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**Working Paper**

## Does physician dispensing increase drug expenditures?

Discussion Papers, No. 13-03

**Provided in Cooperation with:**

Department of Economics, University of Bern

*Suggested Citation:* Kaiser, Boris; Schmid, Christian (2013) : Does physician dispensing increase drug expenditures?, Discussion Papers, No. 13-03, University of Bern, Department of Economics, Bern

This Version is available at:

<https://hdl.handle.net/10419/103842>

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**Does Physician Dispensing Increase  
Drug Expenditures?**

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13-03

July 2013

**DISCUSSION PAPERS**

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# Does Physician Dispensing Increase Drug Expenditures?

Boris Kaiser\*      Christian Schmid\*\*

July 9, 2013

## Abstract

We analyze whether the possibility for physicians to dispense drugs increases health care expenditures due to the incentives created by the markup on drugs sold. Using comprehensive physician-level data from Switzerland, we exploit the fact that there is regional variation in the dispensing regime to estimate policy effects. The empirical strategy consists of doubly-robust estimation which combines inverse-probability weighting with regression. Our main finding suggests that if dispensing is permitted, physicians produce significantly higher drug costs in the order of 30% per patient.

**Keywords:** health care costs; drug expenditures; physician dispensing; supply-induced demand; treatment effects.

**JEL:** I11, I18, C21

**Acknowledgements:** The authors are grateful to Sasis AG for providing access to the data, and in particular, to Oliver Grolimund for his help and assistance. The findings and conclusions expressed are solely those of the authors and do not represent the views of Sasis AG or any other third parties. For perceptive and valuable comments, we thank various conference and seminar participants in Bern (2013) and Aarhus (2013), Robert Leu, Michael Gerfin, Stefan Boes, Andreas Bachmann and Kaspar Wüthrich.

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# 1 Introduction

As many developed countries are faced with continuously rising health care costs, knowledge on inefficiencies in health care provision is very important in shaping reforms. One potential source of such inefficiencies are the conflicting incentives for physicians who act both as entrepreneur and as the patient's agent. Therefore, many countries maintain policies that attempt to disentangle the physician's double role. One important such policy, and the central issue of this paper, is the regulation of drug dispensing by physicians. While some Western countries including the United States, the United Kingdom, Japan and Switzerland (partly) allow physicians to dispense drugs<sup>1</sup>, several OECD countries fully ban physician dispensing. This is due to concerns that physicians are incentivized to induce demand or substitute towards more expensive drugs if they earn a profit margin on drug sales. However, the literature on the effects of physician dispensing on drug or health care expenditures is very small and empirical evidence is scarce (an overview is provided by Lim et al., 2009). Therefore, the objective of this paper is to examine empirically whether the possibility for physicians to dispense affects prescription drug expenditure. To our knowledge, we are the first to estimate the causal effect of physician dispensing on health care expenditures.

Examining the Swiss case is particularly interesting for at least three reasons. First, both dispensing rules (banned/allowed) co-exist in Switzerland because the rule is determined on the cantonal (i.e. state) level. This regional variation in the dispensing regime allows for the identification and estimation of policy effects. Second, drug prices are set by federal regulators, i.e. prices are the same throughout the country such that the comparison of drug costs is not confounded by regional price differentials. Moreover, fully regulated prices imply that the only channel through which physicians can affect drug expenditure is through quantities or the composition of drugs prescribed. Third, the coverage of mandatory health insurance is the same for the whole permanent resident population of Switzerland such that drug expenditures are not likely to be affected by insurance choice.<sup>2</sup>

For our empirical analysis, we study a comprehensive dataset on the prescribing costs of specialized physicians in Switzerland. Applying doubly-robust regression methods (see Imbens and Wooldridge, 2009), our benchmark estimates suggest that

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<sup>1</sup>In the United States for example, physicians are allowed to dispense drugs in most states with the exception of Massachusetts, Montana, New York, Texas, and Utah (see Rodwin and Okamoto, 2000).

<sup>2</sup>Further details on the Swiss health care system can be found in Health Care Systems in Transition report on Switzerland (European Observatory on Health Care Systems, 2000) and The OECD Review of Health Systems: Switzerland (OECD/World Health Organization, 2011).

physician dispensing increases drug expenditures per patient by roughly 30% (CHF 75). In addition, total non-drug expenditures increase as well by about 20% (CHF 100) per patient. These results suggest that, in the context of policy-induced health care expenditures, drugs costs and non-drug costs are complementary. This lends support to the notion that over-prescribing raises consultation costs through an increase in the total time of treatment. On the whole, our findings suggest that the dispensing policy clearly affects the prescribing behavior of physicians in ways that affect health care expenditures.

The remainder of this paper is organized as follows. Section 2 reviews the relevant literature and Section 3 provides information on the institutional background in Switzerland. In Section 4, we discuss identification and estimation of treatment effects in our framework. Section 5 contains the empirical analysis: we explain the construction of the dataset, present descriptive statistics, perform robustness checks with regard to the common support, and discuss the main results (in Section 5.4). Section 6 contains some concluding remarks.

## 2 Related Literature

In spite of a large literature on physician behavior and demand inducement (see, e.g., Labelle et al., 1994; McGuire, 2000), comparatively little evidence exists regarding their dispensing practice. The most thoroughly analyzed aspect of physician behavior related to drugs is their prescription practice in terms of generic and trade-name medicine (see, e.g., Hellerstein, 1998; Coscelli, 2000; Lundin, 2000). However, the results in this literature are mostly based on data from countries without physician dispensing. Three exceptions are Liu et al. (2009), Rischatsch et al. (2009), and Iizuka (2007, 2012), where the authors find for Taiwan, Switzerland and Japan, respectively, that markup differentials between generic and trade-name drugs affect physicians' dispensing behavior. In addition, Park et al. (2005) find that the introduction of a dispensing ban in South Korea led to a reduction in antibiotic prescriptions. A similar result is found by Filippini et al. (2013) who compare antibiotic prescriptions between dispensing and non-dispensing doctors in Switzerland. While these results consistently suggest that physicians respond to financial incentives, no conclusions can be drawn as to how physician dispensing affects aggregate health care expenditures.

A much smaller strand of the literature focuses on the impact of physician dispensing on health care expenditures. Chou et al. (2003) analyze the impact of the dispensing ban that was implemented sequentially in Taiwan and find a substan-

tial decrease in the drug expenditures per visit. However, the effect on aggregate health care expenditures remains unclear due to a simultaneous increase in consultation fees. Baines et al. (1996) and Dummermuth (1993) find for Lincolnshire (United Kingdom) and two Swiss cantons, respectively, that dispensing physicians trigger more drug expenditures per patient than their non-dispensing counterparts. However, these studies essentially compare differences in means and do not control for compositional differences in the patient populations of dispensing and non-dispensing physicians. In a more recent study for Switzerland, Beck et al. (2004) estimate the effect of the cantonal (i.e. state) dispensing policy controlling for other determinants of drug demand. Using canton-level data, the authors find that physician dispensing considerably increases drug expenditures. However, comparisons only on the canton level are problematic due to small sample size and unobserved canton-specific heterogeneity.

While some of the mentioned studies credibly suggest that financial incentives affect physicians' prescription choice (e.g. generic vs. brand-name), the question as to how dispensing affects health care expenditures has largely remained unanswered due to inconclusive empirical evidence. Our main contribution to the literature is therefore to estimate causal effects of the dispensing policy rule (allowed vs. banned) on drug as well as non-drug expenditure. Such estimates provide direct information on the health care costs of physician dispensing and are therefore of high relevance for health care policy. In addition, they add to the literature on physician behavior in the presence of monetary incentives.

### **3 Drug Dispensing and Pricing in Switzerland**

The Swiss pharmaceutical market is highly regulated on the federal level with respect to drug approval, pricing and dispensing. In principle, all licensed pharmacists are allowed to sell prescription drugs, but they cannot issue prescriptions. In contrast, all medical doctors can issue prescriptions by federal regulation, but cantons either prohibit or (partially) permit doctors to dispense drugs. In the time period considered in this paper (2008-2010), five cantons prohibit physician dispensing completely and four cantons have policies that result in a de facto ban. Thirteen cantons allow physician dispensing without any restrictions and four cantons partially allow it in rural areas or communities with low pharmacy densities (see Appendix A.1 for further details). These canton-specific regulations did not change during the period of interest and had been in place since at least the 1980s. Thus, the observed variation in the dispensing regime is rooted in historical differences between cantonal health

care policies.

It is important to note that, while German-speaking cantons have some variation in the dispensing rule, all cantons in the French and Italian speaking parts of Switzerland ban physician dispensing. This means that we cannot control for language region in the estimation procedures. As a consequence, we restrict our analysis to the German-speaking area given the evidence of culture-specific differences in health care consumption and expenditures (see, e.g., Vatter and Ruefli, 2003; Crivelli et al., 2006; Reich et al., 2012).

An important fact about the Swiss drug market is that ex-factory prices as well as retail prices of all drugs covered by mandatory health insurance are determined by federal regulations. The price of pharmaceuticals charged by a dispensing physician corresponds to the retail price plus 2.5% VAT, such that his gross profit margin corresponds to the difference between the retail price and the ex-factory price. For example, for drug with an ex-factory price of CHF 100, the physician charges the patient CHF 128, thus earning a markup of CHF 28 per package (see Appendix A.2). Another crucial feature is that the absolute markup is increasing in the ex-factory price.

In addition, pharmacies charge higher prices than physicians due to two additional lump-sum fees. Given this price setting regime, the pharmacist earns an excess markup compared to the physician (see Appendix A.2 for further details). To appropriately compare the costs between dispensing and non-dispensing physicians, our analysis focuses on *real* expenditures, which means that all drug costs are based on retail prices excluding the pharmacists' excess markup.<sup>3</sup> The reason is that the effect of dispensing on real costs is more informative about physician behavior in the face of monetary incentives.

## 4 Methodology

### 4.1 Identification of Treatment Effects

To estimate the effect of the dispensing policy on drug expenditures, we use the potential-outcomes framework which has become standard in causal analysis (Rubin, 1974). Consider a large population of physicians indexed by  $i = 1, 2, \dots, N$ . Let  $D_i$  be an indicator variable with  $D_i = 1$  if physician  $i$  is allowed to dispense drugs directly and  $D_i = 0$  if physician  $i$  is forbidden to do so. We regard the permission

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<sup>3</sup>The term “real” refers to constant prices in the *cross-section* dimension. However, we also adjust costs across different years for inflation, such that “real” also applies to the *time-series* dimension. We explain how we adjust for pharmacy prices in the Online-Appendix C.

to dispense as the “treatment” and thus refer to the two groups as treatment group and control group, respectively. Denote  $Y_{1i}$  and  $Y_{0i}$  the potential expenditures that physician  $i$  triggers if he is either dispensing or non-dispensing, respectively. Since we only observe  $Y_{1i}$  if  $D_i = 1$  and  $Y_{0i}$  if  $D_i = 0$ , the realized outcome may be written as  $Y_i = D_i Y_{1i} + (1 - D_i) Y_{0i}$ .

The quantity of interest is the average causal effect of dispensing on drug expenditures in the physician population

$$\tau = E[Y_{1i} - Y_{0i}], \quad (1)$$

commonly known as the population average treatment effect (ATE). This effect is informative about the average costs associated with the policy that admits dispensation for all physicians relative to the policy that bans dispensation for all physicians. Moreover, it is instructive to consider the average effect for the group of dispensing physicians:

$$\rho = E[Y_{1i} - Y_{0i} | D_i = 1]. \quad (2)$$

This quantity is referred to as the population average treatment effect on the treated (ATT). This effect is informative about the cost consequences of the *current* policy relative to the policy that bans dispensation for all physicians. Obtaining consistent estimates of the objects in (1) and (2) is the main goal of the empirical analysis in this paper.

The main issue with treatment effects is to ensure identification. Let  $X_i$  denote a vector of covariates capturing personal characteristics of physician  $i$  and various supply and demand conditions at physician  $i$ 's location (e.g. physician density, health status and insurance coverage, demographic and socio-economic factors). A prominent result from the causal inference literature states that (1) and (2) are identified under the following assumptions (cf. Imbens, 2004):

$$(Y_{i0}, Y_{i1}) \perp\!\!\!\perp D_i | X_i = x \quad (3)$$

$$0 < p(x) < 1 \quad \forall x \in \mathcal{X}, \quad (4)$$

where  $p(x) \equiv P(D_i = 1 | X_i = x)$  is the propensity score and  $\mathcal{X} \subset \mathbb{R}^k$  is the support of  $X_i$ . The *conditional independence assumption* in (3) states that once we control for the characteristics in  $X_i$ , treatment status is as good as randomly assigned and thus independent of potential outcomes. Put differently, comparing observations with the same covariate values, any systematic difference in outcomes across treatment and control group is due to the dispensing policy  $D_i$ . Although (3) is intestable, we argue



that our setting renders (3) likely to be satisfied. First, our empirical framework has the advantage that the dispensing policy is determined by institutions on the regional level, such that the possibility for physicians to influence their treatment group assignment is strongly restricted. At the same time, other institutional features such as drug prices and health insurance regulations are the same throughout the country. Second, we can control for several personal characteristics of physicians. This eliminates any potential bias that arises if these characteristics affect both practice style *and* treatment assignment. Finally, as shown in the empirical analysis, we can effectively control for health care market conditions in the practice location. This ascertains that only dispensing and non-dispensing doctors are compared that face similar supply and demand conditions in the local health care market.

The *common support assumption* in (4) implies that, for all possible values of  $X_i$ , we can match dispensing doctors with non-dispensing doctors. This assumption is testable and we will show in the empirical analysis how the sample is restricted to the common support if (4) is violated for a subset of the observations.

As shown for example in Imbens (2004), assumptions (3) and (4) imply that the ATT and the ATE are identified as follows:

$$\rho = E[\Delta(x)|D_i = 1] \tag{5}$$

$$\tau = E[\Delta(x)] \tag{6}$$

where  $\Delta(x) \equiv E[Y_i|X_i = x, D_i = 1] - E[Y_i|X_i = x, D_i = 0]$ . Given this identification result, the remaining task is to estimate the conditional expectation functions with appropriate econometric techniques.

## 4.2 Estimation

There are several empirical strategies available for estimating treatment effects under assumptions (3) and (4). We choose to focus on a method that combines regression with propensity score weighting, also referred to as "doubly robust" regression. The doubly robustness property is particularly appealing because, as discussed below, the estimator is consistent under two separate sets of assumptions (Robins et al., 2007). Imbens and Wooldridge (2009) recommend this approach explicitly because it is found to perform especially well if covariate distributions differ substantially across groups.<sup>4</sup>

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<sup>4</sup>Note that we do not consider matching methods because we have clusters of repeated observations. Unfortunately, for matching estimators, the bootstrap is generally not valid (Abadie and Imbens, 2008) and analytical variances of matching estimators are difficult to compute for

Estimation proceeds in several steps. First, the propensity score is estimated with a binary probability model. The predicted values,  $\hat{p}(X_i)$ , are then used to compute inverse-probability weights (IPW):  $\hat{\lambda}^\rho(X_i) = \left( D_i + \frac{\hat{p}(X_i)}{1-\hat{p}(X_i)}(1 - D_i) \right)$  for the ATT and  $\hat{\lambda}^\tau(X_i) = \left( \frac{D_i}{\hat{p}(X_i)} - \frac{1-D_i}{1-\hat{p}(X_i)} \right)$  for the ATE. For the second step, define the parametric regression models for the treatment group and the control group by  $m(X_i, \beta^1)$  and  $m(X_i, \beta^0)$ , respectively. The doubly robust regression estimator  $\hat{\beta}^{d,e}$  is obtained by solving the following IPW-augmented moment conditions:

$$\sum_{i:D_i=d}^{N_d} \hat{\lambda}^e(X_i)[Y_i - m(X_i, \hat{\beta}^{d,e})]X_i = 0, \text{ for } d_i = \{0, 1\}, e = \{\rho, \tau\} \quad (7)$$

Finally, the estimated coefficients are used to estimate the ATT and the ATE, respectively, as follows:

$$\hat{\rho} = \frac{1}{N_1} \sum_{i:D_i=1}^{N_1} m(X_i, \hat{\beta}^{1,\rho}) - m(X_i, \hat{\beta}^{0,\rho}) \quad (8)$$

$$\hat{\tau} = \frac{1}{N} \sum_{i=1}^N m(X_i, \hat{\beta}^{1,\tau}) - m(X_i, \hat{\beta}^{0,\tau}) \quad (9)$$

Using the appropriate propensity-score weights  $\hat{\lambda}^e(X_i)$  in the regressions ensures that treatment effects are consistently estimated under two separate sets of assumptions. That is, consistency is achieved if *either* the outcome model is correctly specified (in which case  $\text{plim}(\hat{\beta}^{d,e}) = \beta^d$ ), *or* the propensity score model is correctly specified (in which case  $\text{plim}(\hat{\lambda}^e(X_i)) = \lambda^e(X_i)$ ), *or* both. See for example Robins et al. (2007) or Wooldridge (2007) for the formal derivation and a detailed discussion of the doubly robustness result. The main advantage of this type of estimator is that it guards against misspecification more effectively than traditional methods based on the propensity score alone or on regression alone. In practice, the outcome model is normally specified as a linear model such that (7) becomes a weighted least squares (WLS) estimator. If the outcome model is assumed to be exponential, (7) is the weighted Poisson quasi-maximum-likelihood estimator (WPQML), see Wooldridge (2007).<sup>5</sup> We will consider both of these specifications in our analysis. Finally, inference is based on the bootstrap, which takes into account the fact that the

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the case of clustered data. In addition to the cluster problem, matching on covariates has the disadvantage that the asymptotic bias increases in the number of continuous covariates (Imbens, 2004). Matching on the propensity score has the drawback that analytical standard errors do not exist for the case where the propensity score is estimated.

<sup>5</sup>This estimator is equivalent to the generalized linear model (GLM) with log-link and Poisson family augmented with the appropriate weights.

propensity score in the first step is estimated.

## 5 Empirical Analysis

The empirical analysis is structured in the following manner. We first describe the construction of the dataset from the various sources we draw on. Second, we briefly present descriptive statistics on outcomes and covariates. Third, as a preliminary robustness check, we determine the common support of the covariate distributions. Finally, we estimate and discuss the effect of the dispensing policy on health care expenditure.

### 5.1 Data Sources and Variables

We have access to physician-level data for the period 2008-2010 provided by the operator of the nationwide database of Swiss health insurers (Sasis AG). We have expenditure data on medical specialists delivering outpatient care in private practices. The dataset basically aggregates the mandatory health insurance claims of nearly all Swiss health insurance companies.<sup>6</sup> The included insurance companies cover about 90% of the permanent resident population of Switzerland. Although we do not have individual prescription items as the unit of observation, we estimate that the data aggregates about 30 million prescriptions annually.<sup>7</sup> We consider this comprehensiveness an important advantage of our data because problems resulting from adverse selection into *individual* insurance firms are minimized. Two points about the data on prescription drugs should be added. First, as is mostly the case with insurance claims data, out-of-pocket expenditure that patients do not report to their insurer is not included. This is most likely for patients who only incur small amounts of health care costs in a given year and are therefore unlikely to exceed the deductible.<sup>8</sup> Second, although the vast majority of drugs covered by mandatory health insurance require prescriptions, there are some exceptions; roughly 8% are over-the-counter (OTC) products.<sup>9</sup>

In the ensuing discussion on the construction of the dataset, the reader is referred to Table A.3 in Appendix B for a detailed exposition of all variables. For each

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<sup>6</sup>Three out of 61 insurance companies are not included; these are Assura, EGK and SLKK.

<sup>7</sup>This estimate is based on detailed insurance claims data from one major health insurer with more than half a million customers. The estimate is based on the assumption that the average number of prescriptions per person is the same for the remaining population.

<sup>8</sup>Based on national accounting data on the consumption of pharmaceuticals, we estimate that our data contains at least 84% of the relevant drug expenditures.

<sup>9</sup>These include for example painkillers with low dosage or certain herbal products.

physician, the data contains two separate components of total annual prescribing costs triggered. First, we observe **direct costs** (dispensing costs) which arise when a doctor directly sells drugs to his patients. Second, we observe **indirect costs** which arise when a doctor issues prescriptions that are filled in pharmacies. In other words, the cost for every prescription drug sold in pharmacies can be attributed to the physician who originally issued the prescription. This feature of the data is of paramount importance; without it, we would not be able to appropriately compare expenditures between dispensing and non-dispensing physicians. Besides drug expenditures, we also observe non-drug medical costs, the number of patients, the patients' average age, and the distribution of office visits across age groups (5-year intervals) and gender.

A further advantage of the data is that every physician can be identified through his global identifier number (GLN). The GLN can be used to match the expenditure data to individual characteristics of physicians taken from the register of medical professionals (MedReg), which is publicly accessible on the internet.<sup>10</sup> Variables include gender, nationality, age and experience. The MedReg database also includes information on the treatment indicator,  $D_i$ , i.e. the dispensing permission (cf. Table A.3 in Appendix B).

Since the dispensing policy varies by region, we want to account for the fact that physicians face different demand and supply conditions depending on the local health care market in which they operate. To do this, we exploit the fact that, for each physician, we observe the distribution of office visits across patients' place of residence (municipality). Since we have this detailed knowledge on a physician's catchment area, we can effectively control for location-specific determinants of the health care market. On the supply side, we take into account the local physician density. On the demand side, we control for the health status and insurance choice of the local population and for demographic and socio-economic characteristics that are relevant for consumption of prescription drugs. The latter include, for example, the degree of urbanization, average income, unemployment, immigration and education level. For each physician, we average all these variables across municipalities using the number of visits from these municipalities as a weight. Since a physician may draw patients from a much larger area than from just "his" municipality, the weighted averages effectively capture the characteristics of the location-specific health care market that are relevant for a particular physician.

In the empirical analysis, we consider medical specialists but exclude psychiatrists because the nature of their health care provision, the forms of treatment, and

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<sup>10</sup>see <http://www.bag.admin.ch/themen/berufe/00411/>

the role of medication are quite different compared to other physicians. In terms of further sample selection, we decide to drop physicians with less than 10 patients per year (5% of sample).<sup>11</sup> Finally, after combining the datasets and applying the selection criteria outlined above, we are left a vast majority (roughly 90%) of all specialized physicians running independent practices in German-speaking Switzerland.

## 5.2 Descriptive Statistics

Table 1 presents descriptive statistics for our dataset. Expenditure variables are annual measures expressed in constant 2010 Swiss francs (CHF). As explained previously, we focus on real expenditures (in the cross-section sense), i.e. all costs are expressed in retail prices, to facilitate comparability.

In the top panel of Table 1 we see that dispensing doctors have a slightly higher drug-intensity of treatment than non-dispensing doctors given the larger cost share of drugs. Furthermore, total annual drug costs are higher by about 50%. One important issue with total cost measures is that, taken at face value, they are hard to compare because systematic differences in the composition of the two groups remain unaccounted for. We see for example that dispensing physicians have larger practice size, i.e. more patients on average. For these reasons, it makes sense to consider expenditures *per patient*.<sup>12</sup> Table 1 reveals that dispensing physicians also have higher drug expenditures per patient compared to their non-dispensing counterparts. Finally, it is important to note that the shares of dispensing costs in total drug costs are neither zero nor one. This reflects the fact that non-dispensing physicians are normally allowed to dispense limited amounts of drugs in case of emergencies. Conversely, dispensing physicians may have a limited range of pharmaceuticals in stock such that patients must collect unavailable products at pharmacies.

Turning to the covariates, we see that in areas with dispensing, physicians are more often male, foreign and somewhat younger. In contrast, the average age and gender structure in the patient pool appears quite similar across groups. The most striking differences across groups can be found in the bottom half of Table 1. For example, regions where dispensing is allowed have considerably smaller physician

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<sup>11</sup>Many of these doctors only report a single patient. According to Sasis AG, these are likely to be physicians who have retired or quit working for other reasons, but who are still allowed to prescribe drugs to themselves and/or family member and then file these insurance claims to their health insurer.

<sup>12</sup>We do not consider costs per visit because the number of visits in a calendar is directly affected by the physician's behavior with regard to the inducement of follow-up visits. In contrast, the number of patients should not be affected by this.

Table 1: Descriptive Statistics (2008-2010)

	non-dispensing $D_i = 0$		dispensing $D_i = 1$	
	mean	st.dev.	mean	st.dev.
<u>real annual expenditure per physician</u>				
cost share of drugs	0.203	0.171	0.213	0.169
drug costs	97974.228	188175.201	150256.099	313663.858
drug costs per patient	243.156	461.466	274.059	704.880
non-drug costs	259054.027	265050.604	376394.745	365633.773
non-drug costs per patient	919.262	868.270	620.105	756.306
share of dispensed drugs	0.120	0.154	0.865	0.194
<u>physician characteristics</u>				
female	0.311		0.243	
German nationality	0.090		0.126	
other foreign nationality	0.017		0.018	
age	52.910	8.800	51.052	8.211
work experience	17.339	8.915	16.378	8.005
<u>patient pool variables</u>				
# patients	596.860	725.702	942.422	891.956
# visits	1699.686	1657.619	2319.689	1893.149
# visits per patient	5.670	5.008	3.550	2.988
patients' average age	48.982	11.083	50.244	9.102
cases aged > 80y	0.052	0.071	0.060	0.062
cases aged 66 – 80y	0.176	0.150	0.205	0.138
cases aged < 25y	0.128	0.186	0.119	0.128
cases of males	0.362	0.202	0.361	0.203
<u>characteristics of the local health care market</u>				
physician density	3.793	1.305	3.031	1.299
share with very good subj. health	0.206	0.028	0.219	0.035
share with good subj. health	0.674	0.027	0.671	0.036
share with very good obj. health	0.313	0.028	0.325	0.039
share with good obj. health	0.351	0.025	0.354	0.035
share with chronic health problems	0.503	0.035	0.490	0.046
share with high health risk	0.273	0.027	0.253	0.036
share of immigrants	0.217	0.056	0.206	0.045
fraction of urban area	0.354	0.153	0.271	0.107
net income per capita in 1000	80.457	10.824	81.905	15.918
unemployment rate	2.767	0.517	2.506	0.520
share of medium educated	0.500	0.036	0.512	0.023
share of high educated	0.231	0.041	0.212	0.042
population density	0.408	0.732	0.006	0.641
share with managed-care plan	0.174	0.037	0.169	0.055
share with supplementary insurance	0.554	0.037	0.568	0.043
share with high deductible	0.431	0.043	0.429	0.043
# observations	6162		4544	

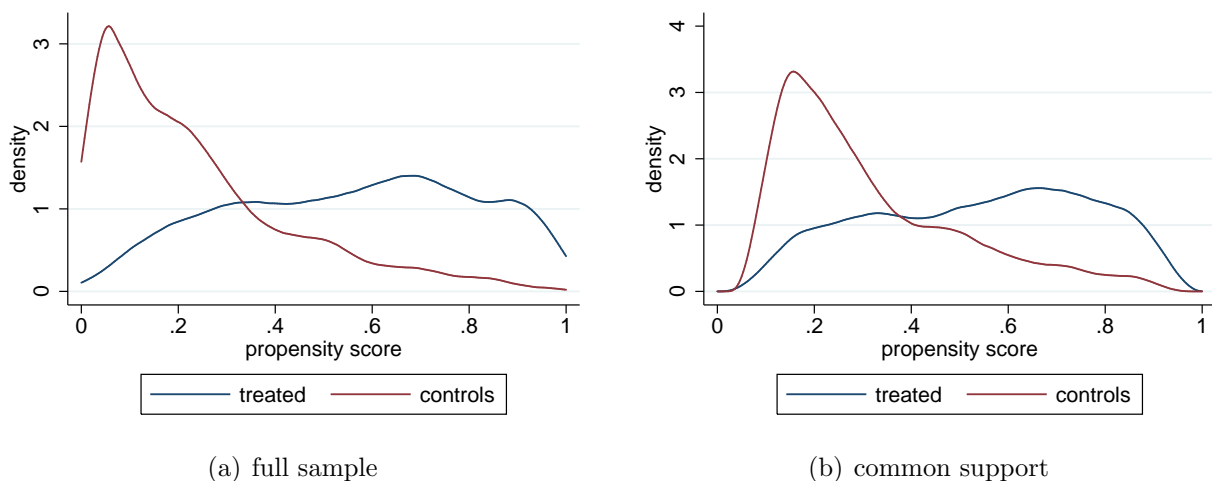
Notes: Based on the sample of physicians in German-speaking Switzerland for the period 2008-2010. All variables are measured annually on the physician level. Costs are in 2010 Swiss francs and measured in retail prices as charged at the doctor's office.

density. In addition, the local population in these regions are on average somewhat healthier, less urbanized and have smaller shares of immigrants and unemployed. The next section investigates these differences in covariates more closely.

### 5.3 Assessing Common Support

Since treatment effects (policy effects) can only be estimated for the common support, assessing the overlap of covariate distributions between treatment and control groups is of great importance. A useful starting point is to inspect scale-free normalized differences (instead of t-statistics), which indicate how difficult it is to adjust for differences in covariates when estimating policy effects.<sup>13</sup> Imbens and Wooldridge (2009) state that, as a rule of thumb, normalized differences exceeding 0.25 in absolute value should invoke caution when using simple regression methods. Table 2 shows that for some covariates, normalized differences clearly exceed one quarter in absolute value such as for the number of patients, the physician density, and several of the socio-economic variables. These findings suggest that appropriately adjusting for differences in covariates might be difficult when estimating policy effects, and moreover, attention must be paid to missing overlap in the covariate distributions. To proceed further, we estimate the propensity score with a logit model to investigate the group-specific densities of the propensity score values, see Figure 1.

Figure 1: Kernel Densities of Estimated Propensity Scores



As we can see from the high probability mass at the boundaries of the  $[0, 1]$

<sup>13</sup>The normalized difference of covariate  $j$  is computed as  $(\bar{x}_{j1} - \bar{x}_{j0})/\sqrt{\hat{V}_{j1} + \hat{V}_{j0}}$ , where  $\bar{x}_{jd}$  and  $\hat{V}_{jd}$  are the sample mean and the sample variance, respectively, estimated in the subsample with  $D_i = d \in \{0, 1\}$ . Imbens and Wooldridge (2009) argue that t-statistics are not useful in this context because the problem of differences in covariates is invariant to sample size.

Table 2: Normalized Differences of Covariates Means (2008-2010)

	full sample	c.s. sample		full sample	c.s. sample
<u>physician characteristics</u>			share of immigrants	-0.108	-0.049
female	-0.018	0.002	fraction of urban area	-0.381	-0.266
German nationality	0.073	0.062	net income per capita in 1000	0.095	-0.025
other foreign nationality	-0.046	-0.026	unemployment rate	-0.309	-0.179
age	-0.147	-0.103	share of medium educated	0.201	0.160
work experience	-0.124	-0.089	share of high educated	-0.269	-0.243
<u>patient pool variables</u>			population density	-0.340	-0.245
# patients	-0.269	-0.243	share with special managed-care plan	-0.078	0.014
# visits per patient	0.157	0.126	share with supplementary insurance	0.254	0.158
patients' average age	-0.172	-0.113	share with high deductible	0.029	0.002
cases aged > 80y	-0.107	-0.042	<u>speciality type</u>		
cases aged 66 – 80y	-0.111	-0.070	gynecologist	0.006	-0.014
cases aged < 25y	0.171	0.128	angiologist	-0.013	-0.021
cases by males	-0.004	0.002	cardiologist	-0.061	-0.032
<u>characteristics of the local health care market</u>			invasive specialist	0.018	0.006
physician density	-0.362	-0.261	other type of specialist	0.119	0.127
share with very good subj. health	0.276	0.201	$\hat{\alpha}$		0.099
share with good subj. health	-0.070	-0.044	# control obs. (non-dispensing)	6162	5339
share with very good obj. health	0.211	0.194	# treated obs. (dispensing)	4544	3889
share with good obj. health	0.109	0.058	# control individuals (non-dispensing)	2215	1908
share with chronic health problems	-0.209	-0.130	# treated individuals (dispensing)	1658	1416
share with high health risk	-0.402	-0.334			

*Notes:* common support refers to the subsample of observations inside the interval  $[\hat{\alpha}, 1 - \hat{\alpha}]$ . The normalized difference of covariate  $j$  is computed as  $(\bar{x}_{j1} - \bar{x}_{j0}) / \sqrt{\hat{V}_{j1} + \hat{V}_{j0}}$ , where  $\bar{x}_{jd}$  and  $\hat{V}_{jd}$  are the sample mean and the sample variance, respectively, estimated in the subsample with  $D_i = d \in \{0, 1\}$ . *Data Sources:* Sasis AG, MedReg, SHS, SFSO, SFFA.

interval in Figure 1 (a), there are a fair number of observations for which treatment status is almost perfectly predicted. That is, these observations are in areas of the covariate space where there are no units of the opposite group and therefore lie outside the common support. To restrict the sample to the common support, we follow the approach recently proposed by Crump et al. (2009). Compared to other methods where researchers have to choose trimming parameters in an ad hoc fashion (cf. Smith and Todd, 2005), this method has the advantage that it is purely data-driven and straightforward to implement because it is solely based on the marginal distribution of the propensity score. The aim is to compute the cut-off parameter  $\alpha$  that solves the minimization problem derived in Crump et al. (2009, p. 193) and then to restrict the sample to those observations satisfying  $\hat{p}(x) \in [\hat{\alpha}, 1 - \hat{\alpha}]$ . Using their algorithm, we find  $\hat{\alpha} = 0.099$  and consequently about 14% of the sample are dropped. Given the common-support sample, the propensity score model is then re-estimated (cf. Crump et al., 2009). For detailed results of the propensity score



estimation, the reader is referred to the Online-Appendix C.

The effect of this procedure is illustrated in two ways. First, the re-estimated propensity score should no longer include values close to zero and one. Indeed, Figure 1 (b) shows that the probability mass close to the boundaries is practically reduced to zero such that the common support assumption now seems likely to be satisfied. Second, the covariate distributions of the treatment and the control group become more balanced as can be seen from normalized differences calculated in the common-support sample in Table 2. Comparing values, we see that, as expected, the magnitude of normalized differences becomes sizably smaller (and thus covariate distributions more balanced) when moving from the full sample to the common-support sample. Importantly, the largest values in the full sample (e.g. population density, physician density, share of urban area, unemployment rate), drop markedly by about 25 to 50 percent. Overall, this means that adjusting for differences in covariates will be much less critical when estimating treatment effects.

## 5.4 Policy Effects of Dispensing

We now proceed to estimate the effect of the dispensing policy on the health care expenditures triggered by physicians. As mentioned, we use expenditures *per patient* as dependent variables to reduce the variation induced by differences in practice size. Note further that we do not take logarithms because it changes the interpretation of average policy effects in an undesired way.<sup>14</sup> The set of covariates used in the doubly robust regression methods is essentially the same as the one presented in Table 2 plus year-specific intercepts, but without the number of patients (because we consider per-patient outcomes). Due to the fairly large number of covariates, only main effects are included in the specification.<sup>15</sup> Standard errors are obtained by 1000 bootstrap iterations, taking into account the clusters of repeated observations across years and the sampling bias resulting from the first-step estimation of the propensity score. When assessing the two different specifications of the outcome model, we have to rely on appropriate goodness-of-fit measures because the linear and exponential models are non-nested. Applying the Bayesian Information Criterion (BIC), the exponential model is found to fit the data better, which implies that the WPQML

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<sup>14</sup>When outcomes are in logs, treatment effects measure the approximate percentage difference in *geometric* means, and not in *arithmetic* means. From a policy perspective, the geometric-mean interpretation is not meaningful at all.

<sup>15</sup>We also tested extended specifications including higher-order terms. While the qualitative results did not change, estimates were found to be less precise.

is preferable relative to the WLS.<sup>16</sup> For detailed estimation results, the reader is referred to Appendix C.

The main results are presented in Table 3. We start by discussing the effect

Table 3: Policy Effects of Dispensing, 2008 – 2010

	Drug Costs per Patient			Non-drug Costs per Patient		
	Coef.	S.E.	% of mean	Coef.	S.E.	% of mean
<b>Unadjusted Difference</b>	56.03**	(23.03)	25.89%	88.54**	(18.98)	21.24%
<b>Average Treatment Effect (ATE)</b>						
WLS	138.83**	(29.74)	64.15%	106.99**	(13.73)	25.66%
WPQML	96.04**	(21.67)	44.38%	105.62**	(12.80)	25.34%
<b>Average Treatment Effect on the Treated (ATT)</b>						
WLS	76.48**	(25.69)	30.74%	98.82**	(17.08)	21.11%
WPQML	71.87*	(32.04)	28.88%	97.67**	(16.78)	20.87%

*Notes:* The estimation sample consists of all physicians in the years 2008-2010 that lie in the common support. Outcomes are measured annually on the physician level. The set of covariates used is the one presented in Table 2 including year-specific intercepts. Standard errors are bootstrapped and clustered at the physician level using 1000 replications. Significance levels: \*\*  $p < 0.01$  and \*  $p < 0.05$ . *Data Sources:* Sasis AG, MedReg, SHS, SFSO, SFFA.

of dispensing on physicians’ drug costs displayed on the left-hand side. First, the estimated policy effects imply that dispensing raises a physician’s total annual drug expenditure in per-patient terms by roughly CHF 75 (30%) in the population of dispensing physicians (ATT). The estimates from WLS and WPQML are very similar in terms of magnitude. In the overall physician population (ATE), effects are larger and differ somewhat between estimators. (Note that the WPQML estimate is more precise.) These results lend support to the notion that physicians prescribe larger amounts or more expensive drugs when allowed to dispense. Next, it is interesting to note that all estimated policy effects are larger than the observed difference in means which corresponds to a negative selection effect (selection effect = unadjusted difference – ATT). That is, if we were to estimate the policy effect naively by the raw difference in means, we would underestimate the true effect of dispensing. Put differently, the interpretation of the negative selection effect is that regions with dispensing would have lower average costs in the absence of dispensing than regions where dispensing is banned under the current regime. The normalized differences (Table 2) suggest that health care market conditions are likely to be the most rele-

<sup>16</sup>The BIC is defined as  $\ln\left(\frac{SSR}{N}\right) + \frac{k \ln(N)}{N}$ . For drugs per patient, for instance, estimation of the outcome model in the two subsamples ( $D_i = 0$ ;  $D_i = 1$ ) produce BICs of (12.55; 12.41) in the exponential specification and (12.61; 13.06) in the linear specification. Since the former has smaller values in both subsamples, the WPQML estimator is preferred to the WLS estimator.

vant “drivers” of the selection effect. Furthermore, there is some discrepancy in the relative magnitude of the ATE and the ATT which is an indication for treatment effect heterogeneity among dispensing and non-dispensing physicians. On the whole, the findings discussed so far suggest that physicians respond quite strongly to the financial incentives created by the markup they earn when dispensing prescription drugs. In this sense, our results are in line with Liu et al. (2009), Rischatsch et al. (2009), and Iizuka (2007, 2012), who find that the prescription choices of dispensing physicians are influenced by the markup (see Section 2).

Besides drug costs, we also examine whether dispensing affects non-drug medical expenditure, as presented on the right-hand side of Table 3. We find that dispensing also increases non-drug costs by around 20-25%. All estimates are significant on the 1% level and very similar across the two estimators. These results point to a complementary relationship between drug expenditure and other forms of medical expenditure *in the context of policy-induced health care expenditures*. (In our extended analysis, we find that dispensing does not significantly affect the cost share of drug expenditures, see the left-hand side of Table 4. This is the same as saying that the relative effect on drug costs is not significantly different from the relative effect on non-drug costs.) There are two explanations for this complementarity. First and more likely, prescribing additional medication is likely to increase the total time of treatment. This may come about because physicians must spend additional time entering the prescription information into the patient’s record, fetching the drug from storage, informing the patient on recommended drug intake and potential side effects, and adding the prescription to the patient’s invoice. This automatically results in higher non-drug costs because consultation costs are increasing in consultation time. Second, a dispensing physician may be tempted to perform additional tests or examinations he would not otherwise perform in order to “justify” the dispensing of additional prescription drugs to the patient. While this represents a rather severe form of malpractice, we consider this second explanation to be less likely than the first explanation.

To explore these issues somewhat further, we test whether the reported increase in non-drug expenditure may be due to the possibility that dispensing physicians induce more visits than non-dispensing physicians, *ceteris paribus*. The results on the right-hand side in Table 4 suggest that the dispensing status does not significantly affect the number of visits per patient. Thus, we can exclude the possibility that the positive effect on non-drug expenditures is due to induced follow-up visits. This implies that dispensing leads to more medical services *per visit*, which lends support to the arguments presented above.

Table 4: Policy Effects of Dispensing, 2008 – 2010

	Share of Drug Costs			Visits per Patient		
	Coef.	S.E.	% of mean	Coef.	S.E.	% of mean
<b>Unadjusted Difference</b>	-0.020**	(0.006)	-9.25%	-0.007	(0.059)	-0.28%
<b>Average Treatment Effect (ATE)</b>						
WLS	0.004	(0.006)	1.89%	0.050	(0.031)	1.95%
WPQML	0.001	(0.006)	0.63%	0.049	(0.030)	1.95%
<b>Average Treatment Effect on the Treated (ATT)</b>						
WLS	0.005	(0.006)	2.63%	0.003	(0.067)	0.13%
WPQML	0.003	(0.006)	1.62%	-0.003	(0.059)	-0.14%

*Notes:* The estimation sample consists of all physicians in the years 2008-2010 that lie in the common support. Outcomes are measured annually on the physician level. The set of covariates used is the one presented in Table 2 including year-specific intercepts. Standard errors are bootstrapped and clustered at the physician level using 1000 replications. Significance levels: \*\*  $p < 0.01$  and \*  $p < 0.05$ . *Data Sources:* Sasis AG, MedReg, SHS, SFSO, SFFA.

## 6 Conclusions

When physicians can earn a markup on dispensing prescription medication, they have financial incentives to over-prescribe or administer more expensive treatment combinations. This paper has empirically tested whether the dispensing policy affects outpatient health care costs. Using comprehensive insurance-claims data from Switzerland, we estimate treatment effects (policy effects) by exploiting the fact that there is regional variation in the dispensing policy. We employ estimation procedures that combine inverse probability weighting and regression, which are more robust to misspecification than methods based on regression alone or the propensity score alone.

Our benchmark estimates suggest that the dispensing permission increases drug expenditures considerably. On top, dispensing is also found to raise non-drug medical costs, which points to a complementary relationship between drug and non-drug costs in the context of policy-induced demand. These findings are of high relevance for health care policy because they imply that outpatient health care expenditure can be reduced considerably by completely separating the prescribing and dispensing of prescription medication.

There are some limitations to our analysis. In particular, our dataset does not include out-of-pocket expenditure where over-prescribing could also be relevant. Furthermore, the data does not allow us to investigate as to how health outcomes are

affected by dispensing. These issues could be tackled if more detailed data were available. Nevertheless, our study presents clear evidence that the dispensing permission affects the prescribing behavior of physicians in ways that have important implications for health care policy.

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# Appendix

## A Drug Dispensing and Pricing in Switzerland

### A.1 Physician Dispensing (2000 – 2010)

Switzerland consists of 26 cantons (states). As Table A.1 shows, 18 cantons have “pure” policies, i.e. physician dispensing is either completely allowed or prohibited. However, four cantons have policies that result in a de facto ban. For example, the canton of Aargau applies a distance-to-pharmacy criterion, i.e. if the nearest pharmacy from the physician’s office cannot be reached within one hour by public transport, the physician is allowed to dispense. This regulation leads to an almost complete ban of physician dispensing.

In the four cantons with a mixed system, the dispensing rule is determined on the municipal level. In the cantons Bern and Graubünden, physician dispensing is banned in communities where the (emergency) supply and accessibility of drugs is sufficiently guaranteed by pharmacies. Concretely, at least one pharmacy with all-day emergency supply has to be available in municipalities of the canton of Graubünden and in the canton of Bern at least two pharmacies are required. Otherwise, physicians are allowed to dispense drugs without restrictions. As a result, physicians in these two cantons are mainly allowed to dispense drugs in rural areas. Finally, physician dispensing is not allowed in the two largest cities of the canton of Zürich (Zürich and Winterthur) and the canton of Schaffhausen (Schaffhausen and Neuhausen), respectively, while it is allowed in all other municipalities.<sup>17</sup>

Table A.1: Physician dispensing regulations (2008 – 2010)

Dispensing	Cantons	Total
allowed	Appenzell Innerrhoden, Appenzell Ausserrhoden, Basel-Landschaft, Glarus, Lucerne, Obwalden, Nidwalden, St. Gallen, Solothurn, Schwyz, Thurgau, Uri, Zug	13
banned	Aargau <sup>1</sup> , Basel-Stadt, Fribourg <sup>1</sup> , Geneva <sup>2</sup> , Jura <sup>1,2</sup> , Neuchatel <sup>2</sup> , Ticino <sup>2</sup> , Vaud <sup>2</sup> , Valais <sup>1</sup>	9
mixed	Bern, Graubünden, Schaffhausen, Zürich	4

*Notes:* (1) drug dispensing banned with exception, (2) only French- or Italian-speaking

### A.2 Drug Price Regulations

The retail prices of all drugs that are covered by the mandatory health insurance are determined by government regulations. The retail price consists of the ex-factory price<sup>18</sup> plus two additional distribution fees that are charged to cover the cost of the retailer, e.g.

<sup>17</sup>Recently, the cantons Zürich (ZH) and Schaffhausen (SH) changed the regulations. After a ballot in the canton of Zürich in 2008, where 53.7% of the cantonal electorate voted for physician dispensing, physicians in Zürich and Winterthur are allowed to dispense drugs as of May 2012. Similarly, physician dispensing will be completely allowed in the canton of Schaffhausen after a ballot in 2012 that concluded with 71.5% yes votes.

<sup>18</sup>The ex-factory price is set by the federal government and depends on prices of drugs with the same drug action and on drug prices in other European countries with similar pharmaceutical sectors (Austria, France, Germany, Denmark, the Netherlands and the United Kingdom).

shipping or warehousing costs. Since the distribution fees increase with the ex-factory price, the absolute markup for the physician (and the pharmacist) is increasing in the ex-factory price (see Table A.2).

Table A.2: Additional distribution fees

ex-factory price (CHF)	additional fee, price dependent	additional fee (CHF), per package	Retail price (CHF), excl. VAT
0.05 - 4.99	12.0%	4.00	4.06 - 9.59
5.00 - 10.99	12.0%	8.00	13.60 - 20.31
11.00 - 14.99	12.0%	12.00	24.32 - 28.79
15.00 - 879.99	12.0%	16.00	32.80 - 1001.59
880.00 - 2569.99	7.0%	60.00	1001.60 - 2809.89
> 2570.00	0.0%	240.00	> 2810.00

The price of pharmaceuticals charged by dispensing physicians corresponds to the retail price plus 2.5% VAT. However, the price charged by pharmacies is higher due to two additional lump-sum fees. First, the pharmacist receives CHF 4.20 for checking the prescription. This fee is charged only once per drug and prescription. Second, the pharmacist maintains records about patients' drug consumption and checks for potential adverse interactions with other drugs. This fee (CHF 1.89) is charged each time the patient buys at least one package. Given this particular price setting regime, the pharmacists earn an excess markup compared to the physician.

## B Variable Definitions and Construction

Table A.3: Variable Definitions and Construction

Variable Name	Description/Construction	Aggregation	Source
direct drug costs	a physician's total real annual drug costs resulting from direct dispensing. Inflation-adjusted to 2010 constant retail prices (Swiss francs) by using the appropriate sub-indices from the Swiss CPI.		Sasis AG
indirect drug costs	a physician's total real annual drug costs resulting from issued prescriptions that are filled in pharmacies. Inflation-adjusted to 2010 constant retail prices (Swiss francs) by using the appropriate sub-indices from the Swiss CPI. Note: the excess markup of pharmacies is adjusted for by the procedure explained in the Online-Appendix C.		Sasis AG
drug costs	direct + indirect drug costs		Sasis AG
nondrug costs	a physician's total annual non-drug medical expenditure. In constant 2010 Swiss francs. Canton-specific price level differentials are adjusted for. Source: <a href="http://www.praxishilfe.ch/seiten/tpw_tarmed.html">http://www.praxishilfe.ch/seiten/tpw_tarmed.html</a> .		Sasis AG
cost share of drugs	drug costs/non-drug costs		Sasis AG
drug costs per patient	drug costs/# patients		Sasis AG
nondrug costs per patient	non-drug costs/# patients		Sasis AG
share of dispensed drugs	direct drug costs/drug costs		Sasis AG

*Continued on next page*



Table A.3 – *Continued from previous page*

Variable Name	Description/Construction	Aggregation	Source
dispensing status, $D_i$	=1, if physician is dispensing, =0 if physician is not dispensing. For physicians where information is not available in MedReg (15%), we define the dispensing status based on the policy rule prevailing in their office location. Moreover, similar to the approach taken by Iizuka (2012), we attempt to reduce measurement error in $D_i$ by re-defining a physician to be dispensing (non-dispensing) if this share of dispensed drugs exceeds 75% (is below 25%). This results in effective changes of dispensing status for roughly 3% of the sample.		MedReg, Sasis AG
female	=1 if physician is female, =0 if physician is male		MedReg
German nationality	=1 if physician has German nationality, =0 otherwise		MedReg
other foreign nationality	=1 if physician has foreign nationality other than German, =0 otherwise		MedReg
age	current year - year of graduation from medical school + 26, where 26 is the average age at graduation		MedReg
work experience	current year - year of attainment of specialty title		MedReg
# patients	the total number of patients who come to the physician's office in the calendar year		Sasis AG
# visits	the total number of visits to the physician's office in a calendar year		Sasis AG
# visits per patient	# visits/# patients		Sasis AG
patients' average age	sum of patients' age/# patients		Sasis AG
cases aged > 80y	# visits by patients aged above 80/# visits		Sasis AG
cases aged 66 – 80y	# visits by patients aged btw. 66-80/# visits		Sasis AG
cases aged < 25y	# visits by patients aged below 25/# visits		Sasis AG
cases of males	# visits by male patients/# visits		Sasis AG
physician density	The physician density is the total number of physicians per 1000 inhabitants in a municipality.	1	MedReg, SFSO
share with very good subj. health	the share of the population who self-report very good health in the region.	2	SHS
share with good subj. health	cf. previous	2	SHS
share with very good obj. health	symptom-based measure. The share of the population in a region who have very good objective health. Objective health is constructed by summing the following indicators: pain in the back, adynamia, abdominal pain, looseness or costiveness, sleep disorder, headache, heart palpitation or ventricular extrasystole, pain or pressure in the chest, joint pain or pain in the limbs, and pain in the hands. Each variable can take the values 0 (=no symptoms), 1 (=light symptoms) or 2 (=strong symptoms). The sum of these variables is recoded as follows: 0-1 (=very good health), 2-3 (=good health), 4-6 (=fair health) and >6 (=poor health).	2	SHS
share with good obj. health	cf. previous	2	SHS
share with chronic health problems	treatment-based measure. The share of the population in a region that was under medical treatment due to at least one of the following chronic conditions: migraine, asthma, diabetes, arthrosis, stomach ulcer, osteoporosis, chronic bronchitis, high blood pressure, heart attack, apoplexy, renal disease, cancer, allergy, and depression.	2	SHS
share with high health risk	diagnosis-based measure. The share of the population in a region diagnosed with high blood pressure, high cholesterol level, and/or diabetes.	2	SHS

*Continued on next page*

Table A.3 – *Continued from previous page*

Variable Name	Description/Construction	Aggregation	Source
share of immigrants	percentage of non-Swiss citizens in the permanent resident population of a municipality	1	SFSO
fraction of urban area	percentage of urbanized acreage relative to total acreage of a municipality	1	SFSO
net income per capita in 1000	average net income per-capita (2008) in 1,000 Swiss francs in municipality	1	SFFA, SFSO
unemployment rate	percentage of unemployed in total workforce in municipality	1	SFSO
share of medium educated	percentage of vocational and secondary school graduates relative to total adult population in municipality	1	SFSO
share of high educated	percentage of college and university graduates relative to total adult population in municipality	1	SFSO
population density	log of population in 1000 per square kilometre in municipality	1	SFSO
share with managed-care plan	percentage of the population enrolled in managed-care insurance plans (HMO and others) in region	2	SHS
share with supplementary insurance	percentage of the population with supplementary health care insurance in region	2	SHS
share with high deductible	percentage of the population who choose a high deductible (>1000 Swiss francs) in region	2	SHS
non-invasive specialist	reference group. =1 if specialty includes dermatology, venereology, specialty for allergies and immunology, endocrinology, pneumology, nephrology, neurology, hematology, gastroenterology, oncology, physical medicine and rehabilitation, specialty for infectious diseases, tropical medicine, metabolic pathology and neuropathology, =0 otherwise		Sasis AG
gynecologist	=1 if gynecologist, =0 otherwise		Sasis AG
angiologist	=1 if angiologist, =0 otherwise		Sasis AG
cardiologist	=1 if cardiologist, =0 otherwise		Sasis AG
invasive specialist	=1 if specialty is surgery, pediatric surgery, ophthalmology, orthopaedy, vascular surgery, urology, jaw and facial surgery, plastic surgery, or hand surgery, =0 otherwise		Sasis AG
other type of specialist	=1 if specialty is anesthetics, radiology, industrial medicine, pathology, pharmaceutical medicine, radio-oncology, intensive-care specialty, nuclear medicine, clinical pharmacology and toxicology, genetics, or other non-classified specialty, =0 otherwise		Sasis AG

Aggregation 1: For each physician  $i$ , we compute a weighted average across municipalities. The share of visits at physician  $i$ 's office due to people living in these municipalities is used as a weight.

Aggregation 2: For each physician  $i$ , we compute a weighted average across regions. The share of visits at physician  $i$ 's office due to people living in these regions is used as a weight. Note: the SFSO divides Switzerland into 106 so-called mobility regions.

Data Sources: Sasis AG: nationwide operator of the insurance claims database of Swiss health insurers, MedReg: federal register of medical professionals, SFSO: Swiss Federal Statistical Office, SHS: Swiss Health Survey, SFFA: Swiss Federal Finance Administration

## C Online Appendix

Supplementary information and tables to this paper can be found on the homepage of the corresponding author: [http://staff.vwi.unibe.ch/schmid/downloads/pd\\_wp\\_appendix\\_jul13.pdf](http://staff.vwi.unibe.ch/schmid/downloads/pd_wp_appendix_jul13.pdf)