Fixed Budgets as a Cost Containment Measure for Pharmaceuticals

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June 2005 version. The final version is published in:
The European Journal of Health Economics 7, 37-45, 2006
(available at www.springerlink.com).

Abstract

In the county of Västerbotten, Sweden, there are two health centres which (contrary to all other health centres in the region) have a strict responsibility over their pharmaceutical budget. The purpose of this paper is to examine if the prices and quantities of pharmaceuticals prescribed by physicians working at these health centres differ significantly from those prescribed by physicians at health centres with open-ended budgets. Estimation results using matching methods, which allows us to compare similar patients at the different health centres, show that the introduction of fixed pharmaceutical budgets did not affect physicians' prescription behaviour.

Key words: Fixed budgets, pharmaceuticals, cost containment, propensity score matching.

JEL classification: I11, I18

1 Introduction

In Sweden, there is an ongoing debate concerning the high cost for drugs. In the last twenty-five years, the expenditures on drugs have risen with on average 10 per cent per year and in 2002 the cost reached 31.7 billions SEK. Prescription pharmaceuticals accounted for 24.5 billion, while hospital and over-the-counter drugs accounted for 2.7 and 4.5 billion SEK, respectively. The Swedish development parallels that of many other European countries, which has spurred an interest in different methods to contain pharmaceutical costs (see e.g. Dukes, 1993, Chapter 7 and Mossialos and Le Grand, 1999). Within the market for pharmaceuticals, cost containing measures operate both on the pharmaceutical industry, e.g. price controls, and reference prices, and on the demand side, e.g. payment systems for physicians and pharmacists, positive or negative lists suggesting which pharmaceuticals physicians should prescribe, generic substitution requirements, and/or patient co-payment plans.

During the period under study in this paper, the most widely used form of cost containment in Sweden was patient co-payments. In Sweden, the pharmaceutical insurance system is non-linear, making patient co-payments decrease as total expenditure for pharmaceuticals increase. All costs for the patient exceeding 1800 SEK per year are financed through the Swedish pharmaceuticals insurance system. Another well known cost containment measure is the so called reference price system which was introduced in Sweden in 1993. The reference price system stated that all costs above a predetermined reference price had to be borne by the patient. Similar systems have also been introduced in the Netherlands, Finland, Norway and Germany, among other countries. The Swedish system specified that any costs exceeding the price of the least expensive generic substitute by more than 10 per cent had to be borne by the patient. On October 1, 2002, the reference price system in Sweden was abolished. Instead a law (SFS 2002:160) was introduced, requiring pharmacists to substitute the prescribed pharmaceutical to the cheapest equivalent pharmaceutical product available at the local pharmacy.

Exclusions of pharmaceuticals from the pharmaceutical reimbursement plan have on occasion been used in Sweden. In the dataset used in this study, approximately 2.2 per cent of all prescriptions relates to pharmaceuticals excluded from the reimbursement plan. Another possible cost containment measure is to restrict the number of diagnoses for which a pharmaceutical product can be prescribed. This has, however, been seen by policy makers as to much of a restriction on physician autonomy, a concept central in medicine (Dukes 1993, p. 138). Note also that under the Swedish pharmaceutical insurance system there are usually no formal restrictions on physician prescription behavior or pecuniary incentives for the physician to internalize a proportion of the insurers utility.

Since 1998, the Swedish county councils are responsible for the costs of prescription pharmaceuticals which is not paid by the patient. It is thus in the interest of the county council to contain drug costs. In spite of this, health centers located in Västerbotten, which are the main source of pharmaceutical prescriptions made in the region, usually work with open-ended budgets for pharmaceutical expenditure, meaning that the prescribing agency has little incentive to contain costs.

The purpose of the present paper is to study the effects of an attempt by the local county council to introduce 'hard' budget constraints in pharmaceutical budgets in two of the health centers in Västerbotten, Sweden. In 2001, these two health centers were given fixed budgets for pharmaceutical expenditures, giving them an incentive to decrease expenditures as they were allowed to keep any surplus (and being forced to repay any deficit) generated during the year.

Whether or not introducing fixed budgets will help contain costs in public agencies is, however, an open question. On the one hand, fixed budget constraints are often considered a necessary condition for the budgetary process to be effective. On the other hand, fixed budget constraints are seldom seen as sufficient cost containing measures in public agencies. One problem is that fixed budgets must be credible. The principal may be tempted to adjust bud-

gets according to history, creating an incentive to overspend in order not to get future budget cuts, the so called ratchet effect (see Kornai et al 2003 for a discussion of soft budget constraints). Another problem is that lack of information may render the principal to introduce more or less fuzzy contracts, making incentives unclear (see e.g. Prendergast, 1999, for a lengthier discussion of problems associated with optimal incentive contracts). Thus, the efficacy of introducing hard budget constraints on cost containment is an empirical question.

This paper contributes to the literature on cost containment in health care by comparing pharmaceutical expenditure at health centers with fixed pharmaceutical budgets to those with open-ended budgets. Changes in the expenditures on pharmaceuticals are decomposed into three parts; the number of prescriptions, the size of prescriptions (the number of defined daily doses per prescription), and the price of the pharmaceutical. Prescriptions are matched according to a large number of background variables, including age and diagnosis, using the method of propensity score matching. This makes it possible to study if physicians respond to the budgetary rules by prescribing cheaper medicine or fewer doses of medicine per prescription. Finally, the number of prescriptions in health centers with fixed budgets are compared to those with open-ended budgets.

The results show that the number of prescriptions in the two health centers with fixed budgets declined relative to the control group after the introduction of the fixed budgets. However, there is no systematic difference regarding either price or quantity per prescription between the two types of health centers. A possible explanation is that the prescribing physicians do not believe the fixed budgets to be credible.

The rest of the paper is organized as follows: section 2 presents the main hypotheses to be explored in this paper. Section 3 presents the empirical analysis. The data to be used are presented in subsection 3.1. Subsection 3.2 describes the matching method to be used, while subsections 3.3 and 3.4 contain the results from the propensity score estimations and from the

matching, respectively. Finally, section 4 concludes the paper.

2 Background and hypotheses to be explored

As mentioned, the purpose of this paper is to study if differences in budgetary rules among health centers in Sweden affect physicians' prescription behavior. From 1998, the county councils in Sweden are responsible for the costs of prescription pharmaceuticals which are not paid by the patient. For the county council in Västerbotten, these costs are approximately 500 million SEK per year. The county councils are to some extent reimbursed by the central government through the Swedish National Insurance Board (SNIB). An agreement between the local county council in Västerbotten and the SNIB states that during the years 2000 to 2004, the SNIB will pay a total of 1.5 billion SEK for pharmaceutical insurance costs in Västerbotten, Sweden. This means that approximately 375 million SEK per year is paid by the SNIB, while all additional costs for pharmaceutical insurance is paid by the local county council in Västerbotten.

In the county of Västerbotten, health centers have since 2001 budgetary responsibility for pharmaceutical costs. The county council works with target budgets for the health centers, which are decided partly upon on the basis of population characteristics, primary age and gender, within each health center's reception area and partly upon each health center's result in 2001. However, budgets are not fixed in the sense that surpluses or deficits are carried over to the next year's budget. This means that the economic incentives to reduce drug costs are limited.

Starting in 2001, two health centers located in Västerbotten (Burträsk and Moröbacke health centers) obtained fixed budgets as a part of an agreement intended to increase the centers' self-autonomy. A fixed budget is here defined as a budget that is determined on the basis of characteristics which are exogenous to the decisions made by the health center, and where surpluses and deficits carry over to the next year's budget.

There are several ways by which an introduction of fixed budgets may affect prescription behavior in health centers. First, the amount of prescriptions made out can be decreased. Having a fixed budget reasonably means that physicians will be more reluctant of writing prescriptions at given prices and quantities. Second, since marginal co-payment decreases with the total purchase of medicine during a full year, physicians can 'choose' to change the mix of patients who obtain prescription drugs. In order to engage in patient substitution, a physician must have information regarding the position of the patient in the price schedule. Such information has not been available during the period of our study.

Third, the price of medicine for a given treatment can be decreased by, for example, an increased use of generic drugs. Availability of information regarding the price of different pharmaceuticals has increased during the period under study. Finally, prescriptions can be made to reduce the number of defined daily doses (DDD:s) per prescription. A well known problem with prescription pharmaceuticals is that large amounts of the pharmaceuticals are wasted, indicating that a major problem is overprescribing. However, note also that if the number of doses is reduced, there is an increased likelihood that the patient returns to the health center for a new prescription, which may increase costs. In the present paper, we address the two last channels, i.e. if the introduction of a fixed budget decreases prices for drugs within specific ATC-groups, and if the number of doses per prescription is affected. We also include some tentative information on the total number of prescriptions made out at the different health centers.

In order to study the price and quantity effects, a comparison is made between the two health centers with fixed budgets (the treatment group) to a group of health centers which have target budgets (the comparison group). Since prescription behavior can differ before the introduction of fixed budgets, a time dimension is also included. A difference-in-difference method is used to study price and quantity effects. If fixed budgets have the intended effects, we expect the prices and/or number of DDD:s to drop in the treatment group

relative to the comparison group.

Finally, on October 1, 2002, the so called substitution reform was introduced, requiring pharmacists to substitute the prescribed pharmaceutical to the cheapest equivalent pharmaceutical product available at the local pharmacy. This reform has had a large effect on drug costs, in particular prices, in health centers (Socialstyrelsen 2003, p. 15). The substitution reform offers another possibility to test our hypotheses. Since the introduction of fixed budgets was made in 2001, prices and quantities should already have dropped in the health centers with fixed budgets before the introduction of the substitution reform. Therefore, making a similar comparison before and after the introduction of the substitution reform, we would expect a price decrease in the comparison group relative to the group of 'treated' health centers.

To summarize, our main hypotheses are as follows: (i) comparing the period January to September 2002, when fixed budgets are newly introduced, with the pre-reform period, we expect prices and quantities in treated health centers to fall relative to the comparisons; and (ii) for the period after the introduction of the substitution reform, we expect the relative price difference to be positive. A potential problem with the data is that the year 2001, which forms the basis for our first comparison, is an adjustment period for the health centers where fixed budgets are to be introduced. This means that the treated health centers may have acted so as to increase their future budgets. In order to at least shed some light on whether such behavior might have influenced our data, we also compare post and pre-reform periods in the year 2001.

3 The empirical analysis

3.1 Data

The data used in this study has been provided by the county council of Västerbotten, Sweden. It contains a total of 6.2 million observation, covering all pharmaceuticals sold in the county of Västerbotten or sold in other parts of Sweden to residents of Västerbotten between January 2001 and June 2003. From this population a random sample of twenty-five per cent is drawn.

The county of Västerbotten (population 255 122, June 2003) consists of fifteen administrative areas. The two health centres Burträsk and Moröbacke, which both are located in the administrative area of Skellefteå, received fixed budgets for prescription pharmaceuticals on May 1, 2001, and November 1, 2001, respectively. Since these are the two health centres of interest in this paper and since we want to avoid potential differences between the areas to influence the results, all observations not originating from physicians working at health centres in Skellefteå are excluded. In this step, we also exclude nearly six per cent of the observations lacking information about the physician's workplace. In addition, nearly six per cent of the observations are dropped since the cost per DDD can not be calculated or because they indicate resell of the pharmaceutical product to the pharmacy. The final sample to be used in the empirical analysis consists of 292 419 observations.

The dataset includes information regarding the patients age, gender and area of residency. Patients are however not traceable over time. The information about the prescription contains the month in which it is sold, the pharmaceuticals ATC-code and an indicator if the pharmaceutical is packed in patient-doses rather than their ordinary packages. Patients who have relatively stable medication and can be expected to have some problem keeping track of how big doses they should take, often receives their prescriptions in patient-doses. In addition, the dataset includes information about the cost of the prescription as well as the patient's co-payment for the prescription. Using these variables and information about the different phases in the re-

imbursement system, we have calculated the co-payment bracket in which the patient was prior to paying for the current prescription. The calculations have been left out in order to save space, but are available from the authors upon request. The dummy variables Copay100, Copay50, Copay25, Copay10 and Copay0 are defined so as to correspond to marginal rates of co-payment of 100, 50, 25, 10 and 0 per cent, respectively. Some prescriptions are always free of charge for the patient and others are excluded from the reimbursement plan (for example cough medicine), irrespective of the patient's co-payment bracket. For these observations the patients' co-payment rates bear no information of previous cost for prescription pharmaceuticals. The dummy variables Free and Unsub indicate that a prescription is free of charge and unsubsidized, respectively.

Descriptive statistics by health centres are presented in Table 1. For the different indicator variables the percentage of the material which belongs to each category are presented. For continuous variables the means and standard deviations are presented instead.

Table 1 about here.

The data contains 146 four-digit ATC-groups. Of these groups 52 have less than 100 observations, 39 have between 100 and 1 000 observations, 55 have more than 1 000 observations. The observations are quite evenly distributed between the thirty months included in the study, with fewest pharmaceuticals (7 830) sold in June 2002 and most pharmaceuticals (11 178) sold in October 2002.

Descriptive statistics of the key-variables of interest both prior to and after the introduction of fixed budget constraints are presented in Table 2 below. Table 2 show that the Price/DDD and the number of DDD:s/prescription have been reduced for Burträsk after fixed budgets were introduced. The opposite is true for Moröbacke, but the number of prescriptions per month have decreased for Moröbacke. For the entire sample the average cost per DDD is 6.5 SEK, the average number of DDD:s per prescription equals 41.7 and the

average cost per prescription is 163 SEK.

Table 2 about here.

3.2 Econometric Method

To test whether the average price per DDD and the average number of DDD:s per prescription are affected by the introduction of fixed budgets, we would like to estimate the average treatment effect on the treated for these two outcome variables. Treated in this case means that the prescription is written by a physician working at a health center with fixed pharmaceutical budget.

When evaluating the average treatment effect on the treated, we can view the problem as if there for every observation, at every time, are two possible outcomes, labelled Y_1 if the observation is treated, and Y_0 if the observation is untreated. We can only observe one outcome for each observation at each time. The average treatment effect on the treated could be expressed as

$$E(Y_1 - Y_0|D = 1) = E(Y_1|D = 1) - E(Y_0|D = 1),$$

where D is a dummy which takes the value 1 if the observation actually is treated and the term $E(Y_0|D = 1)$ is unobservable. That is, we cannot observe what the outcome would have been for the treated, if they had not been treated. Rosenbaum and Rubin (1983) showed that if this term is estimated with the outcome for the untreated we get a bias which can be written as

$$Bias = E(Y_0|D = 1) - E(Y_0|D = 0).$$

Both the OLS and the matching method deal with this bias problem by adjusting for a set of observable variables, X, which are differently distributed in the groups and can influence the outcome. One advantage of the matching method is that it is semiparametric and therefore avoids the functional form restrictions of the OLS equation.

In this study, a variant of propensity score matching will be used, in which untreated observations are determined to be suitable matches for treated observations if they have similar probability of being treated. Rosenbaum and Rubin (1983) prove that if the following three conditions are satisfied, when the outcome is independent of treatment assignment conditioned on the propensity score, $Pr(D = 1|X)$, and the bias is zero. The first condition is that all relevant differences between the groups have to be captured by their observables X. The second condition states that the matching must be done over a region where the probability of being treated conditioned on X is between zero and one, that is:

$$0 < Pr(D = 1|X) < 1.$$

As shown by Smith and Todd (2003a) the condition $0 < Pr(D = 1|X)$ is not required when the parameter of interest is the average treatment effect on the treated, since it only guarantees good matches for each untreated observation. The final condition is that the observables must be independent of treatment assignment conditioned on the propensity score.

By using both pre- and post-treatment data we avoid having to fulfill the first condition. We do this by using a difference-in-difference extension of propensity score matching which can take account of time-invariant unobservable heterogeneity. This method makes it possible to get a unbiased estimate of the treatment effect on the treated even if unobservable differences exists between Burträsk and Moröbacke and other health centers, as long as these differences are time-invariant. We estimate the differences between the treated-control outcome differences after treatment and the treated-control outcome differences before treatment, that is:

$$\Delta^{DID}_{D=1} = [E(Y_1|D = 1) - E(Y_0|D = 1)] - [E(Y'_1|D' = 1) - E(Y'_0|D' = 1)],$$

where $'$ indicates the period before treatment and $D\prime$ is a dummy which takes the value one for observations originating from Burträsk or Moröbacke before

they received fixed budgets for prescription pharmaceutical. The effect will be estimated separately for Burträsk and Moröbacke since they received fixed pharmaceutical budgets at different times.

Matching methods differ by placing different weights on the control observations. Nearest neighbor matching, which will be used in this paper, puts all weight on the observation in the non-treated group which has the most similar propensity score. This reduces bias since only the best matches are used, but could lead to increased variance compared to the other matching methods which uses more observations from the control group. We impose a common support condition by specifying that the maximum allowed distance between the propensity score of the treated and the control is 0.01. Treated observations for which no matches can be found within this caliper are excluded from the analysis.

3.3 Estimation of propensity score

The first step in estimating the difference in difference is to estimate the propensity scores before and after treatment. To be able to get robust estimates we want to use variables which affect treatment, as well as variables which affect the outcome, when estimating the propensity score. We include gender, using an indicator variable equaling one if the individual is a female, and age by using indicator variables for each five-year group. We also include a dummy variable, denoted Area, which takes the value one if the patient's area of residency is some other than Skellefteå, and include indicator variables for the month in which the pharmaceutical is sold.

As can be seen in Table 1, the share of the prescriptions made out in patient-doses differs between health centres. Coscelli (2000) has shown that patients exhibit strong state dependence, that is, they prefer the drug they have been prescribed before. This gives the usually more expensive brand-name drug a first mover advantage. The state dependence is probably more important among the patients receiving their prescriptions in patient-doses, since these patients usually have relatively stable medication. We therefore

include a dummy variable, referred to as Doses below, which takes the value one if the prescription is made out in patient doses.

Previous studies have shown that the patient's co-payment is likely to affect the prescription being made (see e.g. Lundin, 2002, and Rudholm, 2004). We include the dummy variables Copay, Unsub and Free to take account of this effect. We use the patient's co-payment bracket prior to receiving the prescription in question, to avoid the endogenity problem of co-payment being affected by the price and volume of the drug being prescribed.

Another variable which is important to include is the four digit ATC-groups of the pharmaceuticals. The ATC-group affects treatment as the health centers have physicians with different specialities, and affects outcome since average price per DDD and number of DDD:s per prescription vary considerable between ATC-groups. We view ATC-group as a predetermined health indicator which is a function of the patient's health status and therefore exogenous of treatment. The same argument goes for Doses, Unsub and Free.

Among a general set of models, all including the variables discussed above but including different interaction terms, the final specifications are chosen to maximize the within-sample correct prediction rates using the hit-or-miss method (e.g. Heckman, Ichimura, and Todd, 1997). This method ascribes observations a "1" if the estimated propensity score is larger than the portion of the sample which is treated and a "0" otherwise. The propensity scores have been calculated using logit estimations of versions of the following equation;

$$
\begin{aligned}
Treat_i \;=\; & \beta_0 + \beta_1 Gender_i + \sum_{a=1}^{A-1} \beta_a Age_{ai} + \beta_2 Area_i + \beta_3 Doses_i \\
& + \sum_{c=1}^{4} \beta_c Copay_{ci} + \beta_4 Unsub_i + \beta_5 Free_i + \sum_{m=1}^{M-1} \beta_m Month_{mi} \\
& + \sum_{g=1}^{4} \beta_g GenderCopay_{gi} + \sum_{d=1}^{D-1} \beta_d ATC_{di} + \beta_6 AgeGender_i \\
& + \beta_7 Age^2 Gender_i + \beta_8 GenderUnsub_i + \beta_9 GenderFree_i \\
& + \beta_{10} AgeDoses_i + \beta_{11} Age^2 Doses_i + \beta_{12} GenderDoses_i \\
& + \beta_{13} GenderYear_i + \varepsilon_i,
\end{aligned}
$$

where $Treat$ equals one if the prescription is made out by a physician at Burträsk or Moröbacke and A, M and D indicate the number of age-groups, months and four digit ATC-groups in each subsample, respectively.

The results from the logit estimations (available from the authors upon request) show that all models are significant at the 0.0001 level, according to a F-tests made. The within-sample correct prediction rate range between 56 and 62 per cent for the models referring to Burträsk and between 58 and 65 per cent for those referring to Moröbacke. The corresponding figures for the pseudo R^2 values are two to six per cent and two to nine per cent, respectively.

Two balancing tests presented in Appendix 1 show that we have been able to balance most subsamples by matching on propensity score. However, for all models some variables remain unbalanced. Descriptive statistics for the model which is least balanced, reported in Table A2, indicates that the remaining bias due to observables is small in magnitude also for this subsample.

3.4 Matching Results

As discussed in section 2, we expect the difference-in-difference estimates to be non constant over time. We will therefore divide the data into sub periods in order to test the hypotheses that prices and quantities are affected by the introduction of fixed budgets. Three post-treatment periods are identified. First, for the period 2002 and after, the treated health centers had an incentive to decrease prices and quantities. However, on October 1, 2002, the substitution reform was introduced, which may have had a large impact on health centers which prescribed relatively expensive medicine. If fixed budgets had an impact on prices and quantities, we would most likely find a difference in prices and quantities for the period up to the substitution reform. Therefore, the first comparison is made between the period January to September 2002 and the period 2001 prior to the introduction of fixed budgets. Second, after October 1, 2002, when the substitution reform is enforced, we expect the prices in the comparison group to be affected more strongly than those in the treated health centers, since, if fixed budgets had the hypothesized effects, prices should already have adjusted in the treated health centers. Third, the fixed pharmaceutical budgets are in part determined by outcomes in 2001, which gives the treated health centers an incentive to overspend in 2001 in order to increase their future budgets. We therefore compare the period 2001 after the formal introduction of the new rules with the period 2001 prior to the introduction of fixed budgets (January to April for Burträsk and January to October for Moröbacke) in order to study if this incentive led to changes in prescription behavior.

Of the prescriptions made out by physicians at Burträsk and Moröbacke, 2.7 and 4.9 per cent respectively, are made out to patients not listed at the respective health centres, which means that they do not have to bear the costs for these prescription. Neither are the health centers budgets charged for the 0.6 per cent of the observations which refers to special pharmaceuticals. The results to be presented are not affected when dropping these observations and the observations which refer to unsubsidized pharmaceuticals. The

results are also robust against small changes in the specification of the selection models. In order to investigate if the introduction of fixed budgets and/or the substitution reform had different effects on different pharmaceutical treatments, we also distinguish between ATC-groups. The three groups chosen are A, C and N, which refer to drugs related to digestive organs and metabolism, heart and circulation, and the nervous system, respectively. The groups constitute approximately 12, 24 and 28 per cent of the prescriptions, respectively.

Let us start by comparing the period January to September 2002 with the period prior to treatment. The average treatment effects on the treated of introducing fixed budgets for this period are presented in Table 3. As mentioned in our first hypotheses in section 2, we expect both prices and quantities to decrease relative to the comparison group. However, the estimated quantity difference for Moröbacke is positive and significant when comparing all ATC-groups, and there is a large variation in the estimated quantity differences between ATC-groups. For Burträsk, one can note that the price difference is positive and significant in ATC-group N, the other estimated differences vary in size as well as sign.

Table 3 about here.

In order to test our second hypotheses, we take the differences between the period September 2002 and after, and January to September 2002. These results are presented in Table 4. Burträsk and Moröbacke has lowered the number of DDD:s/prescription after the substitution reform relative to the control group, but prices per DDD are largely unaffected. This is surprising since the reform is aimed to affect price per DDD but not the number of DDD:s.

Table 4 about here.

As discussed previously, a potential problem when testing our first hypotheses is that the price and quantities in the year 2001 could be affected

by an attempt of the treated health centres to increase their future budgets. An increase in price and quantities relative to the comparison group, when comparing the post and pre-reform periods in the year 2001, would indicate that such behavior might have influenced our data. The results presented in Table 5 show that quantities are significantly increased in Moröbacke when comparing over all the ATC-groups, and there is a general tendency of positive effects also within ATC-groups. For Burträsk, quantity differences are generally negative, although insignificant. There is no systematic differences in prices between the groups. Again, significant results are found within ATC-group N. Even though the results for quantities in Moröbacke have the expected sign, no systematic difference in prescription behavior can be observed when comparing the post and pre-reform periods in the year 2001. This is however a weak test of strategic behavior since such behavior could have started before the formal introduction of the new rules. If the results for the first hypotheses are affected by strategic behavior, this would lead to an overestimation of potential reductions in price and quantities due to the introduction of fixed pharmaceutical budgets. To conclude, independent of comparison, only a few of the estimated effects turns out in favour of our hypotheses. Most of the estimates are not significantly different from zero. Often, significant estimates are contrary to expectations. Thus, the data display no support for the hypothesis that fixed budgets affect physician prescription behavior.

Table 5 about here.

4 Discussion

Tentative results from this paper show that when health centers in Västerbotten were given a fixed budget, the number of prescriptions of pharmaceutical products declined. This can be seen as an indication that the prescribing physicians adopted to the new budgetary rules by reducing the number

of prescriptions, as compared with the previous situation when they were given open-ended budgets. Other possible explanations for this result include changes in the health status of the population using these two health centers, or demographic factors in the community were the health centers are located. However, the short time period under study makes such explanations less probable, since health status and demographic factors are, in most cases, relatively constant over short periods of time.

The results also show that there are no systematic differences regarding either price or quantity per prescription between health centers using fixed and open-ended budgets for pharmaceutical products, after controlling for several confounding factors. One possible explanation is that the prescribing physicians do not believe the fixed budgets to be credible, in the sense that they expect the county council to cover deficits or to cut future budgets in the case of surplus. Another possible explanation is that the new incentive structure is not clear to the prescribing physicians, or that physicians believe that any surplus generated could be used in ways that they dislike.

A potential problem is that introducing the new budgetary system with fixed budgets was voluntary for both the health centers and the local county council. As such, the health centers which entered an agreement with the county council to have fixed pharmaceuticals budgets might be centers which had low initial pharmaceutical expenditures. In addition, both of the health centers which were given fixed budgets are located in Skellefteå, a region were several other cost containment measures, as for example recommended lists, have been used. Such recommended lists often include not only which pharmaceutical product to use, but also recommendations regarding the optimal package size to prescribe in order to avoid overprescribing. This could mean that decreasing the pharmaceutical costs per prescription further, for example by introducing fixed budgets, might be difficult. As such, the only way to reduce pharmaceutical costs is to reduce the number of prescriptions made, as observed in the two health centers which were given fixed budgets.

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Appendix 1. Balancing tests

As mentioned above, to guarantee that propensity score matching will eliminate all the bias which the observables can account for, the observables must be independent of the treatment assignment conditioned on the propensity score. Several different test of this condition are proposed in the literature. The tests have different limitations and little is known about their statistic properties.

In this paper we employ two balancing tests. The first is a Hotelling T^2 test of the joint null hypothesis of equal means between the treatment groups and their control groups. This test can, however, fail to reject the null hypothesis even if X is dependent of treatment assignment, conditioned on the propensity score. One example of this could be if some variables have higher values in the treated group, compared to the matched group, for high values of the propensity score but lower values for low values of the propensity score.

The second balancing test we conduct is a regression based test described in Smith and Todd (2003b). For each variable used in estimating the propensity score we estimate the following regression:

$$\begin{aligned} X_k &= \beta_0 + \beta_1\hat{P}(X) + \beta_2\hat{P}(X)^2 + \beta_3\hat{P}(X)^3 + \beta_4\hat{P}(X)^4 \\ &\quad +\beta_5 D + \beta_6 D\hat{P}(X) + \beta_7 D\hat{P}(X)^2 + \beta_8 D\hat{P}(X)^3 + \beta_9 D\hat{P}(X)^4 + \varepsilon, \end{aligned}$$

where D is an indicator variable which takes the value one if the observation is treated. In an attempt to test whether D provides additional information about X_k conditioned on $P(X)$ we test the joint null hypothesis that all the coefficients of the terms involving D equal zero. A shortcoming to this test is that the choice of the order of the polynomial may affect the result.

Table A1 about here.

The results from the tests appear in Table A1. The Hotelling test reject balance for three of the subsamples for Moröbacke. The results from the regression based test show that between 10 and 33 per cent of the independent variables remain unbalanced conditioned on the propensity score in all models. Descriptive statistics, available from the authors upon request, indicate that the remaining bias is small in magnitude.

Table 1. Descriptive statistics by health centres

Variable	Category	Sample	Control	Burträsk	Moröbacke
Gender	Female	65.40	65.76	63.39	64.44
	Male	34.60	34.24	36.61	35.56
Area of	Skellefteå	99.30	99.32	99.04	99.33
residency	Other area	0.70	0.68	0.96	0.67
Patient-	Yes	48.73	48.53	45.11	52.6
doses	No	51.27	51.47	54.89	47.4
Patient's	Copay100	29.33	28.72	34.53	29.71
Payment	Copay50	14.10	14.02	15.48	13.65
	Copay25	12.65	12.71	12.83	12.1
	Copay10	11.07	11.18	10.06	11.01
	Copay0	29.89	30.57	24.35	29.98
	Unsub	2.19	2.09	2.08	2.93
	Free	0.69	0.71	0.68	0.61
Variable					
Age	Mean	71.07	71.64	72.51	66.26
	Std. dv.	18.76	18.49	17.07	20.89
Price/DDD	Mean	6.49	6.46	5.68	7.22
	Std. dv.	19.21	19.83	13.46	18.43
DDD:s/prescr.	Mean	41.67	41.91	44.91	37.76
	Std. dv.	56.24	56.17	58.08	55.20
Cost/prescr.	Mean	162.96	164.46	155.23	158.43
	Std. dv.	286.51	284.27	308.82	284.90
NOBS.		292 419	232 799	24 622	34 998

Table 2. Descriptive statistics for the treatment and control groups

		Pre treat. Burträsk		Pre treat. Moröbacke	
Variable		Burträsk	Control	Moröbacke	Control
Price/DDD	Mean	6.24	6.24	7.01	6.21
	Std. dv.	13.46	13.36	16.69	21.25
DDD:s/prescr.	Mean	46.42	41.38	35.94	41.47
	Std. dv.	63.10	55.97	51.87	56.10
Cost/prescr.	Mean	171.07	160.13	151.28	159.69
	Std. dv.	307.60	276.96	268.96	273.78
NOBS.		3 217	29 817	11 948	75 586
Prescr./month[1]		3 378.25	30 330.50	4 804.30	30 943.50

		Post treat. Burträsk		Post treat. Moröbacke	
Variable		Burträsk	Control	Moröbacke	Control
Price/DDD	Mean	5.59	6.50	7.33	6.58
	Std. dv.	13.46	20.61	19.27	19.11
DDD:s/prescr.	Mean	44.68	41.99	38.71	42.12
	Std. dv.	57.29	56.20	56.83	56.20
Cost/prescr.	Mean	152.85	165.10	162.14	166.75
	Std. dv.	308.94	285.32	292.76	289.14
NOBS.		21 405	202 982	23 050	157 213
Prescr./month[1]		3 317.31	31 764.15	4 595.65	31 887.75

[1]Prescr./month refer to the average numbers of prescriptions per month for the whole population and no standard deviations are therefore reported.

Table 3. Estimation results, Difference in Difference, Hypothesis 1

	Burträsk		Moröbacke	
	Price/DDD	DDD:s/Pres.	Price/DDD	DDD:s/Pres.
Sample	0.05	-1.76	-0.61	3.57*
	(0.37)	(1.99)	(0.46)	(1.27)
ATC-group A	-0.79	1.37	-1.11	-2.74
	(0.91)	(3.72)	(0.64)	(3.44)
ATC-group C	0.30	-0.56	1.24	-0.22
	(0.50)	(2.63)	(0.94)	(3.15)
ATC-group N	1.68*	-2.95	-0.85	1.52
	(0.64)	(1.92)	(0.55)	(0.99)

* indicates that the estimate are significantly different from zero at the five per cent significant level.

[1]The number of bootstrap repetitions used to calculate the confidence interval is 50.

[2] In the subsamples referring to Burträsk and in the subsamples referring to ATC-group C or N for Moröbacke 1 to 10 observations are not used in the estimation since they are of common support. The corresponding figures for the other subsamples in Moröbacke is 26 to 49.

Table 4. Estimation results, Difference in Difference, Hypothesis 2

	Burträsk		Moröbacke	
	Price/DDD	DDD:s/Pres.	Price/DDD	DDD:s/Pres.
Sample	0.15	-3.11*	-0.08	-3.55*
	(0.33)	(1.57)	(0.48)	(1.41)
ATC-group A	-0.21	-8.36*	0.90	8.35*
	(1.02)	(4.01)	(0.69)	(3.96)
ATC-group C	-0.27	-5.73*	-1.67	3.13
	(0.63)	(2.73)	(1.24)	(4.00)
ATC-group N	-0.91	0.46	0.00	-0.79
	(0.89)	(1.31)	(0.55)	(0.95)

See notes in Table 3.

Table 5. Estimation results, Difference in Difference, 2001

	Burträsk		Moröbacke	
	Price/DDD	DDD:s/Pres.	Price/DDD	DDD:s/Pres.
Sample	0.38	-4.25	-0.55	5.32*
	(0.46)	(2.19)	(0.72)	(1.58)
ATC-group A	-0.65	-1.16	-0.08	4.68
	(0.50)	(4.25)	(0.75)	(5.98)
ATC-group C	-0.01	-4.13	0.11	3.37
	(0.57)	(2.67)	(1.42)	(4.34)
ATC-group N	1.74*	-3.66	-2.12*	0.92
	(0.70)	(2.07)	(0.85)	(1.97)

See notes in Table 3.

Table A1. Results, Balancing Tests

	Burträsk		Moröbacke	
	H.T.	R.T.	H.T.	R.T.
Sample, pre	1.00	0.15	0.09	0.24
ATC-group A, pre	1.00	0.12	0.75	0.31
ATC-group C, pre	1.00	0.29	0.92	0.22
ATC-group N, pre	1.00	0.18	0.02	0.33
Sample, 2001 post	1.00	0.17	1.00	0.20
ATC-group A, 2001 post	0.99	0.30	1.00	0.18
ATC-group C, 2001 post	1.00	0.13	1.00	0.10
ATC-group N, 2001 post	0.88	0.15	0.97	0.16
Sample, Jan.02-Sep.02	0.98	0.29	0.06	0.28
ATC-group A, Jan.02-Sep.02	0.95	0.21	1.00	0.26
ATC-group C, Jan.02-Sep.02	1.00	0.13	0.93	0.15
ATC-group N, Jan.02-Sep.02	0.75	0.30	0.04	0.19
Sample, Oct.02-June03	0.27	0.22	0.00	0.27
ATC-group A, Oct.02-June03	0.97	0.18	0.87	0.30
ATC-group C, Oct.02-June03	0.75	0.22	1.00	0.15
ATC-group N, Oct.02-June03	0.75	0.18	0.05	0.31

Note: The P-values are presented for the Hotelling test (H.T). For the regression test (R.T.), the percentage of the variables which are unbalanced on the five per cent significant level are reported.