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Does Physician Dispensing Increase Drug Expenditures?

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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.

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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 entrepreneurs and as agents for their patients. One important such policy, and the central issue of this paper, is the regulation of drug dispensing by physicians. If physicians are allowed to sell drugs to patients, they may prescribe more drugs or substitute towards more expensive prescriptions in order to generate additional income. In other words, dispensing can create financial incentives for physicians to induce demand and thus raise health care expenditures. While most OECD countries fully ban physician dispensing, there are some notable exceptions: the United States, the United Kingdom, Japan and Switzerland (partly) allow medical doctors to dispense drugs.[1] Although some recent evidence exists that the dispensing behavior of physicians is affected by the markup on drugs (see for example Iizuka, 2007, 2012), empirical evidence on the effects of physician dispensing on drug or health care expenditures is scarce. 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 several 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, the variation in the dispensing policy is rooted in historical differences

[1] 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).

of cantonal health care systems and therefore represents a credible source of exogenous variation. Third, 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 differences in price setting. 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. Fourth, 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.[2]

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

[2]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).

support, present descriptive statistics, 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 (see Lim et al., 2009, for an overview). 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 substantial 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 Lin-

colnshire (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 for prescription drugs is highly regulated on the federal level with respect to drug approval, pricing, prescribing and dispensing. In principle, pharmacists are allowed to sell prescription drugs, but they *cannot* issue prescriptions. Thus, only medical doctors are allowed to issue prescriptions to patients. The fact that only medical doctors can prescribe is an important institutional feature because prescribing costs of dispensing and non-

dispensing physicians can be adequately compared. Moreover, it is unlikely that differences in the availability of pharmacies confound our analysis as patients *must* visit a physician to obtain prescription medication independent of the dispensing regime.

As far as dispensing is concerned, cantons either prohibit or (partially) permit doctors to dispense drugs.[3] 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 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 ample 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). In other words, the analysis based on the whole of Switzerland would not allow for the identification of policy effects because it would be confounded with unobserved cultural effects.

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

[3]Switzerland has 26 cantons.

gross profit margin corresponds to the difference between the retail price and the ex-factory price. For example, for a 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). A 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.[4] 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 $i = 1, 2, ...N$. 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 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

[4]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 Appendix C.

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

$$\tau = E[Y_{1i} - Y_{0i}], \tag{1}$$

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:

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

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 characteristics of physician i and his patients as well as 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):

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

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

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. Second, differences in dispensing policies are historical (see Section 3), which eliminates concerns that observed policy rules are endogenous. Third, other institutional features such as drug prices and health insurance regulations are the same throughout the country and are therefore guaranteed not to confound the analysis.[5] Fourth, we can mitigate concerns that physicians select into treatment groups based on unobservables by controlling for personal characteristics of physicians. This eliminates any potential bias that arises if these characteristics affect prescribing behavior *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.

[5]However, it is conceivable that patients' deductible choices vary across regions, which is why we control for these potential differences.

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.[6]

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

[6]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 the case of clustered data. In addition to the clustering problem, matching on covariates has the disadvantage that the asymptotic bias increases in the number of continuous covariates (Imbens, 2004).

$\hat{\beta}^{d,e}$ is obtained by solving the following IPW-augmented moment conditions:

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

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

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

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

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 $plim(\hat{\beta}^{d,e}) = \beta^d$), *or* the propensity score model is correctly specified (in which case $plim(\hat{\lambda}^e(X_i)) = \lambda^e(X_i)$), *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).[7] We will consider both of these specifications in our analysis. Finally, inference is based on the block bootstrap, which takes into account two things: first, the potential serial correlations within panels (i.e. physicians), and second, the uncertainty from the

[7]This estimator is equivalent to the generalized linear model (GLM) with log-link and Poisson family augmented with the appropriate weights.

first-step estimation of the propensity score.

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 determine the common support of the covariate distributions. Third, we briefly present descriptive statistics on outcomes and covariates in the common support sample. 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. Primary care physicians (general practitioners) are not included. The dataset basically aggregates the mandatory health insurance claims of nearly all Swiss health insurance companies.[8] 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.[9] This comprehensiveness strengthens the external validity of our analysis because issues of adverse selection into *individual* insurance firms are not of concern. Two points about the data on prescription drug expenditures 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 relevant

[8] Three out of 61 insurance companies are not included; these are Assura, EGK and SLKK.

[9] 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.

for patients who only incur small amounts of health care costs in a given year and are therefore unlikely to exceed the deductible.[10] 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.[11]

In the ensuing discussion on the construction of the dataset, the reader is referred to Table B.I in Appendix B for a detailed exposition of all variables. For each 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.[12]

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).[13] 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 B.I in Appendix B).

Since the dispensing policy varies by region, we want to account for the fact

[10]Based on national accounting data on the consumption of pharmaceuticals, we estimate that our data contains at least 84% of the relevant drug expenditures.

[11]These include for example painkillers with low dosage or certain herbal products.

[12]Note that the data does not contain information on *individual* patients.

[13]see http://www.bag.admin.ch/themen/berufe/00411/

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 the municipality he works in, 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 the role of medication are quite different compared to other specialized physicians. Furthermore, we only consider physicians reporting at least 10 patients per year (5% of sample are excluded).[14] 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.

[14]Many of these doctors with less than 10 patients 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.

5.2 Determining Common Support

Since treatment effects (policy effects) can only be estimated for the common support, a prior analysis of 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.[15] 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 I 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.

– Insert Table I about here –

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.

– Insert Figure 1 about here –

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

[15] 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.

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 α 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 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 I. 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%. Overall, this means that adjusting for differences in covariates will be much less critical when estimating treatment effects. Finally, it bears emphasizing the restriction to the common support means that we can identify and estimate policy effect only for a subset of the population. In our case, however, the common support sample represent a large majority of the initial sample (86%).

5.3 Descriptive Statistics

Tables II presents descriptive statistics. The dataset includes 1,908 non-dispensing doctors and 1,416 dispensing doctors. In total, this amounts to a panel of 9,228 observation because most individuals are observed during all three years (2008–2010). Expenditure variables are annual measures expressed in constant 2010 Swiss francs (CHF) and averaged across the sample period. 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. We focus on expenditure measures in per-patient terms to take into account that the average practice size between the two groups differs, i.e. dispensing physicians have more patients on average. For these reasons, it makes sense to consider expenditures *per patient*.[16] Table II reveals that dispensing physicians have higher drug expenditures per patient compared to their non-dispensing counterparts. The same is true for non-drug health care expenditures. Moreover, 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 cases of emergencies. Conversely, dispensing physicians may have a limited range of pharmaceuticals in stock such that patients must collect unavailable products at pharmacies.

– Insert Table II about here –

Turning to the covariates, we see that in areas with dispensing, physicians are more often foreign and somewhat younger. In addition, the average gender mix in the patient pool is almost identical across groups but dispensing physicians have somewhat younger patients. Regions where dispensing is allowed have smaller physician density, are less urbanized, exhibit smaller shares of immigrants, and the local population are on average less unemployed. Overall, these differences

[16] We do not consider costs per visit because the number of visits in a calendar year could be 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 physician behavior.

are rather small and the covariates seem to be quite well-balanced across dispensing and non-dispensing physicians.

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.[17] The set of covariates used in the doubly robust regression methods is essentially the same as the one presented in Table I, 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.[18] Because the estimation sample pools data from several years (2008−2010), we include year-specific intercepts to account for aggregate changes in group-specific average outcomes across years. Standard errors are obtained by 1,000 bootstrap iterations, taking into account the clusters of repeated observations across years and 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 is preferable relative to the WLS.[19] For detailed

[17] 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.

[18] We also tested extended specifications including higher-order terms. While the qualitative results did not change, estimates were found to be less precise.

[19] The BIC is defined as $\ln(\frac{SSR}{N}) + \frac{k \ln(N)}{N}$. 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 the former has smaller values in both subsamples, the WPQML estimator is preferred to the WLS estimator.

estimation results, the reader is referred to Appendix C.

The main results are presented in Table III. We start by discussing the effect 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 hypothesis 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 I) suggest that health care market conditions are likely to be the most relevant "drivers" of the selection effect. This indicates that controlling for differences in covariates is crucial in the estimation of the policy effects. 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).

– Insert Table III about here –

Besides drug costs, we also examine whether dispensing affects non-drug medical expenditure, as presented on the right-hand side of Table III. 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 IV. 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 intake and potential side effects, answering questions the patient might have about the prescribed medication, 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.

– Insert Table IV about here –

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 IV 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 reported in Table III comes about by a stronger inducement of follow-up visits. This implies that dispensing leads to more medical services *per visit*, which lends support to the arguments presented above that dispensing increases non-drug costs through an increase in consultation costs.

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 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 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.I 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.

– Insert Table A.I about here –

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.[20]

[20]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.

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[21] plus two additional distribution fees that are charged to cover the cost of the retailer, e.g. 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.II).

– Insert Table A.II about here –

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

– Insert Table B.I about here –

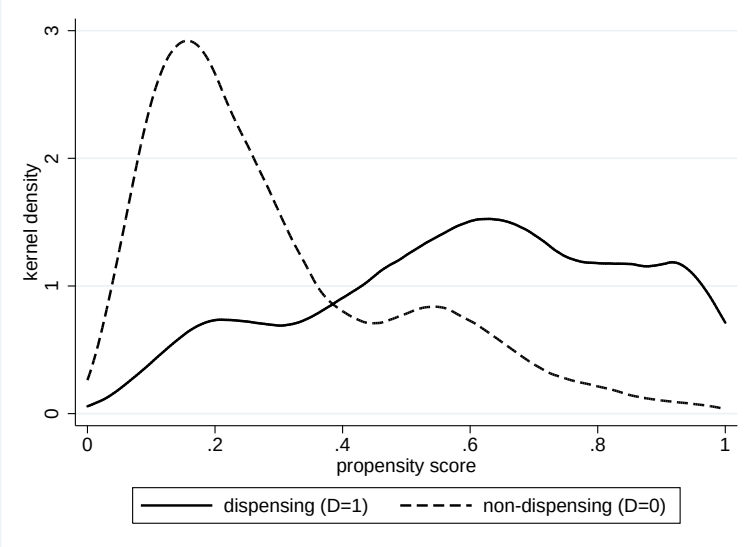
C Online Appendix

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_jan14.pdf

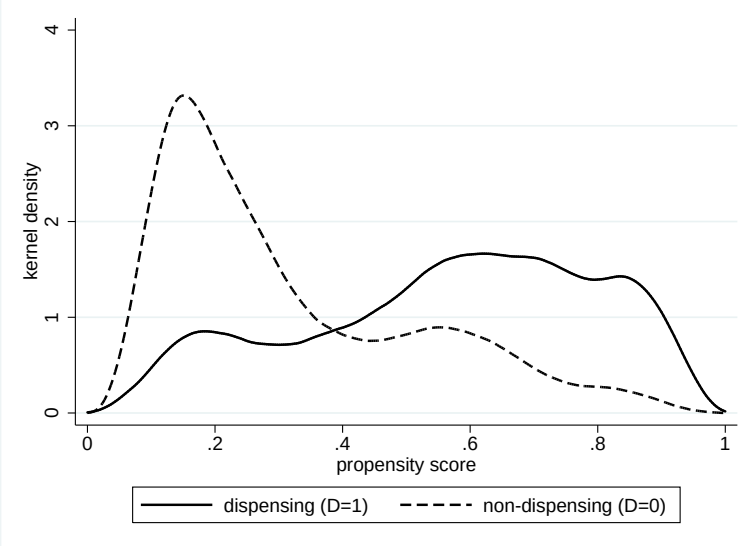
[21] The ex-factory price is set by the federal government and depends on prices of drugs with the same drug action and on drug prices in other European countries with similar pharmaceutical sectors (Austria, France, Germany, Denmark, the Netherlands and the United Kingdom).

Figures and Tables

Figure 1: Kernel Densities of Estimated Propensity Scores



(a) full sample



(b) common support

Table I: Normalized Differences of Covariates Means (2008–2010)

	full sample	c.s. sample
physician characteristics		
female	-0.018	0.002
German nationality	0.073	0.062
other foreign nationality	-0.046	-0.026
age	-0.147	-0.103
work experience	-0.124	-0.089
patient pool variables		
# patients	-0.269	-0.243
# visits per patient	0.157	0.126
patients' average age	-0.172	-0.113
cases aged $> 80y$	-0.107	-0.042
cases aged $66 - 80y$	-0.111	-0.070
cases aged $< 25y$	0.171	0.128
cases of males	-0.004	0.002
characteristics of the local health care market		
physician density	-0.362	-0.261
share with very good subj. health	0.276	0.201
share with good subj. health	-0.070	-0.044
share with very good obj. health	0.211	0.194
share with good obj. health	0.109	0.058
share with chronic health problems	-0.209	-0.130
share with high health risk	-0.402	-0.334
share of immigrants	-0.108	-0.049
fraction of urban area	-0.381	-0.266

Continued on next page

Table I – *Continued from previous page*

	full sample	c.s. sample
net income per capita	0.095	-0.025
unemployment rate	-0.309	-0.179
share of medium educated	0.201	0.160
share of high educated	-0.269	-0.243
population density	-0.340	-0.245
share with special managed-care plan	-0.078	0.014
share with supplementary insurance	0.254	0.158
share with high deductible	0.029	0.002
speciality type		
gynecologist	0.006	-0.014
angiologist	-0.013	-0.021
cardiologist	-0.061	-0.032
invasive specialist	0.018	0.006
other type of specialist	0.119	0.127
$\hat{\alpha}$		0.099
# control obs. (non-dispensing)	6162	5339
# treated obs. (dispensing)	4544	3889
# control individuals (non-dispensing)	2215	1908
# treated individuals (dispensing)	1658	1416

Notes: common support refers to the subsample where observations outside the interval $[\hat{\alpha}, 1-\hat{\alpha}]$ have been dropped. The raw 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 mean and the variance, respectively, estimated in the subsample with $D_i = d \in \{0, 1\}$.

Table II: Descriptive Statistics (2008–2010)

	non-dispensing $D_i = 0$		dispensing $D_i = 1$	
	mean	st.dev.	mean	st.dev.
real annual expenditure per physician				
cost share of drugs	0.228	0.189	0.208	0.180
drug costs per patient	192.797	563.658	248.824	768.969
non-drug costs per patient	379.584	311.369	468.126	663.169
share of dispensed drugs	0.193	0.155	0.866	0.217
physician characteristics				
female	0.221		0.222	
German nationality	0.084		0.110	
other foreign nationality	0.015		0.011	
age	52.402	8.489	51.191	8.147
work experience	17.830	8.321	16.815	7.856
patient pool variables				
# patients	940.438	795.079	1091.844	905.011
# visits	2162.634	1878.317	2510.100	2015.847
# visits per patient	2.541	1.988	2.534	1.600
patients' average age	53.237	8.846	51.821	8.850
cases aged $> 80y$	0.074	0.069	0.070	0.063
cases aged $66 - 80y$	0.251	0.135	0.238	0.131
cases aged $< 25y$	0.094	0.103	0.114	0.110
cases of males	0.352	0.217	0.353	0.212
characteristics of the local health care market				
physician density	3.449	1.092	3.033	1.162
share with very good subj. health	0.207	0.028	0.216	0.034
share with good subj. health	0.674	0.027	0.672	0.036
share with very good obj. health	0.314	0.030	0.324	0.041
share with good obj. health	0.351	0.024	0.353	0.035
share with chronic health problems	0.499	0.036	0.492	0.046
share with high health risk	0.270	0.026	0.256	0.036
share of immigrants	0.208	0.053	0.205	0.045
fraction of urban area	0.325	0.139	0.278	0.103
net income per capita	80.210	11.154	79.774	13.496
unemployment rate	2.685	0.505	2.560	0.484
share of medium educated	0.509	0.032	0.515	0.023
share of high educated	0.225	0.042	0.211	0.042
population density	0.269	0.715	0.034	0.641
share with managed-care plan	0.172	0.037	0.173	0.055
share with supplementary insurance	0.556	0.038	0.565	0.043
share with high deductible	0.425	0.046	0.425	0.042
# observations	5,339		3,889	

Notes: Based on the common support sample of physicians in German-speaking Switzerland and averaged across 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.

Table III: 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
Unadjusted Difference	56.03**	(23.03)	25.89%	88.54**	(18.98)	21.24%
Average Treatment Effect (ATE)						
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%
Average Treatment Effect on the Treated (ATT)						
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 I including year-specific intercepts. Standard errors are bootstrapped and clustered at the physician level using 1000 replications. Significance levels: ** $p<0.01$ and * $p<0.05$.

Table IV: Policy Effects of Dispensing, 2008–2010

	Share of Drug Costs			Visits per Patient		
	Coef.	S.E.	% of mean	Coef.	S.E.	% of mean
Unadjusted Difference	-0.020**	(0.006)	-9.25%	-0.007	(0.059)	-0.28%
Average Treatment Effect (ATE)						
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%
Average Treatment Effect on the Treated (ATT)						
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 I including year-specific intercepts. Standard errors are bootstrapped and clustered at the physician level using 1000 replications. Significance levels: ** $p<0.01$ and * $p<0.05$.

Table A.I: 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[1], Basel-Stadt, Fribourg[1], Geneva[2], Jura[1,2], Neuchatel[2], Ticino[2], Vaud[2], Valais[1]	9
mixed	Bern, Graubünden, Schaffhausen, Zürich	4

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

Table A.II: 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

Table B.I: 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 Section 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 (TARMED) are adjusted for. Source: http://www.praxishilfe.ch/seiten/tpw_tarmed.html.		Sasis AG
cost share of drugs	drug costs/(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 B.I – *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.		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

Continued on next page

Table B.I – *Continued from previous page*

Variable Name	**Description/Construction**	**Aggregation**	**Source**
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

Continued on next page

Table B.I – *Continued from previous page*

Variable Name	**Description/Construction**	**Aggregation**	**Source**
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
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

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Table B.I – *Continued from previous page*

Variable Name	**Description/Construction**	**Aggregation**	**Source**
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

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Table B.I – *Continued from previous page*

Variable Name	**Description/Construction**	**Aggregation**	**Source**
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

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