The Demand for Physician Services. Evidence from a Natural Experiment[¤]

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Abstract

This study exploits a natural experiment in Belgium to estimate the e¤ect of co-payment increases on the demand for physician services. It shows how a di¤erences-in-di¤erences estimator of the price e¤ects can be decomposed into e¤ects induced by the common average proportional price increase (income e¤ects) and by the change in relative prices (substitution e¤ects). The price elasticity of a uniform proportional price increase is relatively small (-.13 for men and -.03 for women). Substitution e¤ects are large, especially for women, but imprecisely estimated. Despite the substantial price increases, the e¢ciency gain of the reform, if any, is modest.

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1 Introduction

On the 1st January, 1994 the health authorities in Belgium increased the co-payment rates of three types of physician services in Belgium: o¢ce visits to general practitioners (GPs), GP home visits and specialist visits. In this study we analyse the impact of this measure on the demand for these outpatient health care services. We decompose this impact into the e¤ect of a common average proportional price increase (an income e¤ect) and into the e¤ects induced by changing relative prices (substitution e¤ects). On the basis of these measured impact e¤ects, we calculate the (gross) social bene…t of this policy and its determinants.

In the literature, numerous studies have estimated the price elasticity of the demand for health services.[1] However, unsatisfactory treatment of methodological problems results often in unreliable estimates (see Newhouse et al. (1980) for a review of these problems). In order to cope with these di¢culties the federal authorities of United States initiated a social experiment in the seventies: the Rand experiment (Manning et al., 1987, Newhouse et al., 1993). The results of the experiment con…rm that an increase in the individual's cost sharing (from 0% to 95% for instance) implies a reduction in the average health care expenditures (of 46% in the example), in the probability of any medical use (27:5%) and in the unconditional probability of inpatient use (33; 8%). The corresponding price elasticity of the demand for health care services is not very large, ranging from ¡0:2 to ¡0:1, but signi…cantly di¤erent from zero. The price elasticity of the demand for ambulatory care is higher, ranging from ¡0:17 for co-payments between 0% and 25% to ¡:31 for higher co-payment rates.

More recently, some studies have attempted to estimate the price sensitivity of the demand for medical services in Europe. Nolan (1993) studies the Irish health care system. Using data from a national household survey carried out throughout Ireland in 1987, the author estimates that the individuals bene…ting from free access to primary health care services are more likely to consume outpatient and inpatient services and, conditional on positive consumption, report a signi…cantly higher number of medical visits. This study faces, however, an endogeneity problem common to cross section studies. Rather than being a consequence of a change in behaviour induced by a higher coverage (a lower co-payment rate), the increased consumption could result from a positive correlation between coverage and a higher (unobserved) propensity to consume, such as predicted in an insurance market with adverse selection (Chiappori et al. 1998, Newhouse et al., 1980).

Chiappori et al. (1998) exploit a natural experiment[2] to estimate the e¤ect of cost sharing on the demand for physician services in France. In France employees can buy additional insurance on the private market in order to bring the cost sharing rate down to 0%. However, on the …rst of July 1993, a point in time when the government's health insurance became less generous, some, but not all, private insurance companies decided

[1] See Zweifel and Manning (2000) for a recent survey.

[2] See Meyer (1995), Besley and Case (2000) and Rosenzweig and Wolpin (2000) for a general discussion of the validity of the inferences drawn on natural experiments.

to increase the co-payment rate from 0% to 10%. The study estimates the e¤ect of cost sharing on the demand by comparing a group of employees for whom the co-payment increased to a group for whom it did not[3]. On the basis of a panel probit model, contrasting the control and treatment groups, the study …nds that the participation rate in GP home visits is signi…cantly a¤ected by the co-payment level. No signi…cant e¤ect, however, is found for GP o¢ce visits.

Our study relies on a similar natural experiment. On the 1st of January 1994, the co-payment rates for physician services of the mandatory public health insurance scheme in Belgium increased substantially for all but one category of individuals: in real terms, the rate increased by 48% for GP o¢ce visits, by 35% for GP home visits and by 60% for specialist visits. As in the French study, we can therefore contrast the impact of the change on the treatment group to a control group[4]. Since, for reasons of con…dentiality, we have only access to grouped data, a simple linear di¤erences-in-di¤erences (DD) estimator implements such a contrast.

In independent work Van de Voorde et al. (2001) apply such DD estimators on similar grouped data for Belgium.[5] We argue, however, that the price elasticities deduced from this study are incorrect, since they implicitly ignore the substitution e¤ects induced by the relative price variations of these physician services.[6] In this paper we explicitly allow for substitution e¤ects within the group of three physician services considered. To this purpose we estimate a system of demand equations as derived from the classic theory of consumer demand and the principles of two-stage budgeting (Deaton and Muellbauer, 1980a, Barten and Bohm, 1982).

A second objective of this paper is to evaluate the (gross) e¢ciency gain of this policy reform. The e¢cient co-payment rate in health insurance trades-o¤ the e¢ciency gains from risk sharing and the e¢ciency costs induced by moral hazard (see Arrow, 1963; Pauly, 1968; Zeckhauser, 1970). In the US substantial research e¤orts have been undertaken to determine this e¢cient rate. Until recently, this research concluded that the cost of moral hazard is large relative to bene…ts and that higher co-payment rates were warranted (Feldstein 1973; Feldstein and Friedman, 1977; Feldman and Dowd, 1991; Manning and Marquis, 1996). However, the analysis in this research was based on a partial equilibrium framework. Such a framework ignores that (uncompensated) price e¤ects decompose in income and substitution e¤ects and that only the latter a¤ect the e¢ciency of an insurance scheme. In a recent paper, Nyman (1999) argues that income e¤ects are important and their neglect in previous studies have led to prescribe cost

[3] Besley and Case, (2000, p. 674) question whether these groups are comparable: the private companies that increased the copayment rate might have done so, because they needed worry more about over-consumption. If so, the e¤ect on demand is under-estimated.

[4] Note that there is no complementary private insurance for ambulatory physician services in Belgium. We may therefore be con…dent that the reduction of coverage is not undone by increased coverage of private insurance companies.

[5] In earlier work for Belgium, Van Doorslaer (1984) estimated the price elasticity of the demand for prescription drugs and Carrin and Van Daal (1991) for dental care. More recently, Adriaensen and De Graeve (2000) studied the demand for GP and specialist services. However, this study is based on cross-section data and does not control for the selection bias induced by unobservables.

[6] There are additional reasons why our …ndings deviate from theirs (see Section 4.4 below).

sharing rates in the US at too high levels.[7] In addition, in a theoretical contribution, Besley (1988)[8] demonstrates that not only the own compensated price effect, but also cross-price effects matter in the design of the optimal cost sharing rule. These are generally not allowed for in the empirical literature.[9] Nevertheless, one type of interaction effect among different health services has retained interest. It has been argued that, by reducing coverage of outpatient services, preventive care might be deterred thereby inducing more expenditure on inpatient (curative) services. However, Manning et al.'s (1987, p.271) report that the findings of the Rand experiment suggest, if anything, that outpatient and inpatient services are complements, not substitutes.

In this study we do not estimate the net, but the gross efficiency gain of the price reform. For, data limitations do not allow evaluating the cost of increased risk induced by the lower insurance coverage. Moreover, for the same reason, even if we decompose the price effect into income and substitution effects within the group of the three higher mentioned physician services, we can account neither for the income effect of the increased co-payments on total expenditures nor for substitution effects between the three physician services and other (health) goods and services. We can therefore only calculate an upper bound for the gross efficiency gain.

The paper is organised as follows. The following section describes the Belgian health care system. In Section 3, we describe the data. In Section 4 we present the standard DD estimator and propose an alternative one that can be decomposed in income and substitution effects. In Section 5 we formulate the Rotterdam demand system (Barten, 1966 and Theil, 1965) assuming two-stage budgeting. We show how the alternative DD can be decomposed. On the basis of the estimation results, we calculate the gross proportional efficiency gain of the increase in the co-payment rates and decompose this gain into its determinants. A final section concludes.

2 The Belgian Health Care System[10]

In Belgium, almost all individuals are covered by a (semi-)public insurance system. All workers with a professional activity must contribute to the scheme. Premiums are set proportionally to earnings. Competition is introduced in that individuals are offered a

[7] See Blomqvist (2001) for a critical discussion of this view.

[8] Besley (1988) argues that the cost of a health insurance scheme induced by moral hazard should not only be traded-off against the efficiency gains in terms of risk sharing, but also against the enhanced efficiency of a redistributive policy in a second best world in which optimal lump-sum taxes and transfers do not exist.

[9] Davis and Russell (1972) and Newhouse and Phelps (1976) are notable exceptions of studies that take account of the interdependencies between the demands for different physician services. They study the interrelationship between the demand for hospital days and the demand for physician services. Newhouse et al. (1980) argue, however, that the insurance variable in the first study is misspecified and that their results are therefore suspect.

[10] In this section, we only give some information that we consider necessary for the understanding of our analysis. For a larger description of the Belgian health care system, we refer our readers to Hurst (1992) and Crainich and Closon (1998).

choice between a number of non-pro…t sickness funds[11] administrating the reimbursement of health expenditures and o¤ering a number of additional services entailing product di¤erentiation. In return for the premium, patients are partially or totally reimbursed for the cost of their health care expenditures. Patients can buy additional insurance on the private market to cover the share of health expenditures not covered by the public system. However, this relates only to hospital services and to some speci…c services, such as the transportation of patients. Additional insurance cannot be bought for physician services on which we focus in this paper.

In Belgium, the choice of physician, GP or specialist, is free. Specialists can be consulted directly without GP referral. Both the fee due for health care services and the patient's co-payment are …xed jointly in negotiations involving the government, representatives of the sickness funds and of the physicians. The latter are not obliged to apply the fee agreed upon in negotiations, but the large majority of physicians do apply it. In practice, this implies that the price change recorded in January 1994 can be expected to have a¤ected most of the patients.

The co-payment rates …xed in the agreements between the above-mentioned parties di¤er across patients depending on their 'social category'. The …rst social category consists of individuals that by their (past[12]) professional activity have contributed to the Social Insurance system. This group of individuals and their dependants (ascendants, descendants or spouse), the so-called 'titulaires indemnisables' (tip), bene…t from the standard conditions o¤ered by the public health insurance. In 1995 The second social category consists of the widowed, disabled, retired or orphaned individuals (vipo) without any (past) professional activity This group is exempted from contributions. It is further divided up according to the income level of the household. The 'vipos' with an income below a certain threshold acquire a preferential status and are called 'vipos préférentiels' (vipo pref). This group bene…ts from a reduced co-payment rate for health expenditures. The other group, the 'vipos non préférentiels' (vipo nopr), are imposed the same conditions as the tip.

These social categories are further classi…ed into three schemes: the 'general scheme', the 'self-employed' and the 'special schemes'. The …rst scheme groups most bene…ciaries from the public health insurance system whereas the 'special schemes' only concern individuals in speci…c professions, such as miners and sailors from the merchant navy. As to the 'self-employed', the compulsory social insurance only covers 'large risks', such as hospital services. These workers can voluntarily decide whether they buy insurance against 'small risks'.

[11] The insurance market is dominated in Belgium by …ve sickness funds, with the Christian and Socialist sickness funds grouping in 1995, respectively, 45% and 27% of the a¢ liated (Janssens, 1998, p. 39). The sickness funds are decentralised into 'federations' that group local entities.

[12] For unemployed workers entitled to Unemployment Insurance bene…ts.

3 The Data

The analysis relies on administrative data originating from a Belgian sickness fund (the 'Mutualités Chrétiennes'). They contain only data on individuals entitled to health insurance within the 'general scheme'. For reasons of confidentiality, the sickness fund did not authorise access to individual data. We thus acquired grouped data on the average[13] number of physician visits of each type for the years 1993 and 1994 for two federations of the sickness fund, 'Liège' and 'Gent'[14]. For each year and for each federation the entitled individuals are grouped according to each combination of the following personal characteristics: the sex, the age (age < 30, $30 \leq$ age < 50 and age ≥ 50), the type of household (with or without dependants), the social category (tip, vipo nopr and vipo pref) and the gross annual professional earnings (E) expressed in euro ($E = 0$, $0 < E \leq 12{,}500$, $12{,}500 < E \leq 25{,}000$, $E > 25{.}000$). Table 1 reports descriptive statistics of these data.

Table 1 also contains figures on the average number of visits to or by the physician for two sub-samples: the 'treated group/treated' and the 'control group/controls'. The former group comprises the social categories tip and vipo nopr. It is this group that has been imposed substantial increases in the co-payment rates for physician visits on the 1st of January 1994:[15] in real terms this amounted to +48% for GP office visits, +35% for GP home visits and +60% for specialist visits (see Table 2). The latter group consists in the social category vipo pref. We refer to it as the 'control group', since the co-payment rates for physician visits of this group were not modified during the 1993-94 period. Observe, however, that even if the rates did not change, this group had to a pay slightly different price for these physician services in 1994: slightly more in real terms for GP office visits, but slightly less for the other two physician services. This is because physicians simultaneously, in January 1994, negotiated an increase of their fees, slightly higher than the increase of the Consumer Price Index (CPI) for GP office visits, but slightly lower for the two other physician services. This variation in relative prices will prove to be important for purposes of identification discussed below.

The descriptive statistics differ slightly between 1993 and 1994. This is because the number of affiliated is not constant in each federation: people may move, decide to change affiliation to another sickness fund or they may die. Even if Gent has a

[13] This average is calculated by dividing the number of visits by the number of entitled individuals in a group. One entitled individual may refer to several individuals, since the consumption of dependants (e.g. spouse or children) cannot be distinguished.

[14] Liège is the largest city in Wallonia, the region in the south of Belgium in which French is spoken. Gent is the second largest city in Flanders, the Northern region in Belgium in which Flemish (Dutch) is spoken.

[15] In order to make the copayment increases socially acceptable, two types of income related stop-loss arrangements (the 'social deductible' and the 'fiscal deductible') were simultaneously adopted. These impose an upper limit on the total charge of copayments to be supported by the patients. Our data do not allow to identify individuals (if any) who attained this upper limit and for whom therefore the degree of cost-sharing drops to zero. This could bias our estimator. However, those individuals represent only a marginal fraction of all patients. Moreover, few patients were aware of this mechanism at the time of its introduction, since information transmission was poor in this initial period and the excess expenditures are only reimbursed ex post.

smaller population than Liège, the Christian sickness fund seems to count more affiliates than in the Flemish than in the Walloon city. This corresponds to a general pattern in Belgium. The Christian sickness fund is traditionally more dominant in Flanders and the Socialist sickness fund is more important in Wallonia. Within the sample, the share of the youngest age group (age < 30) is larger than the two other age groups ($30 \leq$ age < 50 and age ≥ 50) which are roughly equally represented. The older age groups contain proportionally less entitled individuals, because dependent spouses, more numerous within these age groups, are not recorded as separate individuals. Men are generally identified as the head of the household, explaining the higher fraction of men in charge of dependants. The earnings are only reported for individuals belonging to the social category 'tip'. Finally, observe that the vast majority are 'tip' and that the control group ('vipo pref') represents only a small fraction of the total sample.

The average number of visits to or by the physician according to the treatment status are the variables of interest in this study. For men, this number is larger for all three types of visits if one is a member of the control group. This is most pronounced for visits of general practitioner (GP) at home: this number is nearly five times as large as the corresponding figure for the treatment group. For women, the control group visits GP and specialists less at the office, but this is more than compensated by the increase of GP home visits. The GP visits these women nearly six times more often at their home than women in the treatment group.

One could argue that this different pattern in the demand for physician services is induced by the significantly lower co-payment rates to be paid by the control group (see Table 2). Moreover, the relatively low price differential between GP home and office visits is likely to more than outweigh the differential time costs between these services for the majority of the patients, especially for those belonging to the control group. The latter would explain the pronouncedly higher average number of GP home visits for controls. However, this different pattern could also reflect a differential in the structural health conditions or in the preferences between the two groups.

By exploiting the differential price variation between 1993 and 1994 and by (reasonably) assuming that both the structural health conditions and preferences are constant over time, we can disentangle both explanations. For, any difference in the time evolution of the number of visits between the two groups cannot be explained by time-constant factors. Moreover, in order to eliminate the effect of time factors, such as a flew epidemic, affecting the consumption pattern of both groups proportionally, we subtract the time evolution of the control group from the one of the treatment group. The remaining differential estimates, for the treatment group, the effect of the increase in the co-payment rate between 1993 and 1994 on the demand for physician services. This is the differences-in-differences (DD) estimator of the price effect discussed in the following section.

4 Differences-in-Differences (DD) Estimators

We could calculate the DD on the basis of the average evolutions of physician visits of the treatment and the control group as a whole. However, this does not exploit all available information and is therefore not efficient. We can calculate, for each type of physician visit, many such DD estimators, since we can distinguish sub-groups within these control and treatment groups. By crossing the indicator variables reported in Table 2 one can deduce that $M_0 = 10$ of such sub-groups can be formed for the control group and $M_1 = 58$ for the treatment group. In this sub-section we show how these 68 sub-groups can be combined to form the Efficient DD estimator. Subsequently, we propose an alternative specification for the dependent variable of the DD estimator. We do so, since we show in Section 5 that such a specification allows a decomposition of the DD point estimates income and substitution effects if the Rotterdam system describes the demand for physician services. We denote this specification by the 'Rotterdam DD estimator'. In a third subsection, we argue that higher earnings groups behave significantly differently from other ones and that this justifies restricting the DD estimator to the lower earnings groups only. Moreover, heterogeneous behaviour justifies the introduction of interaction effects. Finally, we compare our estimation strategy and results to those of Van de Voorde et al. (2001) on similar data for Belgium.

4.1 The Efficient DD Estimator

We introduce the following notation: $i = 1$ for GP office visits, $i = 2$ for GP home visits and $i = 3$ for specialist visits; $d = 0$ for the controls and $d = 1$ for the treated; $m = 1; 2; \ldots M_d$ for each sub-group belonging to treatment group d; $t = 0$ for the year 1993 and $t = 1$ for the year 1994. The average demand for a visit of type i for individuals belonging to sub-group m and treatment group d in the year t is denoted as q_{imdt} and its growth rate by $\Delta \ln q_{imd}$. Without loss of generality, this growth rate can be specified in the following way:

$$\Delta \ln q_{imd} = \alpha_i^0 + \beta_i^0 d + \omega_{imd}^0 \quad (1)$$

where α_i^0 and β_i^0 are unknown parameters. If we assume that ω_{imd}^0 is a random unobserved group specific effect that is uncorrelated with the treatment status d; such that $E\left[\omega_{imd}^0 \mid i; m; d\right] = 0$, then β_i^0 is the expected value of the DD estimator:[16]

$$\beta_i^0 = E\left(\Delta \ln q_{im1} - \Delta \ln q_{im0}\right) \quad (2)$$

[16] If the effect differs among members of the treatment group, the DD estimator identifies the Latent Average Treatment Effect (LATE). This is the average effect of those individuals in the treatment group who are induced to change their demand for physician services following the price change (Imbens and Angrist, 1994). In the remainder of this paper we either assume a constant treatment effect or a treatment effect that varies parametrically with the size of the budget share attributed to the service (see the 'Rotterdam DD estimator' below).

With this assumption (1) defines a regression equation for which Ordinary Least Squares (OLS) yields an unbiased estimate of β^0_i. However, since our sample is finite, we do not observe $\Delta \ln q_{imd}$, but only an estimate, namely $\Delta \ln \hat{q}_{imd}$, where

$$\hat{q}_{imdt} = \frac{\sum_{n=1}^{N_{mdt}} q_{imdt}(n)}{N_{mdt}}; \tag{3}$$

$\hat{q}_{imdt}(n)$ is the realisation of the random number of visits of type i in the year t demanded by individual n belonging to group $(m; d)$ and N_{mdt} is the number of individuals in the sample belonging to group $(m; d)$ in the year t. If we replace $\Delta \ln q_{imd}$ by $\Delta \ln \hat{q}_{imd}$, the relation (1) is no longer exact. This suggests estimation by the Minimum Chi-Square method (Berkson, 1944; Amemiya, 1981; more recently, Cockx, 1997; Cockx and Ridder, 2001). For, expanding $\Delta \ln \hat{q}_{imd}$ in a Taylor expansion around $(q_{imd0}; q_{imd1})$ yields

$$\hat{y}^0_{imd} \equiv \Delta \ln \hat{q}_{imd} = \alpha^0_i + \beta^0_i d + \omega^0_{imd} + \xi^0_{imd} \tag{4}$$

in which ξ^0_{imd} represents an approximation error. Generalised Least Squares (GLS) yields an asymptotically efficient estimator of the parameters in regression equation (4) and, as such, the Efficient DD estimator, $\hat{\beta}^0_i$. In Appendix 1 we explain how one can find an estimate the variance-covariance matrix of the residual terms to construct a feasible GLS estimator.

The weighted sum of squared residuals (WSSR) can be used as a goodness-of-fit test statistic, testing whether the estimated model is to be rejected against the saturated model. It is distributed χ^2 with $M_0 + M_1 - P$ degrees of freedom (DF), where P is the number of estimated parameters (see e.g. Amemiya, 1981).

The DD estimates are reported in column 0 of Tables 3a and 3b, respectively for men and women. The goodness-of-fit test statistic indicates that the estimated model cannot be rejected against the saturated: the P-value is 66% for men and 87% for women. According to these estimates, the price increase affected the demand for all three types of physician services negatively. However, for men, the effect is insignificant (at the 5% level) for GP office visits and, for women, it's insignificant for specialist visits. Observe also that the effect on the demand for GP home visits is the largest, even if the proportional price increase was the smallest (see Table 2). This suggests higher price sensitivity for home visits.

4.2 The Rotterdam DD Estimator

Below we decompose the DD estimator in income and substitution effects. To this purpose, we will specify the Rotterdam demand system. In this specification the dependent variable does not correspond to the one defined in regression equation (4) above, $\hat{y}^0_{imd}$. We therefore propose a DD estimator defined with respect to this alternative dependent variable. In fact the dependent variable in the Rotterdam model pre-multiplies $\hat{y}^0_{imd}$ by a moving average of the budget share spent on service i, $\hat{w}_{imd}$.

Introduce the following notation. If p_{idt} denotes the price of this service for an individual belonging to treatment group d at time t, then for an individual belonging to group (m, d) in year t the total budget available for buying physician services is on average given by

$$x_{mdt} = \sum_{i=1}^{8} p_{idt} q_{imdt} \tag{5}$$

and the budget share spent on service i is defined as

$$w_{imdt} = \frac{p_{idt} q_{imdt}}{x_{mdt}}. \tag{6}$$

The moving average of the budget share is defined between $t = 0$ and $t = 1$, such that

$$\bar{w}_{imd} \equiv (w_{imd0} + w_{imd1})/2 \tag{7}$$

Replacing population averages by their estimates, this yields the following alternative to regression equation (4):

$$\hat{y}^1_{imd} \equiv \hat{\bar{w}}_{imd} \cdot \ln \hat{q}_{imd} = \alpha^1_i + \beta^1_i d + \omega^1_{imd} + \upsilon^1_{imd} \tag{8}$$

in which β^1_i is the expected value of the 'Rotterdam DD estimator'. Again the parameters of this regression equation can be estimated efficiently by GLS (see Appendix 2 for details).

In column 1 of Tables 3a and 3b the DD estimates are reported for men and women respectively. To facilitate comparison with the previous DD estimates reported in column 0, we also report the parameter estimates divided by the average budget share among the treated. From these we can conclude that the DD estimators are not very sensitive to the model specification: the point estimates of the two models lie in each others 95% confidence intervals. Observe, however, contrary to specification 0, the price reform now seems to have reduced the demand for GP office visits for men significantly. On the basis of the χ^2 goodness-of-fit test statistic neither model can be rejected. For women the Rotterdam DD model fits best (a P-value of 97% versus 87%), but for men the initial DD regression model (4) performs slightly better (a P-value of 66% versus 64%). In the sequel, we only retain the Rotterdam DD model, since only this model allows the announced decomposition of the price effects.

4.3 The Rotterdam DD Estimator Accounting for Heterogeneous Behaviour

Up to now we assumed that the behaviour both within and between treatment groups is homogeneous. In this sub-section we depart from this assumption by allowing both, the intercept and the slope of regression equation (8), to interact with the discrete explanatory variables described in Table 2. We start off with a model in which all first-level interaction effects are allowed for. Such a model contains, apart from the coefficients for the reference group, four interaction effects for the intercept (federation (Gent), age $30 - 50$, age > 50, household type (with dependants)) and eight for the slope (the previous four plus the social status (tip) and the earning levels in euro: $0 < E \leq 12{,}500$;

$12,500 \leq E <= 25,000$; $E > 25,000$). This model (not reported) is estimated to test whether interaction effects can be ignored.

We proceed in two steps. In a first step we test whether the slope interaction effects can be set to zero at a P-level of 5%. Since substitution effects relate the three physician services (see Section 5), we only retain zero interactions to the extent that these cannot be rejected for all three services jointly. The difference between the WSSR of the restricted and the unrestricted model is distributed χ^2 with as many degrees of freedom as the number of restricted parameters. In this first step, we cannot reject a model in which all slope interactions but three (times three for each service) are set to zero. For men, the remaining interactions consist of the two highest earnings classes and the federation; for women, these coincide with the three earnings classes referring to strictly positive earnings.

Recall that the control group, by construction, does not contain any individuals with strictly positive reported earnings. The significant interaction effects for the groups with positive earnings suggest therefore that the behaviour of these individuals cannot be compared to those belonging to the control group. To avoid bias induced by this non-comparability, we therefore exclude the higher earnings groups from any further analysis. For women, this involves all groups with strictly positive earnings; for men, only the two highest earnings classes are eliminated. This reduces the number of cells available for analysis considerably: from 204 to 132 and 96, respectively for men and women. It is on this restricted sample that we apply our second step of the testing procedure.

In this second step, we estimate again the model with all first level interaction effects. Subsequently, we test whether we can constrain this model by setting intercept and slope coefficients to zero according to the above-mentioned rule. As such, we impose all but two (for each physician service) interaction effects to zero. All slope interactions can be ignored, implying that, within this restricted sample, all groups reacted similarly to the price reform. Intercept interactions indicate that expenditures on physician services would have evolved differently between groups even in the absence of a price reform. These reveal (not reported) that expenditures growth of all three physician services for men and women in charge of dependants was lower than for other groups. Similarly, this growth was below the reference for men aged between 30 and 50 and for women affiliated to the federation of Gent (rather than Liège). This model, reported in column 1^* of Tables 5a and 5b, respectively for men and women, could not be rejected against the full interaction model at a P-value of 6% for men and 26% for women. Any further restriction is rejected at the 5% level.

It is striking that the DD point estimates are much smaller in absolute value as compared to models 0 and 1. Moreover, whereas in model 1 only specialist visits by women were not significantly (at a 5% level) affected by the price reform, now only the coefficients of GP home visits for men and of GP office visits for women are significant.

We can test whether the parameters of interest, i.e. β_i^1 $(i = 1, 2, 3)$ of model 1 and 1^* are significantly different. For, under the null hypothesis of equal coefficients, model 1 is consistent and more efficient than model 1^*; but under the alternative hypothesis it yields an inconsistent estimator. This suggests comparison on the basis of a Hausman test (Hausman, 1978).[17] On this basis we reject the null hypothesis of equality with a P-value of 0:06% for men ($\chi^2(3) = 17:3$) and of 0:25% for women ($\chi^2(3) = 14:3$).

We conclude that our findings resemble those reported by Chiappori et al. (1998) for France. The demand for GP home visits is more price elastic than the other two physician services. We estimate that, in spite of a lower proportional price increase, the reform reduced the demand for this service most: as compared to the control group, the demand of the treatment group decreased by 14% for men and by 9% for women. However, for women this estimate is very imprecise and is not maintained when we restrict the Rotterdam DD model to allow for a decomposition in income and substitution effects (see Section 5.2 and 5.3 below). The demand for other physician services is not significantly different from zero, except for GP office visits by women, the demand for which decreased by 7%.

Contrary to Chiappori et al. (1998), we do not believe that the time costs, not accounted for in the price of office visits, can explain the differential response: these are fixed over time and eliminated by differencing. We explain the differential price sensitivity of the demand for physician services in terms of differing income and substitution effects.

4.4 A Comparison with Van de Voorde et al. (2001)

Van de Voorde et al. (2001) also exploit grouped data on physician visits originating from the same sickness fund. Their data set is richer in that they have access to pooled data on a period of 10 years (1986-1995)[18] and this for all regional offices in Belgium. Recall that our data only refer to 1993 and 1994 and to only two regional offices, Liège and Gent. On the other hand, these researchers could only distinguish between three categories of users (tip, vipo nopr and vipo pref) and could therefore neither control for the sex of the user of physcian services nor for any other explanatory variables, as in our study (Section 4.3).

Van de Voorde et al. (2001) estimate both, a DD model and a level model containing a linear time trend as control for time-varying factors other than prices. Their DD estimates of the price elasticity are not significantly different from zero if the control group (vipo pref) is contrasted to one treatment group (vipo nopr), but significantly negative if compared with the other (tip). This is consistent with our findings, since we also found larger treatment effects for the highest earnings groups within the contributing population, tip (Section 4.3). Van de Voorde et al. (2001) conclude, as we do for the higher earning groups, that the control group (vipo pref) is not adequate for the contributing population (tip).[19] The authors argue, however, that the control group is neither adequate for the other treatment group (vipo nopr), since they "are really a very

[17] To ensure that the difference of the variance-covariance matrices between the two models is positive definite, we re-estimate model 1 imposing for the observations retained in model 1^* the estimated variance-covariance matrix of the residuals of the consistent model 1^* and apply the test to this model.

[18] This longer time period is, however, not much more informative, since the copayments hardly changed apart from the 1994 increase.

[19] Note that this does not necessarily imply that the sensitivity of demand for the tip group is higher than for the controls. A larger treatment effect could also reflect a more pronounced autonomous decrease

selective group among the socially weakest" (p. 13). We contest this conclusion, since the homogeneity of the treatment effect for the lower earnings groups including vipo nopr (Section 4.3) suggests that the control group is only inappropriate for the higher earnings groups.

Van de Voorde et al. (2001) have more confidence in the estimates of their level model yielding for the contributing population (tip) significantly negative price elasticities of the same order of magnitude of those found in the Rand experiment in the US. We question the validity of this conclusion, since it crucially depends on the assumption that the time effects can be completely captured by a linear time trend.[20] We rather conclude that we cannot identify the impact of the increase of co-payments for the higher earning groups on the basis of this natural experiment. We can do so for the lower income groups, however.

There is a more fundamental reason why we do not believe in the elasticities reported in the above-mentioned article. The authors implicitly assume that the relative prices of the three physician services were affected in the same proportion, such that the treatment effect of physician service i only depends on the own proportional price change ($\Delta \ln p_{id}$). This assumption is incorrect (see Table 2). Consequently, if substitution effects between these services are non-zero, the parameter estimates do not reflect the price elasticities of demand for these services.[21] The correct price elasticities can only be deduced from the estimated parameters of a demand system capturing the substitution effects induced by the changes in relative prices.

5 The Demand System

We now propose a method to decompose the price effects in income and substitution effects. To that purpose we rely on the classic theory of consumer demand and assume that consumers behave according to the principles of two-stage budgeting. In the first stage the consumer decides upon the budget to allocate to expenditures on physician services. In the second stage the consumer decides which type of physician service he will buy taking the budget allocated in the first stage as given.

The classic theory assumes that the consumer decides in a certain environment. The demand for health care is, however, typically conditioned on the realisation of an uncertain event, i.e. on illness. We provide two justifications for our approach. First, the decision at the second stage does not involve uncertainty. The choice of the type of physician service is only taken after one has become ill and is conditional on a budget determined under uncertainty in the first stage. Since by lack of adequate data we cannot impose much theoretical structure, we can interpret the first stage as a reduced form of a model of demand under uncertainty.

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in demand for the tip group.

[20] The authors are aware of this limitation, since they report it in the main text (p.9).

[21] Equation (22) below reveals why a constant elasticity demand equation depending just on own prices is only justified if substitution effects are zero ($\forall j : \epsilon^*_{ijmd} = 0$) or if the proportional price change is uniform ($\forall j : \Delta \ln p_{jd} = \Delta \ln P_{Gd}$).

Second, following the literature on "physician agency" (McGuire, 2000), one could argue that it is the GP or specialist, rather than the patient, who decides on the quantity and type of medical care services consumed. The physician could then determine, ex ante, for each group of patients, in function of prices and of their personal characteristics, both, the average health expenditures and their allocation among the different services. If these groups coincide approximately with those constructed for the empirical analysis, then the classic allocation model under certainty will apply to these grouped data.

In fact, the by European standards extremely high density of physicians in Belgium is favourable to the second interpretation: In 1995 Belgium had 660 inhabitants per practising GP and 630 per specialist (Van de Voorde et al. 2001, p. 4). Moreover, since the empirical analysis is restricted to two large cities, this density will be even larger.

The outline of this section is as follows. We first briefly recapitulate the structure of the Rotterdam demand system in the second budgeting stage and discuss identification and estimation. Subsequently, we describe the first budgeting stage and explain how the parameters of this stage can be estimated by imposing appropriate restrictions on the 'Rotterdam DD estimator'. In a third sub-section we present the estimation results of the Rotterdam model. Finally, we calculate the gross efficiency gain of the price reform and decompose it in its determinants.

5.1 The Second Budgeting Stage of the Rotterdam Model

We assume that the Rotterdam model is on average a correct description of the behaviour of patients demanding physician services.[22] To eliminate fixed effects we formulate the Rotterdam model in its differential form:

$$w_{imd} d\ln q_{imd} = a_i + b_i \sum_j w_{jmd} d\ln q_{jmd} + \sum_j s_{ij} d\ln p_{jd} + e_{imd} \qquad (9)$$

with

$$\sum_j w_{jmd} d\ln q_{jmd} = d\ln x_{md} - \sum_j w_{jmd} d\ln p_{jd} \qquad (10)$$

$$b_i = w_{imd}\eta_{imd} = p_{id}\frac{\partial q_{imd}}{\partial x_{md}} \qquad (11)$$

$$s_{ij} = w_{imd}\varepsilon^*_{ij} = w_{imd}[\varepsilon_{ijmd} + \eta_{imd} w_{jmd}] \qquad (12)$$

where a_i is the autonomous growth rate of the demand for service i (in deviation from the autonomous growth rate of average expenditures on physician services, a_0, defined in Section 5.2), b_i is the marginal propensity to spend on the ith service, s_{ij} is the $(i;j)$th term of the Slutsky substitution matrix S, and e_i is a random term, allowing deviations from rational behaviour. η_{imd}; ε^*_{ijmd} and ε_{ijmd} are, respectively, the expenditure, the

[22] In a sensitivity analysis we compared the findings resulting from the Rotterdam specification to the CBS model of Keller and van Driel (1985). This model did not fit the data well and is therefore not reported. A discussion of the analysis and results can be obtained from the authors upon request. As to the Almost Ideal Demand system (AID) of Deaton and Muellbauer (1980b), we only estimated the second stage OLS version, because negativity (see (16) below) was violated and cannot be imposed globally on AID.

compensated and the uncompensated price elasticities. From (10) it is clear that the regressor of β_i is an index of proportional change in real total expenditure. Moreover, it can also be regarded as a measure of change in utility, so that the Rotterdam demand equations (9) represent Hicksian demands (Deaton and Muellbauer, 1980a, p.68).

Another advantage of this formulation is that the restrictions imposed by theory of rational choice can be expressed in terms of fixed parameters $(\beta_i; \sigma_{ij})$. This makes it easier to impose these restrictions in estimation. The restrictions are the following:

$$\sum_i \alpha_i = 0; \quad \sum_i \beta_i = 1; \quad \sum_i \sigma_{ij} = 0 \quad \text{(Adding-up)} \tag{13}$$

$$\sum_j \sigma_{ij} = 0 \quad \text{(Homogeneity)} \tag{14}$$

$$\sigma_{ij} = \sigma_{ji} \quad \text{(Symmetry)} \tag{15}$$

$$x' \Sigma x < 0 \quad \text{(Negativity)} \tag{16}$$

To write down an estimable Rotterdam system (9) the differentials are approximated. We follow Barten (1967) and replace the differential $d\ln q_{imd}$ by first differences $\Delta \ln q_{imd}$ and replace the budget shares by a moving average between $t = 0$ and $t = 1$, i.e. by $\overline{w}_{id}$ defined in (7). Finally, we need to replace the average demand for service i by an estimate. The estimated dependent variable is denoted by $\hat{y}^1_{imd}$ and defined by (8) and (3).

Using (9), a Taylor expansion of $\hat{y}^1_{imd}$ and the observed independent variable $\sum_j \hat{y}^1_{jmd}$ around $(q_{1mdt}; q_{2mdt}; q_{3mdt})^1_{t=0}$ yields

$$\hat{y}^1_{imd} = \alpha_i + \beta_i \sum_j \hat{y}^1_{jmd} + \sum_j \sigma_{ij} \Delta \ln p_{jd} + e_{imd} + u_{imd} \tag{17}$$

where u_{imd} is the approximation error.

This demand system cannot be estimated as such. First, the adding-up restrictions (13) imply that the rows of the variance-covariance matrix of the residuals add-up to zero[23]. The variance-covariance matrix is therefore singular. For estimation purposes, one demand equation may therefore be deleted: the parameters of the deleted equation can be derived from the two other ones.

Second, the proportional price variation of each service $(\Delta \ln p_{jd})$ takes on only two values for each of the three physician services: one for the control group ($d = 0$) and one for the treatment group ($d = 1$). Consequently, even if we impose homogeneity ($\sigma_{i3} = -\sigma_{i1} - \sigma_{i2}$) and symmetry ($\sigma_{12} = \sigma_{21}$), the intercepts and the price variables of the demand system are linearly dependent. Intuitively, the system of two demand equations contains only four independent relative price changes ($\Delta \ln p_{jd} - \Delta \ln p_{3d}$; $j = 1; 2$; $d = 0; 1$)[24], two for each service, to identify 5 parameters $(\alpha_1; \alpha_2; \sigma_{11}; \sigma_{12} = \sigma_{21}; \sigma_{22})$. This is formally proved in Appendix 3.

[23] This applies also to the approximation error u_{imd}, because the Taylor expansion is applied to both the dependent and the first independent variable. The latter is multiplied by β_i, so that after summing over i the aproximation error of the independent variable cancels out with that of the dependent variable (see Appendix 3).

[24] By homogeneity, the price variation of one good ($j = 3$) is taken as numeraire.

To resolve this identification problem we propose theoretical restrictions of the following kind: $a_1 = 0$, $a_2 = 0$ or $a_3 = 0$ (i.e. $a_1 = -a_2$ by (13)). Such a restriction implies that the autonomous growth in the demand for service i equals the autonomous growth of the average expenditures on physician services, i.e. a_0 defined in Section 5.2. To avoid that this restriction is completely arbitrary, we choose the one that renders the demand system compatible with the corresponding 'Rotterdam DD estimator'. This procedure is explained in the next sub-section.

In the empirical application the Slutsky substitution matrix S is not negative definite (16) without imposing it to be so. In the estimation we therefore impose negativity by a Cholesky decomposition (Barten and Geyskens, 1975). This requires estimation by a non-linear GLS method. The estimation method is further explained in Appendix 4.

5.2 The First Budgeting Stage of The Rotterdam Model

In the first budgeting stage the consumer (induced by the physician) decides how to allocate his total available budget to different groups of goods and services, among which the budget to spend on physician services.[25] Our dataset neither contains information on the total available budget, nor does it on expenditures on other goods or services. The first budgeting stage can therefore only formulated under very restrictive assumptions. First, we assume that the total budget was either constant or has grown at an average uniform rate over the period of analysis. As such, the income effects are captured by the constant term. Second, we assume that there are no interaction effects with other goods or services.

Under these assumptions the first stage of the demand for physician services can be written in the following way:

$$\sum_{j=1} \hat{y}^1_{jmd} = a_0 + b_0 \Delta \ln P_{Gd} + \sum_{j=1} \left(\omega^1_{jmd} + \nu^1_{jmd} \right) \qquad (18)$$

where a_0 captures the autonomous growth rate of average expenditures on physician services and P_{Gd} is an aggregate price index of physician services defined as:

$$\Delta \ln P_{Gd} = \sum_{j=1} \hat{\theta}_j \Delta \ln p_{jd} \qquad (19)$$

where $\hat{\theta}_j$ is an estimate obtained from the second budgeting stage. To the extent that the two higher mentioned (restrictive) assumptions are satisfied, b_0 can be interpreted as the uncompensated price elasticity of demand.[26] Finally, the last two terms denote, respectively, the unobserved group effects and the approximation errors of the Taylor expansion.

[25] See e.g. Deaton and Muellbauer (1980a, 127-133) for the derivation of the first stage Rotterdam model in differential form.

[26] The compensated price elasticity of demand in this first stage could only be obtained by deflating the evolution of the total budget by an appropriate price index. This index must vary between the treatment and control group and therefore violates the first of our two assumptions.

The above-mentioned two-step GLS estimation procedure could be directly applied to (18). However, we prefer an indirect estimation procedure obtained by restricting the 'Rotterdam DD estimator' discussed in Sections 4.2 and 4.3. As such, we can test whether the identifying assumptions required for the estimation of the second stage model (17) are reasonable or, on the contrary, must be rejected.

Consider the Rotterdam DD regression model defined in (8). Interestingly, if the Rotterdam model is a correct description of the demand for physician services, then the estimated parameters of the second budgeting stage (17) impose testable restrictions on this modified DD regression model. Indeed, inserting the first budgeting stage model (18) in the second stage one (17) corresponds to imposing the following restrictions on the intercepts and slopes of the Rotterdam DD model (8):

$$\alpha_i^1 = \hat{a}_i + \hat{\beta}_i (a_0 + b_0 \Delta \ln P_0) + \sum_{j=1} \hat{s}_{ij} \Delta \ln p_{j0} \tag{20}$$

and

$$\beta_i^1 = \hat{\beta}_i \left[b_0 \left(\Delta \ln P_{G1} - \Delta \ln P_{G0} \right) \right] + \sum_{j=1} \hat{s}_{ij} \left(\Delta \ln p_{j1} - \Delta \ln p_{j0} \right) : \tag{21}$$

Note that these restrictions are consistent, since prices of each physician service vary only with the treatment status. As such, both sides of the two restrictions are constants. For $i = 1; 2; 3$, this reduces the number of parameters in 'homogeneous' Rotterdam DD model (8) from 6 to $6/3 = 2$: only a_0 and b_0 remain unknown parameters. If we use the (consistent) estimate of the variance-covariance matrix of the unrestricted model (8) for the GLS estimation of the restricted model, then, under the null hypothesis, the difference between the WSSR of the restricted and the restricted model is distributed chi-squared with $6 - 2 = 4$ degrees of freedom $\left[\chi^2(4) \right]$. If we allow intercept interactions, as in model 1*, than this test statistic is distributed $\chi^2(8)$.[27] Moreover, if not rejected, the estimation of the restricted model directly yields estimates for the parameters of the first budgeting stage of the Rotterdam model (18). If rejected, this suggests that an inappropriate identifying restriction has been imposed on the second budgeting stage. Among the un-rejected models we choose the model that fits the unrestricted DD model (8) most closely on the basis of the χ^2 goodness-of-fit test statistic.

5.3 The Estimation Results

Since the estimation results reported in Section 4.3 imply heterogeneous behaviour, we eliminate the higher earnings groups from the sample and allow the same intercept interactions, both for the first and second budgeting stage, as imposed on the DD model 1^*. Note, imposing $a_1 = 0$, $a_2 = 0$ and/or $a_3 = 0$ on the intercept of reference group is a sufficient identifying restriction. We need not impose it on the interaction effects.

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[27] The unrestricted DD model counts 4 parameters rather than 2 (+2 interactions) for each of the 3 services ($4 \times 3 = 12$) and the restricted model $12/3 = 4$ ($12 - 4 = 8$).

We subsequently report the estimation results of the first and second budgeting stage in Tables 3a and 3b (column 1^{*r}), and Tables 4a and 4b, for men and women respectively. For men, only for the model on which we impose $a_1 = 0$; the restrictions (20) and (21) are not rejected (P-value = 7.4%). For women, we retain the model with $a_3 = 0$ as identifying restriction (P-value = 6.7%). The DD point estimates corresponding to this restricted model are reported in Table 3a and 3b. Because of the restrictions these are more precise and, although they remain within the common confidence intervals, they deviate from those estimated within the unrestricted model (column 1*). It strikes that the point estimates of the demand for home visits are smaller: -8% for men (in stead of -14%) and close to 0% for women (in stead of -9%). Besides, the demand for specialist visits by men is now significantly reduced (-7% in stead of -3%) by the price reform. Note that the restricted DD model (18) fits the data quite well: it cannot be rejected against the saturated model at a significance level of 39% for men and 19% for women.

Even if the price increase had a significant negative impact on the demand for some physician services, the price elasticity on the demand for physician services as a whole is quite small: the estimate of b_0 in the first budgeting stage model (18) indicates that a 100% price increase of all three physician services ($\Delta \ln P_{Gd} = .01$) decreases expenditures (significantly) by 13% for men and (insignificantly) by 3% for women. This is much lower than the price elasticity of $-.31$ for outpatient services reported for comparable cost sharing rates in the Rand experiment in the US. However, recall that we can only report elasticities for the lower income groups (see Section 4.3 and 4.4). Since in the US the use of outpatient services is reported to be significantly lower for lower income groups (Newhouse et al., 1993, p. 262) our findings are not necessarily conflicting with those of Rand experiment. By contrast, the level model of Van de Voorde et al (2001) reports for the non-contributing population (vipo) on average larger elasticities than those reported in this study. For the DD model, the contrast between vipo pref and vipo nopr yields lowers elasticities. However, these authors did not account for substitution effects induced by the change in relative prices.

The above mentioned elasticities are averages. If we insert (18) in (17) and divide both sides by the budget share, $\hat{w}_{imd}$, we obtain $b_0 \eta_{imd}$, the elasticity of the demand for service i with respect to a uniform proportional price increase for all three physician services. This elasticity is the average price elasticity b_0 multiplied by the income elasticity of demand in the second budgeting stage η_{imd}. Table 4a (4b) reports for men (women) the estimated income elasticity evaluated at the average budget share of the treatment group. The price elasticity is larger than the average for luxury services ($\eta_{imd} > 1$) and smaller for necessities ($\eta_{imd} < 1$).

Home visits are luxuries both for men and women: the income elasticity is respectively 1.38 and 2.24. The corresponding price elasticities are therefore $-.18$ and $-.08$, the largest of all three physician services. In contrast, GP office visits are necessities. The income and price elasticities for men (women) are much smaller, respectively .47 (.32) and $-.06$ ($-.01$). Finally, visits to the specialist are luxuries for men ($\eta_i = 1.11$), but not significantly, and necessities for women ($\eta_i = .55$). The corresponding price elasticities are $-.14$ for men and $-.02$ for women.

The policy reform did not, however, increase the prices of all three physician services in the same proportion. According to theory, changes in relative prices should induce substitution effects. The Slutsky matrix in the Rotterdam model (17) reflect these. Recall, apart from theoretical restrictions ((13), (14) and (15)) necessary for identification, we were also required to impose negativity (16) on all estimations (see Appendix 3 for details).

Observe that the Slutsky matrix is very imprecisely estimated. This is because the natural experiment does not induce sufficient relative price variation. It is difficult to test the null hypothesis of a zero Slutsky matrix, S, because at the frontier of the parameter space the test statistic is no longer distributed χ^2, asymptotically. However, we can test whether the restrictions (20) and (21) are rejected if we impose $S = 0$. For men, this test results in a higher P-value than for the model with the non-zero Slutsky matrix: 11.9% as compared to 7.4%. In contrast, for women the restrictions must be rejected (P-value = 5.0% as compared to 6.7%). This is consistent with the, in absolute value, much larger point estimates of the Slutsky terms for women.

The imprecision of the parameter estimates makes interpretation hazardous. Nevertheless, we attempt to draw some conclusions. The own price elasticities of the demand is much larger for GP office visits ($-.95$ for men and -2.20 for women) than for the two other physician services. The sign of cross price elasticities suggests that GP office visits are Hicksian substitutes of home visits and specialist visits. GP home visits and specialist visits are (weak) complements. The latter might reflect that very ill individuals need both to be treated by a GP at home and by a specialist.

We now recapitulate the findings. To this purpose, we insert equation (18), describing the first budgeting stage of the Rotterdam model, into the second stage model (17). If we divide all terms by the budget share of (m, d), $\hat{w}_{md}$, and neglect the intercept and residual terms, we obtain

$$\Delta \ln \hat{q}^S_{imd} = \eta_{imd} b_0 \Delta \ln P_{Gd} + \sum_j \varepsilon^*_{ijmd} \Delta \ln p_{jd} \qquad (22)$$

The left hand side is the expected proportional effect of the price reform on the demand for service i of group (m, d). In Table 5 we report a weighted average of this effect over all treatments.[28] Note, in contrast to the restricted 'Rotterdam DD' estimates, $\hat{\theta}_i$, reported in column 1^{*r} of Tables 3a and 3b, this effect is not evaluated the effect in deviation from the effect of the price reform on the control group. Since prices were also slightly increased for the control group, the total effects reported in Table 5 deviate slightly from the ones in Tables 3a and 3b.

The right hand side of equation (22) decomposes this total effect in income and substitution effects. This decomposition confirms that substitution effects are less important for men than for women. In fact, for men, the column reporting the income effects is a good predictor of the total effect. By contrast, substitution effects are important for

[28] We replace parameters by their point estimates. The effect for each group m_1 is weighted by each group's fraction of the total number of service units of type i demanded in 1993. In contrast to the estimation, we extrapolate our findings by maintaining the higher earning groups in the calculation.

women. For instance, for GP office visits the negative effect is nearly completely induced by the change in relative prices: the positive substitution effects induced by the price increases of the other two physician services are large (.42 + .37 = .79), but cannot compensate for the even larger own price effect (−.85). Also, patients substitute office visits for home visits (+50%) and, for women, this effect is so large that it dominates slightly all three, the negative own price effect (−25%), the negative effect induced by the complementariness with specialist visits (−22%) and the income effect (−3%). This essentially results from both, a relatively higher substitution elasticity ($\varepsilon^*_{21} > \varepsilon^*_{22}$) and that the proportional price increase of home visits was the lowest of all three. Finally, even if the prices of specialist visits increase proportionally more than those of GP office visits, the substitution elasticity, ε^*_{31} is relatively so high that positive substitution effect induced by the price increase of GP office visits (+17%) also more than compensates the same three sources of negative effects. Consequently, for women, the change of relative prices implied by the reform caused consumption of GP home visits and specialist visits to increase rather than to decrease.

5.4 The Gross Efficiency Gain of the Price Reform

The increase of the own contribution charged for the consumption of physician services could benefit to society to the extent that it reduces the excess demand induced by insurance. In this section we provide an estimate of this efficiency gain.

This estimate should be regarded as an upper bound for a number of reasons. First, data limitations do not allow weighing the efficiency gain against the cost of increased risk induced by the lower health insurance coverage. We can therefore only estimate the gross efficiency gain. Moreover, for similar reasons, we cannot account for the income effect of the price increase on total expenditures or for the substitution effects inducing the demand for other (health) goods and services to increase. Feldstein (1973) argued that the cost of medical services should fall as a consequence of less health insurance coverage. This increases the efficiency gain. However, this argument does not hold in the Belgian institutional context. As mentioned in Section 2, prices of health services are not set freely, but fixed nationally in negotiations involving the government, representatives of the sickness funds and of the physicians. Between January 1993 and 1994, these parties negotiated a price increase corresponding approximately to the increase of the Consumer Price Index (CPI).

By contrast, we under-estimate the gross social benefit if the price elasticity of demand increases with earnings, since we extrapolate our findings for the low earning groups.

The gross efficiency gain, EG, as a proportion of total expenditures on physician services is estimated by the following expression:

$$EG = \frac{\sum_d \sum_m \sum_i \left[\hat{\phi}_{imd0} N_{md0} \left(-\Delta \ln \hat{\phi}^S_{imd}\right) k_{id1} \left(1 - \left(\frac{p_{id1}}{k_{id1}} - \frac{p_{id0}}{k_{id0}}\right)/2\right)\right]}{\sum_d \sum_m \sum_i \hat{\phi}_{imd0} N_{md0} k_{id1}} \quad (23)$$

where k_{idt} $(t = 0; 1)$ is the cost of physician service i for treatment group d at time t and p_{idt}/k_{idt} the corresponding co-payment rate. Since the demand for service i falls proportionally at a rate of $-\Delta \ln \hat{q}^S_{imd}$, $\hat{q}_{imd0}N_{md0}\left(-\Delta \ln \hat{q}^S_{imd}\right)$ is the number of units by which it falls. This is valued at the cost of the physician service to society net of the loss in consumer surplus per euro of reduced consumption, calculated in the usual Harberger fashion.

Calculated as such, the total gross efficiency gain, EG, is 2.3% of total expenditures. This is small for real price increases ranging between 35% and 60%. Moreover, recall that this figure must regarded as an upper bound. Since the prices of the control group hardly changed, 99% of this efficiency gain is generated by the fall in consumption of the treatment group. If we calculate the efficiency gain relative to the total expenditures of the treated only, it increases to 3.2%. Finally, note that the largest share of the efficiency gain, i.e. 70%, is induced by an altered consumption pattern of men. This reflects the different pattern of price effects between men and women reported in Table 5.

If we use the decomposition formula (22) of the proportional fall in consumption, $-\Delta \ln \hat{q}^S_{imd}$, and insert this in (23) we can also decompose the gross efficiency gain. In Table 6 we report this decomposition for men and women separately. To facilitate reading, we normalise the total efficiency gain to a value of 100.

Consistent with the findings reported in Table 5, for men, the total gain is largely (103%) induced by the common average price increase of all three services (i.e. by the income effects) and not by a change in relative prices (i.e. by substitution effects, -3%). For women, the change of relative prices accounts for as much as 21%.

For men, the efficiency gain is nearly entirely (96%) caused by a fall in expenditures on GP home visits (45%) and office visits to specialists (51%). For women, in contrast, society benefits only from the reduction in the number of GP office visits. Substitution effects induce the demand for GP home visits and for office visits to specialists to increase. This reduces the efficiency gain by 16% and 18%, respectively.

The reported gross effects of the changes in relative prices are very large. For instance, the efficiency gain induced by the own price effect of GP office visits for women is 16 times as large as the total gain. The model predicts that if only the price of GP office visits had been increased in 1994, then this could have yielded a much larger gross efficiency gain for women: 277% induced by the own price effect and the substitution effects on the other two physician services to which one must add the positive income effect of this price change (7%)[29]. However, the large imprecision by which the Slutsky terms were estimated calls for caution. Further research is required to confirm these findings.

6 Conclusion

This study analysed the effect of a substantial increase of the co-payments of three types of physician services on the 1st of January 1994 in Belgium: GP office and home visits and office visits to the specialist. We proposed a DD estimator of the effect of the price increase on the demand for these services and showed how it could be decomposed into one induced by the uniform proportional increase of co-payments for all three services

[29] This figure is calculated by setting $d\ln P_{Gd} = \hat{\theta}_1 d\ln p_{1d}$ in the three terms reflecting the income effects.

(i.e. an income effect) and into a number of substitution effects induced by the change in relative prices.

The average elasticity of a common proportional price increase is $-.13$ for men (ranging from $-.06$ for GP office visits to $-.14$ for specialist visits and $-.18$ for GP home visits) and $-.03$ for women (ranging from $-.01$ to $-.02$ and $-.08$ for the same services). This is low if one compares it to the average price elasticity of $-.31$ for outpatient services reported for comparable cost sharing rates in the Rand experiment in the US (Newhouse et al., 1993). However, the natural experiment on which this study relies, identifies the price elasticity of lower income groups only. Since the Rand experiment reports an increase in the use of outpatient services for higher income groups, the findings of this study are not necessarily different. By contrast, the level model of Van de Voorde et al (2001) reports for Belgium for the non-contributing population (vipo) on average larger elasticities than those reported in this study. For the DD model, the contrast between vipo pref and vipo nopr yields lowers elasticities. However, these authors did not account for substitution effects induced by the change in relative prices.

As Chiappori et al. (1998) for France, GP home visits are found to be more price elastic than the other two physician services, at least for men. We claim, however, that the higher elasticity follows from GP home visits being a luxury, rather than from the lower time costs, as suggested by Chiappori et al. (1998). For women, a GP home visit is also a luxury, but the income effect is more than offset by the substitution effects induced by the relative price increases of the two other physician services. GP office visits are necessities and specialist visits are only luxuries for men.

The effects of the relative price changes are large, especially for women. For women, the positive substitution effects of the increase in the cost-sharing rates on the demand for GP home visits and specialist visits even more than compensate the negative income effects. GP office visits are Hicksian substitutes for specialist and GP home visits. GP home visits and specialist visits are (weak) complements. Nevertheless, these findings should be confirmed in further research, because the parameter estimates were very imprecise.

Despite the substantial increase of the co-payment rates, we estimate that an upper bound for the gross efficiency gain of the price reform is only 2.3%.[30] This gain results essentially (70%) from the fall in health expenditures for men. To obtain an estimate of the net efficiency gain, we must deduct the efficiency loss of the increased risk due to the lower insurance coverage from this figure. This means that the net welfare gain of the reform, if any, is surely very modest.

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Appendix 1: The Efficient DD Estimator

GLS is an asymptotically efficient estimator of the parameters of regression model (4). This requires further specification and estimation of the variance-covariance matrix of the residual terms. First, consider the unobserved group effects, ω^0_{imd}. Apart from assuming that $E\left[\omega^0_{imd} \mid i; m; d\right] = 0$, we allow these unobserved effects to be correlated between the different types of physician services, i, and this differently according to the treatment status, d:

$$E\left[\omega^0_{imd}\,\omega^0_{jm'd'} \mid i; j; m; m'; d; d'\right] = \delta_{mm'}\delta_{dd'}\sigma^{\omega^0}_{ijd} \tag{24}$$

where δ_{xy} is the Kronecker delta.

The approximation error ν^0_{imd} equals the first order terms of the above mentioned Taylor expansion:

$$\nu^0_{imd} = \sum_{t=0} (-1)^{t+1} \frac{(\hat{q}_{imdt} - q_{imdt})}{q_{imdt}} \tag{25}$$

The higher order terms may be omitted, since it can be shown that its probability limit for N_{mdt} $(t = 0; 1)$ tending to infinity converges to zero at a faster rate than the first order terms (Amemiya, 1985, p.276-7). Hence, this omission does not affect the consistency of the estimator nor its asymptotic distribution.

If we assume that the random number of visits $q_{imdt}(n)$ is independently distributed across individuals with mean and variance equal to q_{imdt} and, as such, compatible with a Poisson distribution, then it can be shown that $E\left[\nu^0_{imd} \mid i; m; d\right] = 0$ and

$$E\left[\nu^0_{imd}\nu^0_{jm'd'}\right] = \delta_{mm'}\delta_{dd'}\delta_{ij} \sum_{t=0} (N_{mdt}q_{imdt})^{-1} \equiv \sigma^{\nu^0}_{ijmd} \tag{26}$$

A consistent estimate is obtained by replacing q_{imdt} by $\hat{q}_{imdt}$.

In a similar context, a feasible GLS procedure was proposed by Amemiya and Nold (1975) and Parks (1980). It consists of two steps. In a first step one estimates (4) by OLS. This allows to calculate, for each d; an estimate of the 3×3 variance-covariance matrix of the unobserved group effects:

$$\hat{\sigma}^{\omega^0}_{ijd} = \frac{1}{M_d} \sum_{m=1}^{M_d} \left(\hat{e}^0_{imd}\hat{e}^0_{jmd} - \hat{\sigma}^{\nu^0}_{ijmd}\right) \tag{27}$$

with $\hat{e}'_{imd}$ the OLS residual of regression equation (4) and $\hat{\sigma}^{\upsilon'}_{ijmd}$ the estimate of (26). Next, observe that the variance-covariance matrix of the residuals in equation (4) is block-diagonal and that the elements of block $(m; d)$ can be estimated by

$$\hat{\sigma}^{\omega'}_{ijd} + \hat{\sigma}^{\upsilon'}_{ijmd} \tag{28}$$

This estimate is then used in a second step to construct a feasible GLS estimator.

Appendix 2: The Rotterdam DD Estimator

The GLS estimator of regression model (8) is constructed in a similar way. First, the variance-covariance matrix of the unobserved group effects ω^{μ}_{imd} is the same. The approximation error is

$$\upsilon^{\mu}_{imd} = \sum_{t=0} \sum_{j=1} \frac{\partial y^{\mu}_{imd}}{\partial q_{jmdt}} \left(\hat{q}_{jmdt} - q_{jmdt}\right) \tag{29}$$

where

$$\frac{\partial y^{\mu}_{imd}}{\partial q_{jmdt}} = (-1)^{t+1}\delta_{ij}\frac{\overline{w}_{imd}}{q_{imdt}} + \frac{\overline{w}_{imd}}{2}\left(\frac{\delta_{ij}}{q_{imdt}} - \frac{p_{jdt}}{x_{mdt}}\right)\Delta q_{imd} \tag{30}$$

Assuming again that the random number of visits $q_{imdt}(n)$ is independently distributed across individuals with mean and variance equal to q_{imdt}, it can be shown that $E\left(\upsilon^{\mu}_{imd} \mid i; m; d\right) = 0$ and

$$E\left(\upsilon^{\mu}_{imd}\upsilon^{\mu}_{jm'd'}\right) = \delta_{mm'}\delta_{dd'}\sum_{k=1}\sum_{t=0}\left(\frac{\partial y^{\mu}_{imd}}{\partial q_{kmdt}}\frac{\partial y^{\mu}_{jmd}}{\partial q_{kmdt}}\frac{q_{kmdt}}{N_{mdt}}\right) \equiv \sigma^{\upsilon\mu}_{ijmd} \tag{31}$$

A consistent estimate is obtained by replacing q_{imdt} by $\hat{q}_{imdt}$ and a feasible GLS estimator is found by the procedure described in Appendix 1.

Appendix 3: Identification of the Second Stage Rotterdam Model

Even if we impose homogeneity ($s_{i3} = -s_{i1} - s_{i2}$) and symmetry ($s_{12} = s_{21}$), the intercepts and the price variables of the demand system (17) are linearly dependent. To see this we rewrite (17) with the mentioned theoretical restrictions in matrix notation:

$$\hat{y}^{\mu}_{m} = X_m\beta + Z\gamma + v_m \tag{32}$$

where

$$\hat{y}^{\mu}_{m} = \left[\hat{y}^{\mu}_{1m0}\quad \hat{y}^{\mu}_{1m1}\quad \hat{y}^{\mu}_{2m0}\quad \hat{y}^{\mu}_{2m1}\right]' \tag{33}$$

$$\beta = \begin{bmatrix} b_1 & b_2 \end{bmatrix}' \tag{34}$$

$$\gamma = \begin{bmatrix} a_1 & a_2 & s_{11} & s_{12} & s_{22} \end{bmatrix}' \tag{35}$$

$$v_m = \begin{bmatrix} e_{1m0} + u_{1m0} & e_{1m1} + u_{1m1} & e_{2m0} + u_{2m0} & e_{2m1} + u_{2m1} \end{bmatrix}' \tag{36}$$

$$X_m = \begin{bmatrix} \sum_j \hat{y}^1_{jm0} & 0 \\ \sum_j \hat{y}^1_{jm1} & 0 \\ 0 & \sum_j \hat{y}^1_{jm0} \\ 0 & \sum_j \hat{y}^1_{jm1} \end{bmatrix} \tag{37}$$

and

$$Z = \begin{bmatrix} 1 & 0 & \Delta \ln(p_{10}/p_{30}) & \Delta \ln(p_{20}/p_{30}) & 0 \\ 1 & 0 & \Delta \ln(p_{11}/p_{31}) & \Delta \ln(p_{21}/p_{31}) & 0 \\ 0 & 1 & 0 & \Delta \ln(p_{10}/p_{30}) & \Delta \ln(p_{20}/p_{30}) \\ 0 & 1 & 0 & \Delta \ln(p_{11}/p_{31}) & \Delta \ln(p_{21}/p_{31}) \end{bmatrix} \tag{38}$$

The reader can verify that $\det(Z'Z) = 0$, implying that γ cannot be uniquely identified from the data. In contrast, if relative prices, both for the treatments as for the controls do not vary proportionally and if for some i we impose $a_i = 0$, then identification is assured.

Appendix 4: The Estimator of the Second Stage Rotterdam Model

The GLS estimator of the second stage Rotterdam model (17) is constructed along similar lines, but is non-linear because of the negativity constraint (16) that must be imposed in this analysis. The first two moments of the unobserved group effects e_{imd} are specified as in Appendix 1 and 2. The approximation error, u_{imd}, of the Taylor expansion is

$$u_{imd} = \sum_{t=0}^{1} \sum_{k=1}^{3} \left[\left(\frac{\partial y^1_{imd}}{\partial q_{kmdt}} - b_i \sum_{j=1}^{3} \frac{\partial y^1_{jmd}}{\partial q_{kmdt}} \right) (\hat{q}_{kmdt} - q_{kmdt}) \right] \tag{39}$$

where $\frac{\partial y^1_{imd}}{\partial q_{kmdt}}$ is defined in (30). The second term in the parenthesis accounts for the approximation error induced by the regressor of the income effect in (17). Specified as such, the approximation error satisfies the adding-up condition (13): $\sum_{j=1}^{3} u_{jmd} = 0$. Since b_i is unknown it is replaced by a consistent estimate: the OLS estimate of (17).[31]

[31] This induces correlation between the regressors and the error term and will therefore bias the estimator. Consistency is not affected, however.

In this second budgeting stage the random number of visits $q_{imdt}(n)$ of type i is no longer independently distributed from the other types, because it is conditioned on a given budget x_{mdtn}. In order to derive the first variance-covariance matrix of the approximation errors, we therefore assume that at time t, the individual n's expenditures on the physician service of type i, $p_{idt}q_{imdtn}$, follow a multinomial distribution with

$$E\left(p_{idt}q_{imdtn}\right) = x_{mdt}w_{imdt} \tag{40}$$

and

$$E\left[p_{idt}q_{imdtn}p_{jd't'}q_{jm'd't'n'}\right] = \delta_{mm'}\delta_{dd'}\delta_{tt'}\delta_{nn'}x_{mdt}w_{imdt}\left(\delta_{ij} - w_{jmdt}\right) \tag{41}$$

Since the prices are non-random the distribution of $q_{imdt}(n)$ can be easily deduced. Using (39), (40) and (41), it can be shown that $E\left(u_{imd} \mid i; m; d\right) = 0$ and

$$E\left[u_{imd}u_{jm'd'}\right] = \delta_{mm'}\delta_{dd'}\sum_{t=0}\frac{x_{mdt}}{N_{mdt}}\sum_{k=1}\left(\frac{\partial y_{imd}}{\partial q_{kmdt}}\right)\times \left[\left(\frac{\partial y_{jmd}}{\partial q_{kmdt}}\right)\frac{w_{kmdt}\left(1 - w_{kmdt}\right)}{p_k^2} - 2\sum_{l\neq k}\left(\frac{\partial y_{jmd}}{\partial q_{lmdt}}\right)\frac{w_{kmdt}w_{lmdt}}{p_k p_l}\right] \tag{42}$$

If one replaces q_{imdt} by $\hat{q}_{imdt}$, the feasible GLS estimator is found by the procedure described in Appendix 1.

The estimated Slutsky matrix S is not negative definite. We follow Barten and Geyskens (1975) and impose negativity (16) using a Cholesky decomposition. By homogeneity (14) and adding-up (13), we may delete one row and one column from the Slutsky matrix. The Cholesky decomposition of the remaining matrix on which symmetry (15) is imposed, is then given by

$$\begin{bmatrix} s_{11} & s_{12} \\ s_{12} & s_{22}\end{bmatrix} = -\begin{bmatrix} 1 & 0 \\ b_{21} & 1\end{bmatrix}\begin{bmatrix} (h_1)^2 & 0 \\ 0 & (h_2)^2\end{bmatrix}\begin{bmatrix} 1 & b_{21} \\ 0 & 1\end{bmatrix} \tag{43}$$

$$= \begin{bmatrix} -(h_1)^2 & -b_{21}(h_1)^2 \\ -b_{21}(h_1)^2 & -(b_{21}h_1)^2 - (h_2)^2\end{bmatrix} \tag{44}$$

in which the Cholesky values $-(h_1)^2$ and $-(h_2)^2$ are negative by construction. Note, this imposes non-linear restrictions on the Slutsky parameters, requiring the regression model (17) to be estimated by non-linear GLS.

If the negativity constraint is binding one of the Cholesky values should tend to zero. However, if it were set exactly to zero the Slutsky matrix and therefore the outer-product of the first derivatives is singular. Since the inverse of the latter matrix is used in our procedure of numerical optimisation, we choose the algorithm proposed by Marquardt (1963) allowing the Cholesky value to converge very closely to zero.[32]

[32] As a consequence of near-singularity the objective function of the numerical optimisation procedure is very flat and tends to converge too rapidly, i.e. far from the minimum. To overcome this problem, we first estimate the model in which we impose the second Cholesky value to be equal to zero. Subsequently, we take the parameter values of this first stage as initial values, apart from the second Cholesky value, which is set at a value very close to zero. We then apply the optimisation routine proposed by Marquardt (1963).

TABLE 1: Descriptive statistics of the sample

	Men		Women	
	1993	1994	1993	1994
Age				
$age < 30$	43.6%	43.3%	39.7%	39.3%
$30 \leq age < 50$	28.1%	28.5%	27.4%	27.8%
$age \geq 50$	28.3%	28.2%	32.9%	32.9%
Federation				
Gent	58.6%	58.5%	57.6%	57.6%
Household type[a]				
with dependants	37.7%	37.0%	10.1%	10.5%
vipo and $age < 30$	1.5%	1.5%	1.3%	1.3%
Earnings (in Euro)[b]				
$E = 0$	62.4%	65.3%	70.8%	72.8%
$0 < E \leq 12{,}500$	2.3%	2.7%	7.2%	7.5%
$12{,}500 \leq E <= 25{,}000$	19.4%	17.9%	16.5%	14.5%
$E > 25{,}000$	15.9%	14.1%	5.5%	5.2%
Social status				
tip	81.6%	81.5%	72.6%	72.4%
vipo nopr	11.0%	11.3%	13.9%	14.5%
vipo pref	7.4%	7.2%	13.5%	13.1%
Total number of individuals	179,360	180,420	195,137	196,025
Average number of visits				
treated				
GP office visits	1.89	1.77	2.16	2.12
GP home visits	1.21	1.03	1.59	1.47
Specialist visits	1.31	1.28	1.94	1.94
controls				
GP office visits	2.35	2.25	1.93	2.02
GP home visits	5.68	5.47	8.32	8.34
Specialist visits	1.81	1.81	1.79	1.83

[a] To avoid too small a cell size, this categorical variable is not defined for individuals belonging to the social category 'vipo' and aged < 30.

[b] This categorical variable is defined only for individuals in the category 'tip'.

TABLE 2 : Evolution of the co-payment rates

	GP o¢ce visits			GP home visits			Specialist visits		
	euro (1994)[a]	rate %	index	euro (1994)[a]	rate %	index	euro (1994)[a]	rate %	index
treated									
1993	2.62	20	100.0	4.22	25	100.0	5.13	25	100.0
1994	3.87	30	147.9	5.68	35	134.7	8.18	40	159.5
controls									
1993	0.99	8	100.0	1.29	8	100.0	1.75	9	100.0
1994	0.99	8	100.1	1.29	8	99.5	1.74	9	99.1

[a] The 1993 …gures are de‡ated by the CPI. The exchange rate is 40.3399 BEF/euro.

Source: Alliance Nationale des Mutualités Chrétiennes (1995), M-Informations, p.13.

TABLE 3a : Differences-in-differences (DD) estimates
(standard errors in parentheses)

Men[a]

i [b] \ model j	0	1	1*	1*r
interactions [c]	no	no	yes	yes
$\hat{b}_0$	-	-	-	-.13
				(.06)
$\hat{b}^j_1=\frac{\hat{b}^j_1}{\overline{w}_{11}}$	-.027	-.017/-.05	-.004/-.01	.001/.01
	(.038)	(.008/.03)	(.007/.02)	(.003/.01)
$\hat{b}^j_2=\frac{\hat{b}^j_2}{\overline{w}_{21}}$	-.230	-.047/-.19	-.038/-.14	-.022/-.08
	(.031)	(.010/.04)	(.012/.04)	(.009/.03)
$\hat{b}^j_3=\frac{\hat{b}^j_3}{\overline{w}_{31}}$	-.082	-.033/-.08	-.012/-.03	-.030/-.07
	(.026)	(.011/.03)	(.013/.03)	(.011/.02)
# of cells	204	204	132	
WSSR	189.5	190.4	117.5	131.9 [d]
DF	198	198	120	128
P-value	0.655	0.638	0.546	0.389

[a] The estimated intercepts and the variance-covariance matrix of the residuals are not reported.

[b] i=1 for GP office visits, i=2 for GP home visits, i=3 for specialist visits.

[c] A different intercept for individuals with dependants and for those aged between 30 and 50.

[d] Restrictions on 1* cannot be rejected (P-level=7.4%; $\chi^2(8)=14.3$).

0 Logarithmic DD model (4).

1 Rotterdam DD model (8).

1* as 1, but on a restricted sample excluding groups with earnings > 12,500 euro and including intercept interactions.

1^{*r} Restrictions (20) and (21) imposed on 1*.

TABLE 3b : Differences-in-differences (DD) estimates
(standard errors in parentheses)

Women[a]

$i^b \backslash$ model j	0	1	1^*	1^{*r}
interactions[c]	no	no	yes	yes
$\tilde{b}_0$	-	-	-	-.03
				(.07)
$\tilde{b}^j_1 = \frac{b^j_1}{\bar{w}_{11}}$	-.139	-.032/-.12	-.017/-.07	-.013/-.06
	(.039)	(.010/.04)	(.009/.04)	(.002/.01)
$\tilde{b}^j_2 = \frac{b^j_2}{\bar{w}_{21}}$	-.202	-.035/-.16	-.028/-.09	-.000/-.00
	(.047)	(.019/.09)	(.020/.07)	(.017/.06)
$\tilde{b}^j_3 = \frac{b^j_3}{\bar{w}_{31}}$	-.012	-.013/-.02	.005/.01	.001/.00
	(.031)	(.010/.02)	(.017/.02)	(.007/.01)
# of cells	204	204	96	
WSSR	176.1	162.6	89.2	103.8 [d]
DF	198	198	84	92
P-value	0.866	0.969	0.329	0.188

[a] The estimated intercepts and the variance-covariance matrix of the residuals are not reported.

[b] i=1 for GP office visits, i=2 for GP home visits, i=3 for specialist visits.

[c] Interaction of intercept for individuals with dependants and for those living in Gent.

[d] Restrictions on 1* cannot be rejected (P-level=6.7%; $\chi^2(8)=14.6$).

0 Logarithmic DD model (4).

1 Rotterdam DD model (8).

1* As 1, but on a restricted sample excluding groups with earnings > 0 and including interactions for the intercept.

1^{*r} Restrictions (20) and (21) imposed on 1*.

TABLE 4a: The Second Budgeting Stage of the Rotterdam model (17) (a_2 = 0)
(standard errors in parentheses)

Men [a]

i n j [b]	$a_i^d=\frac{a_i}{\overline{w}_{i1}}$ [e]	b_i=´$_i$ [e]	s_{ij}=²¤$_{ij}$ [e]		
			j = 1	j = 2	j = 3
i = 1	0	.138/.47	-.275/-.95	.088/.30	.187/.64
		(.039/.13)	(.798/2.74)	(.371/1.27)	(.429/1.47)
i = 2	-.025/-.09	.382/1.38	.088/.32	-.028/-.10	-.060/-.22
	(.008/.03)	(.043/.15)	(.371/1.34)	(.146/0.53)	(.227/.82)
i = 3 [c]	.025/.06	.480/1.11	.187/.43	-.060/-.14	-.127/-.29
	(.008/.02)	(.043/.10)	(.429/.99)	(.227/.53)	(.210/.48)
WSSR = 86.803		DF = 88 - 10 = 78		P-value = 0.232	

[a] The variance-covariance matrix of the residuals is not reported.

[b] i=1 for GP o¢ce visits, i=2 for GP home visits, i=3 for specialist visits.

[c] Figures deduced from the estimation of the …rst two equations.

[d] Intercept for the reference individual. Interactions for individuals with dependants and for those aged between 30 and 50 not reported.

[e] Elasticities are calculated on the basis of the average budget share of treatment group (tip and vipo nopr).

TABLE 4b : The Second Budgeting Stage of the Rotterdam model (17) (a_3 = 0)
(standard errors in parentheses)

Women [a]

i n j [b]	$a_i^d = \frac{a_i}{w_{i1}}$ [e]	b_i=´$_i$ [e]	s_{ij}=2¤$_{ij}$ [e] j = 1	j = 2	j = 3
i = 1	.024/.10	.075/.32	-.523/-2.20	.330/1.39	.193/.81
	(.015/.06)	(.039/.16)	(1.677/7.06)	(.828/3.49)	(.852/3.59)
i = 2	-.024/.08 [c]	.668/2.24	.330/1.11	-.209/-.70	-.122/-.41
	(.015/.05)	(.050/.17)	(.828/2.78)	(.389/1.31)	(.442/1.48)
i = 3 [c]	0	.256/.55	.193/.42	-.122/-.26	-.071/-.15
		(.046/.10)	(.852/1.83)	(.442/.95)	(.410/.88)
WSSR = 62.247		DF = 64 - 10 = 54		P-value = 0.206	

[a] The variance-covariance matrix of the residuals is not reported.

[b] i=1 for GP o¢ce visits, i=2 for GP home visits, i=3 for specialist visits.

[c] Figures deduced from the estimation of the …rst two equations.

[d] Intercept for the reference individual. Interactions for individuals with dependants and for those living in Gent not reported.

[e] Elasticities are calculated on the basis of the average budget share of treatment group (tip and vipo nopr).

Table 5: Decomposition of the average price effects of the treated.
The Rotterdam model

	total	income effect	substitution effects			
	$\Delta \ln q_{i1}$	$\eta_i b_0 \Delta \ln P_{G1}$	total $= \sum \varepsilon_{ij} \Delta \ln p_{j1}$			
Men			total	$j = 1$	$j = 2$	$j = 3$
$i = 1$	-0.005	-0.025	+0.020	-0.384	+0.095	+0.309
$i = 2$	-0.081	-0.074	-0.007	+0.133	-0.033	-0.107
$i = 3$	-0.070	-0.060	-0.010	+0.182	-0.045	-0.147
Women			total	$j = 1$	$j = 2$	$j = 3$
$i = 1$	-0.068	-0.004	-0.064	-0.854	+0.418	+0.372
$i = 2$	0.007	-0.030	+0.037	+0.502	-0.246	-0.219
$i = 3$	0.006	-0.007	+0.013	+0.170	-0.083	-0.074

$i=1$ for GP office visits, $i=2$ for GP home visits, $i=3$ for specialist visits.

Table 6: Decomposition of the total gross e¢ciency gain

	total	income e¤ects	substitution e¤ects			
Men			total	$j=1$	$j=2$	$j=3$
$i=1$	4	17	-13	265	-66	-212
$i=2$	45	41	4	-73	18	59
$i=3$	51	45	6	-135	33	108
total	100	103	-3	57	-15	-45
Women			total	$j=1$	$j=2$	$j=3$
$i=1$	134	7	127	1607	-786	-694
$i=2$	-16	54	-70	-883	432	381
$i=3$	-18	18	-36	-447	219	192
total	100	79	21	277	-135	-121

i=1 for GP o¢ce visits, i=2 for GP home visits, i=3 for specialist visits.