Did Adolescents in Norway Respond to the Elimination of Co-payments for General Practitioner Services?

Keywords: co-payments, adolescents, synthetic control method, general practitioner services, health care utilisation

1. INTRODUCTION

Co-payments for primary care services involve a trade-off between efficient use of health care resources and access to health care services. The absence of co-payments 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 under-consumption 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 co-payments. The effect of changes in co-payments on use of health care services has been studied extensively in the literature (see Kiil and Houlberg (2014), Galway *et al.* (2007) for systematic reviews). However, most of these studies have focused on adult responses to increases in co-payments and few have addressed the issue of co-payments on adolescent use of health care services. Several studies report that co-payments 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 *et al.*, 2002, Tylee *et al.*, 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 *et al.*, 2005).

In Norway, starting in 2010, a co-payment reform was introduced that exempted all adolescents between the ages of 12 to 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 co-payments 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 co-payments on demand for primary care services for adolescents and specifically the aim is to estimate whether being exempted from co-payments led to an increased use of general practitioner (GP) visits for the treated age groups. Exempting adolescents from co-payments might also reduce other barriers to access, such as the confidentiality concerns; due to their financial dependency on parents and thus reduces parental involvement. Coupled with the size of the price change (from 17.5 EUR 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 co-payments on adolescents' use of primary care services (Olsen and 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 et.al (2010, 2015). Advantages of the SCM, compared to the Difference-in-Difference (DID) 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, since this allows for relaxing the restrictions.

1.1. Previous empirical studies on co-payments and use of health care services

There have been three recent studies on adolescents and co-payments 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 co-payment fee of 1.2 EUR in 2009. Two studies (Votapkova and 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 co-payments on the use of these services. The authors argue that this was because the size of the co-payments 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 co-payment fee of 10 EUR. Using 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 co-payments that they evaluate. Patients in the Czech Republic have direct access to secondary care services (Alexa *et al.*, 2015), whereas Norwegian patients have limited access due to gatekeeping. Other important substitutes for primary care services that are important to consider are emergency services, or more specifically for adolescents and children - access to a school nurse. In Norway, patients may use emergency primary care services, but this also incurs a co-payment 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 to a GP. For example, a school nurse is on average available only 3.5 hours 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 co-payment (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 co-payment was approximately 17.5 EUR in 2010. Additionally, Norway has a co-payment 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 co-payment is required. (An additional co-payment 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 co-payment exemption card. The limit on co-payments means that the effect of co-payments 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 (1999). Prior to 2010, co-payments for GP consultations were required for patients aged 12 and older. As of 1st January, 2010, this age threshold was increased to age 16, thus exempting adolescents between the ages of 12 and 15 from paying a co-payment 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). The majority (approximately 20 %) of consultations in 2012 for children between the ages of 6 and 15 concerned respiratory diseases and ear infections. Other common diagnoses in this age group are mental health issues (8 %), asthma and allergies (10%) and injuries (7 %) (Statistics Norway, 2012).

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 dataset 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 post-reform period, this ensures a sufficient period of 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 was combined with population data in each age group from Statistics Norway. The population data is measured on 31st December 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.

Gender is unidentified for 0.1 % of the observations in the dataset. 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

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

missing from our dataset for different age groups or for males vs. females.

4. METHODS

This study differs from previous studies on the effect of co-payments since 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 Synthetic Control Method (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 pre-treatment period are assumed to have similar treatment-free outcomes in post-treatment periods (O'Neill *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 pre-intervention outcomes of the treated and control groups are minimised. For a formal exposition of the SCM see papers by Abadie et al (2010, 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 to 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, i.e. constructing a synthetic counterfactual where the control groups are weighted so as to minimise the distance between the pre-treatment 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 pre-treatment 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 co-payment 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:

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

This means that given $Y_{t,pre}^{obs}$ and $Y_{c,pre}^{obs}$ i.e. the observed outcomes in the pre-reform period for the treated and control group we want to estimate values for the intercept, μ and weights, , 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).

Since 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 (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 pre-intervention 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 since matching on pre-intervention values of the outcome alone controls for unobserved factors (Athey and Imbens, 2016). In addition, since 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 dataset did not include use of GP services in nursing homes, only office visits to the GP. This means that elderly 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 and 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 suggest 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 (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 pre-treatment 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 pre-treatment fit. We therefore estimate the fit by calculating the pre-treatment mean square prediction error (MSPE) and eliminate from the placebo analysis age groups with a MSPE that is ten 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 pre-reform 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 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 pre-reform 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 co-payment reform, the number GP consultations per person increased by 12 % and 14.8 % from 2009 to 2010 for males and females respectively.

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---Figure 2 here---

5.2. MAIN RESULTS

Figures 3 and 4 display the trajectory of use in GP services for the pre 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 pre-treatment 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 pre-treatment fit. The SCM gives a positive weight to four age groups (ages 9, 10, 11, and 17), while the elastic net gives a positive weight to three age groups (ages 9, 18 and 19). As seen in table I, the predicted average effect of the reform for the post reform 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 pre-treatment 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%, since 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 post-reform period (see Table I).

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 to 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 post-period reform, thus capturing the reform effect for 4 years compared to the one year increase calculated for the descriptive results.

---Figure 3 here---

---Figure 4 here---

---Table I here---

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 different 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.

---Table II here---

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.

---Figure 5 here---

--- Figure 6 here--

6. DISCUSSION

Using the preferred model, we find that exempting adolescents from paying a fee to see their GP results in a 22 % increase in consultations for females and 13.8 % increase for males on average for the post-reform 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% 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 co-payment exemption achieved its goal. However, with regards 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 co-payments 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 socioeconomic status and analyse whether the effect of the exemption differed for different subgroups.

We performed stratified analysis by gender and showed that adolescent females are more responsive to the co-payment reform. This suggests that for this age-group, prior to the reform, girls were more vulnerable than boys to co-payments. There is limited research on gender differences in adolescents' response to co-payments with which we can compare our findings, moreover, studies on this topic based on data for adults provide mixed evidence (Pendzialek *et al.*, 2016). Nevertheless, some studies report that adolescent girls have a greater need for confidential GP consultations i.e., without their parents' involvement (Edman *et al.*, 2010, Klein *et al.*, 1999), thus it could be that the co-payment exemption had a greater effect among adolescent girls than boys partly because it encouraged confidential consultations with GPs.

Compared to the previous studies on how co-payments affect adolescent use of primary care services (Paul and Nilsson, 2014, Votapkova and 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 relatively larger co-payment (approximately 70 % greater compared to adolescents in Sweden). Moreover, the Norwegian reform targeted an age group that has both a greater need for GP services without parent involvement, compared to 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.

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

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[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.

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 co-payment 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 to 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 pre-reform 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 non-treated 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 to 0.75 visits for the non-treated cohorts. This does not indicate that the results are driven by a large cohort effect.

Table II shows how the effect varies by including different age groups as control groups. Data with few pre-treatment periods, like ours, is susceptible to the effects of idiosyncratic shocks, where the method constructs the synthetic control group from units that appear to be similar and thus have similar unobserved characteristics in the pre-treatment period, but then are not similar in the post-treatment period. By increasing the number of potential control groups to 70 it seems as if the method selects groups that only appear to be similar in terms of pre-treatment trend in the outcome.

Lastly, it's important to consider other events in the post-treatment period that affect the control groups. An example of an event that might affect our results is that in June 2010, the cap on co-payments 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 since the automatic registration could give incentives for increased number of GP visits for the control groups in the post-reform period.

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Figure 1 Number of consultations per person, averaged over the pre-reform period (2006 to 2009) by age and gender

Number of consultations per person (2006-2009) by age and gender
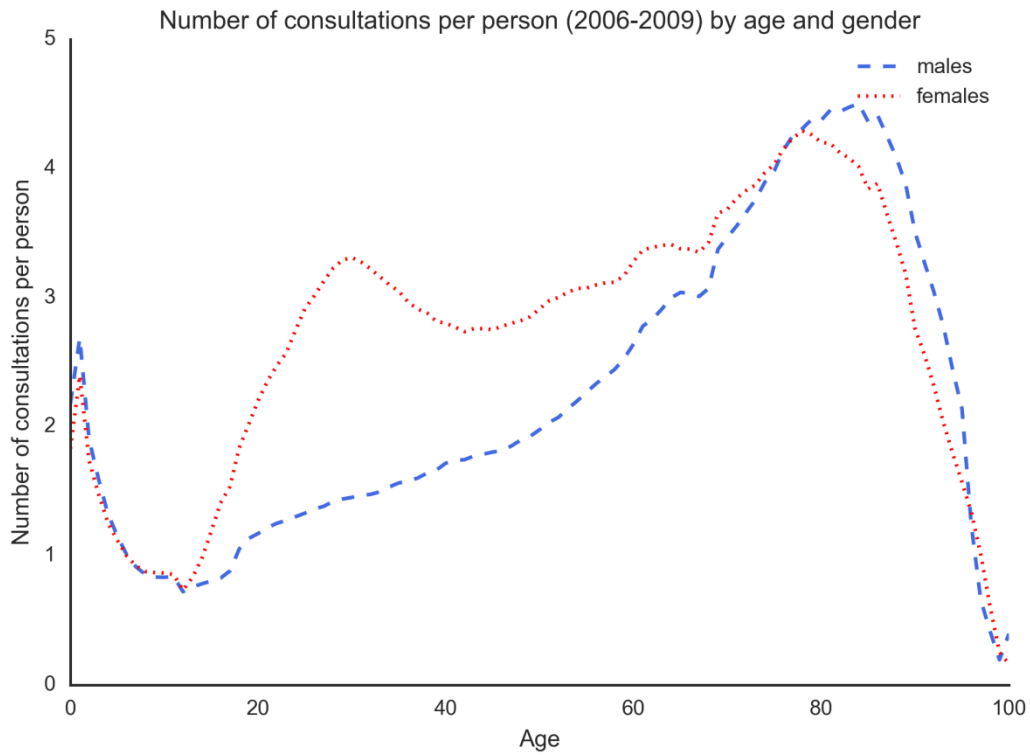
Figure 2 Trend in use of GP services for the treated age group (age 12 to 15) in the pre-reform (2006-2009) and post-reform (2010-2013) period
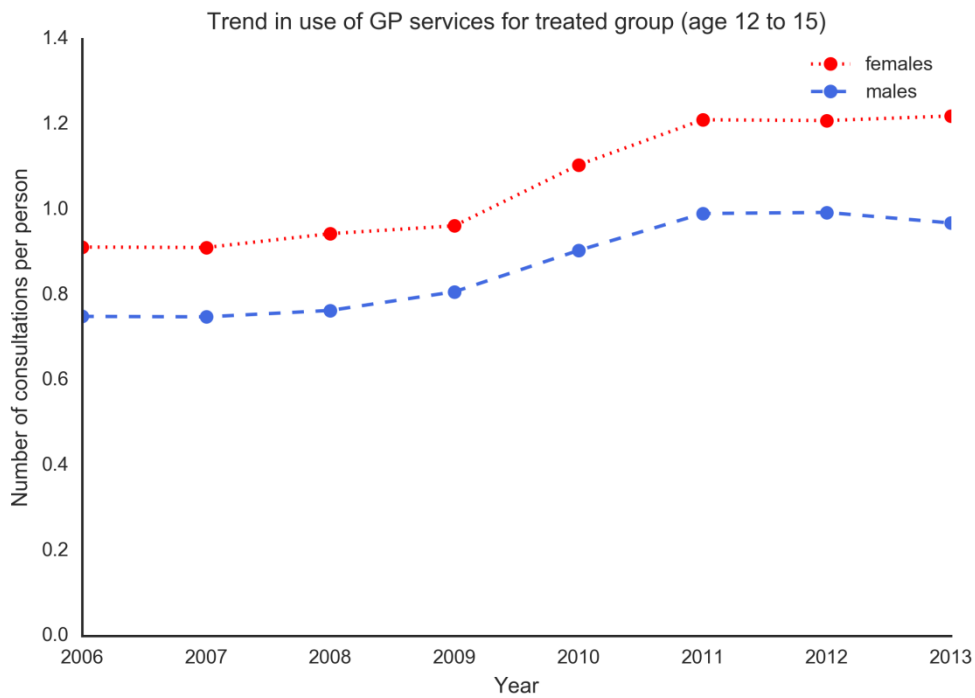

Trend in use of GP services for treated group (age 12 to 15)

Figure 3 Number of consultations per person for the female treated group and the constructed control group for each method
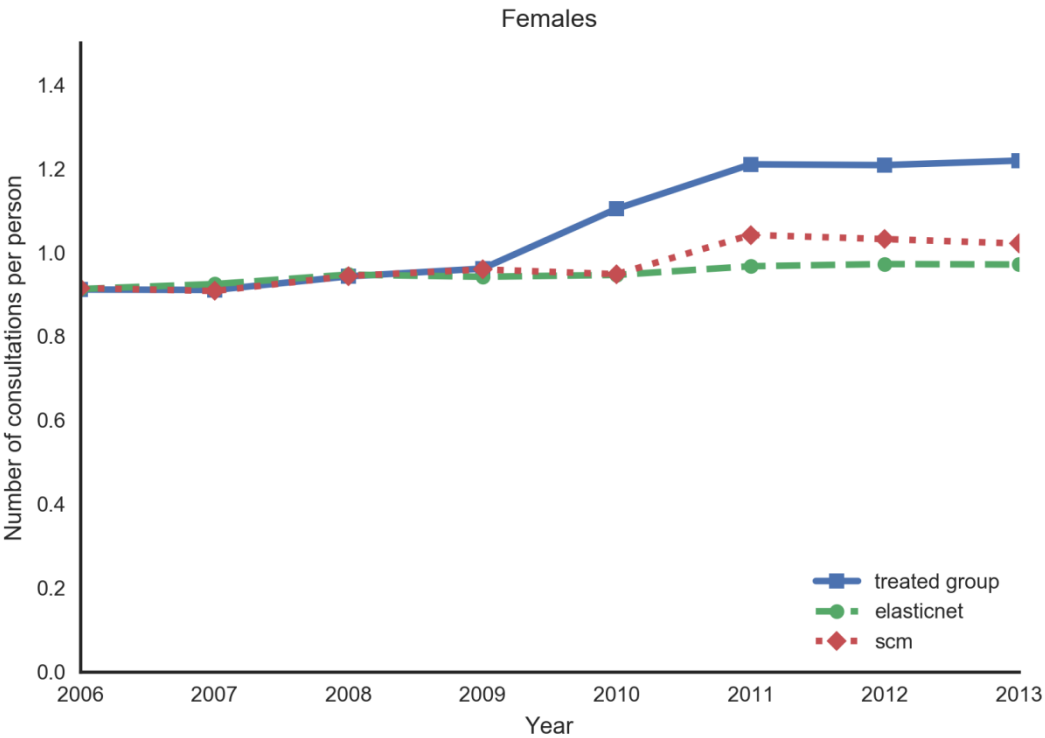
Figure 4 Number of consultations per person for the male treated group and the constructed control group for each method
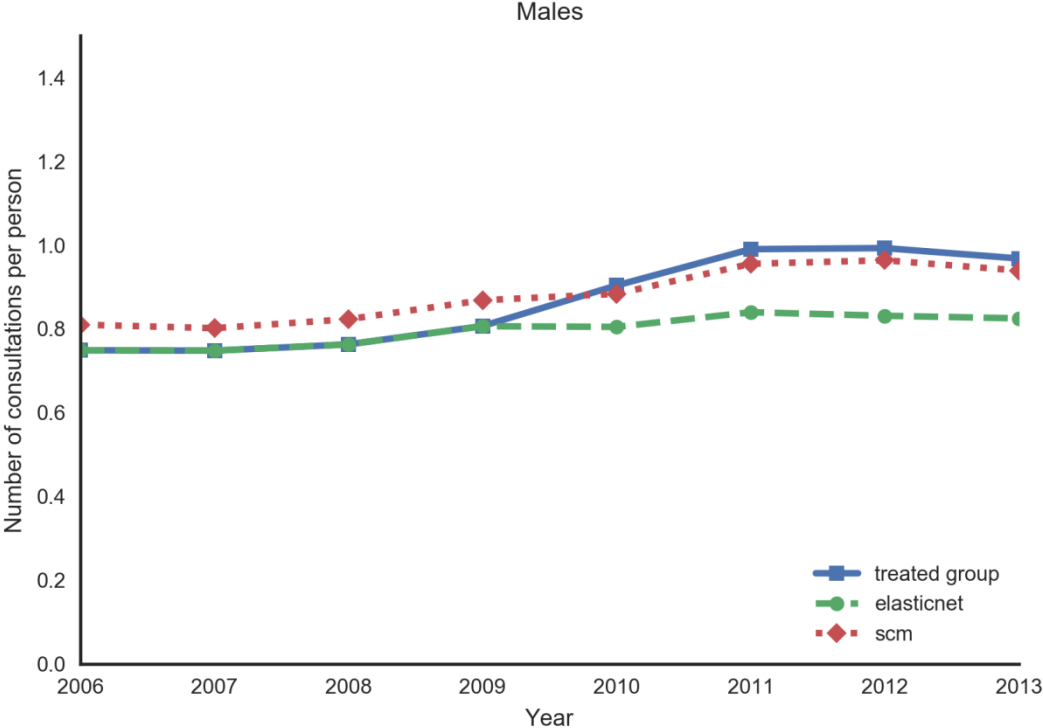

Table I Estimated size of the effect of the co-payment reform for the different methods. The effect estimate is an average over the entire post-reform period (2010-2013).

	SCM	Elastic Net
Females	17.5 %	22.1 %
Males	2.8 %	13.8 %

Table II Estimated size of the effect of the co-payment reform for the different age groups included as control units. The effect estimate is an average over the entire post-reform period (2010-2013).

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 %

Figure 5 Estimated effect size for the female treated group and placebo effect size for the other age groups
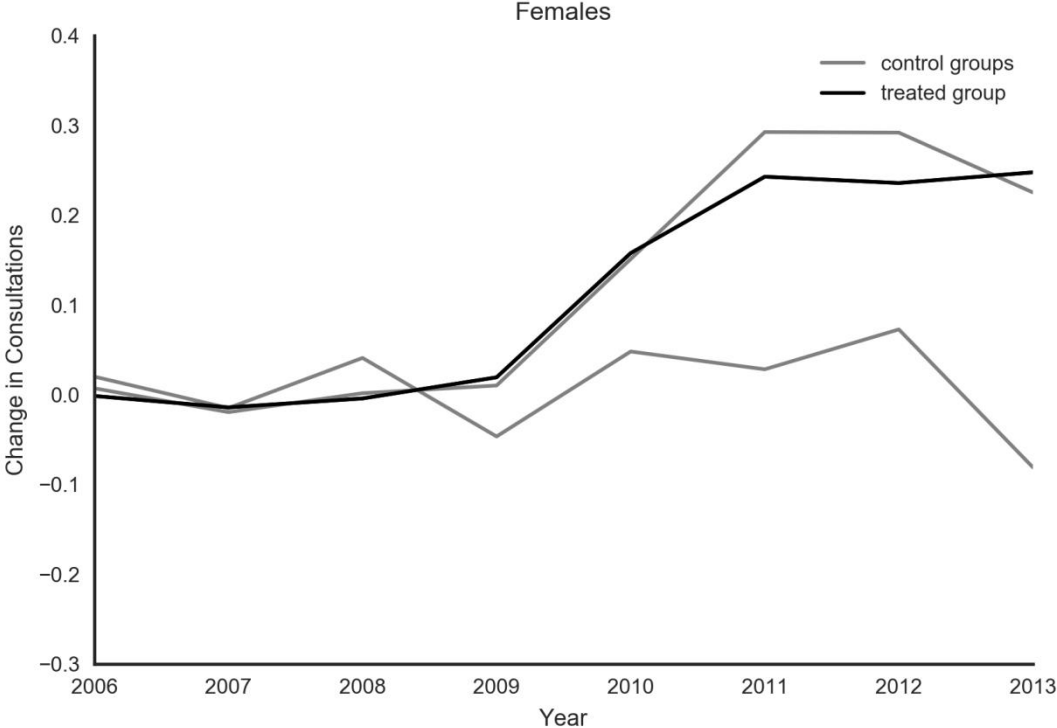
Figure 6 Estimated effect size for the male treated group and placebo effect size for the other age groups