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Abstract

When health insurance reforms involve non-linear price schedules tied to payment periods (for example, a quarter or a year), the empirical analysis of its effects has to take the within-period time structure of incentives into account. The analysis is further complicated when demand data are obtained from a survey in which the reporting period does not coincide with the payment period. We illustrate these issues using as an example a health care reform in Germany which imposed a per-quarter fee of €10 for doctor visits and additionally set an out-of-pocket maximum. This co-payment structure results in an effective "spot" price for a doctor visit which decreases over time within each payment period. Using this variation, we find a substantial effect of the new fee, in contrast to earlier studies of this reform. Overall, the probability of visiting a physician decreased by around 2.5 percentage points in response to the new fee for doctor visits. We verify the key assumptions of our approach using a separate data set of insurance claims in which the reporting period effects are absent by construction.

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1. Introduction

Insurance firms try to implement incentives to avoid excessive claims. This is particularly important in health insurance markets because some therapies depend on patient choice. The first visit to a doctor for a new illness, for instance, is solely a patient's decision. Here co-payments could be an appropriate instrument to reduce moral hazard. They do not only have a direct fiscal effect, but might also help to avoid excessive use of health care services. Such an inhibiting effect on utilization was the explicit aim of the € 10 fee for doctor visits which was introduced in Germany in 2004. A special characteristic of this fee is that patients who have already paid the fee are exempt from paying it for the rest of the calendar quarter. The out-of-pocket maximum is thus € 10 per calendar quarter and the price for doctor visits drops from € 10 to € 0 after the first visit.

There are several studies which use non-linearities in price schedules to estimate price elasticities. For instance, Kowalski (2009) uses exogenous variation in the marginal price of medical care which arises from family deductibles and out-of-pocket maxima. Marsh (2011) uses the sharp change in marginal prices that occurs around the deductible. While these two studies focus on changes in spot prices, Aron-Dine *et al.* 2012 show that the expected end-of-period price (i.e., the spot price at the end of the coverage or payment period) also influences the behavior of individuals when they face a non-linear price schedule. In this study we use the fact that individuals' behavior depends on spot prices. We show how the interaction between coverage period, which is a calendar quarter in this application, and reporting period, which corresponds to three months, can be used to induce exogenous variation in the initial price (i.e., the spot price at the beginning of the reporting period). While this variation in general can affect the interpretation of survey results, it can also be used to assess the causal effect of co-payments.

The identification strategy exploits the fact that the interaction between reporting and coverage period determines the prices that the respondents face at the beginning of the reporting period. Respondents interviewed at the end of the coverage period face a higher level of initial prices relative to respondents interviewed in the middle of the coverage period. The reason for this is that the fee has to be paid only once in a calendar quarter. Hence, the spot price for a doctor visit decreases over time. Some individuals who are interviewed in the middle of the coverage period have already paid the fee in the unobserved part of the previous calendar quarter. Therefore they initially face a price of € 0 for a non-negligible part of their reporting period. On the other hand, the initial price of those interviewed at the end of the coverage period is always € 10 since for them the reporting period starts at the beginning of the coverage period. Although the level of initial prices varies between these two groups, the expected end-of-period price is the same since both groups are random samples of the population.

There are two studies dealing with the introduction of the € 10 fee. Both are based on the German Socio-Economic Panel (GSOEP). Augurzky *et al.* (2006) tried to assess the effect of the reform on the probability of seeing a physician using a differences-in-differences approach. They compared statutory health insured participants with privately insured persons, and youths, because the latter two groups are exempt from the fee. Schreyögg and Grabka (2010) applied a similar estimation strategy. Furthermore, they used a zero-inflated negative binomial regression and a negative binomial hurdle model to directly model the number of doctor visits. Both studies concluded that the co-payment for doctor visits had failed to reduce the demand for doctor visits and argued that this ineffectiveness stems from the fact that it is just a *per-quarter* fee.

The present study, however, reveals that this characteristic does not make the fee ineffective; rather, it is the reason why the effect cannot be observed in the GSOEP using simple differences-in-differences approaches. In addition to a simple comparison of physician visits over time between privately and statutorily insured individuals, this study uses a second natural experiment that exploits exogenous variation in the initial prices. This allows us to disentangle the impact of the per-quarter fee from the effects of other parts of the reform. Using survey and claims data, we show that the reform as a whole decreases the probability of visiting a physician by 5 percentage points. The per-quarter fee causes at least half of this effect.

This study is organized as follows. The next section gives some institutional background about the health insurance system in Germany. Section 3 describes the natural experiment in the initial prices which identifies the causal effect of the new fee. Section 4 explains the data sets used in this analysis and the estimation strategy. Section 5 shows that the new fee alters the observable behavior in the survey data in a special manner. The effect of the new fee can only be observed once the model accounts for the structure of the data. Section 6 concludes.

2. Institutional background

According to the OECD (2008), around 90% of the German population are covered by statutory health insurance (SHI). The regulation of SHI is heavily influenced by governmental decisions. One example is the implementation of a broad health care reform in 2004 which tried to strengthen cost consciousness and personal responsibility by increasing co-payments. An important part of this reform was the introduction of co-payments for doctor visits. Since 2004, most SHI-insured adults have had to pay €10 for the first visit to a doctor in a calendar quarter. Children and teenagers up to the age of 18 are exempt from co-payments. Moreover, there are also exemption rules for adults. They can apply for an exemption by paying one or two percent of their income in advance. Alternatively, they can choose a gate-keeping model. In this case they often have to pay only €10 a year but must visit a general practitioner (GP) first. When more specialised care is required, the patients receive a referral from this GP.

The €10 fee also covers additional doctor visits within a calendar quarter. So it is a "per-quarter" fee, which is independent of the volume of services rendered in connection with this or later visits within a quarter. This characteristic distinguishes the co-payment from "per-visit" fees. The effects of a per-visit co-payment have been analyzed in several studies (Roemer *et al.*, 1975; Jung, 1998; van de Voorde *et al.*, 2001). For instance, Jung (1998) investigated the effects of implementing such a fee in Korea. He found an remarkable decrease in the number of doctor visits and in the probability of seeking medical care. The effects of a per-quarter co-payment, however, should be different because this fee is not intended to affect all parts of the distribution. It creates a new incentive to avoid the first visit to a doctor in a quarter. However, in contrast to a per-visit fee, it generates no incentives to reduce the number of doctor visits within a quarter once the fee is paid.

Additionally, co-payments for prescription drugs have been increased at the same time as the introduction of the €10 fee and this complicates the evaluation of the fee. Prior to the reform, patients had to pay €4 for small, €4.50 for medium and €5 for large quantities of drugs. Since 2004 it has been a function of the retail price and the patient has had to bear 10% of the drug price. The co-payment amounts at least to €5 and at most to €10. The effects of increasing co-payments for prescription drugs on the demand for doctor visits were extensively investigated by Winkelmann (2004a, 2004b, 2006). He analyzed the influence of an earlier health care reform implemented in 1997. The most radical element of this reform was the increase of co-payments for prescription drugs (Winkelmann 2004a). All three studies found a link between the propensity to visit a doctor and co-payments for prescription drugs. Therefore, the health care reform of 2004 could affect the behavior of health care consumers through both the increased prescription fees and the introduction of co-payments for doctor visits. This study, however, introduces a method to disentangle these two effects and to uncover the impact of the co-payment for doctor visits.

3. Identification strategy

The GSOEP is an annual survey started in 1984 which, among other things, includes a question about the number of visits to a doctor in the last three months before the interview.[1] Thus the observed three-month period (the reporting period) depends on the day of the interview. The interviews are conducted every day from January to October. This variation can be used to identify the causal effect of the new fee if, depending on the day of the interview, the participants are differentially affected by the fee. As already mentioned, a special characteristic of the fee is that it must only be paid at the first visit in a quarter. This characteristic makes it possible to identify random samples of the population that face different initial prices for a doctor visit. The term "initial" refers to the beginning of the reporting period. The following example is to show that the initial price for a doctor visit depends on the day of the interview.

[1] The question reads as follows: Have you gone to a doctor within the last three months? If yes, please state how often.

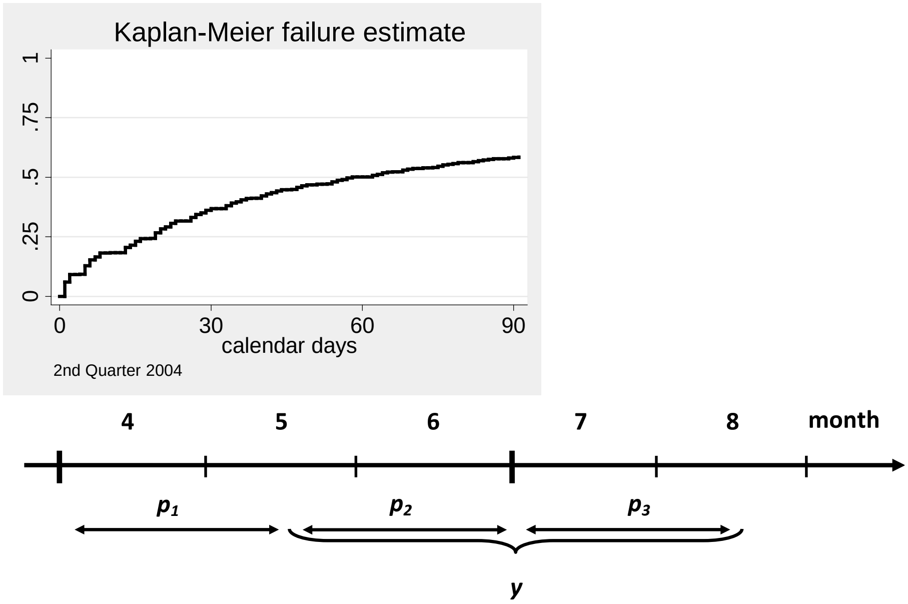

Figure 1: Degree of misclassification in the GSOEP data according to the AOK sample

By way of illustration, Figure 1 shows the reporting period for an interview conducted at August 15th. The reporting period can be separated into two equal periods - one period before and one period after the end of the calendar quarter (p_2 and p_3 in Figure 1). Period 1, on the other hand, is the unobserved part of the previous calendar quarter. The initial price for a doctor visit is either €0 or €10 depending on whether or not the individual has seen a doctor in period 1. According to the 2004 claims data set, 45% of the population visit a doctor in period 1 and have already paid the fee (see Figure 1). Hence, the initial price in period 2 is indeed €0 for a large fraction of the participants who are interviewed on August 15th. This group has access to free visits in the second period which is a non-negligible part of the reporting period. Compared to the years before the reform, the out-of-pocket costs during period 2 are thus unchanged in this group. On the other hand, the respondents who are interviewed at the end of a calendar quarter always face an initial price of €10. This is due to the fact that period 1, which is defined as unobserved part of the calendar quarter, now contains zero days and thus no one could have visited a doctor in this period. There is, hence, a variation in the initial prices whenever the reporting period differs from the coverage period, like it is, for instance, often the case in a survey.

The variation in the initial prices can also be seen as a misclassification of the treatment status in a simple before-after comparison. To see this, we express the probability of at least one doctor visit within the reporting period as

$$
\begin{aligned}
Pr(y>0) &= 1 - Pr(y=0) \\
&= 1 - Pr(s_2 = 0, s_3 = 0) \\
&= 1 - Pr(s_2 = 0) Pr(s_3 = 0) \\
&= 1 - \left[Pr(s_1 = 0, s_2 = 0) + Pr(s_1 > 0, s_2 = 0) \right] Pr(s_3 = 0)
\end{aligned} \tag{1}
$$

where the number of doctor visits in period p_k is s_k for $k = 1, 2, 3$ and the number of visits in the reporting period is $y = s_2 + s_3$. For illustration purposes, we assume that the doctor visits follow a Poisson process. This justifies the third equality because all periods are disjoint time intervals. The Law of Total Probability then gives the fourth equality. It simply separates the individuals into two groups. Firstly, the individuals who have not visited a doctor in the first period and therefore have to pay the fee at the first visit in the second period. Secondly, those individuals who have seen a doctor in the first period and thus have access to free visits in the second period. The initial price for a doctor visit is €10 in the former group and €0 in the latter. The treatment status is not clear-cut when we define a treated individual as someone who faces an initial price of €10. Actually, $Pr(s_1 > 0)$ is the probability of a false-positive treatment status. We can thus observe the true reform effect only if $Pr(s_1 > 0) = 0$. The group of participants who are interviewed at the end of a calendar quarter is the only group where we know for sure that this condition is true because then there is no unobserved period in this calendar quarter. We therefore hypothesize that the true reform effect and in particular the causal effect of the per-quarter fee can only be observed in the group of participants who are interviewed at the end of the coverage period. To get rid of the misclassification problem, we use different models that account for the day of the interview. The details of these models are explained in the next section.

4. Data and estimation

We use two separate data sets to verify our identification strategy. The primary source of data is the GSOEP, which is an annual survey started in 1984. The second data source is a claims data set from the largest German sickness fund. We have used this data set to imitate a survey with two randomly assigned interview days. This enables us to verify essential assumptions of our identification strategy that are untestable with survey data. In the following, we finalize our identification strategy and state our hypotheses. Then, we explain how the claims data set can be used to investigate the validity of the assumptions.

We created a data set using the GSOEP and a data set using claims data from the AOK. We selected a period of four years centered around the health care reform of 2004 and used the years 2002/03 to observe the behavior before the reform and 2005/06 as post-reform years.[2] The sample includes men and women aged 20 to 60. The basic estimation strategy is to pool the data over the four years and evaluate the effect of the fee on the probability of at least one visit to a doctor in the observed three months.[3] We use linear probability models (LPM) to determine the effect of the reform. The conditional probability of at least one doctor visit is $Pr(y > 0|\boldsymbol{x}_k, \boldsymbol{w}) = \boldsymbol{x}'_k\boldsymbol{\beta}_k + \boldsymbol{w}'\boldsymbol{\gamma}$ where y is the number of doctor visits. The index k refers to different parameterizations of the linear index $\boldsymbol{x}'\boldsymbol{\beta}$ which have been estimated to evaluate the effect of the reform. They are explained in more detail in the following paragraph. The vector $\boldsymbol{w}$ stands for other characteristics controlled for in the regressions. It contains a second-order polynomial in age, two indicators for self-reported health status, three indicators for interview season and employment status. Furthermore, we include the variables *female, years of education, married, household size, welfare recipient and household income.*[4]

The LPM have been estimated using different parameterizations. One current method to evaluate health care reforms in Germany is to compare privately and statutorily insured persons with a differences-in-differences approach because privately insured persons are unaffected by these changes. Under the assumption of a common trend between privately and statutorily insured persons, this approach can identify the effect of the entire reform only if this effect is independent of when the interview took place. Here $\boldsymbol{x}'_k\boldsymbol{\beta}_k$ is

$$\boldsymbol{x}'_1\boldsymbol{\beta}_1 = \beta_{1,1}\text{after} + \beta_{1,2}\text{SHI} + \beta_{1,3}\text{after} * \text{SHI} \tag{2}$$

where the variable *after* indicates the post-reform years and the variable *SHI* is an indicator of whether a person is SHI-insured. The interaction between *after* and *SHI* denotes a statutorily insured observation after the reform.

As hypothesized in section 3, SHI-insured participants in the GSOEP are, depending on the day of their interview, differently affected by the new fee. The estimation strategy in equation (2), which has been used in previous studies, ignores the variation in the initial prices. The models discussed in the following use the information about the day of the interview to assess the reform effect and in particular the causal effect of the per-quarter fee:

$$\boldsymbol{x}'_2\boldsymbol{\beta}_2 = \beta_{2,1}\text{after} + \beta_{2,2}\text{SHI} + \beta_{2,3}\text{after} * \text{SHI} * \text{q} + \beta_{2,4}\text{after} * \text{SHI} * \text{(1-q)} \tag{3}$$

where q measures the degree of misclassification which rises with decreasing overlap between reporting and coverage period. We use a dichotomous and a continuous measure of the misclassification. In the latter case q is the distance of the day of the interview to the nearest end of a calendar quarter and $q = 0$ indicates individuals who were interviewed

[2] The year 2004 has to be ignored because many interviews in the GSOEP take place in the first three months and thus the observed three-month period lies partly in the pre- and post-reform time.

[3] Generally, it is possible to analyze the effect on the number of visits using a count data model. In this study, however, we are primarily interested in the binary decision whether an individual visits a doctor or not because not visiting a doctor is the only way to avoid the fee.

[4] In the claims data set we can observe only individuals' age and gender.

at the end of a quarter where the initial price is €10 for all individuals and the misclassification is thus zero. $\beta_{2,4}$ therefore reveals the true reform effect. The assumption that the reform effect is independent of the day of the interview can be rejected once $\beta_{2,4}$ is significantly different from $\beta_{2,3}$. Additionally, it is possible to identify the reform effect by a dichotomous variable that splits the participants into two groups - similar to the example discussed in section 3. In group A the interview took place at the end of a quarter.[5] Group B contains the remaining sample.[6] The results from the dichotomous measure are very similar to the results from the continuous measure, indicating that the misclassification in group A is close to zero. We therefore rely on the dichotomous measure in the following analysis.

The final estimation strategy makes it possible to identify the causal effect of the new fee without using privately insured persons as control group. Here we use the fact that group B itself is a contemporaneous control group for group A. While group A and B are equally affected by the increase in prescription fees and other macro effects, they face different initial prices for doctor visits. The following regression can thus reveal the impact of new fee:

$$\boldsymbol{x}_3'\boldsymbol{\beta}_3 = \beta_{3,1}\text{after} + \beta_{3,2}\text{A} + \beta_{3,3}\text{after} * \text{A} \tag{4}$$

where $\beta_{3,2}$ is zero if both groups are random samples of the population. The parameter $\beta_{3,3}$ identifies the post-reform difference between both groups, which is caused by the gap in the initial prices. A testable implication of our identification strategy is that $\beta_{3,3}$ should be larger in magnitude in the sicker population because they visit a doctor on average more often than healthy people. Hence, more of them have access to free visits implying that the gap in the initial prices widens between group A and B.

There are two essential assumptions of our identification strategy. The first assumption is only relevant in the GSOEP data set. The question is here whether the distance to the end of a calendar quarter was assigned to each survey participant in a way that can be considered as random. The evidence in the GSOEP data strongly suggests a random assignment. Nevertheless, we additionally verify this assumption using the claims data set. Here we can randomly split the sample to simulate certain interview or reporting periods and compare the results of equation (4) with the corresponding results from the GSOEP. The first "interview period" starts on July 1st and ends on September 30th of

[5] In the GSOEP data the group A indicator must contain some days around the end of a calendar quarter since too few participants were interviewed exactly at the end of a quarter. We choose plus and minus 10 days.

[6] Schreyögg and Grabka (2010) apply a similar approach but do not use the variation in the initial prices to identify the causal effect of the new fee. They restrict their sample to those respondents who gave their interview within 15 days *before* the end of a quarter. This classification, however, incorrectly assigns persons to their group B that were interviewed close to the end of a calendar quarter where the misclassification is close to zero - namely, those participants who were interviewed at the beginning of a calendar quarter. This classification, thus, decreases the exogenous variation in the initial prices and it is not surprising that they only found slightly larger effects for their group A.

each year, i.e. it covers a full calendar quarter. This is group A in the claims data set. The "interview period" of group B is from May 16th to August 15th of each year. We used the claims data set to calculate the number of doctor visits in both "interview periods".

The second key assumption is that the probability of visiting a doctor in the different interview periods would have been the same in the absence of the new fee. Here seasonal fluctuations are a potential concern since both "interview periods" are not completely overlapping. We used the 16 to 17 year olds to investigate this assumption. Given a common trend, this group makes it possible to separate seasonal effects, since people younger than 18 do not have to co-pay at all. Hence, in the absence of seasonal effects there should be no difference between both "interview periods" in the group of 16 to 17 year olds. The random assignment and the absence of seasonal effects would suggest that there are also no differences between both "interview periods" in the adult population – apart from the variation in the initial prices.

5. Results

Table 1 shows that the per-quarter fee alters the observable behavior of the SHI-insured persons in the GSOEP in a special manner. It displays the sample means for the years before and after the reform grouped by whether or not the respondents were interviewed at the end of a quarter. Interestingly, after the reform the share of respondents with at least one doctor visit is significantly lower when participants were interviewed at the end of a quarter (group A) compared to the second group of interviews which took place sometime in the middle of a quarter (group B). This is, however, not the case before the reform. In both groups 64% visit their doctor at least once in three months before the reform. The unconditional probability decreases to 61.6% in group A after the reform, whereas it stays unchanged at around 64% in group B. Apart from the stronger decline in group B, the results are very similar in the claims data set from the AOK Hesse. Here we can also see a difference between group A and B after the reform but no difference before the reform.

Table 1 also gives evidence that the distance to the end of a calendar quarter is quasi-randomly assigned. There are namely no relevant differences between group A and B in important predictors of need for medical care. For instance, the average age in the GSOEP is 40 in both groups and around 54% of the respondents are female. Self-reported health (SRHS) is also very similar in both groups. In contrast to the AOK Hesse data set, the assignment to both groups was not by definition random in the GSOEP. Therefore it is here particularly important to see that there is neither a difference in the outcome before the reform nor any differences in important explanatory variables.

In this paragraph we verify our identification strategy using the claims data set. Table 2 compares and contrasts the estimation results from the GSEOP data with the results from the AOK claims data. The corresponding estimation strategy is described in equa-

Table 1: Group means before and after the reform

	GSOEP		AOK Hesse	
	2002 & 2003	2005 & 2006	2002 & 2003	2005 & 2006
At least one doctor visit	**0.640**	**0.616**	**0.658**	**0.581**
	0.640	0.642	0.659	0.610
Age	**39.77**	**40.36**	**40.62**	**40.86**
	39.94	40.65	40.69	40.96
Female	**0.532**	**0.548**	**0.471**	**0.480**
	0.534	0.543	0.477	0.485
SRHS (1: very good, ..., 5: very bad)	**2.475**	**2.529**		
	2.495	2.525		
Education in years	**11.75**	**12.01**		
	11.82	11.90		
Married	**0.626**	**0.616**		
	0.620	0.594		
Household size	**3.063**	**3.024**		
	3.035	2.955		
Welfare recipient	**0.037**	**0.053**		
	0.038	0.069		
Ln(income)	**7.719**	**7.767**		
	7.695	7.695		
Observations	**3,680**	**3,430**	**152,086**	**147,923**
	19,664	16,770	152,091	147,563

Only SHI-insured observations are used in the GSOEP sample (**Group A** / Group B).

Note: The lower fraction of women in the AOK Hesse sample is in accordance with official figures. See e.g. "GKV-Versicherte nach Alter und Wohnort GKV-Statistik KM6 zum 1. Juli 2005", Federal Ministry of Health.

Table 2: Estimation results from the different data sets

	GSOEP	GSOEP AOK only	AOK Hesse 20-60	AOK Hesse 16-17
Age/10	-0.0861	-0.0475	-0.1473	0.3875
	(0.0185)	(0.0326)	(0.0051)	(0.0542)
$Age^2/100$	0.0161	0.0135	0.0234	
	(0.0023)	(0.0040)	(0.0000)	
Female	0.1495	0.1583	0.1719	0.1654
	(0.0058)	(0.0102)	(0.0016)	(0.0061)
After	-0.0024	-0.0064	-0.0523	-0.0130
	(0.0047)	(0.0085)	(0.0016)	(0.0086)
A	0.0015	0.0143	0.0011	-0.0105
	(0.0085)	(0.0147)	(0.0020)	(0.0088)
After x A	-0.0260	-0.0382	-0.0279	-0.0076
	(0.0123)	(0.0217)	(0.0023)	(0.0122)
Observations	43,544	13,760	599,663	27,763

Dependent variable: at least one doctor visit. Parameter estimates after separate linear regressions using only SHI-insured observations. Cluster-robust standard errors in parentheses.

tion (4). The first two columns are based on the survey data, the third column shows the corresponding results from the claims data set and the last column shows the results for the 16 to 17 year olds which allows us to separate potential seasonal effects. There are some differences between the survey data and the claims data set. Firstly, while we can observe many potential covariates in the survey, we only observe individuals' age and gender in the claims data set. For comparison reasons, the covariates in Table 2 are thus restricted to a second-order polynomial in age and a gender indicator. Secondly, while the individuals in the claims data set are insured with AOK, the survey participants can be insured in all existing statutory sickness funds. This is particularly important because if someone is SHI-insured, he can choose between all statutory sickness funds. As a result, the risk pool of AOK may differ from the other sickness funds. According to official figures, AOK insurees are slightly older than the entire population.[7] Therefore we also provide the estimation results for the group of survey participants who are insured with AOK (see column 2). Finally, there is a regional difference. While all individuals in the claims data set live in Hesse, the GSEOP is a German-wide survey. However, we do not believe that this affects the comparability of both samples since Hesse is a large federal state and certainly representative of Germany.

The results are striking. Although we randomly split the claims data set into two groups, there is a significant difference in the probability of visiting a physician between

[7] Source, available only in German: "GKV-Versicherte nach Alter und Wohnort GKV-Statistik KM6 zum 1. Juli 2005", Federal Ministry of Health.

these two groups after the reform which was not the case before the reform (see column 3 of Table 2). On the other hand, splitting the 16 to 17 year olds into two groups with different "interview periods" does not lead to a significant difference (see column 4), indicating that the effect on the adult population is not due to seasonal fluctuations. While the post-reform difference in the adult population is significant, the pre-reform difference between both groups is not. This is the expected result when the group assignment is random and when there are no seasonal differences between both "interview periods" in the years before the reform. Given the random assignment in the claims data set and the likely absence of seasonal influences, we therefore conclude that the post-reform difference stems from the variation in the initial prices as hypothesized in section 3.

The results are very similar in the survey data set indicating that our identification strategy also works in the GSOEP. But while the decline in group B, which can be interpreted as the general effect of the reform, is about 5.2% in the claims data set, it is insignificant in the survey data set. Given the accuracy of the claims data set, this may point to a survey effect in the response behavior. The estimates from the differences-in-differences regression, which are discussed in more detail later in this study, strengthen this suggestion. When we use the privately insured as contemporaneous control group, the overall effect is between 4.2% and 5.4% in the GSOEP sample (see Table 3). This is distinctly larger than the overall effect of 2.8% reported in Table 2, and also closer to the overall effect of 8.0% in the AOK sample. The following part of the results is based on the GSOEP data because in this data set we can take advantage of the richer set of covariates and moreover can observe the privately insured as an additional contemporaneous control group.

Table 3: Estimation results from the GSOEP data set

	full sample			if $A = 1$
Age / 10	-0.0982	-0.0984	-0.0981	-0.0529
	(0.0179)	(0.0179)	(0.0179)	(0.0392)
$Age^2/100$	0.0123	0.0123	0.0123	0.0073
	(0.0022)	(0.0022)	(0.0022)	(0.0047)
Female	0.1350	0.1351	0.1350	0.1413
	(0.0058)	(0.0058)	(0.0058)	(0.0120)
Education / 10	0.0791	0.0795	0.0792	0.0910
	(0.0110)	(0.0110)	(0.0110)	(0.0230)
Married	0.0293	0.0292	0.0293	0.0311
	(0.0065)	(0.0065)	(0.0065)	(0.0141)
Household size	-0.0247	-0.0247	-0.0248	-0.0232
	(0.0024)	(0.0024)	(0.0024)	(0.0052)
Good health	-0.1691	-0.1693	-0.1692	-0.1780
	(0.0053)	(0.0053)	(0.0053)	(0.0121)
Bad health	0.1629	0.1628	0.1628	0.1520
	(0.0061)	(0.0061)	(0.0061)	(0.0144)
Welfare recipient	-0.0114	-0.0117	-0.0116	-0.0282
	(0.0134)	(0.0134)	(0.0134)	(0.0340)
Ln(income)	0.0375	0.0376	0.0377	0.0313
	(0.0058)	(0.0058)	(0.0058)	(0.0129)
After	0.0146	0.0135	0.0141	0.0128
	(0.0118)	(0.0118)	(0.0118)	(0.0296)
SHI	0.0312	0.0316	0.0314	0.0474
	(0.0104)	(0.0104)	(0.0104)	(0.0238)
After x SHI	-0.0226			-0.0537
	(0.0125)			(0.0314)
q is		continuous	dichotomous	
After x SHI x q		-0.0082*	-0.0184*	
		(0.0136)	(0.0126)	
After x SHI x (1-q)		-0.0417*	-0.0430*	
		(0.0143)	(0.0146)	
Observations	49,326	49,326	49,326	8,084

Dependent variable: at least one doctor visit in the reporting period.

Models also account for seasonal effects and employment status.

Cluster-robust standard errors in parentheses.

* The parameter estimates are significantly different at the 1%-level.

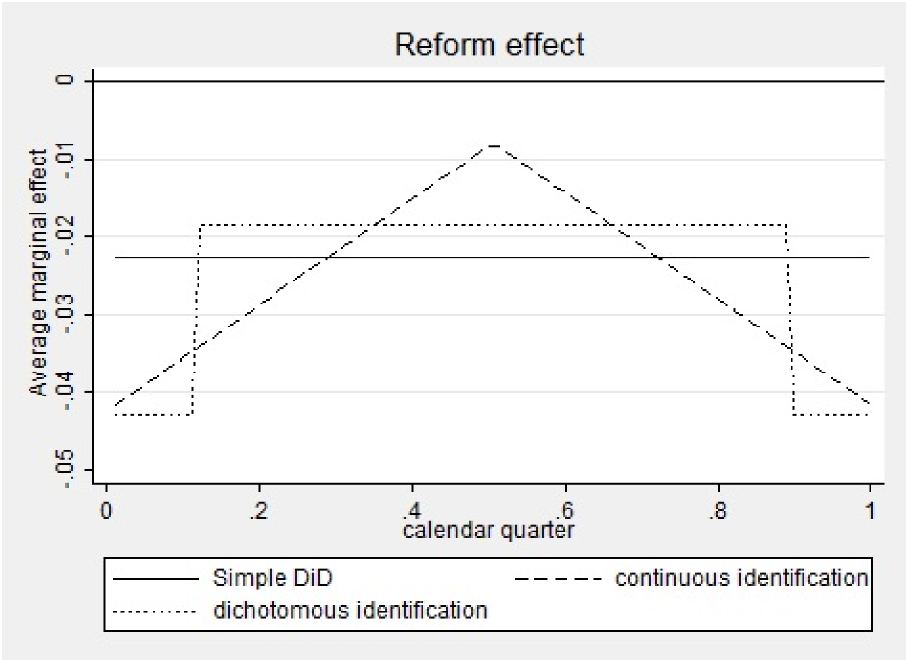

Figure 2: Average marginal effects for each day of the interview (GSOEP)

Table 3 displays the average marginal effects of the probit regressions that compare privately and statutorily insured individuals. Most effects are very similar to those found in Winkelmann (2004a). The probability of visiting a doctor is u-shaped in age and women are more likely to see a physician than men. The effects of education and household size are larger in the present study and married persons are somewhat more likely to visit a physician in Winkelmann's sample. The estimation strategy in the first column is a simple differences-in-differences approach conditional on covariates (see equation (2)). According to these estimates, the reform leads to a slight decrease in the probability of visiting a physician in the group of SHI-insured persons. It is only weakly significant at the 10%-level despite the large sample size. This result is in line with previous research which concluded that the per-quarter fee had failed to reduce the demand for doctor visits (Augurzky *et al.*, 2006; Schreyögg and Grabka, 2010). However, this conclusion changes once the reform effect can vary with the degree of misclassification as in equation (3) (compare columns 2 and 3 of Table 3). Now, there is a strong and highly significant reform effect in both models given the misclassification is zero ($q = 0$), which also means that the initial price is €10 for all individuals.

Figure 2 displays the reform effect over the entire range of q. The average marginal effect of the reform is significantly stronger at the end of a calendar quarter, while we wrongly assume that the reform effect is constant in the simple differences-in-differences regression in equation (2). Comparing the reform effect at the end of the coverage period with the effect in the middle of the period, allows us to assess the effect of the per-quarter

Table 4: Comparison of trends for different subgroups of the population (GSOEP)

Regressions conditional on SRHS	Parameter estimates: After	A	After x A	Pre-reform: Number of doctor visits	Probability of any use	Obs.
very good	0.0277	0.0009	0.0239	0.99	0.43	4,075
	(0.0169)	(0.0289)	(0.0429)			
good	-0.0018	0.0013	-0.0392	1.35	0.56	20,138
	(0.0076)	(0.0129)	(0.0186)			
satisfactory	-0.0156	0.0156	-0.0253	2.49	0.72	13,525
	(0.0084)	(0.0152)	(0.0210)			
poor	-0.0021	0.0341	-0.0675	5.02	0.88	4,769
	(0.0112)	(0.0179)	(0.0275)			
bad	0.0122	-0.0014	-0.0717	9.35	0.94	1,030
	(0.0171)	(0.0295)	(0.0466)			
Entire sample	-0.0028	0.0084	-0.0314	2.24	0.64	43,544
	(0.0047)	(0.0085)	(0.0120)			

Dependent variable: at least one doctor visit in the reporting period.

Parameter estimates after separate linear regressions using only SHI-insured observations.

Covariates are the same as in Table 3. Cluster-robust standard errors in parentheses.

fee. According to Figure 2, at least half of the reform effect is caused by the per-quarter co-payment for doctor visits. The underlying estimates are significantly different at the 1%-level (see Table 3).

The model in the first column of Table 3 is inappropriate to evaluate the new co-payment for doctor visits. It assumes that the reform effect is independent of when the interview took place although the observable behavior in the GSOEP is differently affected by the new fee. Column 4 shows the estimation results for the group of participants who were interviewed around the end of a calendar quarter. The reporting period thus coincides with the coverage period which implies that all individuals in this group face an initial price of €10. Here one can see a significantly stronger decline in the probability of visiting a physician in the group of statutorily health insured individuals than in the group of privately insured. The average reform effect is -0.054 which is very similar to the results from the second and third column (-0.042 and -0.043). The reform effect in column 4 is, however, less significant, which is probably due to the distinctly smaller sample size.

To further strengthen our identification strategy, we now alter the gap in the initial prices. This should affect the post-reform differences. Table 4 reports the estimation results for equation (4) which compares the different trends between participants interviewed around the end of a calendar quarter with the remaining sample. The estimation strategy in Table 4 is the same as in Table 2 but now we only use the GSOEP data set and can thus take advantage of the richer set of covariates. The primary variable of interest is the post-reform difference between group A and B. According to the last row of Table 4, the post-reform difference is significant and similar in magnitude to the results in Table

2. The upper panel in Table 4 shows the estimation results and the sample means of the outcome variable grouped by self-reported health status. The rise in the probability of visiting a doctor at least once and in the number of doctor visits indicate that the group of individuals with access to free visits increases with decreasing health status. This means that gap in the initial prices widens. As expected, the point estimate for the post-reform difference between group A and B is largest in the sick population. However, it is not significant (p-value=0.124) which might be caused by the distinctly smaller sample size. Apart from the group which reports a satisfactory health status, the point estimate rises in magnitude with decreasing health status. This indicates that the sicker population contributes more to the identification, which might be due to a true reduction in demand for medical care.

However, since sick people have a high need for medical care, it is unrealistic that they permanently reduce their visits to zero in order to avoid paying €10 per quarter. The difference between both groups in the sicker population may therefore also be caused by a second effect of the fee. It may have generated an incentive to cluster a given level of care in as few as possible calendar quarters. So once people have access to free visits, they may be tempted to group their visits into this quarter. This would affect the observable behavior in group B stronger because some members of group B are exempt from the fee already at the beginning of their reporting period. The post-reform difference between both groups could therefore also be caused by an incentive to cluster visits. Such a behavior would also lead to a larger variance in the number of doctor visits. In the claims data set there is indeed an increase in the variance after the reform. While the sample variance was 26.7 before the reform, it rises to 30.5 after the reform. We will investigate this issue in more detail in a follow-up analysis.

6. Conclusion

A key contribution of this study is to show that the interaction between reporting and coverage period determines the prices that the respondents face at the beginning of the reporting period. Ignoring this interaction can lead to an underestimation of the price effect. Using as an example the introduction of a per-quarter fee for doctor visits in Germany, we show how this can affect the interpretation of survey results. Respondents interviewed at the end of the coverage period face a higher level of initial prices relative to respondents interviewed in the middle of the coverage period since the spot price for a doctor visit decreases over time. This leads to a dilution of the reform effect in survey data sets.

Furthermore, the exogenous variation in the initial prices can be used to assess the causal effect of the fee on utilization. This approach is appealing because it compares random samples of the population that are differentially affected by the new fee. Therefore, a differences-in-differences regression makes it possible to disentangle its influence from potential macro effects. In particular, it separates the influence of the fee from the effect of the contemporaneous increase of co-payments for drugs.

Taking the variation in the initial prices into account, the probability of visiting a physician decreases by around 5 percentage points. Due to our identification strategy, we can attribute at least half of this effect to the per-quarter fee for doctor visits.

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