined with population data in each age group from Statistics Norway. The population data are measured on December 31 each year. Using this data, we calculated the average number of GP visits per capita for each gender and age group per year, in order to reduce bias from size of the age cohorts.

[1]The value in Euro is calculated using the exchange rate for 2010; 1.00 EUR: 8.00 NOK

Gender is unidentified for 0.1% of the observations in the data set. In addition, according to HELFO, 3.2% of the records on GP visits are missing for year 2006, because some GPs had not yet switched to the electronic reporting system. We assume that the missing consultations are not systematically missing from our data set for different age groups or for males versus females.

4 | METHODS

This study differs from previous studies on the effect of copayments because we applied a data-driven method to select the most relevant control groups to construct a counterfactual for the treated group. Specifically, we used a modified version of the SCM developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) to select and construct a synthetic control group. Abadie et al. (2010, 2015) argue that for aggregate data a weighted combination of control units is better at depicting the characteristics of the treated unit than any single unit alone. The identification strategy of the SCM is that groups with similar outcomes in the pretreatment period are assumed to have similar treatment-free outcomes in post-treatment periods (O'Neill, Kreif, Grieve, et al., 2016). Thus, units with similar past outcomes are likely to also be similar in terms of unobserved confounders. To construct the synthetic control group, the potential control groups are weighted such that the difference between the preintervention outcomes of the treated and control groups are minimised. For a formal exposition of the SCM, see papers by Abadie et al. (2010) and Abadie et al. (2015).

The SCM imposes some restrictions, and by doing so ensures that it finds unique weights for each control group, avoids extrapolation and that the control and treated group are similar in the levels of the outcome prior to the intervention period. First, the weighting of the control groups is estimated without an intercept, which is referred to as the "no-intercept" constraint. Second, the weights are constrained to be non-negative and sum up to one. However, Doudchenko and Imbens (2016) argue that these constraints are not necessarily the most appropriate for all types of data settings and instead restrictions are to be imposed based on their merit and not just a matter of routine. They point out that imposing the "no-intercept" and "summing-up-to-one" constraints makes it difficult to find relevant control groups if the treated group is systematically smaller or larger than the control groups or if it is at the extreme end of the distribution in terms of the outcome.

There are several reasons why the SCM is unable to construct a synthetic control group when applied to the data in this paper, and particularly for the males. First, the treated groups are at the lower end of the distribution in terms of the outcome compared with the other age groups (see Figure 1). By excluding an intercept, this will attempt to force the synthetic and treated data to have same means. Second, constraining these weights to equal to one will inflate the slope of the synthetic control group larger than the actual data (Li, 2017).

Doudchenko and Imbens (2016) propose a similar strategy to the SCM, that is, constructing a synthetic counterfactual where the control groups are weighted so as to minimise the distance between the pretreatment outcomes for the treated and potential control groups. However, they relax the sum of weights equal to one and no-intercept constraints. Li (2017) shows that modifying the SCM in this way, by relaxing some of the constraints, gives more unbiased results for data settings where the SCM and its constraints lead to poor pretreatment fit.

In addition, for data settings where the number of control units is larger than the number of time periods, then there may be several combinations of the weights that satisfy the restrictions. To regularise the estimator for the weights, Doudchenko and Imbens suggest using the elastic net penalty. The preferred method for estimating the effect of the copayment reform is thus the modified approach proposed by Doudchenko and Imbens (which will be referred to as the elastic net in the results). Using their notation, the objective function for constructing the synthetic control group is thus:

$$Q\left(\mu, \omega | Y_{t,pre}^{obs}, Y_{c,pre}^{obs}\right) = \left\| Y_{t,pre}^{obs} - \mu - \omega^T Y_{c,pre}^{obs} \right\|_2^2 + \lambda\left(\frac{1-\alpha}{2}\|\omega\|_2^2 + \alpha\|\omega\|_1\right)$$

This means that given $Y_{t,pre}^{obs}$ and $Y_{c,pre}^{obs}$, that is, the observed outcomes in the prereform period for the treated and control group we want to estimate values for the intercept, μ and weights, ω, such that the distance between the observed outcomes are minimised. The first part of the equation refers to the exact balance restriction and is similar to the original SCM except that the intercept, $\mu \neq 0$. In addition, $\sum_{i=1}^{N} \omega_i \neq 1$. The second part of the equation is the elastic net penalty term, where the parameter λ is the ratio of l1 (Lasso) and l2 (Ridge) type penalties and α is the degree of regularisation. For more details on the elastic net penalty, see Zou and Hastie (2005).

Because the reform targeted several age groups, there are multiple treated units. The treated units are aggregated to a single treated unit by dividing the sum of the consultations for ages 12 to 15 by the population for this age group (as suggested by Abadie et al. (2010) and Kreif et al. (2016)).

The data set only includes data for the outcome variable, which is the use of GP services. Therefore, only the preintervention values for the outcome variable are used to construct the synthetic control group. Other covariates and determinants of visits to the GP may be included to match on when constructing the synthetic control group; however, this is not strictly necessary because matching on preintervention values of the outcome alone controls for unobserved factors (Athey & Imbens, 2016). In addition, because the effect is estimated at the age group level, any potential determinants of health care demand would have to determine GP visits for a whole age group.

The method applies a data-driven approach to choosing the most relevant control groups within a pool of potential control groups, but the researcher still needs to define a subset of suitable groups that is thought to be driven by the same structural processes and not subject to structural shocks during the observation period (Abadie et al., 2015). The following aspects were considered when choosing the subset for inclusion in the analysis. First, the data set did not include use of GP services in nursing homes, only office visits to the GP. This means that older adult patients may be using GP services, but this is not represented in the data and thus they may appear to have similar levels, but their use of GP services differs from the treated group. Second, Norwegian citizens are allowed early retirement from the age of 62 (as of 2011), and the statutory retirement age is 67. Demand for health care services may change as a result of retirement, but the empirical evidence is mixed (see, e.g., Behncke, 2012; Coe & Zamarro, 2011). Third, parents in the same households as the adolescents may prioritise their own need for GP services differently as a result of the reform. This suggests that younger age groups may be more suitable as comparison groups and limiting the subset to age groups below the age of 21 enables comparison between groups that are more similar in terms of age. The analysis is performed using ages between 0 and 20 as the main comparison units, but results are also presented using different age groups in the donor pool as sensitivity analysis.

The effect is measured as the difference in the outcome for treated and synthetic control group in the post-treatment period and the numbers presented in Table 1 are an average for the post-intervention period. Estimates from both the original SCM and the modified SCM are presented in Figures 3 and 4 to show the improved fit of the preferred modified approach.

We follow Abadie et al.'s (Abadie et al., 2010) suggestion of performing placebo analysis to check the significance of the results. Specifically, each control group is iteratively assigned to the treated status, and the elastic net procedure is applied to each age group (ages 0 to 20) to compute a distribution of estimated effects. The estimated effect for the treated group (ages 12 to 15) is then compared with the distribution of estimated effects for each control group. Placebo results that show effect sizes that are as large as the effect estimated for the treated group suggest that the effect of the reform is not significant.

When running the placebo analysis, it is important to evaluate the fit for each of the control groups in the pretreatment period. Abadie et al. (2010) state that placebo runs with poor fit prior to the reform do not provide information to measure the relative rarity of estimating a large effect for age groups with a good pretreatment fit. We, therefore, estimate the fit by calculating the pretreatment mean square prediction error (MSPE) and eliminate from the placebo analysis age groups with a MSPE that is 10 times larger than the MSPE for the treated group (as suggested in Abadie et al., 2010).

5 | RESULTS

5.1 | Descriptive results

Figure 1 displays the level of GP services by age and gender and represents an average for the prereform period (2006 to 2009). The number of consultations is about 0.8 per person at the age of 12, which is lower than the population average of

TABLE 1 Estimated size of the effect of the copayment reform for the different methods

	SCM (%)	Elastic net (%)
Females	17.5	22.1
Males	2.8	13.8

Note. The effect estimate is an average over the entire postreform period (2010–2013).

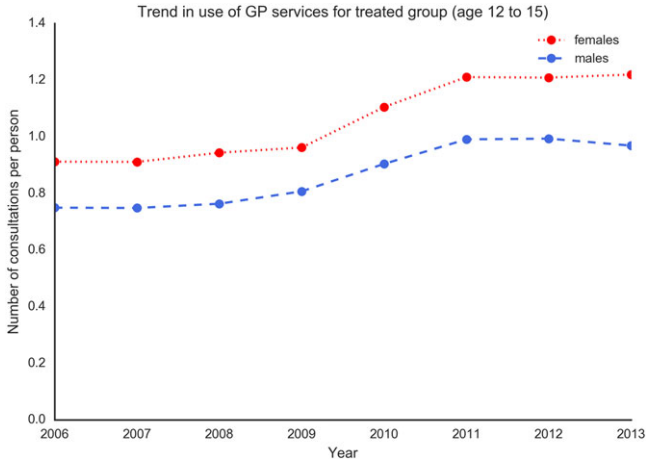

FIGURE 2 Trend in use of GP services for the treated age group (age 12 to 15) in the prereform (2006–2009) and postreform (2010–2013) period [Colour figure can be viewed at wileyonlinelibrary.com]

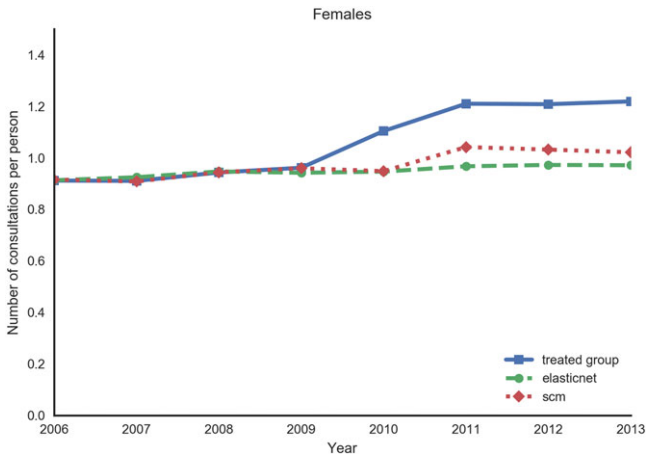

FIGURE 3 Number of consultations per person for the female treated group and the constructed control group for each method [Colour figure can be viewed at wileyonlinelibrary.com]

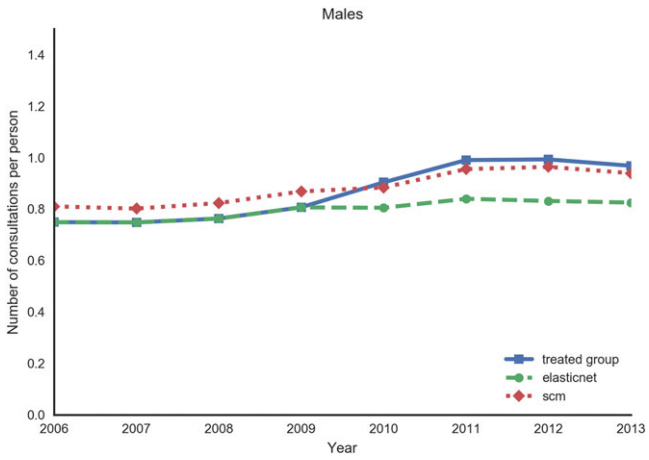

FIGURE 4 Number of consultations per person for the male treated group and the constructed control group for each method [Colour figure can be viewed at wileyonlinelibrary.com]

2.2 and 2.7 GP visits per capita for males and females, respectively. Males and females diverge in their visits to the GP from the age of 12; therefore, we do the analysis separately for males and females. In addition, the use of GP services seems to increase for both groups after the age of 12 and declines after the age of 80.

Figure 2 shows the number of consultations for the treated group in the years prior to and after the reform was introduced. Across the prereform years, the number of consultations per person for females in the treated group was just below 1 consultation for females and around 0.77 for males. After the copayment reform, the number GP consultations per person increased by 12% and 14.8% from 2009 to 2010 for males and females, respectively.

5.2 | Main results

Figures 3 and 4 display the trajectory of use in GP services for the pretreatment and post-treatment periods for the treated group (12–15 years), and its counterfactual constructed using both the SCM and elastic net procedures. According to Abadie et al. (2010), the SCM is to be assessed on the pretreatment fit, which is assessed qualitatively based on the output figure. If the fit is close to perfect, then the SCM is unbiased. For females, the counterfactual based on the SCM and elastic net procedures has good pretreatment fit. The SCM gives a positive weight to four age groups (ages 9, 10, 11, and 17), whereas the elastic net gives a positive weight to three age groups (ages 9, 18, and 19). As seen in Table 1, the predicted average effect of the reform for the postreform period is 22.1% and 17.5% for the elastic net and SCM methods, respectively.

For males, the SCM is not able to find a good pretreatment fit, indicating that constraining the method to find control groups that are similar in levels is too restrictive. The SCM predicts an average effect of 2.8%, because the synthetic control group exhibits higher levels of GP consultations than the treated group. The elastic net approach is able to find a better fit for the data. The SCM only gives a positive weight to age 16, whereas the elastic net gives a positive weight to ages 1, 9, 10, 16, and 17. The elastic net predicts an average effect of 13.8% for the postreform period (see Table 1).

The predicted effect estimates of the reform are larger than the observed increase in GP visits from the descriptive results (12.0% and 14.8%). This may be explained by the methods predicting a declining trend in GP visits for the synthetic control group. Thus, compared with the predicted effect for the synthetic control group, the treated group experienced an effect of 13.8% and 22.1%. In addition, the estimates are predicted for the entire postperiod reform, thus capturing the reform effect for 4 years compared with the 1 year increase calculated for the descriptive results.

5.3 | Sensitivity analysis

Table 2 shows the effect estimates from the sensitivity analyses by including different age groups in the pool of potential control groups. The effect estimates are relatively stable for the groups up to age 60. However, including ages between 60 and 66 and 67 and 70 seems to increase the estimated effect, especially for the males.

5.4 | Placebo results

Figures 5 and 6 display the results from the placebo analysis. The estimated effect of the reform is larger for our treated group than for the majority of the other age groups for the males. For the females, on the other hand, one age group (age 4) also experienced an increase in use of GP consultations after 2010 as well. However, restricting the number of control groups by assessing the MSPE prior to the reform, results in relatively few placebo groups.

TABLE 2 Estimated size of the effect of the copayment reform for the different age groups included as control units

Age groups as control units	Females (%)	Males (%)
20	22.12	13.85
30	22.76	16.32
40	20.89	15.61
50	20.89	13.11
60	21.01	12.84
66	24.79	19.71
70	24.38	19.65

Note. The effect estimate is an average over the entire postreform period (2010–2013).

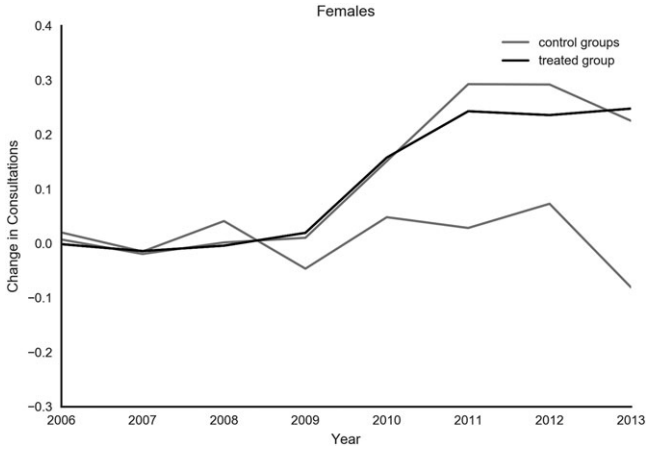

FIGURE 5 Estimated effect size for the female treated group and placebo effect size for the other age groups

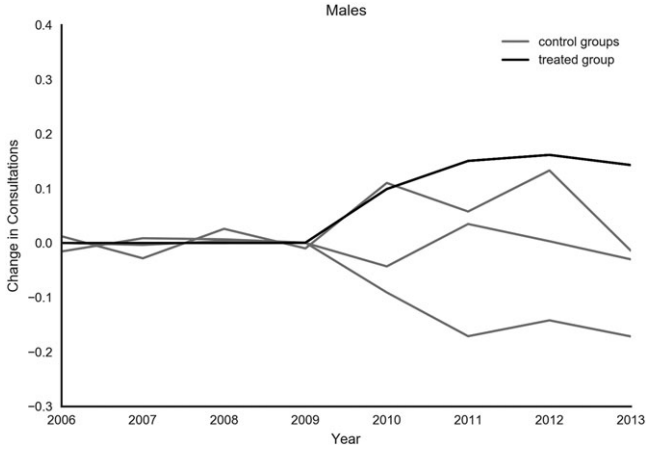

FIGURE 6 Estimated effect size for the male treated group and placebo effect size for the other age groups

6 | DISCUSSION

Using the preferred model, we find that exempting adolescents from paying a fee to see their GP results in a 22.1% increase in consultations for females and 13.8% increase for males on average for the postreform period. Considering that there are approximately 120,000 adolescents between 12 and 15 years old in a given year for each gender, an effect of 22.1% and 13.8% will result in 24,000 extra GP visits for girls and 13,500 extra GP visits for boys on average per year. Given remuneration of 17.5 EUR for a standard consultation in 2010, the extra visits have resulted in an additional 656,000 EUR of healthcare spending per year.[2]

In light of the aim of the reform, which was to increase access to GPs for adolescents, the copayment exemption achieved its goal. However, with regard to the trade-off discussed at the beginning concerning access versus efficient use of health care services, the results can be interpreted in two ways. First, given the large increase in the number of visits for this group suggests that GP services were under-consumed prior to the reform and that exemption from copayments was important for access. On the other hand, the exemption may have led to an increase in consumption of unnecessary health care services for this age group. Diagnostic data for GP visits in this age group could give us information on why adolescents visit their GP, but further research is needed on the health benefit for this age group. In addition, further research using individual-level data can account for factors such as household income and socio-economic status and analyse whether the effect of the exemption differed for different subgroups.

[2]This calculation has only taken into account standard consultations and does not include any potential increase and costs associated with diagnostic tests, additional treatments and so on. Thus, the actual costs to the government may be slightly larger.

We performed stratified analysis by gender and showed that adolescent females are more responsive to the copayment reform. This suggests that for this age group, prior to the reform, girls were more vulnerable than boys to copayments. There is limited research on gender differences in adolescents' response to copayments with which we can compare our findings; moreover, studies on this topic based on data for adults provide mixed evidence (Pendzialek, Simic, & Stock, 2016). Nevertheless, some studies report that adolescent girls have a greater need for confidential GP consultations, that is, without their parents' involvement (Edman, Adams, Park, & Irwin Jr, 2010; Klein, Wilson, McNulty, Kapphahn, & Scott Collins, 1999); thus, it could be that the copayment exemption had a greater effect among adolescent girls than boys partly because it encouraged confidential consultations with GPs.

Compared with the previous studies on how copayments affect adolescent use of primary care services (Paul & Nilsson, 2014; Votapkova & Zilova, 2015; Zápal, 2010), our results suggest a larger effect of the reform. One possible explanation is that the Norwegian adolescents were exempted from a larger copayment (approximately 70% greater compared with adolescents in Sweden). Moreover, the Norwegian reform targeted an age group that has both a greater need for GP services without parent involvement, compared with the younger children included in the other studies and who (from the age of 12) are permitted to contact their GP without their parents being informed. Lastly, the results could be influenced by the number of control groups included. For instance, results from an approach using many age groups (such as in the Swedish and Czech studies) will fluctuate less. The modified SCM approach used fewer age groups and will therefore have higher variance, which may contribute to explaining the larger estimate in our study.

A decrease in the price for GP services may have spill-over effects, both within the primary care sector and to other types of health care services. One important spill-over effect to consider is the use of school nurses or public health nurses at health clinics. The services provided by a school nurse may be considered as both substitutes and supplements to the services offered by a GP. For example, they provide services such as sexual education and prescription for contraceptives, dietary advice, prevention of mental health issues, and vaccination (Norwegian Directorate of Health, 2004). An increase in GP visits after the copayment reform may be a result of adolescents using GPs rather than a nurse for similar types of services or they may be using GP services in addition to the services offered by the school or public health nurse, but there is no data available in Norway on adolescent use of school or health clinic services.

A potential concern with our results is that they could be driven by a cohort effect and not only a price effect. For example, the birth cohort turning 12 years in 2010 might be unhealthier or have a greater need for health care services due to a cohort effect compared with those who turn 12 in 2009. If this is true, then the observed increase in GP visits captures both a price effect and a sickness effect. The extent of a cohort effect can be estimated by comparing GP visits for the cohorts affected by the reform in the observed prereform time periods to cohorts not affected to see if the treated cohorts have a higher number of GP visits on average. For similar age groups (ages 5 to 7), the treated cohorts have an average number of visits of 0.99, whereas the nontreated cohorts have an average of 1.06 visits. Similarly, for ages 12 to 14, the treated cohorts have an average of 0.76 visits compared with 0.75 visits for the nontreated cohorts. This does not indicate that the results are driven by a large cohort effect.

Another potential concern is the stability of the results. Table 2 shows how the effect varies by including more age groups as control groups. It should be noted that with a limited time period before the interventions, and as the number potential control groups are expanded, including more age groups may lead to control groups that randomly happen to match well in the pretreatment period. For instance, with four pretreatment time periods and more than 50 potential control groups (i.e., age groups), some of the age groups may have a pattern that randomly happen to fit well. In the analysis, the results were stable when we included controls up to 60 years old. They were less stable after including ages above 60, and for this reason we have focused on the results using the controls with an age 20 and below that were close to the treated age groups (12–15).

Furthermore, a potential limitation for the interpretation is that one of the placebo tests also showed an increase in the number of visits to GP for one age group. This may represent random errors that are natural when one tries many placebo controls.

Lastly, the credibility may be compromised if other events in the post-treatment period affected the control groups. An example of such an event is that in June 2010, the cap on copayments was automatically registered for patients older than 16, resulting in more patients in the control groups to reach the cap and be registered for free consultations with their GP. However, this may underestimate the effect because the automatic registration could give incentives for an increased number of GP visits for the control groups in the postreform period.

Overall, the results indicate that adolescents between the ages of 12 and 15 were sensitive to having to pay a copayment before the reform and that copayments affect adolescent use of GP services.

CONFLICT OF INTEREST

The authors have no conflict of interest.

ETHICS

Only aggregate level data were used for the analysis, and no ethics approval was required.

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