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Co-payments for prescription drugs and the demand for doctor visits – Evidence from a natural experiment

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Summary

The German health care reform of 1997 provides a natural experiment for evaluating the price sensitivity of demand for physicians' services. As a part of the reform, co-payments for prescription drugs were increased step up to 200%. However, certain groups of people were exempted from the increase, providing a natural control group against which the changed demand for physicians' services of the treated, those subject to increased co-payments, can be assessed. The differences-in-differences estimates indicate that increased co-payments reduced the number of doctor visits by about 10% on an average. Copyright © 2004 John Wiley & Sons, Ltd.

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Introduction

Expenditures for health services make up a substantial part of total GDP in all OECD countries. For most countries, health expenditures as a share of total GDP have trended upward over the last few years and decades. In Germany, for example, the share increased from 8.4% in 1980 to 10.5% in 1996 [1]. The most commonly cited reasons for this increase are expanding technological possibilities in the health service sector as well as the ageing of the population, coupled in many countries with a large public health service where the incentive structure does not promote an efficient resource allocation.

One such country with a large publicly funded health sector is Germany. There have been regular attempts to reform the health care system in order to reduce cost (or limit its growth). The purpose of this paper is to evaluate the effects of such a major reform that took place in Germany in 1997. In that reform, co-payments for prescription drugs were raised by up to 200%.

Among the professed goals of the reform was to mitigate the moral hazard problem inherent in providing free or low-cost insurance. A number of indicators collected and published by insurance companies suggest that the problem of excessive use of prescription drugs was real and substantial [2]. According to this source, an estimated quarter of all prescriptions has no confirmed therapeutic benefit. Drugs worth 4 billion DM are disposed of each year as waste, a fifth of these in packages that were never opened.

In this paper, I will evaluate whether the reform, judged by its own objectives, was a success. Thus, I will study whether the reform lowered the demand for health services using the framework of a natural experiment, i.e. comparing outcomes for a treatment group with outcomes for a control

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group. This is possible, since certain groups of the population were exempt from the increase. These people provide a natural control group against which the changed demand for physicians' services of the treated, those subject to increased co-payments, can be assessed. The preferred differences-in-differences estimator indicates that increased co-payments reduced the number of doctor visits by 15% on average.

The German health care system and the reform of 1997

More than 90% of the German population is covered by statutory health insurance, which always includes coverage for prescription pharmaceuticals. The insurance system is mostly financed by mandatory payroll deductions. For employees, the premium is proportional to earnings (up to a contribution ceiling), and the coverage automatically extends to non-working spouses and dependent children. Employees with earnings above the contribution ceiling have the option to remain insured, buy private insurance, or have no insurance at all. Special provisions exist for other groups, such as the unemployed or students. All in all, the system mixes insurance aspects with strong redistributive elements.

Once a person is part of the system, the insurance reimburses the cost of doctor visits (including visits to general practitioners, specialists, and dentists), hospital stays, and qualifying pharmaceuticals. Pharmaceuticals are either sold over the counter, or they are available against a prescription. Only physicians can issue such a prescription, so that a doctor consultation must in general precede the purchase of the drug. All outpatient care prescription drugs are dispensed by retail pharmacies who charge uniform prices all over Germany.

Over-the-counter drugs are paid for directly by the patient, without any involvement of the insurer. In the case of prescription drugs, retail pharmacies directly charge the insurance companies for the price of the prescription, minus any out-of-pocket contribution that may be required of the patient. One consequence of this reimbursement system is that the patient in most cases is unaware of the full cost of the medicine. There are two potential types of out-of-pocket expenses. The most important one is the prescription fee – referred to as co-payment in this paper – that must be made for all prescriptions. The amount varies by package size. Such co-payments were increased substantially on 1 July 1997, by a fixed amount of DM 6 relative to a year earlier. Since the absolute amount of the co-payment is a function of the package size, after the reform DM 9 for small, DM 11 for medium and DM 13 for large sizes, the relative effect of the 1997 reform was largest for small sizes, where it amounted to a 200% increase.

An additional, second type of out-of-pocket expense refers to a maximum reimbursement for certain classes of pharmaceuticals where both brand products and generics are sold in the market. In such cases, a regulatory board can set reference prices, usually above the price of the generic and below the price of the brand. In this case, the reference price constitutes the maximum contribution covered by the insurer, and a patient buying the brand would need to pay the extra amount herself. This system has some noticeable effects on producer prices, as shown in [3]. In particular, producer prices for brand pharmaceuticals fell substantially following the introduction of reference pricing in 1989. However, this aspect is not central to the present analysis, as it likely affects only the relative demand for brands and generics, rather than the overall demand for prescription drugs.

Potential consequences of the reform

How large was the effect of the reform on the demand for prescription drugs and other aspects of health care utilization, and how successful was the reform in reducing perceived excess demand for health services? In assessing the effects of the reform on the demand for health services, one can usefully distinguish between a direct and an indirect effect. The direct effect is a movement up the demand curve for prescription drugs, i.e. a reduced number of drug purchases after the reform, as the increased co-payment directly increased the patient's out-of-pocket expenses for drug purchases. The indirect effect is a potential inward shift of the demand curve for doctor visits, to be explained below.

According to [4], the number of prescriptions paid for by the social health insurance fell by 11% between 1996 and 1997. However, one has to

be cautious when interpreting this number. First, the number of reimbursed prescriptions falls automatically, since prescription drugs priced below the level of the co-payment no longer require a payment by the insurer, and therefore are not counted in the statistics. Thus, even if the demand is completely inelastic, the instances of drug reimbursements will decline following an increase in the co-payment. Moreover, the reform changed the relative prices of the various package sizes. In particular, larger packages became less expensive relative to smaller packages, and one would expect to find a shift in demand towards these larger packages. Hence, a mere drop in the number of prescriptions does not necessarily indicate a decline in the total quantity of drugs demanded.

The indirect effect is related to the impact a price change for prescription drugs may have on the demand for doctor visits. There are good reasons to think that the indirect effect may be quantitatively as important as the direct one. Since all prescriptions are issued by physicians, the demand for doctor visits and the demand for prescription drugs are intrinsically linked. The two are close complements, and one would expect to observe a negative cross-price elasticity.

From the insurance point of view, therefore, increased co-payments can lower overall expenditures through two related channels, and any evaluation of the reform should reasonably take both into account. This is even more so since, as remarked before, people may have switched to larger package sizes. In this case it is possible, for instance, that the total amount of drugs demanded remained unchanged while the number of doctor visits went down (since fewer prescriptions needed to be issued). Of course, the link between doctor visits and the patient price for prescription drugs is more complicated than this simple example suggests. While all prescriptions require a doctor visit, only a fraction of all visits is undertaken in order to obtain a prescription. For example, one might see a doctor in order to seek advice on non-prescription or self-treatment, and one might not comply with the prescription and just not buy the drug. In either case, the number of visits would tend to be unaffected by the increased co-payment. If there is a combination of the two effects, the number of visits will go down, and it is an empirical question to quantify the size of the overall effect (Figure 1).

For all these reasons, it can be argued that evaluating the effect of the 1997 reform on the number of doctor visits is an important part of any overall evaluation. Mainly due to data considerations, the present paper will deal with this single aspect, although a future evaluation that includes direct evidence on the demand for pharmaceuticals is clearly desirable. The goal is then to provide estimates of the extent to which the individual demand for doctor visits changed due to the reform.

Two prior studies in this area are [4, Winkelmann R. Health care reform and the number of doctor visits – an econometric analysis. *J Appl Econ* 2003, in press.]. The former study uses evidence from a survey of 695 visitors to pharmacies in Cologne in 1998. The limitations of this approach were pointed out in the latter study, where an alternative evaluation based on simple pre–post-reform comparisons using data from the *German Socio Economic Panel* (GSOEP) was performed. The present paper improves on this previous research by exploiting an important aspect of the reform, namely that it did leave some people unaffected, in order to perform differences-in-differences estimation.

First, there is the group of people with private insurance. The content of private insurance

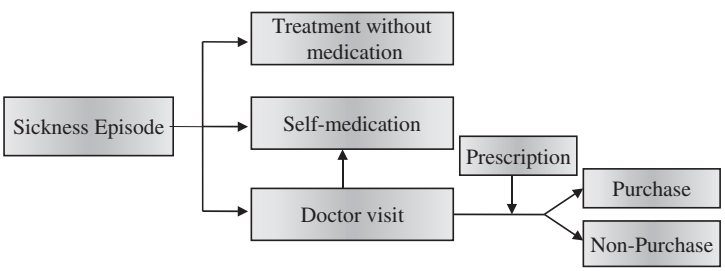

Figure 1. Prescription drugs and the demand for doctor visits [4].

contracts is quite diverse, and some of the contracts are designed to provide a coverage similar to the social insurance, albeit at lower cost. These contracts usually have a number of safeguards against moral hazard in place, such as a substantial annual deductible and a premium payback for non-users. Thus, it is unlikely that prescription drug co-payments, even if their level would have been synchronized with the reform (which again was not the case in most contracts), play a significant role for this group of people.

A second group explicitly exempted from the increased co-payments are co-insured children under the age of 18. A third type of exemption status has been defined by the law through a combination of income and exposure, in an attempt to limit the financial burden related to the provision of health services for low-income families. In particular, low-income households with family gross income under DM 1700 (for singles) or DM 2350 (for couples) were exempt, as were all amounts beyond the cumulative annual co-payments of 2% of annual gross income. Since I cannot measure exposure directly, I will proxy the group of people qualifying for this exemption status by defining a group of 'poor' persons as those whose income is in the first decile of the income distribution and who receive social assistance. Thus, there are three exempt populations, people with private insurance, young individuals, and the poor. All three groups will serve as 'controls' in the following analysis, taking the perspective of a 'natural experiment'.

Data and methods

The GSOEP was initiated in 1984 [5]. It is an annual survey that is ongoing. For the purpose of this study, I selected a period of 5 years centred around the year of the reform, i.e. 1995–1999. The GSOEP has a handful of variables relating to the usage of health service. One of them is the number of visits to a doctor during the previous 3 months. In some earlier years, this question was asked separately for visits to a general practitioner visits to a specialist, separately by field, and visits to a dentist. However, only the aggregate count is available in the 1995–1999 waves. I use observations on men and women from Sample A, i.e. persons associated with non-guestworker-households in the original sample for West Germany. Privately insured individuals (about 5% of the sample) are included.

The basic empirical strategy is to pool the data over the 5 years and estimate the effects of the reform by comparing the expected number of visits before and after the reform *ceteris paribus*, i.e. for an individual with constant characteristics. Let the model be

$$y_{it} = \exp(\beta_0 + \beta_1 \,\mathrm{treat}_i + \beta_2 \,\mathrm{post}_t + \beta_3 \,\mathrm{treat}_i \times \mathrm{post}_t + z_{it}'\gamma) + u_{it} \quad (1)$$

where y_{it} is the number of doctor visits of person i at time t, post_t is an indicator whether the increased co-payment is applicable in period t, treat_i is an indicator whether person i is subject to the increased co-payment, and the interaction between treat_i and post_t denotes an observation of a treated person after the reform. The vector z_{it} stands for all other socio-economic characteristics controlled for in the regression. These include a second-order polynomial in age, three indicators for the quarter of the interview, four indicators for the employment status plus the variables *years of education*, *married*, *household size*, *active sport*, *good health*, *bad health*, *private insurance*, *welfare recipient* (the coding of these variables is explained in the Appendix).

In Equation (1), β_3 measures the reform effect. If it is negative, the demand for doctor visits in the treatment group falls relative to the demand in the control group after the imposition of the increased co-payments. Since the conditional expectation function is log-linear, the coefficient identifies the relative change in expected demand. The dependent variable being a count (the number of doctor visits in the previous quarter), efficient estimation requires that the conditional expectation function implied by (1) is embedded into a count data model.

I will employ in the following analysis two different samples and two different econometric models. Remember that the reform took place in mid-1997. Thus, depending on the month of the interview, 1997 observations relate either to the pre-reform or to the post-reform period. For this reason, and in order to avoid transition affects, it seems prudent to drop 1997 observations from the sample. In a first set of estimations, then, 4 years of data are used, with 1995 and 1996 being pre-reform years, and 1998 and 1999 being post-reform years. Deleting observations with missing values on any of the dependent or independent

variables, this sample comprises 37 319 observations. I will refer to it as Sample A. To check the robustness of the results, I also use an alternative Sample where only 2 years of data are kept, namely 1996 and 1998. The sample size then drops to 18 683 observations. This is my Sample B. Note that in either case, the critique by Bertrand *et al.* [6] that tests based on conventional differences-in-differences estimators overstate the significance level does not apply, since the panel is short.

The second issue is how to account for the panel structure of the data. Clearly, repeated person-specific observations cannot be independent in the strict sense, since individual effects are likely present. I account for correlated observations in either one of the two possible ways. First, I use a Poisson probability function

$$f(y_{it}|\lambda_{it}) = \frac{\exp(-\lambda_{it})\lambda_{it}^{y_{it}}}{y_{it}!} \tag{2}$$

where

$$\begin{aligned}\lambda_{it} &= E(y_{it} \mid \lambda_{it}) \\ &= \exp(\beta_0 + \beta_1 \,\mathrm{treat}_i + \beta_2 \,\mathrm{post}_t \\ &\quad + \beta_3 \,\mathrm{treat}_i \times \mathrm{post}_t + z_{it}'\gamma)\end{aligned} \tag{3}$$

The model parameters are estimated by pseudo-maximum likelihood, using the random sampling likelihood function

$$L = \exp\left[-\sum_{i=1}^{N}\sum_{t=1}^{T_i}\lambda_{it}\right]\prod_{i=1}^{N}\prod_{t=1}^{T_i}\frac{\lambda_{it}^{y_{it}}}{y_{it}!} \tag{4}$$

and a covariance matrix estimator that accounts for heteroscedasticity of an unknown form and correlation between observations for the same person. As long as the conditional expectation function is correctly specified this approach leads to consistent parameter estimates and valid inference although it is inefficient [7].

The alternative is to use a count data model with random effects. Let

$$E(y_{it}|\lambda_{it}, \alpha_i) = \lambda_{it}\exp(\alpha_i) \tag{5}$$

where the conditional distribution of y_{it} given λ_{it} and α_i is a Poisson distribution, λ_{it} is defined as in (3), and α_i is independently gamma distributed. The resulting marginal model is a panel Poisson model of a negative binomial variety, with joint probability function for individual i

$$\begin{aligned}f(y_{i1}, \ldots, y_{iT}) &= \frac{\Gamma\left(\sum_t y_{it} + \gamma\right)}{\Gamma(\gamma)}\left(\frac{\gamma}{\gamma + \sum_t \lambda_{it}}\right)^{\gamma} \\ &\quad \times \frac{1}{\left(\gamma + \sum_t \lambda_{it}\right)^{\sum_t y_{it}}}\prod_{t=1}^{T}\left(\frac{\lambda_{it}^{y_{it}}}{y_{it}!}\right)\end{aligned} \tag{6}$$

where γ is an additional parameter. This model provides potentially more efficient parameter estimates than the Poisson model [8].

Results

Sample means and definitions of the variables involved in the analysis are given in the appendix. For example, the average number of doctor visits per quarter is 2.8. 53 percent of the person-year observations are associated with 'good' health, based on a subjective self-evaluation, and 17% are associated with bad health.

Two sets of full regression results, one for the Poisson model and the other for the panel Poisson model with gamma heterogeneity, denoted here as a 'panel negbin' model, are displayed in Table 1. The estimates are based on Sample A, i.e. they include observations for the years 1995, 1996, 1998 and 1999. No differences in differences are used here, but rather a simple pre–post reform comparison, which makes these results directly comparable to those in [Winkelmann R. Health care reform and the number of doctor visits – an econometric analysis. *J Appl Econ* 2003, in press.]. In the Poisson model the standard errors are adjusted to account for heteroscedasticity of the unknown form and correlation between observations for the same person. A formal likelihood ratio test clearly rejects the Poisson model against the panel negbin model. A comparison of the estimated standard errors suggests that some efficiency is gained indeed by estimating the panel model.

The estimated parameters differ somewhat between the two specifications, although they tend to give the same qualitative results. In particular, both models estimate a drop of 9–10% in the expected number of doctor visits in the post-reform period. Many of the other results are common in the literature: men have fewer doctor visits than women. The health indicators have the largest effect among all variables. A person in bad health is estimated to have four times as many

Table 1. Maximum likelihood results. Dependent variable: number of doctor visits during the previous quarter

	Poisson	Panel negbin
Age/10	0.082**	0.066**
	(0.033)	(0.027)
Age squared/1000	−0.069**	−0.011
	(0.033)	(0.028)
Male	−0.160**	−0.250**
	(0.021)	(0.017)
Years of schooling/10	−0.000	−0.027
	(0.040)	(0.035)
Married	0.078**	0.060**
	(0.024)	(0.016)
Active sport	0.060**	0.040**
	(0.020)	(0.011)
Good health	−0.602**	−0.479**
	(0.019)	(0.011)
Bad health	0.809**	0.648**
	(0.021)	(0.010)
Welfare recipient	0.054	0.027
	(0.047)	(0.025)
Logarithmic income	0.051**	−0.018
	(0.023)	(0.013)
Private health insurance	0.059	0.028
	(0.043)	(0.022)
Post reform	−0.097**	−0.091**
	(0.014)	(0.006)
Log-likelihood	−101 630	−81 770

Source: *German Socio-Economic Panel*, years 1995, 1996, 1998 and 1999. The model includes furthermore a constant, three-indicator variable for a quarter of the interview (winter, spring, fall), four indicators of employment status (full-time, part-time, self-employed, unemployed) and household size. The Robust standard errors are in parentheses. Coefficients with ** are significant at the 5% level. Coefficients with * are significant at the 10% level. $N = 37\,319$.

doctor visits as a person in good health. Interestingly, engaging actively in sports increases the number of visits once we control for general health.

The full differences-in-differences estimates based on Sample A are shown in Table 2. The purpose of these comparisons is to see whether the reform effect can be causally attributed to the increased co-payments (rather than other aspects of the reforms, or third aggregate influences unrelated to the 1997 reform). The strategy is to compute the change in expected visits before and after the reform separately for a treatment group and a control group. Since the data are not from a real experiment, the assignment to treatment and control has to be based on the evidences available in the survey. Three comparisons are considered. All of them are based on count data regressions with the full set of other control variables included. Specifically, I compare

- the pre–post change for privately insured persons (control) versus non-exempt statutory insured (treatment),
- the pre–post change for youth aged 16–18 (control, since exempted from co-payment) versus those aged 19 and above who are not otherwise exempt,
- the pre–post change for poor individuals (control, since presumably exempted from co-payment) versus others (excluding the privately insured and the youth).

The division into treatment and control is admittedly imperfect. Most problematic is the grouping based on welfare and low income status. For example, income is likely subject to considerable reporting error. Moreover, a fraction of all persons identified as belonging to the treatment may be exempt as well, because they have reached their maximum cumulative contribution by past co-payments during the year. Arguably, the first two comparisons are less error prone. In addition, Table 2 also shows estimates from an additional model, where all the three exempt groups are combined into a single control group. In this last comparison, I have 3547 observations as controls and 33 772 observations for the treatment.

The reform effect is labelled here as 'differences in differences'. It is negative in all cases, and also statistically significant except in the panel negbin estimates with poor persons as control group. The combined effect, based on the panel negbin model, is a 13% reduction in the expected number of visits of the treatment group relative to the control group. I also show in the table the two components of the differences-in-differences estimates, namely the pre–post change for the control group and the pre–post change of the treatment group. In the combined sample, and again based on the panel negbin model, the expected number of visits for the control group increased by 3%, although this change is insignificant. The expected number of visits of the treatment group decreased by 10%, so that the overall reform effect is the aforementioned reduction by 13%. This reasoning assumes that in the absence of the reform, the change in the treatment group would have been equal to the observed change in the control group.

In Table 3, I check how robust these results are with respect to the definition of the sample.

Table 2. Differences-in-differences estimates for the reform effect, Sample A

Change pre–post	Poisson model		Panel negbin	
	Effect	Standard error	Effect	Standard error
Private insurance				
Control group (yes)	0.031	(0.065)	0.044	(0.029)
Treatment group (no)	−0.098**	(0.013)	−0.090**	(0.006)
Difference in difference	−0.130*	(0.067)	−0.134**	(0.030)
Youth				
Control group (age 16–18)	0.117	(0.102)	0.118	(0.073)
Treatment group (age 19–)	−0.099**	(0.013)	−0.088**	(0.006)
Difference in difference	−0.216**	(0.103)	−0.207**	(0.074)
Poor				
Control group (yes)	0.125	(0.114)	−0.019	(0.053)
Treatment group (no)	−0.112**	(0.014)	−0.102**	(0.007)
Difference in difference	−0.238**	(0.115)	−0.082	(0.053)
Combined[a]				
Control group (yes)	0.056	(0.052)	0.027	(0.025)
Treatment group (no)	−0.112**	(0.014)	−0.102**	(0.007)
Difference in difference	−0.168**	(0.055)	−0.129**	(0.027)

Note: These estimates are based on count data regression models with the same controls as in Table 1. Coefficients with * are significant at the 10% level. Coefficients with ** are significant at the 5% level.
[a]Combines the three exempt sub-populations (privately insured, young, poor) into a single control group. Maximum number of observations: 37319.

Sample B uses only 1 year of data before and after the reform each, 1996 and 1998. Clearly, this leads to a reduced precision of the parameter estimates, and a number of effects that were significant in Sample A are now no longer statistically significant. However, based on either the youth comparison or the combined effect, and considering the preferred panel negbin estimates, the main result stays the same, namely that the expected number of doctor visits dropped between 1996 and 1998 for the treatment group relative to the control group, and that this effect is statistically significant. The point estimate is slightly smaller in this case, yielding a 7% reduction in the expected number of visits that can be causally linked to the reform.

Discussion

To summarize, the evidence strongly suggests a causal effect of the reform, in the sense that the control group displayed no reduction or even an increase in the demand for doctor visits between the pre- and post-reform periods, whereas the treatment group did reduce its demand by large amounts. The point estimates depend somewhat on the sample definition, ranging from 7 to 13% in the negbin models with combined control groups. The results therefore confirm the previous findings based on simple differences, where a reform effect of 9–10% was found – about the average between the overall effects in Samples A and B.

Is the effect uncovered by this analysis really causal? The identification is through differences in variation over time between the affected and non-affected persons. By comparing treatment and control, we allow for an unspecified time trend, beyond the individual socio-economic characteristics controlled for in the regression. We require, however, that this unspecified time trend is the same for the two groups. This seems to be a reasonable assumption, as it is hard to imagine any other systematic factor responsible for changes in the relative demand for doctor visits between 1996 and 1998.

In particular, the differences-in-differences set-up also addresses another concern, namely that

Table 3. Differences-in-differences estimates for the reform effect, Sample B

Change pre–post	Poisson model		Panel negbin	
	Effect	Standard error	Effect	Standard error
Private insurance				
Control group (yes)	0.008	(0.094)	−0.008	(0.045)
Treatment group (no)	−0.084**	(0.016)	−0.073**	(0.008)
Difference in difference	−0.092	(0.095)	−0.065	(0.046)
Youth				
Control group (age 16–18)	0.122	(0.138)	0.099	(0.089)
Treatment group (age 19–)	−0.085**	(0.016)	−0.071**	(0.008)
Difference in difference	−0.208	(0.139)	−0.170*	(0.089)
Poor				
Control group (yes)	−0.040	(0.136)	−0.119	(0.079)
Treatment group (no)	−0.101**	(0.019)	−0.081**	(0.010)
Difference in difference	−0.061	(0.138)	0.037	(0.080)
Combined[a]				
Control group (yes)	0.016	(0.073)	−0.009	(0.041)
Treatment group (no)	−0.101**	(0.019)	−0.083**	(0.010)
Difference in difference	−0.118	(0.076)	−0.073*	(0.042)

Note: Coefficients with * are significant at the 10% level. Coefficients with ** are significant at the 5% level.
[a]Combines the three exempt sub-populations into a single control group. Maximum number of observations: 18 683.

even if the health care reform of 1997 was instrumental for reducing the subsequent number of visits, it is still an additional step to attribute this effect to the increased co-payments. The reforms consisted of a bundle of measures, the demand side policy of increased co-payments being one of them. It was already argued elsewhere that these other measures were unlikely to have produced the patterns found in data [Winkelmann R. Health care reform and the number of doctor visits – an econometric analysis. *J Appl Econ* 2003, in press.]. The results presented here provide an additional support for this view. Since these additional changes (such as changed budgeting procedures for doctor's offices) affected treatment and control equally, their effect, if any, should have cancelled out and left the overall results unaffected.

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Appendix: Variable definitions, means and standard deviations (Sample A)

Doctor consultations is the self-reported number of visits to a doctor during the 3 months prior to the interview; (2.80; 4.53)

Male is an indicator variable taking the value of one if the individual is male; (0.487)

Education is educational levels, measured in years of schooling; (11.19; 2.48)

Married is an indicator variable taking the value of one if the individual is married; (0.635)

Household size is the number of persons living in the household; (2.96; 1.37)

Active sport is an indicator variable taking the value of one if the individual participates in sports at least once a week; (0.264)

Good health is an indicator variable taking the value of one if the individual classifies its own health as either 'very good' or 'good'; (0.525)

Bad health is an indicator variable taking the value of one if the individual classifies its own health as either 'very bad' or 'bad' ('fair' is the omitted reference category); (0.165)

Fulltime employed is an indicator variable taking the value of one if the individual is in fulltime employment at the time of the interview; (0.448)

Parttime employed is an indicator variable taking the value of one if the individual is in parttime employment at the time of the interview; (0.086)

Unemployed is an indicator variable taking the value of one if the individual is unemployed at the time of the interview; (0.065)

Non-participation is the omitted employment status category

Log(*income*) is the logarithmic household equivalent income, where the OECD scale has been applied (weight of one for the first person, 0.7 for the second person, and 0.5 for each additional person). The income is expressed in 1995 values using the CPI deflator published in [9]; (7.53; 0.45).

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