The effect of copayments on primary care utilization: Results from a quasi-experiment

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Abstract

This paper analyzes how health care utilization is affected by copayments in a tax financed health care system. The paper utilizes a natural experiment in which a health care region in Sweden changed the price of health care in such a way that primary care general physician prices increased by 33 percent. We use daily visit data in the treatment region and a neighboring control region where no price change took place and analyze the effect using differences-in-differences (DD) as well as differences-in-differences-in-differences models (DDD). The results from the preferred models indicate no effect on health care utilization due to the price change, a result that also holds across different socio-economic sub-regions in the treatment region.

Key words: copayments**,** health care, demand, moral hazard, natural experiment

JEL codes: C33, D12, I11, I18

I. Introduction

The funding of the public sector in general and health care in particular is at the top of the agenda in countries around the globe. The increased demand for health care and access to more expensive treatments increases the strain on public health care budgets. Today most economically developed countries spend around 10 percent (or more) of their gross domestic product on health care, and it is believed this share increases over time (OECD, 2012). In the typical health care system today a patient only pays a small share of the full cost of a health care visit directly when utilizing primary or emergency health care. The share of health care expenditures paid directly by households varies somewhat across countries; the OECD average is 19.5 percent (OECD, 2012). The lion's share of health care is paid for via either taxation, as in the UK and Sweden, or social (or private) insurance systems, as in many continental European countries and the US.

There is a long ongoing debate on the role of copayments as a means of cost containment, financing, and steering patients to the preferred provider/care unit. However, knowledge regarding the effects copayments has on health care utilization and health equity issues in the population is still sparse outside the US system, with a dearth of papers from different institutional settings using experimental or quasi-experimental approaches.

Cost containment may be achieved by increasing copayments if there is moral hazard in demand, i.e. patients over-utilize health care when there is a difference between the cost the patient actually bears and the total societal cost (Zweifel & Manning, 2000). Additionally, patient charges may be preferred to general taxation to cover increasing health care costs given the substantial welfare cost of raising additional taxes. Recent empirical evidence indicates that the marginal cost of public funds is around 1.3 in e.g. Sweden, implying that a one Euro increase in tax revenue will induce a welfare cost of 1.3 Euros (Sörensen, 2010).

However there are also potential downsides with relying on copayments. If health care utilization decreases this may lead to fewer preventive health care visits, which may worsen population health and increase costs in the long run. Further, if low-income groups are more price sensitive than high-income groups, as indicated in some international studies (Skriabikova, Pavlova, & Groot, 2010), health inequalities in the population may increase if copayments increase. Higher copayments may also be seen as an "extra tax" on individuals with greater health care needs, which may be considered unfair. Additionally, if patients are already highly "conscientious" in their health care-seeking behavior, copayment increases will not lead to a reduction in "unnecessary" health care utilization and there are no efficiency improvements to be made. On the contrary – if individuals are risk averse and moral hazard is not an important aspect of health care demand, higher patient prices can decrease efficiency and welfare.

In summary, there are potentially both positive and negative consequences with higher (or lower) copayments for health care. In order to evaluate the consequences of different health care policies with respect to the costs and pricing structure, there is a need for a greater understanding of how individuals/patients respond to differences in prices in different health care and institutional contexts.

In this paper we analyze the impact of primary general physician (GP) health care utilization caused by a price reform and how it may vary across different socio-economic/demographic areas of the population. We use data from a natural experiment setting, allowing us to move beyond examining correlations and instead address the more important issue of causality. More specifically, we use this research design to address the following two research questions: (1) Do copayment changes affect consumption of primary health care? (2) Do the (potential) effects of copayment changes on primary health care consumption differ across different socio-economic and socio-demographic areas?

Our contribution to the literature is that we focus on a health care system that relies heavily on gate-keeping (using a phone triage system) and, to some extent, waiting times that already exist at primary care level.[1] Thus, price sensitivity could be expected to be especially low in such a context and our findings could potentially be seen as a lower bound from an international perspective and more relevant for many health care systems than previous estimates from e.g. the US health care market. Furthermore, our data makes it possible to extend the differences-in-differences analysis from previous European studies, allowing us to inspect and control the common trend assumption. The results from the preferred models indicate no effect on health care utilization due to the price change, a result that also holds across different socio-economic sub-regions in the treatment region.

The rest of the paper is structured as follows. In section two we give a brief overview of the previous literature, focusing on the evidence from experimental and quasi-experimental studies. Section three provides a very brief description of the Swedish health care system. Section four describes the natural experiment, data and econometric approach used in this paper. Sections five show the results and section six robustness checks. Section seven ends the paper with a concluding discussion.

[1] The share of patients getting to see their primary doctor within 7 days in Sweden is low in a European context. Only Portugal has a lower share of accessibility based on this measure (Björnberg, 2013).

II. Previous evidence

There is a previous theoretical as well as empirical literature on how copayments affect health care utilization. The theoretical research is primarily focused on issues of moral hazard, see the overview in Zweifel and Manning (2000), and distinguishes between three types of moral hazard; the insured's tendency (1) to reduce preventive effort, (2) to demand more medical services, and (3) to opt for the newest medical technology, compared to the situation where the individual paid the full cost medical cost. In basic models of consumer choice there is a clear negative price effect on the utilization of health care (Zweifel and Manning (2000).

This paper is empirical in nature and we therefore focus here on the relevant empirically based research. There are several reviews of the empirical research on the effect of insurance in health care prices on the use of health care services (Broyles & Rosko, 1988; Kiil & Houlberg, 2014; Newhouse, Phelps, & Schwartz, 1974; Rice & Morrison, 1994; van de Ven, 1983). In the more recent reviews of the published international (empirical) literature addressing the issue of how demand is affected by patient payment systems in general practice health care, between 40 and 50 studies were identified (Kiil & Houlberg, 2014; Skriabikova et al., 2010). The studies span a relatively long time period and the majority of them are based on data from the US. However, despite the number of studies listed in this review, there is little empirical evidence on how copayments affect health care utilization due to the pure price effect. The reason for this is that most studies are based on cross-sectional data and do not address the issues of selection bias and endogeneity. To exemplify the methodological problem in many studies: in an insurance system individuals' with worse health status (partly unknown to the insurer) will be more likely to select an insurance plan with higher coverage and lower copayments. This will create a relationship between copayments and health care utilization that is significantly affected by the selection bias. Hence, experimental or quasi-experimental approaches are necessary in order to address the research question outlined in this project, and such studies are relatively rare. In the following, we discuss the most relevant existing studies using credible techniques (experimental or quasi-experimental) to identify the effects of prices on health care utilization.

The most influential study in the field is still the RAND health care experiment conducted in the US between 1974 and 1982. The RAND study was based on a randomized experimental design covering about 6,000 patients (Manning et al., 1987). In the experiment, families were randomly assigned to different insurance plans where the rate of coinsurance differed (the share of expenditures paid by the individual/family). The main result in the RAND experiment was that the price of health care significantly affects utilization. It was further shown that it was primarily the number of individuals seeking health care that was affected, rather than the number of visits among frequent health care patients. Results from the RAND study have also been used together with modeling assumptions to estimate the price elasticity of demand. The price elasticity of demand was in the range of -0.1 to -0.2. These estimates have been influential in subsequent health policy discussions and reforms; see further e.g. Aron-Dine, Einav, and Finkelstein (2013). A more recent experimental study investigates the effects of health insurance for low-income individuals in Oregon, USA (Finkelstein et al., 2012) and finds clear effects on utilization. A recent quasi-experimental study by Chandra, Gruber, and McKnight (2014) estimates the effects of copayments on health care utilization for low-income earners in Massachusetts, USA, using exogenous increases in copayments introduced in 2007. They find a price elasticity of -0.16 for this group, and somewhat smaller estimates for the chronically ill.

In the European context there are a few studies based on quasi-experimental approaches. Chiappori, Durand, and Geoffard (1998) used the fact that in a group of employed individuals (in a specific region of France), the patient price increased for a subset of the employed (via the introduction of a 10 percent co-payment for physician visits). A possible problem with their analysis is that choices made by the firms where the employees work determine the copayment level faced by employees. Thus, the copayment level may not be randomized, something that is also suggested by the fact of observable age differences between the treatment and control group. They found a small effect such that the number of general practitioner home visits was reduced in the group with a 10 percent copayment compared to the group with no copayment, but they found no difference in general practitioner surgery consultations. A Belgian study using a difference-in-difference design analyzed demand effects from a change in patient prices that took place in Belgium during the 1990s (Cockx & Brasseur, 2003). They found a relatively low level of price sensitivity, with a price elasticity of -0.13 for men and -0.03 for women. Ziebarth (2010) estimated price elasticities for convalescent care programs (medical rehabilitation therapy and preventive therapy) in Germany based on a price reform in 1997 that doubled the daily copayment rates for people insured under the German health insurance system (but not the private system). Using a difference-in-difference design, results showed that the doubling of the copayment rate reduced the overall incidence of convalescent care programs by 20 to 25 percent, which implied a price elasticity of around -0.3

In summary, the international literature highlights the fact that changes in patient prices affect health care utilization differently across different health care contexts. But there is a lack of research from a European context and especially a lack of research from health care systems that rely heavily on some form of gate-keeping at the primary care level (e.g. as in Sweden with a telephone triage system) and (partly) on waiting time as a rationing mechanism, where it is likely that the effect of copayments may be quite different. Our paper thus attempts to fill this gap and by using a natural experiment in this context as described further below.

III. The Swedish health care system

The central government, county councils, and municipalities share the responsibility for healthcare in Sweden. The role of the central government is to establish principles and guidelines, while the responsibility for providing health care is devolved to the 21 county councils and, in some cases, municipal governments (mainly responsible for nursing-home and long-term care). County residents elect the representatives to the county councils every four years alongside national elections. Most of the work of Swedish county councils concerns health care, but they also deal with other areas such as regional communication/infrastructure, tourism, and cultural issues.

About 12 percent of Swedish healthcare is carried out by private care providers but financed through the county councils. Patients are covered by the same regulations and fees that apply to public care facilities.[2] Costs for health and medical care represent 9.6 percent of Sweden's gross domestic product, which is on par with most other European countries, with the OECD average at 9.5 percent (OECD, 2012). Proportional county council and municipal taxes pay for the bulk of health care costs in Sweden, while patient out-of-pocket costs and state grants are a smaller financing source. Counting all health care expenditures direct household expenditures for health care stands at 16.8 percent, which is below the OECD average of 19.5 percent (OECD, 2012).

Within each county council general practitioners in primary care are generally employed at primary health care centers ("vårdcentraler") that provide primary care to residents within a geographical area. Patients are, however, free to sign up for any primary care center within their own county council, but note that the level of copayment does not differ between health centers within each council. If a patient wishes to consult their general practitioner they usually have to phone to the primary care center where they are listed, describe their health condition, and ask for an appointment. The number of physicians per capita is relatively high in Sweden with almost 4 physicians per 1,000 inhabitants, to be compared with the OECD average of 3.1 (OECD, 2014). Despite this the num-

[2] However, there is also a very small entirely private market of physicians and hospitals, with free price setting generally covered by private insurance contracts. Only a very small part of the population is covered by these contracts (which are over and above the public system).

ber of doctor consultations per capita and year is substantially lower in Sweden (3 per person and year) compared to the OECD average (6.4 per person and year).

The patient fee to visit a physician is decided at the county council level and varies between 150 to 350 SEK across counties (€14 to €40, SEK 8.54 = EUR 1). The level of copayments has remained fairly stable in almost all councils for the last ten years with only some minor changes. All councils also apply an out-of-pocket cost ceiling. When a patient reaches the ceiling during a moving 12-month period, the patient price falls immediately to zero for the remainder of the 12-month period for ordinary medical consultations. The out-of-pocket cost ceiling also varies across councils but tends to be slightly above 1,000 SEK (€117) per moving 12-month period.

In international comparisons, the Swedish health care system usually fares well on most outcomes, with a relatively low level of infant mortality, high life expectancy (81.7 years), relatively good cancer survival rates, and relatively few problems with Methicillin-resistant Staphylococcus aureus (MRSA) infections, etc. However, accessibility and waiting times are a historically problematic issue in the Swedish health care system. Waiting time data shows that 77 percent of patients wishing to see their primary care doctors get an appointment the same or following day. Almost 10 percent have to wait a week or more (Björnberg, 2013).

IV. Data and econometric approach

Data

On May 1, 2012, a Swedish county council (Värmland) implemented a price harmonization of health services. The patient cost for office visits to GPs increased from 150 SEK to 200 SEK (€17.5 to €23), while the patient cost for emergency care visits was lowered from 300 SEK to 200 SEK (approximately €35 to €23, SEK 8.54 = EUR 1). These price changes were implemented in order to harmonize patient costs to 200 SEK (€23) for different types of care.[3] The price levels are relatively similar to most other OECD countries in terms of copayments for a GP visit. In the political decision it is stated (freely translated): *"The suggested changes implies a simplified system for copayments and the out-of-pocket cost ceiling and a simplified administration for both patients and staff"* (LIV, 2012, p.18). Since price setting in health care is a purposeful action by politicians, possibly affected by economic and political conditions, it is important that the price change that we are investigating is not a response to anticipated changes in demand for health care. Both readings of the political decisions as well as several interviews with the respon-

[3] The out-of-pocket cost ceiling ("högkostnadsskydd") was constant during this period.

sible politicians and decision makers indicate that demand and supply issues was not a motivation for the price harmonization.[4] Because the price changes we are examining were not implemented as a response to changes in the demand or supply of health services, it is possible to treat the price change as a natural experiment (Besley & Case, 2000).

In the present paper we focus on visits to general practitioners. Thus, the question we are answering is: Did visits to GPs decrease as the price increased and as the price of the potential substitute, emergency care, decreased? In order to evaluate the price change we need a credible control region where no price changes occurred. The control region (Örebro) was chosen because it is the geographically closest region and is also in our examined population characteristics the most similar region to our treatment region. The level of copayment in the control region equaled 120 SEK (€14) during our time period, i.e. the same as the pre-treatment price in the treatment region. The population in the treated region was 273,132 in 16 municipalities (year 2013), the average age was 43.6 years and the regional gross domestic product was 298,000 SEK (year 2011). The population in the control was 283,526 in 12 municipalities, the average age was 41.8 years in Örebro, and the regional gross domestic product was 330,000 SEK (SCB, 2013). Thus, the regions are relatively similar in geographical location, population, average age, and regional GDP.

We have collected data on the number of daily visits to general practitioners at primary care centers in treated as well as in the neighboring control (where no price changes occurred), before and after the price change. We have data from January 1, 2011 to December 31, 2012. We have data on the total number of visits per day aggregated at county council level as well as grouped over the 31 primary care units ("vårdcentraler") in the treatment region. For each primary care unit we also have a summary score measure for their socio-economic status and care need as used by the county councils, which allows us to analyze whether the price reform has had different impact in primary care unit regions with different socio-economic and demographic status. Table 1 below shows the mean and median number of daily visits in the treated and untreated council area. The mean and median number of visits is fairly close, and for the treated council the mean is lower than the median due to a slightly more left-skewed distribution.

[4] As a matter of fact several Swedish county councils are contemplating the same price harmonization scheme in order to being able to market one single price for all health care utilization to patients.

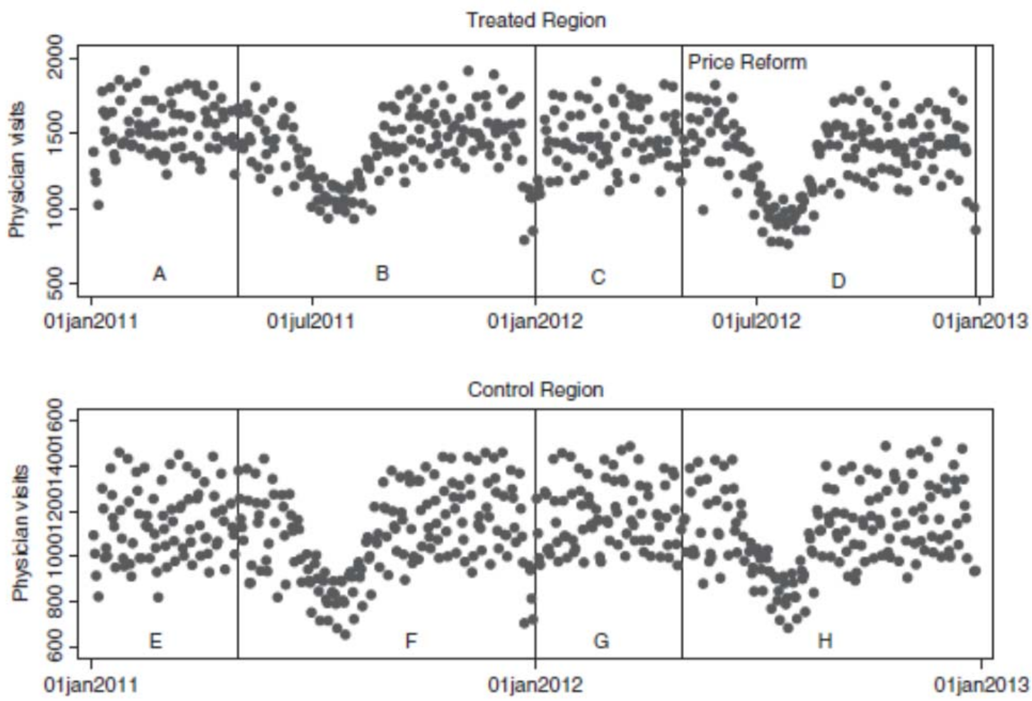
Table 1 Mean and median daily visits

County Council (Region)	Mean daily visits	Median daily visits
Treated (Värmland)	1369.36	1405
Untreated (Örebro)	1096.41	1088.5

Note: Excluding weekends and holidays.

In Figure 1 we present the daily physician visits in the treated and untreated region, as well as dividing the time period into four different time windows that will be used to present our econometric approach. Daily visits are consistently somewhat higher in the treated than in the untreated council, as seen in Table 1. Additionally, we see that visits tend to reduce during the summer and at Christmas in both regions. The number of physician visits is lower in the treated and control regions, as in Sweden in general, compared to most other OECD countries.

Figure 1 Physician visits in treatment and control region 2011–2012



Note: Number of primary care visits are on the vertical axis, and days are on the horizontal axis. Each dot corresponds to the number of visits in one day. The vertical line called "Price Reform" indicates when prices were changed in the treated region. The time windows A-F are used to describe which periods are compared in the econometric analysis.

Since general practitioners in the control council area were unaffected by the price change in the treated council area, comparisons of the changes in the daily number of visits to facilities in the treated council area relative to visits to facilities in the control council yield estimates of the effect of the price changes on health care consumption. This is a typical "differences-in-differences" (DD) setup (see Angrist and Pischke (2009) or Blundell and Dias (2009) for a comprehensive overview) commonly applied to estimate causal effects of public and health care policies (Jones, 2011). The intuition is that the average change in visits to facilities in the control region is subtracted from the average change of visits to facilities in the treated region. This removes the bias in just focusing on change in the treated region that could result from time trends, and it removes the bias in second-period comparisons between the treatment and control group that could be the result of permanent differences. Table 2 below shows a simple comparison in mean visits in the treated and control region before and after the price reform comparing the same time periods in both years (i.e. pre-treatment is May-December 2011 and post-treatment is May-December 2012). The data reveals that the number of visits reduced in both regions after the price reform but also that the reduction was larger in the treated region.

Table 2 Comparison in visits between treated and control region: pre- and post-reform

	Treated region	**Control region**
Mean visits pre-reform	1398.87	1087.46
Mean visits post-reform	1339.15	1105.57
Difference	-59.72	-18.11
	(-4.27%)	(1.67%)

Note: Excluding weekends and holidays.

Econometric approach

In order to analyze the potential effects of the price reform, we outline our two main approaches below. We observe the number of daily visits at the treated and control region from approximately 15 months before the price change to eight months after the price change.[5] Y_{rt} is the number of visits in region r in day t. As a first step, the regression to be estimated is:

$$Y_{rt} = \alpha + \beta_0 Treated + \beta_1 Post + \beta_2 Treated \times Post + \varepsilon_{rt}, \quad (1)$$

where $Treated$ is a dummy for the treated region, $Post$ is a dummy for the post-treatment period, and ε_{rt} is an error term. The $Treated$ indicator captures differences between the regions before the price change, while $Post$ captures factors that would cause changes in

[5] We have also tried different transformations of the outcome variable, e.g. visits per capita and the natural logarithm of visits and visits/capita, without changing any of the conclusions from the model and results shown in the paper.

visits even in the absence of a price change. Given the identifying assumptions, the estimate of β_2 is here the impact of the price change on the total number of visits. Since we have data for many time periods (days) both before and after the price change, we will follow the framework presented by Bertrand, Duflo, and Mullainathan (2004) to cope with serial correlation in DD analyses, and thus collapse the data in a pre- and post-period to produce consistent standard errors. Given the seasonal variation in data shown in Figure 1, in our basic specification we want the pre- and post-treatment period to contain the same months, i.e. May-December 2011 and May-December 2012. In a graphical interpretation from Figure 1 the basic DD setup can thus be seen as (D-B) - (H-F), and we refer to this as Model 1. We will also include two other specifications: in Model 2 we only include data from 2012 and the DD setup can thus be seen as (D-C) - (H-G), and finally in Model 3 we include all time periods and the DD setup can thus be seen as (D- A to C) – (H – E to G). The identifying assumption (for drawing causal conclusions) in our DD models is that both regions would follow the same development in visits albeit from different starting points (levels) in the absence of the price change (common trend assumption). This is a rather strong assumption; although we have two very similar and neighboring regions, there may of course be differences in the seasonal patterns between the two, in which case a DD setup may not isolate the causal effect of the price change.

Since we also have access to data from 2011 where no price changes or other policy reforms that could affect visits occurred, we can complement our analyses with a "Differences-in-Differences-in-Differences" (DDD) setup to relax the common trend assumption using another approach. See e.g. Gruber (1994) and Bell, Blundell, and Reenen (1999) for applications of this method. To obtain the DDD estimate we start with the change over time in daily visits in the treatment region in 2012, and then "net out" the change in visits in the control region during the same period (i.e. the ordinary DD analysis). We then exploit the changes in visits in 2011 to account for seasonal variation in visits and thus separate the true impact from the price change from the differential trend. The idea is that this controls for two kinds of potentially confounding trends: changes in visits across regions (that would have nothing to do with the policy) and seasonal changes in visits in the policy-change region. The regression to be estimated is:

$$Y_{rt} = \alpha + \beta_0 Treated + \beta_1 Post\ May1 + \beta_2 2012 + \beta_3 2012 \times Treated + \beta_4 2012 \times Post\ May\ 1 + \beta_5 Treated \times Post\ May\ 1 + \beta_6 2012 \times Treated \times Post\ May\ 1 + \varepsilon_{rt}, \quad (2).$$

In equation (2) *PostMay1* is a dummy for days between May 1 and December 31 in both years (2011 and 2012), thus β_6 is the coefficient of interest, or the DDD estimate, that captures the effect on visits in the treatment region after May 1 in 2012 (holding the same effect in 2011 constant). In a graphical interpretation from Figure 1 the DDD setup can be seen as (D-C)-(B-A) – (H-G)-(F-E).

These analyses will be used to address our two research questions. It can be directly utilized to answer the first research question, i.e. how the price changes affects utilization of primary care. To address our second research question, whether the effects differ across socio-economic areas, we will use the fact that we have access to data on daily visits at the level of each separate primary health care unit in the treatment region (together with data on their Care Need Index). We will run primary care-specific analyses in order to examine whether patient payment changes have heterogeneous effects across different primary care units.

V. Results

Table 3 below shows the results from the DD model as outlined in equation (1) using our three different models including different time periods, where the interaction between the treated region and post-May 1, 2012, is the impact of the price reform on the total number of visits ($Treated \times Post$). We see that in Model 1 and 3 the effect is statistically significant at the 1 percent level with coefficients of -77.83 and -91.60, i.e. the number of visits in the treated region decreases by approximately 78 and 92 visits after the introduction of the price reform based on Model 1 and 3 respectively. The result from Model 2, only including data from 2012, is such that we cannot reject the null hypothesis of no effect of the price reform. The coefficients on $Treated$ and $Post$ captures that the number of visits is significantly higher in the treatment region in general ($Treated$) and that the number of visits is lower after May 1 in both the treatment and control region ($Post$).

Table 3 Impact of price reform on physician visits: DD results

Time periods from Figure 1→	**(D-B) - (H-F)**	**(D-C) - (H-G)**	**(D-A/C) – (H-E/G)**
Variables	**Coeff. (Std.err)**	**Coeff. (Std.err)**	**Coeff. (Std.err)**
Treated	311.41***	280.21***	325.18***
	(23.67)	(32.18)	(16.05)
Post	18.11	-76.30***	-17.44
	(23.81)	(27.84)	(19.70)
Treated ×Post	-77.83**	-46.63	-91.60***
	(33.68)	(39.37)	(27.86)
Constant	1087.46***	1181.87***	1123.01***
	(16.74)	(22.75)	(11.35)
R^2	0.28	0.29	0.35
N	676	500	1006

Note: Two-way significance tests: *** p<0.01, ** p<0.05, * p<0.1. Regression based on weekdays, i.e. excluding weekends and holidays. Model 1 uses the period May-December 2012 and 2011, Model 2 uses only data from year 2012, and Model 3 uses all data from January 1, 2011 to December 31, 2012.

As discussed in the previous section the identifying assumptions in the DD model requires a common trend assumption. As test of the identifying assumptions we conduct “placebo regressions” and introduce a placebo price reform on May 1, 2011. The results from the “placebo regression” are shown in Table 4. The placebo regressions can only be performed using Model 2 and 3 from Table 3, since a placebo regression of Model 1 would require data from year 2010. In model 2 we use only data from 2011 (rather than only from 2012). Interestingly, we find a statistically significant effect of the “placebo price reform” taking place on May 1, 2011, in the treatment region in both Model 2 and 3. In model 2 the placebo price reform implies that the visits in the treated region decreases by 88 visits per day after the placebo price reform. In model 3 the result implies that the number of visits in the treated region decreases by 66.5 visits per day after the placebo price reform.

Table 4 Impact of price reform on physician visits: DD results from a placebo regression

	Model 1	**Model 2**	**Model 3**
Time periods from Figure 1→	**(D-B) - (H-F)**	**(B-A) - (F-E)**	**(B/D-A) – (F/H-E)**
Variables	**Coeff. (Std.err)**	**Coeff. (Std.err)**	**Coeff. (Std.err)**
Treated	272.96***	399.43***	339.46***
	(16.90)	(31.54)	(22.65)
PostMay1	-	-50.10*	-63.44***
	(-)	(27.13)	(19.70)
Treated ×PostMay1	-	-88.02**	-66.50**
	(-)	(38.63)	(27.64)
Constant	1096.41***	1137.56***	1159.85***
	(11.94)	(22.30)	(16.02)
R^2	0.28	0.43	0.36
N	676	506	1006

Note: Two-way significance tests: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regression based on weekdays, i.e. excluding weekends and holidays. Model 1 is not possible to estimate in a placebo-framework since PostMay1 does not vary over the data. Model 2 uses only data from year 2011, pretending that the price reform took place on May 1, 2011. Model 3 uses all data from January 1, 2011 to December 31, 2012 also pretending that the price reform took place on May 1, 2011.

The fact that we have data that allows us to carefully analyze the common trend assumption is thus seen to be extremely important, since we can strongly reject the common trend assumption given the results in our placebo regression. As an additional test, we conduct a DD regression in the treated council using data from 2011 as the control group, in terms of Figure 1 we thus estimate (D-C) – (B-A). The results (not shown here) indicate no significant effect of the price reform taking place on May 1, 2012, and the coefficient estimate is approx. -11. Hence, we conclude that the standard DD model is not suitable for identifying the effect of the price reform on the number of physician visits. We therefore move on to equation (2) and estimate the DDD model. Table 6 shows the results from the DDD model; the identification of the price reform is given by the interaction between the treated region, dates after May 1, and year 2012 (*Treated × PostMay1 × Year 2012*).

As can be seen, the impact of the price reform on physician visits is far from being statistically significant and, as in the DD model with time trends, we cannot reject the null hypothesis of no effect on the number of visits due to the price reform. This is in contrast to earlier evidence in the US and Germany (Manning et al., 1987; Ziebarth, 2010) but similar to a quasi-experimental study in a French setting (Chiappori et al., 1998).

Table 5 Impact of price reform on physician visits: DDD results

Physician visits	**Coef.**	**Std. Err.**	**p-value**
Treated	399.43	31.96	0.00
PostMay1	-50.10	27.48	0.07
Year 2012	44.31	31.86	0.17
Year 2012*Treated	-119.21	45.06	0.01
Year 2012*Post May1	-26.20	38.86	0.50
Treated * Post May1	-88.02	38.87	0.02
Treated * Post May1 * Year 2012	41.38	54.96	0.45
R^2	0.37		
N	1006		

Note: Regression based on weekdays, i.e. excluding weekends and holidays.

Still, even though we do not reject the null hypothesis of no effect of the price reform on average, it may be the case that the price reform has heterogeneous effects across different socio-economic/demographic areas in the treatment region. In order to analyze this we split the data in quartiles based on their Care Need Index (CNI), a socio-economic care need index, placing primary care centers with lower CNI in higher quartiles.[6] We run our preferred DDD model on data from each of the quartiles and Table 6 shows regression coefficients for $Treated \times PostMay1 \times Year\ 2012$, which identifies the effect of the price reform. Other variables included but results suppressed here.

[6] The CNI is calculated based on the patients listed at each primary care center and is a summed mean based on the following patient variables: age over 65 and living alone, born outside EU, unemployed, single parent, low education, and age below 5. The variables are scored on a scale where a higher score means higher care need and lower socio-economic status.

Table 6 DDD results for primary care centers with heterogeneous Care Need Index

Physician visits	Coef.	Std. Err.	p-value
DDD regression 1st quartile			
Treated * Post * Year 2012	16.09	35.83	0.65
DDD regression 2nd quartile			
Treated * Post * Year 2012	29.29	35.41	0.41
DDD regression 3rd quartile			
Treated * Post * Year 2012	44.55	37.50	0.24
DDD regression 4th quartile			
Treated * Post * Year 2012	41.67	36.81	0.26

Note: Regression based on weekdays, i.e. excluding weekends and holidays. Number of observations is 1006 in all four regressions.

As seen in Table 6 the results are similar across primary care centers with different CNIs, i.e. we cannot reject the null hypothesis of no effect of the price reform on the number of visits in any of the sub-group analyses.

VI. Robustness checks

As a final step we implement some robustness checks to assess our findings. In Table 7 we examine the robustness of our main results to two alternative specifications. First, since patients may be seeking care in anticipation of the price increase we might overestimate the effect, they may know about the price change and therefore go to the doctor before the date of the price change. We thus exclude visits from the month before and the month after the price change (April and May) in order to see if the results are sensitive to excluding the period around the price change. As can be seen in the first specification in Table 7 the effect is still not statistically significant. In the second specification we exclude the summer months (June–August) since visits clearly decrease during these months, the decrease may be due both to less patient demand but also due to less personnel during this period We do not find a price effect in this specification either, and the point estimate is actually positive in both specifications, which is not what should be expected if there was an actual effect of the increase in price. These exercises show that our findings are not due to the inclusion or exclusions of certain time periods.

As a final step, we also implement a regression discontinuity (RD) design to assess if there is a clear break in visits in the treatment region at the date of the price change (Imbens & Lemieux, 2008; Lee & Lemieux, 2010). The rationale is that if the price is important, this would show by an immediate change in visits after the change in the price. In the final specification in Table 7 we show the results after a parametric RD regression using a linear specification, but the results also hold for other implementations of the RD design (using small time-windows around the implementation date and/or including nonlinear specifications using higher order polynomials of days around the implementation date). Also in this specification we do not find a statistically significant effect, and we thus still end up with the conclusion that we cannot reject the null hypothesis of no price effect.

Table 7 Impact of price reform on physician visits: DDD results

Physician visits	**Coef.**	**Std. Err.**	**p-value**	**N**
Month before and after excluded				
Treated * Post * Year 2012	31.92	62.47	0.61	844
Summer months excluded				
Treated * Post * Year 2012	65.81	52.42	0.21	752
Regression discontinuity	-24.18	91.60	0.79	250

Note: Regression based on weekdays, i.e. excluding weekends and holidays.

VII. Concluding discussion

In this paper we have evaluated the effect on the number of GP visits of a price reform in which the price of GP visits was increased by 33 percent and the price of a potential substitute good, ER visits, was decreased by 33 per cent. The price reform hypothesis was thus that we would expect, if individuals are price sensitive in their health care-seeking behavior, that the number of GP visits would decrease. We used DD and DDD models in our econometric approach. The analyses showed problems with the identifying assumptions in our DD model (common trend assumption). Hence, our preferred models were the DD model including time trends and the DDD model. The results from the latter two models clearly showed that we could not reject the null hypothesis of no effect of the price reform. This result was stable across sub-groups of primary care centers with difference levels of Care Need Index as well. The conclusion is thus that the price reform does not seem to influence the utilization patterns in the population and there is no evidence of moral hazard in the demand for GP visits. This is in contrast to earlier evidence in the US and Germany (Manning et al., 1987; Ziebarth, 2010) but similar to a quasi-experimental study in a French setting (Chiappori et al., 1998). The latter paper was also based on an institutional setting in which the price change was relatively minor and the absolute price level for a visit also relatively small. A potential reason for the absence of any price effect may thus be the, in absolute terms, relatively low levels of price (and price change) as a share of the typical individual's budget.

Another possible reason for the non-rejection of the null hypothesis of no price effect may be the existence of the "out-of-pocket ceiling." Patients pay the patient price for each visit up to a total cost of 1,100 SEK in the treated region (€130) over a moving 12-month period, after which the patient price drops to zero. Hence, for patients making enough visits over a 12-month period to reach the "ceiling", the marginal cost of an additional GP visit is not affected by the price reform (and only consists of the waiting time costs, transportation costs, etc.).

But in the Swedish health care context the most likely reason for our "null-results" is probably the supply restriction/gate-keeping. For the price of a GP visit to be able to affect utilization patterns there cannot be a supply restriction putting demand effects out of the game. However, this is generally what we believe may be the case in a Swedish setting. In order to get an appointment to your primary GP you need to book an appointment by phone, and nurses and/or your GP apply a "phone triage" system.[7] This is likely to reduce the potential moral hazard behavior of patients (but may obviously also reduce patient satisfaction).

Finally, we should mention a limitation with our study in the sense that standard errors were unfortunately not very small, indicating that we did not estimate our null effects with the desired precision. As shown in the Results section, we would require a reduction in visits by 5 percent in order to detect a statistically significant effect. However, in comparison with the price increase this is still a rather small effect. Hence, we can confidently conclude that the price reform had, if not null effects, at least very small effects.

From an economic efficiency perspective the results in this paper show that the price increase in GP visits did not reduce any potential moral hazard and there were thus no efficiency gains due to a moral hazard reduction. The price reform instead created an increase in the direct income flow for the county council paid for by the relatively less healthy population (GP visitors). In sum, from an efficiency and financing perspective it may thus be argued that the price increase in relatively low levels is an efficient instrument to raise revenue compared to increasing general taxation (that creates substantial

[7] For example, a patient phoning his/her primary GP documenting symptoms of an ordinary cold may be asked to "wait and see" and will not be offered a visit. Or in cases where the physician believes that the benefit of a visit to be very low, the patient may be assigned an appointment several days later (which may imply that the patient by the "conservative treatment" gets better in time and cancels his/her appointment).

welfare costs) and that using copayments for GP visits may be preferable to services with more preventive and elastic demand (e.g. vaccination and screening campaigns).

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