

Are private physicians more likely to veto generic substitution of prescribed pharmaceuticals?

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HUI Working Paper No 14

Abstract

Physicians' decisions whether or not to veto generic substitution were analyzed using a sample of 350,000 pharmaceutical prescriptions from the county of Västerbotten, Sweden. The primary purpose was to test if physicians working at private practices were more likely to oppose substitution than county-employed physicians working on salary. It was found that private physicians were 50-80% more likely to veto substitution. Also, the probability of a veto was found to be increased as patients' copayments decreased. This might indicate moral hazard in insurance, though other explanations are plausible.

*I am grateful to Kurt Brännäs, Kerstin Gustafsson, Mikael Lindbeck, Karl-Gustaf Löfgren, Carl Lönnbark, Andréa Mannberg, Niklas Rudholm and participants at seminars at Umeå and Gävle Universities for helpful comments and suggestions, and to the Swedish Competition Authority for a research grant that supported the work. I also thank Västerbotten County Council and IMS Sweden for providing the datasets, Eva Lindberg for contributing the information about the physicians. Previous versions of this paper have circulated under the titles "Does physicians' employments affect the probability of their vetoing generic substitution?" and "The effect of economic incentives on double agents: evidence regarding generic substitution".

1 Introduction

Imperfect information often results in “Principal-agent problems” when one person, the agent, is employed to act on behalf of another, the principal (Ross, 1973). In the healthcare sector the problem is complex since efficiency requires physicians to act not only as agents for their patients, but also for third-party payers, insurers (Blomqvist, 1991, and Shortell, 1998). This study analyzes how economic incentives affect physicians’ decisions whether or not to veto generic substitution, and also whether their decisions suggest that they internalize differently the costs occurring to their two principals.

Since October 2002, pharmacists in Sweden have been required to substitute the prescribed pharmaceutical product to the cheapest available generic when neither the prescribing physician nor the patient opposes it. Patients who oppose substitution have to pay the difference in price themselves, but if the physician vetoes it for medical reasons, patients are subject only to the normal copay requirement under Swedish pharmaceutical insurance.

Although similar reforms have been introduced in many European countries and American states, what determines whether physicians’ veto substitution has, to my knowledge, not been studied previously. One explanation could be lack of data: Hellerstein (1998) noted that the US NAMCS-data unfortunately lack information about whether substitution was vetoed, while Mossialoa, Walley and Rudisill (2005) noted on a general scarcity of good prescription data for several European countries.

To study physicians’ decisions regarding generic substitution is important since it not only directly affect patients’ and insurers’ costs for pharmaceuticals, but also indirectly since more bans against substitution likely reduces price-competition between pharmaceutical firms. In the sample used for this study, brand-name products for which substitution was vetoed by physicians were on average 218% more expensive than the cheapest generic alternative, whereas the corresponding figure for other brand-name products was only 15%. This correlation might indicate that physicians’ decisions whether or not to veto generic substitution have an important effect on price-competition among pharmaceutical firms.

The primary purpose of this study was to analyze whether privately employed physicians were more or less inclined to oppose substitution, compared to county-employed physicians. Private physicians have a stronger incentive to

please their patients in order to keep them, since their income depends on the number of patient-visits, whereas county physicians work on salary. Opposing substitution, if the patient suggests that, might be a costless way of doing this. Allowing substitution might also be time-consuming for the physician if it worries the patient. Hence, each consultation could take longer, resulting in fewer of them, and again less income. Private physicians might also have stronger brand-name loyalty since, for example, they are less restricted, compared to county-employed physicians, from participating in education organized and paid for by pharmaceutical companies. The hypothesis to be tested is thus that private physicians were more likely than county physicians to veto substitution.

Another purpose was to analyze the effect of patients' copayments on physicians' decisions. The Swedish pharmaceutical insurance is non-linear, with patient-copayments decreasing as total expenditure increases. This provided an opportunity to study whether physicians internalized patients' costs more than costs to the insurer, indicating what Pauly (1968) called moral hazard in insurance (also called *ex post* moral hazard).

The analyses were done using a sample of 350,000 observations drawn from a micro-dataset covering all prescriptions dispensed in the county of Västerbotten, Sweden - or dispensed elsewhere in Sweden to inhabitants of Västerbotten - during 43 month after the substitution reform. The dataset includes information about the patients, prescribers, prices, copayments, pharmaceuticals prescribed and dispensed, and about whether the physician or patient opposed substitution. Patients and prescribers are, however, not traceable over time.

Since the values were observed at micro-level, the risk of estimators being biased towards zero was reduced; this is otherwise a common problem when aggregated data are used as proxies for micro-variables. Using register-data also eliminated recall-bias, as well as selection-bias, which can be a problem if for example not everyone participates in an experiment or answers a questionnaire. The size of the dataset also substantially reduced the risk of accepting a false null-hypothesis which is otherwise a common problem when studying questions, such as here, where a large part of the variation, for various reasons, cannot be explained by the observables.

Gosden, Pedersen and Torgerson (1999) reviewed the literature on the effects of salary payments on physicians' behavior. They reported some evidence that

payments of salary was associated with fewer referrals and tests compared both with fee-for-service (FFS) payments and capitations. Compared with FFS payments, salary payment also correlated with fewer procedures per patient, fewer patients per physician, longer consultations, more preventive care, and different patterns of consultation. Nassiri and Rochaix (2006) found that primary-care physicians in Quebec reacted both to temporary removal of expenditure caps and to changes in the relative price of consultations by changing their treatment pattern. Dusheiko, Gravelle, Jacobs and Smith (2006), studying the effect of financial incentives on general medical practices in England, found that abolishing foundholding increased elective surgery by 3-5%.

Leibowitz, Manning and Newhouse (1985) and Hellerstein (1998) used U.S. data to study the choice between prescribing brand-name or generic pharmaceuticals and found that the choice was not a function of the insurance plan; however, Leibowitz et al. found that individuals with more generous insurance plans bought more prescription pharmaceuticals. On the other hand, using a Swedish dataset covering seven pharmaceuticals, Lundin (2000) found evidence of moral hazard: patients with low copayments were more likely to receive brand-name pharmaceuticals. Similarly, Mott and Cline (2002) found that insured patients in a Midwestern state were more likely to receive prescription that did not allow generic drug use, either because the prescribed pharmaceutical was patent protected or because the physician opposed generic substitution. The literature analyzing the effects of patients' copayments on the choice of pharmaceutical also includes Crown, Berndt, Baser, Finkelstein, Witt, Maguire and Haver (2004) and Rudholm (2005). Crown et al. found no statistically significant effect of insurance plans' mean copayment-rates on patients' treatment patterns for asthma. Rudholm, however, found significant effects of individual patients' copayment-rates on the price of the chosen pharmaceutical. Rudholm also included a variable indicating for privately employed physicians in his regressions but, except in one subsample, found no statistically significant effects of this variable.

Empirical results presented in this paper show that private physicians were 50-80% more likely to veto substitution than county-employed physicians. Also, the probability of a veto was found to be increased as patients' copayments decreased. This might indicate moral hazard in insurance, though other explanations are plausible.

2 Rules and incentives

2.1 Patients' copayments and the substitution reform

During the study-period, all pharmaceuticals in Sweden were sold through a nation wide government owned monopoly, which at all times charged a nation wide uniform price for each pharmaceutical product.

All residents were (and are still) covered by a mandatory and uniform health insurance, including the pharmaceutical insurance presented below. In the pharmaceutical insurance patients pay all costs up to 900 Swedish crowns (approximately 120 USD)¹ per 12-month period; 50% of the cost from 900 to 1700 SEK; 25% from 1700 to 3300 SEK; and 10% from 3300 to 4300 SEK; after which all costs during the period are paid by the insurance (specifically, by Swedish county councils). However, there are some exceptions: some pharmaceuticals are always free of charge for the patient, and others are not covered by insurance at all. Another exception is that patients who oppose generic substitution have to pay the entire extra cost caused by this.

Under the substitution system, physicians should check a box on the prescription to veto substitution. When physicians veto substitution, the extra costs are covered by the pharmaceutical insurance system, but physicians are allowed to veto only for medical reasons, for example, if the patient is sensitive to inert ingredients in some of the substitutes. Unless substitution has been vetoed by the physicians, pharmacists should inform the patient if substitutes are available, and that the cheapest available generic product considered to be a perfect substitute by the Swedish Medical Products Agency would be provided within the Swedish pharmaceutical insurance system. Patients need not accept substitution, but the entire extra cost will then be charged to them.²

2.2 Physicians

There were nearly 1000 physicians working in the county of Västerbotten during the study-period. Most of them were county-employed, paid on salary, but nearly 40 physicians worked at small private practices, indicated here as *Private*. In addition five physicians worked at private occupational health services, excluded from this analysis.

Twenty of the private physicians worked at practices that were nevertheless paid by the county council according to different contracts, while the remain-

ders were paid privately, either directly by patients, or possibly under contract to private health insurance companies. Of the physicians with county-council contracts, sixteen ran single practices and received only fee-for-service reimbursement; the remaining four worked at two so called “house-doctor practices”, which were paid fee-for-service plus a capitation per patient registered at their practice.

The financial incentives and rules differed between the private and county employed physicians in, at least, two important aspects. First, the revenues of all private practices increased with the number of patient-visits. Higher revenues for the practices also meant higher income for the physicians. This connection was perhaps strongest for the private physicians which ran single practices, but likely strong also for those others who ran practices together with one or a few associates. Private physicians had thus a pecuniary incentive to please their patients in order to keep them and/or attract new patients. The connection between patient-visits and income also gave private physicians a pecuniary incentive to keep consultations short, in order to increase the number of possible patient-visits per day. These incentives were not shared by county-employed physicians since their salaries did not increase with the number of patient-visits.

The second difference regards the rules regulating physicians’ relationships with pharmaceutical firms. The rules restricting physicians to participate in educations and conferences paid for by pharmaceutical firms, or to perform paid assignments for them, are related to the physicians’ responsibilities toward their employers: physicians must obtain approval from their employer before accepting offers from pharmaceutical firms. During the study-period, the county council had stringent policies regarding its employees’ relations with the pharmaceutical firms. This is in sharp contrast to especially the self-employed physicians, who only needed their own approval.

There were also physicians working in other organizational forms, including ten working for the private company Carema, which ran Dragonen’s health center during the last five months of the study-period. Carema received a lumpsum payment for the first 12 months, after which compensation would depend on the number of registered patients at the health center. The incentives for physicians working for Carema probably differed from those for other private physicians in two ways: the incentives for the company differed from those for private

practices and then there were probably internal principal-agent problems.³

2.3 Physicians' decisions regarding generic substitution

Besides concerns for their patients' health, physicians decisions whether or not to veto substitution can be influenced by how a veto would affect patients' and insurers' cost for pharmaceuticals, as well as by time consideration and brand loyalty discussed later.

That the price-difference between the prescribed and the cheapest generic alternative is covered by the insurance if the physician opposes substitution, but not if the patient does it, means that patients who would otherwise refuse substitution could save money if their physician opposed it instead, given that their copayment-rate is below 100%. As the copayment-rate goes down, patients could save more if their physician vetoed substitution; above 4300 SEK total cost, patients have a zero copayment-rate and would pay nothing if their physician opposes substitution, versus paying the entire difference themselves.

Patients who would not refuse substitution could have lower copayments if their physician allowed substitution. For them, the extra copayments caused by a physician's veto declines with their copayment-rates. Thus, for patients, physicians' vetoes become more beneficial or less costly as their copayment-rates decline. The insurers' costs of a veto, on the other hand, goes up as the patients' copayment-rates declines. This tells us that if physicians internalize patients' cost more than the insurers' cost, indicating moral hazard in insurance, we would expect physicians to be more likely to oppose substitution when the patient's copayment-rates is low.

The physician might internalize the consequences of a veto for the patient because of altruistic considerations, or because of pecuniary incentives. Pecuniary incentives could arise, for example, since patients can change physician if they are not satisfied. Private physicians might have stronger pecuniary incentives to please their patients in order to keep them and/or attract new patients, since their income depends on the number of patient-visits. The physicians have generally no pecuniary incentives to internalize the insurers' cost, but since the insurers are the Swedish county councils, one might still suspect that county county-employed physicians care more about the insurers' cost than private physicians do. If private physicians internalize patients' cost more, or insurers' cost less, than county-employed physicians, we would expect private physicians'

likelihood of vetoing substitution to increase faster than that of county-employed physicians, as patients' copayments fall.

As mentioned, physicians' decision regarding generic substitution might also be affected by time considerations and brand loyalty. Allowing substitution might raise questions from the patient about differences between the prescribed and dispensed pharmaceutical. Answering such questions might be time-consuming, reducing the number of possible patient-visits per day. If so, this will be more costly for private physicians since their income depend on the number of patient-visits. Private physicians might also have stronger brand-name loyalty if they, as opposed to county-employed physicians, are not restrained by their employer from participating in education organized and paid for by pharmaceutical companies. If private physicians' decisions are affected by either of these two mechanisms, we would expect them to be more likely to veto substitution irrespective of the patients' copayments.

3 The empirical analysis

3.1 Data

The prescription dataset used in this study was provided by the county council of Västerbotten, Sweden. It contains all prescriptions sold in the county, or sold in other parts of Sweden to residents of the county, from January 2003 through October 2006, except for November and December, 2003, and September, 2004. Data for these three months are not available since the county council's data files for these months were damaged. Prescriptions issued before the substitution reform of October, 2002 and prescriptions of pharmaceuticals packed in patient-doses, were excluded since in these cases physicians were not asked if they opposed substitution. Non-pharmaceutical prescriptions as well as prescriptions issued by others than physicians (e.g. dentist and nurses) were also excluded. Finally, after excluding nearly 270,000 observations originating from other workplaces than health centers, clinics or private practices in Västerbotten (e.g. emergencies, labs, occupational health services, or workplaces in other counties), or unknown workplaces, and 630,000 that lack data on ATC-group or did not belong to any ATC-group, 5.1 million observations of pharmaceutical prescriptions remain. The ATC-groups used here corresponds to the most narrow groups in the World Health Organization's Anatomical Therapeutic Chem-

ical classification system, meaning that only pharmaceuticals with the same active ingredients are grouped together.

In 1.7% of the observations the physician had vetoed substitution and in 2.8% the patient had opposed it. All these observations were used plus a random sample of 2.5% of the remaining observations, resulting in a final sample of 350,180 observations. A sample had to be drawn because of limited computer-capacity for running iterative estimation procedures. Because of the low percentage of physicians opposing substitution, all those observations were used in order to reduce the variance in the logistic regressions, compared to using a random sample from the whole population, resulting in the same number of observations. All observations when the patients refused substitution were used in order to minimize the effect of individual measurement-errors of the copayment variables that may exist for these observations (these measurement errors can arise since the price-difference between the prescribed and the cheapest generic alternatives cannot be perfectly observed). As discussed below, sampling-weights were used in the estimations.

Some descriptive statistics are presented in Table I, where the sample is grouped based on whether the physician vetoed substitution ($V = 1$) or not ($V = 0$). The observations are weighted according to the inverse of their probability of being sampled. For the indicator-variables the percentage of observations in each category are presented. For the continuous variable *Age*, means and standard deviations are presented instead. The variables *County* and *Private* take the value one if the prescribing physician was employed by the county or worked at a small private practice, respectively, while the next three variables indicate which healthcare district (*Umeå*, *Skellefteå* or South of *Lapland*) their workplace was located in. The dataset do not identify the prescribing physician, but do identify which workplace unit the physician work at. For county-employed physicians, the workplace unit is the health centre or clinic where they work. Private physicians are grouped together in the data to one workplace unit per healthcare district.

The dataset includes information about the total cost of the prescription as well as the patient's copayment, from which the copayment-rate the patient had prior to paying for the current prescription was calculated (calculations are available from the author upon request). The indicator-variables *Copay100*, *Copay50*, *Copay25*, *Copay10* and *Copay0* show these predetermined

copayment-rates. Thus, *Copay100* take the value one for patients who had to pay 100% of the pharmaceutical costs themselves and *Copay0* indicate patients who have reached the highest breakpoint of the pharmaceutical insurance and therefore receive pharmaceuticals free of charge. Some prescriptions are always free of charge (*Free*) for the patient and others are excluded from the insurance system (*Unsub*) irrespective of the patient's copayment bracket. The last two variables refer to the gender and age of the patient.

Table I about here

In addition, the dataset includes information about the prescribed pharmaceutical's ATC-code, the patient's municipality of residence, and the date when the prescription was written. Of the 883 seven-digit ATC-groups present in the sample, 276 have less than 10 observations; 334 have 10 to 100 observations; 206 have 100 to 1000 observations; and 67 have more than 1 000 observations. 36% of the prescriptions were written to inhabitants of Umeå, the county's largest municipality; 28% to inhabitants of Skellefteå; 1-5% to inhabitants of each of the county's other municipalities; and 3% to individuals not living in the county. 1-3% of the observations were issued in each of the 49 months from the substitution reform, effective in October 2002, through October 2006.

The descriptive statistics provide some support for the hypotheses tested here. First, private physicians are over-represented among the subsample where substitution was vetoed. Second, the same is true for patients' with low copayments whereas the opposite is true for those with high copayments.

To be able to study how the probability of a veto related to whether or not the prescription was for a brand-name pharmaceutical, the dataset described above was linked with a dataset provided by the company IMS Sweden, that classified 50% of the prescribed pharmaceuticals as brand-names pharmaceutical.

3.2 Empirical specifications

Specification 1 is written

$$\begin{aligned} \Pr(V_i = 1) = & F(a + \beta_1 Private_i + \sum_{c=1}^4 \delta_c Copay_{ci} + \beta_2 Unsub_i + \beta_3 Free_i \\ & + \beta_4 Women_i + \sum_{a=1}^{20} \eta_a Age_{ai} + \sum_{g=1}^{882} \kappa_g ATC_{gi} \\ & + \sum_{m=1}^{15} \lambda_m Mun_{mi} + \sum_{d=1}^2 \mu_d District_{di} + \sum_{q=1}^{16} \tau_q Quarter_{qi} + \epsilon_i). \end{aligned}$$

Private was included to test the main hypothesis in this study, that private physicians were more inclined to veto substitution. The copayment-indicators were included to test the hypothesis that moral hazard in insurance exists. What really influences physicians' decisions is probably their expectation of their patients' copayments at the end of the insurance period, since this determines the share of the cost of a veto borne by the patient. This is not observable, but those with a predetermined copayment-rate of 0% will also have a zero-rate at the end of the insurance period. The other copayment-variables are only proxies, since for example those with a predetermined copayment-rate of 25% will have a rate of 25% or lower at the end of the insurance period.

Predetermined copayment-rates were used in order to avoid endogeneity caused by the value of the dependent variable for observation i affecting the value of independent variables for that observation. Nevertheless, persistence can cause endogeneity. For example, a physician who previously vetoed substitution of a particular pharmaceutical for a particular patient might be more inclined to veto substitution again the next time for the same patient and pharmaceutical. At the same time, the past decision might affect the patient's predetermined copayment-rate. To study whether this possibility affects the results, the baseline specification was also estimated on a subsample of only antibacterial drugs (ATC: J01), since these are very seldom prescribed repeatedly to a patient. Another problem that has to be kept in mind when interpreting the results is that the copayment-variables are correlated with previous pharmaceutical expenditures.

Unsub and *Free* also reflects copayments and were therefore included. However, pharmaceuticals that are always free of charge, or always excluded from the

insurance, belong to a small number of ATC-groups, with which these variables are highly correlated, so high that some ATC-indicators were excluded from the estimations due to multicollinearity. Therefore, the coefficients for *Unsub* and *Free* probably captured other effects besides those relating to moral hazard.

Women, indicator-variables for 5-year age-groups, and the ATC-indicators were used as proxies for differences in health outcome in which the prescribed pharmaceutical and the cheapest generic might result. The ATC-indicators also controlled for the fact that some ATC-groups included no generic products, so that the physicians' willingness to allow substitution had no effect. Finally, the ATC-indicators controlled for heterogeneity among ATC-groups with respect to price-differences between the prescribed pharmaceutical and the cheapest available generic. I did not directly control for price-differences since these depend on the physicians' choices of pharmaceuticals, for example, between brand and generic versions. If the choice of pharmaceutical depends on whether the physician is private or county employed, controlling for price-differences would mean that the estimated odds-ratio for Private no longer would describe the total effect of being a private employed physician. Also, if physicians often veto substitution for a pharmaceutical, the pharmaceutical firm might find it profitable to increase the price of that product, and thus the price-difference between that product and the cheapest generic substitute, meaning that the estimator for the price-differences likely would be positively biased.

The municipality-indicators, including one variable indicating whether or not the patient lived in the county of Västerbotten, were included, together with the demographic variables, to capture socioeconomic differences among the municipalities. Differences among municipalities might be important to control for, since disproportionately many private physicians are located in the two biggest municipalities, Umeå and Skellefteå. Also, I controlled for which healthcare district the prescribing physician belonged to, and in which of the 17 quarters the prescription was written. The estimation results from specification 1 are presented in the first column of Table II (next section).

In specification 2, interaction-terms between *Private* and the six variables reflecting patients' copayment-rates are included to study if private physicians' likelihood of vetoing substitution increased faster than that of county-employed physicians, as patients' copayments fell.

In the first two specifications, private physicians are compared to county-

employed physicians, irrespective of whether they worked at health centers or clinics. Estimation results for county-employed physicians alone indicate that those working at clinics (primarily specialists) were more inclined to veto substitution than were those working at health centers (primarily general practitioners, GPs). Nearly half of the private physicians, but less than 20% of the county-employed physicians, were GPs. It is therefore quite possible that, among private physicians, a higher share of prescriptions was written by GPs, compared to those written by county-employed physicians. But since the data does not indicate whether each individual prescription was written by a GP or not, it is not possible to compare private and county-employed GPs separately, and private and county-employed specialists separately.

If being a GP makes a physician less inclined to oppose substitution, then the estimates for *Private* in the first two specifications will be underestimated, and can then be understood as lower bounds. In specification 3 (and specification 4), upper bounds were estimated by including an indicator-variable that takes the value one for prescriptions written at clinics, so that only physicians working at health centers were used as a control group for the private physicians.

In the first three specification, prescriptions written at Dragonen's health centre after it became private were excluded since the incentives for those writing these probably differed from those of both county-employed physicians and those working at small private practices. The fourth column in Table II presents the results obtained by comparing physicians working at Dragonen's health center with physicians at other health centers. An advantage of studying this health center is that both time series- and cross sectional variation can be used to study if private physicians were more likely to veto substitution. Specification four includes an indicator-variable for all prescriptions written at Dragonen's health center, to control for time invariant heterogeneity regarding the health center, and another indicator-variable for prescriptions written there after it became private (*Dragonen's^{private}*), to help test the hypothesis that private physicians were more likely to veto substitution. Since physicians are not traceable over time, it is, however, not possible to control for changes in the staff of physicians at the health centre, which might affect the estimate for *Dragonen's^{private}*.

In all estimations, a maximum-likelihood logit estimator was used and the error terms (ϵ_i) were allowed to be heteroskedastic and correlated within workplace units. Allowing for this correlation is important since Hellerstein (1998),

Coscelli (2000) and Lundin (2000), among others, found persistence in physicians' prescription behavior. Following Boyes, Hoffman and Low (1989) and Greene (1992) - who also over-sampled observations where the dependent variable took the value one, because of the low share of such observations in the population - I used sampling-weights in the estimations. Manski and Lerman (1977) and Greene (2003, Chapter 21) describe why sampling-weights should be used to avoid bias that otherwise could arise because of choice-based sampling.

To examine the robustness of the results, a large number of other specifications were also estimated. These differ from the specifications above by for example using continuous variables, instead of indicator variables, in order to control for age and the date the prescription was written. The specifications presented above were chosen over these alternatives since they had better (lower) values on the AIC information-criterion. The robustness of the results were also investigated by estimating the above specifications on different samples: for example, including observations that lack data of ATC-group or that do not belong to any ATC-group or excluding observation where no generic substitute was available for the prescribed pharmaceutical. The specifications were also estimated using a probit estimator instead of a logit estimator. The results from all these estimations are available from the author upon request and show that the presented results are robust.

3.3 Results

The estimation results in Table II are presented in terms of odds-ratios. The odds-ratio for an independent variable X is

$$odds-ratio_X = \frac{[\Pr(V = 1)|(X = 1)/\Pr(V = 0)|(X = 1)]}{[\Pr(V = 1)|(X = 0)/\Pr(V = 0)|(X = 0)]}.$$

Hence, an odds-ratio of one means that the variable X does not affect the probability of a veto. This definition also tells us that if $\Pr(V = 0)$ approaches 1, the odds-ratio for a variable X approaches the relative probability: $\Pr(V = 1)|(X = 1)/[\Pr(V = 1)|(X = 0)]$. Since physicians allowed substitution in more than 98% of the cases, the odds-ratios reported here are thus approximately equal to the relative probabilities.

Table II about here

The point estimates from the first two specifications indicate that on average private physicians were approximately 50% more likely to oppose substitution, compared to county-employed physicians, *ceteris paribus*. As noted that is a lower bound. The corresponding figure for the upper bound, obtained from the last two specifications, is about 80%. The different estimates regarding *Private* are not significantly different from each other, but all are significantly different from unity at the 5% level, and thus provide clear support for the main hypothesis in this paper.⁴

A second purpose of the study was to analyze moral hazard in insurance. The odds-ratios increase as the patients' copayment decreases. This can indicate moral hazard but, as noted, can also have other explanations. The same pattern was observed when restricting the sample to only antibacterial drugs, except that the odds-ratio for *Copay25* was below unity, though not significantly so (odds-ratio 0.93; std. err. 0.22). Because of increased standard error, the only copayment estimate that was found to be significantly different from unity was that for *Copay0* (odds-ratio 2.39; std. err. 0.51). The estimates for *Copay50* and *Copay10* are 1.16 (0.21) and 1.31 (0.31). These estimates indicate that at least the results regarding copayments are not driven solely by persistence in the physicians' prescription decisions. However, one cannot conclude whether the results are driven by previous pharmaceutical expenditures and/or moral hazard. An argument for the former is that high pharmaceutical expenditures are probably positively correlated with the number of different pharmaceuticals a patient consumes, and that a high number of pharmaceuticals can be a valid reason for a physician to veto substitution, e.g., due to the risk that the patient otherwise confuse the drugs. On the other hand, the nearly linear relationship with the patients' copayments that the estimates for *Copay50*, *Copay25* and *Copay10* show suggest that the results might be driven by moral hazard. That this pattern is broken by the high point estimates for *Copay0* can be explained by less measurement-error for that variable, and hence less attenuation.

As mentioned, the variables *Unsub* and *Free* are highly collinear with several ATC-groups, and the estimates for these variables should therefore be interpreted with caution. The results suggest that physicians were more inclined to veto substitution for pharmaceuticals which were always unsubsidized, compared to other pharmaceuticals where the patients' copayments were 100%. The odds-ratios for pharmaceuticals that were always free of charge were not

significantly different from unity.

That the patient was a woman was found to increase the probability of a veto. Physicians in the healthcare districts of Skellefteå and South of Lapland were less inclined to veto substitution than those in the omitted healthcare district (Umeå). Estimation results for age-, ATC-groups, municipalities and quarter of prescription are not reported in order to save space, but are available from the author upon request. A Wald test (not reported) shows that these groups of variables had significant effects.

Among the interaction-variables included in the second specification, only the interaction with *Unsub* was significantly different from unity. Thus the results do not indicate any differences between private and county-employed physicians regarding the degree to which they internalize patients' costs relative to the insurer's costs, even though we can not rule out that such differences exists. Estimation results (not reported) show that the difference between private and county-employed physicians' likeliness of vetoing substitution was approximately five times higher when brand-name pharmaceuticals were prescribed, compared to non-brand name ones. This indicates that a large part of the difference between the two physician groups might be explained by private physicians having stronger brand-name loyalty.

The results from the third and fourth specifications clearly show that substitution was more likely to be vetoed if the prescription was written at a clinic instead of a health center. As mentioned, the point estimates for *Private* became larger when controlling for *Clinic*. The estimated odds-ratios for the healthcare districts became closer to unity, which makes sense since a disproportionably high share of the prescriptions originating from the omitted healthcare district (*Umeå*) were written at clinics. Controlling for *Clinic* also resulted in slightly lower odds-ratios for the copayment variables.

The hypothesis that private physicians were more inclined to veto substitution was given further support by the results regarding Dragonen's health center, reported at the bottom of the last column. Physicians there became approximately 50% more likely to veto substitution when the center became private.⁵ However, even though nearly 3000 prescriptions in the sample originated from this health center after it became private, it is still only one health center. Thus the pattern found is only the result from one case study. Also, the results for Dragonen might be affected by changes in the staff of physician

at the center, which can not be controlled for with the present data.

4 Discussion

The importance of the form of compensation that physicians receive and the presence of moral hazard in insurance were analyzed by studying the determinants for whether physicians vetoed substitution or not.

The primary purpose was to test if physicians working at private practices were more likely to oppose substitution than county-employed physicians working on salary. It was found that private physicians were indeed more likely to veto substitution. Depending of how the control group was specified private physicians were estimated to be 50-80% more likely to veto substitution. Also, the results show that the likeliness of a veto on prescriptions written at Dragoon's health center increased with approximately 50% when the center became private.

The difference in the likeliness of private and county-employed physicians vetoing substitution was not significantly affected by patients' copayment-rates. Thus there is no evidence that private physicians were more inclined to please their patients in order to secure a high number of patient-visits. A possible explanation to the observed difference between the two physician groups could be that, as seems possible, private physicians have stronger brand loyalty. Allowing substitution might also be time-consuming for the physician, if it worries the patient. Hence, it could reduce the number of patient-visits per day, which would be more costly for private physicians, since their income depends on that number.

Since a physician can choose whether or not to work privately, it cannot be ruled out that the pattern found was caused by selection. The physicians who chose to work privately might have differed systematically from those that did not, for example, they might have had stronger brand-name loyalty already before becoming private physicians. Similarly, patients that chose to visit private physicians might have had systematic unobserved differences from those that did not.

A second purpose was to analyze if moral hazard in insurance affected the physicians' decisions, that is, if physicians internalized costs borne by their patients more than costs borne by the insurance. The results are consistent with

that moral hazard affected the physicians' decisions, and the point estimates imply that physicians were nearly twice as likely to oppose substitution if all costs were borne by the insurance rather than by the patient. Thus physicians appeared to act more as agents for their patients than for the insurer. The patients' copayment-rates are a function of their previous pharmaceutical expenditures, however, so it cannot be ruled out that the results were caused, for example, by physicians being more likely to veto substitution the more pharmaceuticals a patient was using.

A veto against substitution not only leads to higher cost for the current prescription but also risks reducing price-competition between pharmaceutical firms. Therefore these results are important to consider when designing physicians' contracts, and perhaps also when designing pharmaceutical insurances. However, more research is needed, especially regarding moral hazard in insurance, preferably based on data where patients can be followed over time so that persistence in pharmaceutical consumption can be studied and the number of pharmaceuticals a patient consumes can be controlled for. Further research about physicians' compensation should preferably be based on data where the share of private physicians is largely affected by policy changes, so that selection effects can be separated from treatment effects.

Notes

¹On June 4, 2009, USD/SEK = 7.67 and EUR/SEK = 10.84.

²Some employees were covered by supplemental medical insurance for prescription drugs, provided by their employer. (According to Lundin, 2000, 10% of the employees were covered by such insurances in 2000.) However, even if the entire out-of-pocket cost were covered by such extra insurance, the cost was not reduced to zero for the patient, since such fringe benefits were subject to taxation. Also, many patients were retired (45% in the dataset used here) and thus not covered by extra insurance.

³Another organizational form is the so-called community company, which ran Jörn's health centre since October 2003 with two part-time physicians. The company received its compensation in fixed lumpsum payments. The incentives for this health center therefore seem similar to those of the health centers managed by the county council. In addition, there were two personnel-managed health centers with greater autonomy from the county council, which were also compensated by lumpsum payments. Granlund, Rudholm and Wikström (2006) found no clear effect of increased autonomy on the prescription-behavior of physicians working at these health centers. Therefore, these centers, and Jörn's health center were treated in the empirical analysis here as ordinary county health centers. I tried including indicator-variables for prescriptions written at Jörn's health centre and at the personnel managed health centers, but the odds-ratios for these variables were not significantly different from unity, and the other estimates were only marginally affected by including them.

⁴Conditioned on price-differences, private physicians were approximately 40-75% more likely to veto substitution; as discussed above, however, including price-differences likely results in bias.

⁵Since physicians working at this health centre knew before June 2006 that it would be privatized, it is possible that they started to adjust to the reform before that date. Therefore specification 4 was estimated excluding observations written at Dragonen's health center between February 2006 - when the contract regarding privatization of the health centre was signed - and June 2006. This did not change the qualitative results, but the estimate for *Dragonen's^{private}* became slightly larger, 1.57 (0.10).

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Table I. Descriptive statistics

Variable	Sample	$V = 1$	$V = 0$
V	1.68	100	0
<i>County</i>	92.62	90.95	92.64
<i>Private</i>	7.38	9.05	7.36
<i>Umeå</i>	54.69	64.49	54.52
<i>Skellefteå</i>	28.63	22.46	28.73
<i>Lapland</i>	16.69	13.04	16.75
<i>Copay100</i>	36.05	25.17	36.24
<i>Copay50</i>	13.87	12.51	13.89
<i>Copay25</i>	11.04	11.06	11.04
<i>Copay10</i>	5.33	6.01	5.32
<i>Copay0</i>	29.59	43.60	29.35
<i>Unsub</i>	3.26	1.26	3.29
<i>Free</i>	0.85	0.39	0.86
<i>Women</i>	59.45	58.80	59.46
<i>Age</i>	59.17±20.19	61.93±17.58	59.12±20.23
Population size	5,112,236	85,678	5,026,558
Sample size	350,180	85,678	264,502

Notes: The mean age and, except for Copay25 and Women, the percentage of observations in each category differ significantly between the subsample where the physician vetoed substitution ($V=1$) and the subsample where the physician allowed substitution ($V=0$). For Copay10 the difference is significant only at the 5% level, while the other significant differences are at the 1% level. Data on Age is missing for 280 observations.

Table II. Estimation results, odds-ratio

	1	2	3	4
<i>Private</i>	1.50** (0.24)	1.54*** (0.23)	1.79*** (0.25)	1.75*** (0.25)
<i>Copay50</i>	1.18*** (0.03)	1.18*** (0.03)	1.16*** (0.03)	1.16*** (0.03)
<i>Copay25</i>	1.31*** (0.04)	1.32*** (0.04)	1.28*** (0.03)	1.28*** (0.04)
<i>Copay10</i>	1.39*** (0.04)	1.38*** (0.04)	1.35*** (0.04)	1.35*** (0.04)
<i>Copay0</i>	1.97*** (0.14)	1.99*** (0.15)	1.85*** (0.09)	1.85*** (0.09)
<i>Unsub</i>	1.35*** (0.13)	1.32*** (0.12)	1.34*** (0.13)	1.33*** (0.13)
<i>Free</i>	0.89 (0.52)	0.89 (0.52)	0.82 (0.44)	0.83 (0.44)
<i>Women</i>	1.10** (0.05)	1.10** (0.05)	1.13*** (0.04)	1.13** (0.04)
<i>Skellefteå</i>	0.68*** (0.09)	0.68*** (0.09)	0.77** (0.09)	0.76** (0.09)
<i>Lapland</i>	0.53*** (0.07)	0.53*** (0.07)	0.66*** (0.06)	0.64*** (0.06)
<i>Private * Copay50</i>		1.05 (0.05)		
<i>Private * Copay25</i>		0.96 (0.08)		
<i>Private * Copay10</i>		1.07 (0.05)		
<i>Private * Copay0</i>		0.91 (0.09)		
<i>Private * Unsub</i>		1.31* (0.19)		
<i>Private * Free</i>		1.32 (0.73)		
<i>Clinic</i>			1.63*** (0.23)	1.58*** (0.22)
<i>Dragonen's</i>				0.74*** (0.04)
<i>Dragonen's^{private}</i>				1.51*** (0.11)
AIC	50,891	50,889	50,668	51,077
Pseudo R ²	0.1434	0.1434	0.1471	0.1479
Sample size	346,384	346,384	346,384	349,073

Notes: The asterisks ***, ** and * denote that the odds-ratio is significantly different from one at the 1%, 5% and 10% levels. Robust standard errors are shown in parentheses.

Estimation results for age-, ATC-groups, municipalities and quarter of prescription are suppressed in order to save space, but are available from the author upon request.

In all specifications 280 observations were not used since they lack data on Age. In addition, 822 and 825 observations were not used in specifications 1-3 and specification 4, respectively, since they belong to small ATC-groups, for which no veto against substitution was observed in the sample.