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Copayments for doctor visits in Germany and the probability of visiting a physician - Evidence from a natural experiment

Copayments for doctor visits in Germany and the probability of visiting a physician - Evidence from a natural experiment

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SUMMARY

The German health care reform of 2004 imposes a charge of € 10 for the first visit to a doctor in each quarter of the year. At first glance, there is no inhibiting effect of this fee on utilization in the German Socio-Economic Panel. However, this study reveals that the true effect is diluted by a special characteristic of the fee. Exploiting random variation in the interview date, this study finds a substantial effect of the new fee on the probability of visiting a physician. In addition, the identification strategy makes it possible to disentangle this effect from the influence of the contemporaneous increase of copayments for prescription drugs.

Keywords: copayment; moral hazard; differences-in-differences

July 11, 2009

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1 Introduction

Every insurer tries to implement incentives to avoid excessive claims. This is particularly important in health insurance markets, where claiming insurance often depends only on the insured. The first visit to a doctor for a new illness, for instance, is solely a patient's decision. Here copayments could be an appropriate instrument to reduce moral hazard. The introduction and increase of copayments have been important instruments of past health care reforms in the German statutory health insurance. There are, for instance, copayments for drugs, hospitalization and doctor visits. These instruments have a direct fiscal effect because the insurer covers a lower amount. In addition, however, there might be a reduction in the demand for health care services because the insured avoid excessive use. Such an inhibiting effect on utilization was also a professed goal of the copayment for doctor visits which was introduced in Germany in 2004. This study exploits a new natural experiment which can be used to evaluate the effect of the fee. In contrast to other research (Augurzky *et al.*, 2006; Schreyögg and Grabka, 2008) this study finds a significant decline in the probability of visiting a doctor.

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 personal responsibility by increasing copayments. An important part of this reform was the introduction of copayments for doctor visits. Since 2004 most SHI-insured adults have had to pay € 10 for the first visit to a doctor in a quarter.[1] Importantly, these € 10 payments also cover further therapies within this 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 copayment from "per-visit" fees. The effects of a per-visit copayment have already been analyzed in some studies (Roemer, 1975; Jung, 1998; van de Voorde *et al.*, 2001). Jung (1998) investigated the effects of implementing such a fee in Korea. He found an important decrease in the number of doctor visits and in the probability of seeking medical care. The effects of a per-quarter copayment, however, should be different because this fee is

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[1] Children and teenagers up to the age of 18 are exempted from copayments. Moreover, adults can choose a gate-keeping model. In this case they can avoid the new fee by visiting a general practioner (GP) first. When more specialised care is required, the patients receive a referral from this GP. However, it will turn out that the method of this study is unaffected by this potential self-selection.

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, copayments for prescription drugs have been increased at the same time and this complicates the evaluation of the copayment for doctor visits. Prior to the reform, patients had to pay € 4 for small, € 4.50 for medium and € 5 for large package sizes. Since 2004 it has been a function of the sales price. The patient has had to bear 10% of the drug price. The copayment amounts at least to € 5 and at most to € 10. The effects of increasing copayments 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 copayments for prescription drugs (Winkelmann 2004a). All three studies found a link between the propensity to visit a doctor and copayments for prescription drugs. Therefore the health care reform of 2004 could affect the behaviour of health care consumers through both the increased copayments for prescription drugs and the introduction of copayments for doctor visits. This study, however, shows a method to disentangle these two effects and to uncover the impact of the copayment for doctor visits.

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 exempted from the fee. Schreyögg and Grabka (2008) applied a similiar estimation strategy. Furthermore, they used a zero-inflated negative binomial regression to directly model the number of doctor visits. However, both studies concluded that the copayment for doctor visits had failed to reduce the demand for doctor visits and argued that this ineffectiveness stems from the fact that it is a *per-quarter* fee. This study, however, reveals that this characteristic does not make the copayment ineffective but is the reason why the effect cannot be observed in the GSOEP data at first glance.

The paper is organized as follows. The next section describes the natural experiment which enables the identification of the copayment effect. Section 3 explains the dataset used in this analysis and the estimation strategy. Section 4 shows that the copayment alters the observable behavior in the used dataset 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 5 concludes.

2   A new natural experiment

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.[2] Thus the observed three months depend on the interview day. The interviews are conducted every day from January to October. The majority of interviews take place in the first half of the year. 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 SHI-insured population that are differently affected by the per-quarter fee.

[insert Figure 1 around here]

Two extreme cases of the interview date are indicated by the dots in figure 1. In the first case (group B) the interview took place at the end of a quarter. Here the respondents had a full quarter in mind when they answered the question about their number of doctor visits. In the second case (group A) the interview took place in the middle of a quarter and the observed three-month period extends over two quarters. Assuming that the interview day (not necessarily the month) is randomly assigned to each participant, there should be no differences in the demand for doctor visits between group A and B because you can observe three months in both groups.[3] I argue, however, that the per-quarter copayment differentially affects the observable behavior in these two groups. Since the reform of 2004 almost all SHI-insured persons in Germany have been confronted with a € 10 hurdle which vanishes once it is paid by a patient. If the relevant three-month period goes over two quarters (group A), there is a positive probability that the participant crosses the € 10 hurdle in the unobserved time of the previous quarter - this time is marked by C in figure 1. Thus some members of group A had already paid the fee in the *unobserved* time C

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

[3] The interviews are conducted every day. So in the analysis, of course, group A and B do not only contain these extreme cases. Therefore it is possible to account for seasonal effects and, at least conditional on season, there should be no differences in the demand for doctor visits. This study shows strong evidence that the assignment to both groups is indeed random.

and were therefore unaffected by the copayment in the *observed* period D. All members of group B had a full quarter in mind which, of course, was completely influenced by the fee. So if this copayment works, it will affect group B stronger than group A and this will lead to a lower demand for doctor visits within group B. A differences-in-differences approach can be used to uncover this effect.

The accuracy of this method depends on the comparability of the control group to the treatment group (Besley and Case, 2000). A strong advantage of the above-explained natural experiment is that both groups are random samples of the SHI-insured population. Therefore in the absence of the new fee there would be a common trend in both groups, which is necessary to identify the effect of the copayment. In particular, it is not problematic that copayments for prescription drugs have increased at the same time as the €10 fee has been implemented because the control and treatment group are equally affected by the increased copayments for drugs. Therefore this approach makes it possible to disentangle the influence of the copayment for doctor visits from the effect of the contemporaneously increased copayments for drugs. The concentration on SHI-insured persons also causes a minor disadvantage. This method can only partly identify the effect of copayments for doctor visits because the control group is also to some extent affected by the new fee.

This study exploits variation in the interview day to identify groups of participants that are differently affected by copayments for doctor visits. The main idea is to compare the demand for doctor visits between respondents who had a full quarter in mind (group B) and those who did not (group A). Two problems arise by constructing an indicator for group B. Firstly, it cannot be observed exactly which three months the participant had in mind when he answered the question of the number of doctor visits. But it is very likely that a person thought of a full quarter if he was interviewed at the end or beginning of a quarter. Secondly, too few observations are exactly at the end or beginning of a quarter. Therefore the group B indicator must contain some days around this date. In the following the variable *B* indicates all participants who were interviewed at the end of a quarter plus or minus 10 days. Of course, this assignment leads to some misclassification. A person interviewed at March 21 might not really have a full quarter in mind. To address this concern, I also use the distance to the end of a quarter as a continuous measure of the intensity of treatment and it gives similar results.[4]

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[4] Additionally, the results are very similar for different definitions of group B (e.g. +/-5 days). They are available from the author upon request.

3 Data and methods

I create a dataset using the GSOEP samples A-F and select a period of four years centered around the health care reform of 2004.[5] The sample includes men and women in the age of 20 to 60 who have statutory or private health insurance (PHI). The basic estimation strategy is to pool the data over the four years and evaluate the effect of the copayment on the probability of at least one visit to a doctor in the observed three months. Generally, it is possible to analyze the effect on the number of visits using a count data model. For the purpose of this study, however, a binary outcome model is sufficient because the per-quarter fee is not intended to influence the frequent users of health services. Persons with many necessary doctor visits have severe health problems and it is unrealistic that they would reduce their contacts to zero to avoid paying € 10 per quarter.[6] Therefore a probit model is used to determine the effect of the copayment. The conditional probabilities are

$$
\begin{aligned}
Pr(y \geq 1 | \boldsymbol{x}_k, \boldsymbol{w}) &= \Phi(\boldsymbol{x}_k'\boldsymbol{\beta}_k + \boldsymbol{w}'\boldsymbol{\gamma}) \\
Pr(y = 0 | \boldsymbol{x}_k, \boldsymbol{w}) &= 1 - \Phi(\boldsymbol{x}_k'\boldsymbol{\beta}_k + \boldsymbol{w}'\boldsymbol{\gamma})
\end{aligned}
\tag{1}
$$

where y is the self-reported number of doctor visits and Φ denotes the cumulative distribution function of the standard normal distribution. k refers to four different parameterizations of the linear index $\boldsymbol{x}'\boldsymbol{\beta}$ which will be estimated to evaluate the effect of the new copayment. They will be explained in more detail in the following paragraph. The vector $\boldsymbol{w}$ stands for other characteristics controlled for in the regressions. It contains the same variables that are used in Winkelmann (2004a) except for the variable *active sport* which measures the frequent participation of an individual in physical activities and is constructed to act as a further proxy for good health. This variable cannot be used because there is no corresponding question in the surveys of 2002 and 2006. $\boldsymbol{w}$ thus includes a second-order polynomial in age, two indicators for self-reported health status, three indicators for interview season and employment status. Furthermore, I include the variables *female, years of education, married, household size, welfare recipient and*

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[5] I use the years 2002/03 to observe the behavior before the reform and 2005/06 as post-reform years. 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.

[6] Moreover, as already mentioned, persons can commit themselves to visit a GP first. In this case there are no copayments for doctor visits. Patients who must visit their physicians very often may use this option.

household income. In addition to the indicators for interview season, w contains a third-order polynomial in interview day (coded from 1 to 360) to account more precisely for seasonal effects.

The probit model will be estimated using different parameterizations. A current method to evaluate health care reforms in Germany is to compare privately and statutorily insured persons within a differences-in-differences approach. In this case $x'_k\beta_k$ is

$$x'_1\beta_1 = \beta_{1,1}\text{after} + \beta_{1,2}\text{SHI} + \beta_{1,3}\text{after * SHI} \tag{2}$$

where the variable *after* indicates the post-reform years and the variable *SHI* is an indicator whether a person is SHI-insured. The interaction between *after* and *SHI* denotes a statutorily insured observation after the reform. This estimation strategy supposes a single interaction effect in the overall sample. However, this study reveals two groups of SHI-insured participants in the GSOEP reacting differently to the new copayment. If each participant is randomly assigned to one of these groups, the following approach can be used to estimate the different interaction effects

$$x'_2\beta_2 = \beta_{2,1}\text{after} + \beta_{2,2}\text{SHI} + \beta_{2,3}\text{after * SHI * (1-B)} + \beta_{2,4}\text{after * SHI * B} \tag{3}$$

where the variable *B* indicates the group of participants that were interviewed at the end of a quarter. In this case the observed period is a quarter. As a result, only in this group are all members fully affected by the per-quarter fee. The assumption of a single interaction effect can be rejected once $\beta_{2,4}$ is significantly different from $\beta_{2,3}$. Furthermore a greater decrease over time in the probability of visiting a doctor in group B than in group A shows the effectiveness of the copayment.

A simpler empirical strategy that makes it possible to evaluate the copayment is to use only SHI-insured observations and estimate the decline in demand at the highest intensity of treatment. In this case $x'_k\beta_k$ is

$$x'_3\beta_3 = \beta_{3,1}\text{after} + \beta_{3,2}\text{I} + \beta_{3,3}\text{after * I} \tag{4}$$

where *I* measures the intensity of treatment. If it is coded as an inverse measure, the value zero indicates observations that are most strongly affected by the €10 copayment and $\beta_{3,1}$ identifies the decline in demand within this group. One such inverse measure is the group A indicator. Additionally, it is possible to identify the decline by a continuous measure

of the intensity of treatment. Let *t* measure the distance to the nearest end of a quarter in days. Thus participants who are interviewed t days before or after the end of a quarter get the same value. The logarithm of (*t*+1) is used as a continuous measure of the intensity of treatment.

In both cases $\beta_{3,2}$ is expected to be zero because there should be no differences in the demand for doctor visits between different intensities in the absence of the treatment, whereas $-\beta_{3,3}$ identifies the effect of the copayment for doctor visits.

4   Results

Table 1 shows that the new copayment alters the observable behaviour of the SHI-insured persons in the GSOEP in a special manner. It displays the 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 distinctly lower when participants were interviewed at the end of a quarter compared to the second group of interviews which took place sometime in the middle of a quarter. 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 probability decreases to 61.6% in group B after the reform, whereas it stays unchanged at around 64% in group A. The group means for the other variables are almost identical which gives some first evidence for the random assignment to one of both groups.

[insert Table 1 around here]

Table 2 displays the estimates of the probit regressions that compare the privately and the statutorily insured persons. Most effects are very similiar 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 this study and married persons are somewhat more likely to visit a physician in Winkelmann's sample. The estimation strategy in the first column is simply a differences-in-differences approach conditional on covariables. According to these estimates, the reform only leads to a slight decrease in the probability of visiting a physician in the group of SHI-insured persons and a part of this decline stems from the insignificant increase in the control group. These results are in line with previous research which concluded that copayments for doctor visits had failed to reduce the demand for doctor visits (Augurzky *et al.*, 2006;

Schreyögg and Grabka, 2008). However, this conclusion changes once the coefficient of the interaction term can vary between group A and B (compare column 2). In this model there is a strong and significant effect in group B but no significant decline in group A. The corresponding coefficients are significantly different at the 1%-level.[7] The model in the first column is inappropriate to evaluate the copayment for doctor visits. It assumes a single reform effect in the sample, while there are two groups in the GSOEP that react differently to the new fee and the majority of statutorily insured persons is in group A (compare Table 1). The true reform effect which can be observed only at the end of a quarter is thus diluted in the simpler model.

[insert Table 2 around here]

The control group in table 2 consists of privately insured persons. According to both regressions, the probability that a privately insured person visits a physician is not significantly influenced by time. Therefore estimations with only SHI-insured observations lead to similar conclusions about the effectiveness of the copayment for doctor visits. In the following, this sample will be used to evaluate the decline in demand for doctor services.

[insert Table 3 around here]

According to the estimates in the first column of table 3, there is a small but significant decrease in the probability of visiting a physician. It is, however, questionable to interpret this as a causal effect because identification is simply achieved by comparing the demand for doctor visits over time. Thus the decline in demand could also be caused by other events that occured concurrently. This is particularly important with respect to the increased copayments for drugs. Unlike the regression in the first column, the models in the second and third column allow one to disentangle the influence of the copayment for doctor visits from the effect of the increased copayments for drugs. This is achieved by comparing the decline in demand over time between groups of participants that have statutory health insurance but are differently affected by copayments for doctor visits. According to the estimates in the second column of table 3, there is no difference between group A and B in the probability of visiting a physician prior to the reform. This is the expected result if the interview day is randomly assigned to each participant. After

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[7] A more general model would allow all coefficients to vary between group A and B. The estimates of $\beta_{2,3}$ and $\beta_{2,4}$ are, however, almost the same in this model. In addition, a likelihood ratio test cannot reject the parsimonious model from (3) against the more general specification.

the reform, however, the probability that an individual with given characteristics visits a doctor is significantly lower if he was interviewed at the end of a quarter (i.e. he belongs to group B) compared to the remaining sample. The difference between both groups after the reform stems from the fact that all members of group B were completely affected by the introduction of the copayment, whereas some members of group A visited their physician in the unobserved time of the previous quarter and were thus partly exempted from the fee in the observed three months. Since both groups are random samples of the SHI-insured population, they are, on average, equally affected by time-varying influences apart from the introduction of the copayment. The significantly stronger decline in group B is therefore solely caused by the copayment for doctor visits. Based on this model the average differences-in-differences adds up to around -3.4%. Applying the continuous measure of the intensity of treatment leads to similar results (compare column 3).

[insert Figure 2 around here]

Figure 2 shows the relation between the average marginal effect of *after* and the distance to the end of a quarter for the discrete and continuous measure. In both cases there is a strong and equally sized effect at the end of the quarter but this effect diminishes with increasing distance to the end of a quarter or with decreasing intensity of treatment, respectively.

This study reveals a significant decline in the probability of visiting a physician due to the copayment whereas there is no study showing an effect in the positive part of the number of doctor visits.[8] This suggests an interesting policy implication. To the extent that the number of doctor visits represents the necessity of the visits, it reveals a stronger effect of the copayment on patients with minor illnesses. Persons who frequently have to visit a physician might choose the gate-keeping model to avoid the new copayment.

A crucial question in this study is whether the distance to the end of a quarter was randomly assigned to each participant. The smoothness of figure 2a and 2b gives some first evidence for the random assignment. If there were considerable differences in the distribution of the covariables within each value of *t*, there would be visible peaks in both figures. Moreover, the conditional differences-in-differences in the probability of at least one visit to a physician is around -3.4%, which is very similar to the unconditional differences-in-differences of -2.6% (compare Table 1). This means that the effect in the unconditional probabilities cannot be explained by differences in any variable included in

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[8] I also find no convincing evidence for such an effect of the copayment for doctor visits.

the model. Finally, employing the identification idea in the absence of the € 10 fee does not reveal any differences in the demand for doctor visits between different intensities of treatment. These points together give strong evidence for the random assignment.

5 Discussion

This study exploits variation in the interview date to assess the inhibiting effect of a per-quarter copayment for doctor visits on utilization. This approach is appealing because it compares random samples of the SHI-insured population that are differentially affected by the per-quarter fee. A differences-in-differences regression, therefore, makes it possible to disentangle the influence of the new copayment from potential macro effects. In particular, it disentangles the influence of the € 10 copayment from the effect of the contemporaneous increase of copayments for drugs. An important contribution of this study is to show the necessity of comparing full quarters before and after the reform to assess the effect of the copayment for doctor visits in Germany.

Since the introduction of the fee there have been two groups in the GSOEP. On the one hand, there are the participants who had a full quarter in mind when they answered the question about their number of doctor visits in the last three months. Only members of this group were completely affected by the copayment for doctor visits. As a result the true effect can be observed in this group. On the other hand, there are the remaining participants who had a three-month period in mind which extended over two quarters. In this group some participants crossed the € 10 hurdle in the unobserved time of the previous quarter and were thus partly unaffected by the copayment for doctor visits in the observed three months. The majority of observations in the GSOEP are in the latter group and therefore the true effect of the copayment is diluted in a model which assumes a single reform effect in the overall sample.

This study overcomes this problem by accounting for the structure of the data. It reveals that the probability of visiting a physician is significantly influenced by the implementation of copayments for doctor visits. The effect is about -3.4%, whereas there seems to be no effect in the positive part of the distribution. This result gives some evidence for a stronger effect of the copayment on patients with minor ailments.

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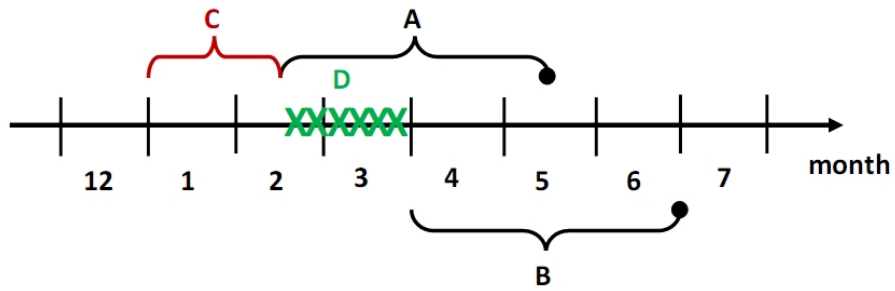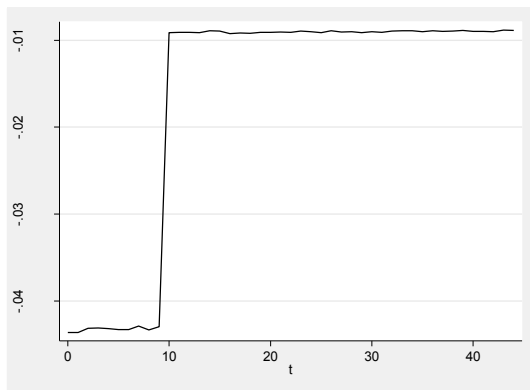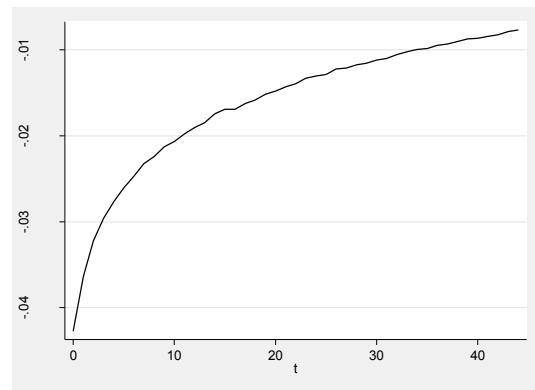
Figure 1: Relationship between interview day and intensity of treatment (two extreme cases)

(a) Discrete measure

(b) Continuous measure

Figure 2: Marginal effect of *after* (averaged by *t*) vs. distance to the end of a quarter

	2002 & 2003	2005 & 2006
At least one doctor visit	**0.640**	**0.616**
	0.640	0.642
Age	**39.77**	**40.36**
	39.94	40.65
Female	**0.532**	**0.548**
	0.534	0.543
Education in years	**11.75**	**12.01**
	11.82	11.90
Good health	**0.581**	**0.545**
	0.561	0.547
Bad health	**0.130**	**0.128**
	0.131	0.138
Observations	**3,680**	**3,430**
	19,664	16,770

Only SHI-insured observations are used (**Group B** / Group A).

Table 1: Group means before and after reform

Age / 10	-0.3025	-0.3022
	(0.0520)	(0.0520)
$Age^2/10$	0.0038	0.0038
	(0.0006)	(0.0006)
Female	0.3842	0.3842
	(0.0169)	(0.0169)
Education / 10	0.2191	0.2194
	(0.0322)	(0.0322)
Married	0.0849	0.0851
	(0.0187)	(0.0187)
Household size	-0.0719	-0.0720
	(0.0069)	(0.0069)
Good health	-0.4621	-0.4624
	(0.0150)	(0.0150)
Bad health	0.6127	0.6127
	(0.0256)	(0.0256)
Welfare recipient	-0.0415	-0.0421
	(0.0389)	(0.0389)
Ln(income)	0.1076	0.1083
	(0.0170)	(0.0170)
After	0.0260	0.0246
	(0.0337)	(0.0337)
SHI	0.0913	0.0919
	(0.0293)	(0.0293)
After x SHI	-0.0673	
	(0.0356)	
After x SHI x (1-B)		-0.0545[1]
		(0.0358)
After x SHI x B		-0.1286[1]
		(0.0414)
Log-Likelihood	-29,806.86	-29,802.60
Observations	49,326	49,326

Dependent variable: at least one doctor visit in three months.

Models also account for seasonal effects and employment status.

Robust standard errors in parentheses.

[1] The estimates are significantly different at the 1%-level.

Table 2: Results of pooled probit regressions using observations with statutory or private health insurance.

After	-0.0419	-0.1249	-0.1218
	(0.0133)	(0.0318)	(0.0505)
A		-0.0301	
		(0.0248)	
After x A		0.0985	
		(0.0345)	
Ln(t+1)			-0.0017
			(0.0128)
After x Ln(t+1)			0.0260
			(0.0160)
Log-Likelihood	-26,187.11	-26,182.55	-26,185.01
Observations	43,544	43,544	43,544

Dependent variable: at least one doctor visit in three months.

Models contain all covariables. Robust standard errors in parentheses.

Table 3: Results of pooled probit regressions using only observations with statutory health insurance.