"Do I really need to go to rehab? I'd say no, no, no."

Estimating Price Elasticities Of Convalescent Care Programs [‡]

Preliminary version. Comments welcome.

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December 9, 2008

Abstract

This study is the first to estimate the price elasticities of demand for medical rehabilitation programs and health spa therapies. In Germany, the Statutory Health Insurance covers both therapy forms if administered in authorized medical facilities upon the referral of a physician. While a health resort stay is prescribed to recover from a general poor health condition and has preventive character, medical rehabilitation implies recovering from a specific illness or an accident. From 1997 on, the German legislator more than doubled the copayments for both health care services from DM 12 (€ 6.14) to DM 25 (€ 12.78) per day for SHI insured. Using SOEP micro data, this exogenous price variation allows us to study the causal effects on demand as a sound control group is available. The data suggest that pull-forward effects in 1996 accounted for up to one-fifth of the subsequent decrease in demand. Accounting for this anticipation effect, we show that the reform induced a decrease in total demand of about 20 percent. We estimate the price elasticity for rehabilitation programs that aim to prevent work incapacity to be about -0.15, whereas the elasticity for rehabilitation programs to recover from work accidents lies around -0.25. In contrast, the price elasticity for spa therapies is elastic and lies between -1 and -2.

Keywords: convalescent care, health spa, medical rehabilitation, price elasticity, natural experiment

JEL classification: C93; H51; I18; J22

[‡]I would like to thank Martin Karlsson, Jochen Kluve, Michael Kvasnicka, Hendrik Schmitz, Tom Siedler, Gert G. Wagner, and participants at various seminars for their helpful comments and discussions. A special thank goes to Deborah Bowen who did the proofreading. I am responsible for all remaining errors and shortcomings of the article.

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1 Introduction

How does the demand for medical care change, when prices change? Since the early days of the profession of health economics, the question of the price elasticity for health services has been central. Joseph P. Newhouse alone, one of the founders of the field of health economics, has published more than 100 articles on health care demand. Newhouse was also the leader of the RAND group that designed and directed the RAND Health Insurance Experiment which predominantly analyzed the impact of cost-sharing on the demand for medical care. Not only Newhouse but many other renowned eonomists have devoted a great amount of time to this topic.

Many studies have tried to causally link the level of cost-sharing to demand for single health services or whole insurance plans. The great majority exploits data from North America and the 60s, 70s, or 80s. Unfortunately, most studies are not representative for the U.S. as they rely on single cities, counties, states, or data on special health insurance plans. In a private health insurance market like in the U.S., appropriately dealing with health status dependent sorting into different health plans is extremely challenging. Studies that try to circumvent this endogeneity problem by means of IV usually lack a convincing identification strategy. Another issue is the poor comparability of the estimates as private health insurances usually offer plans that differ with respect to deductibles, coinsurance rates, stop-losses, and copayments. The change in demand is then calculated as the response to a change in one or more of these parameteres. To make estimates with variations in two or more parameters comparable to those where only one parameter is varied, it becomes necessary to extrapolate the results making more or less strong assumptions.

Most studies estimate the overall price elasticity of the demand for health services to lie around -0.2. Outpatient care is found to be more elastic than inpatient care and mental health care more responsive than outpatient care. There is evidence that price elasticities are higher in the short than in the long-run. Moreover, is has been empirically and theoretically shown that elasticities are lower the more severe the illness and the more urgent the care is (O'Grady et al., 1985; Wedig, 1988; Keeler et al., 1988; Lee, 1995; Zweifel and Manning, 2000). However, since elasticities are most likely subject to cultural influences and health care systems and may change over time, there won't be a conclusive answer to how demand for medical care responds to variations in prices.

This study analyzes how an exogenous increase in the copayment rate affected the demand for medical rehabilitation and health spa programs in Germany in the 90s. We thereby contribute to the existing literature in various ways. Most importantly, this is the first study at all that provides price elasticity estimates for medical rehabilitation and health spa treatments. As only one single parameter is exogenously varied, elasticites are computed without making further assumtions about other cost-sharing elements. Additionally, this study is one of the few on health demand that comes from Europe and uses representative data from the 90s. It is noteworthy that selection into or out of the treatment is no issue in our context. The German majority is compulsorily insured with the German Statutory Health Insurance (SHI) which provides free universal health care coverage. An independent Private Health Insurance (PHI) system coexists for special income and occupational groups. Strict legal regulations prevent switching between the SHI and the PHI making treatment related sorting impossible and guarantees a sound control group for this natural experiment. We show that a doubling of the copayment rate has led to an overall decrease in demand for convalescent care of around 25 percent. As the data suggest that full-foward effects in the year prior to the reform are likely to play a role, the pure policy induced decline in demand is estimated to lie around

20 percent. Relating the increase in prices to the decrease in demand gives us price elasticities for various convalescent care programs.

2 The German Health Care System and the Policy Reform

The German health care system actually consists of two independent health care systems existing side by side. The major of the two is the Statutory Health Insurance (SHI) that covers about 90 percent of the German population. Employees whose income from salary is below a politically defined income threshold (2007: €3,975 per month) are compulsorily insured with the SHI. High-income earners who exceed that threshold as well as self-employed have the right to choose between the SHI, a private health insurance, or to remain uninsured. All SHI insured are subject to a universal benefit package which is determined at the federal level and codifed in the Social Code Book V (SGB V). Coinsurance rates are prohibited in the SHI and thus, apart from copayments, health services are fully covered (German Ministry of Health, 2008).

The second track of the German health care system is the Private Health Insurance (PHI). It basically covers private sector employees above the income threshold, public sector employees, and self-employed. Privately insured people pay risk-related insurance premiums based on a health checkup at the beginning of the insurance period. The premiums exceed the expected expenditures in younger ages as the health insurer makes provisions for rising expenditures in older ages. Coverage is provided according to different health plans and insurance contracts are subject to private law. Consequently, in Germany, public health care reforms affect the SHI rather than the PHI.

It is important to highlight that the great majortiy of compulsorily insured has no right to chose the health insurance system and the benefit package. Once an optionally insured (high earners and self-employed) opts out of the SHI system, a switch back is practically not possible. Employees above the income threshold are legally not allowed to switch back and employees who fall below the income threshold in subsequent years may switch back but loose their provisions as they are not transferable (neither between PHI and SHI, nor between the different private health insurances). In reality, a change to a private health insurance may be regarded as a lifetime decision and switching between the SHI and the PHI system as well as between private health insurances is very rare.

The German Market for Convalescent Care

Apart from small copayments, the German SHI fully covers health spa and medical rehabilitation therapies. However, both therapies require a physician's prescription and an application for treatment needs to be filed to the sickness fund. The role of the patient within the application process is central. On the one hand, well informed patients may push their doctors to write complaisant medical opinions, who in turn, need to fear to loose their patients if they reject, due to the free choice of medical practioners for SHI insured. On the other hand, patient may refuse their doctor's proposal to go to a health spa or to rehab. After the application, the sickness fund scrutinizes whether the premises for a treatment are fulfilled and authorizes the therapy. The wordings of the preconditions can be found in the German social legislation, Social Code Book V (SGB V, article 23 para. 1, article 40 para. 1). In general, the prescription of a spa stay requires that the patient is in a bad health condition which is likely to lead to an illness or disability in

foreseeable time. Hence, health spa therapies have a preventive character although the patient is already confronted with health problems. Medical rehabilitation, by contrast, implies that an illness already exists. After authorization through the sickness fund, both therapy forms are carried out in an approved medical facility that is under contract by the sickness fund. These medical facilities are usually situated in rural scenic state-approved villages called *Kurorte*. For a village to get such a license, several conditions need to be fulfilled, e.g. extraordinary clean air, location at the seaside, or mineral springs. These requirements are established by federal state laws. Hence, patients undergo a change of location which is often integral part of the treatment program.

The German market for convalescent care is said to be the largest world-wide, at least when the booming wellness industry is not considered. In 1995, a total sum of 7.646 billion Euros was spent on convalescent care treatments accounting for more than 4 percent of all health expenditures in Germany (German Federal Statistical Office, 2008). Almost 6 billion Euros thereof were raised by the SHI and the Statutory Pension Insurance (SPI). Around 1,400 medical facilities with 100.000 full time equivalent employees treated 1.9 million patients who stayed on average 31 days. The majority of these cases were medical rehabilitation treatments. Health spa therapies for SHI insured in poor health, most of whom were retirees or family insured, accounted for 300.000 cases (German Federal Statistical Office, 2008).

The German SPI is legally obligated to pay for those medical rehabilitation therapies that help to prevent permanent partial or total work disabilities. This goes back to the basic principle "rehabilitation before pension" in the social insurance system. Thus, the SPI basically comes up for patients in gainful employment whose illness is severe enough to threaten their work capacity. This funding principle will be of relevance later on.

The Policy Reforms on Convalescent Care

At the end of 1996, the German government under chancellor Kohl implemented two health reforms which both affected convalescent care programs. The first stands in the focus of this study. The copayments for inpatient health spa stays and medical rehabilitation therapies were increased from DM 12 (€6.14) per day to DM 25 (€12.78) per day from January 1, 1997 onwards in West Germany. In East Germany, the copayments were increased from DM 8 (€4.09) to DM 20 (€10.23). This reflects an increase of 108 (150) percent.[1] The second law intended to bring down the sickness absence rate and to fight moral hazard. The replacement rate for sickness spells up to six weeks was reduced from 100 percent to 80 percent of foregone gross wages for employees. As employees are eligible for sick pay during a health spa or medical rehabilitation treatment, this law is supposed to affect convaslescent care, too. Moreover, the legislator allowed employers to deduct two days of paid vacation for every five days that the employee was unable to work due to convalescent care.[2]

[1] The correct German name of this law that was passed on November 1, 1996 is *Gesetz zur Entlastung der Beiträge in der gesetzlichen Krankenversicherung (Beitragsentlastungsgesetz - BeitrEntlG), BGBl. I 1996 p. 1631-1633.* Additionally, the time that needs to elapse between two treatment episodes, for the insured to be eligible again, was extended from 3 to 4 years and the regular length of both therapy forms was reduced from 4 to 3 weeks. Both changes were only effective conditionally on the non-existence of severe medical reasons for a treatment. While the reduction of the regular length of stay should be of negligible overall importance, we will assess the impact of the shortening of the qualifying period in Section 5.

[2] The correct German name of this law that was passed on September 15, 1996 is *Arbeitsrechtliches Gesetz zur Förderung von Wachstum und Beschäftigung (Arbeitsrechtliches Beschäftigungsförderungsgesetz), BGBl. I 1996 p. 1476-1479.* The law became effective at October 1, 1996. It should be noted that we are not able to perfectly identify those employees who were affected by the cut in sick pay as employers and unions voluntarily agreed in some collective wage agreements upon the continuity of the old sick pay arrangement.

Table 1: Definition of Subsamples

	Copayment increase (1)	**Paid vacation reduction (2)**	**Sick pay decrease (3)**
Private sector Employees with SHI (1)	yes	yes	yes
Self-employed with SHI (2)	yes	no	no
Non-working with SHI (3)	yes	no	no
Public sector employees with SHI (4)	yes	no	no
Trainees with SHI (5)	yes	no	no
Self-employed with PHI (6)	no	no	no
Non-working with PHI (7)	no	no	no
Public sector employees with PHI (8)	no	no	no
Trainees with PHI (9)	no	no	no

We now define subsamples that were affected differently by the increase in coypayments, the cut in sick pay, and the deduction of paid vacation. These subsamples serve as treatment and control groups in the evaluation of this natural experiment.

We see from Table 1 that subsamples (6) to (9) were completely unaffected by all three legislative changes. Consequently, these subsamples serve jointly as control group.

Subsamples (2) to (5) did not suffer from cuts in sick pay and/or paid vacation. However, as SHI insured, they underlay the copayment increase. We use these subsamples to isoloate the effect of the increase in copayments from the cut in compensation levels and define these subsamples jointly as Treatment Group 2. Disentangling the effects becomes evident when looking at subsample (1) which was affected by all policy changes and which we call Treatment Group 1.

3 Data And Variable Definitions

Dataset

The empirical analysis relies on micro data of the German Socio-Economic Panel Study (SOEP). The SOEP is an annual representative household survey that started in 1984 and sampled more than 20,000 persons in 2006. Wagner et al. (2007) provide further details. We use data of the waves 1995 to 1999 that include time-invariant information, current information, and retrospective information that concerns the previous year. As our main dependent variable contains information about the year prior to the interview, we employ data that is related to the years 1994 to 1998.[3]

[3] If the respondent was interviewed in two subsequent waves, e.g. in 1994 and 1995, we match the time-variant data of the first year that display current information with the data of the second year that concerns the first year. For example, in 1995 respondents were asked about their *current* health status but about their insurance status during the *last* year. Hence, we use the information of 1994 about the health status together with the data of 1995 about the insurance status if the respondent was interviewed in both years.

We use the whole sample size except for respondents under the age of 18 as they are exempted from copayments. Moreover, we necessarily need to drop observations with missings on the exogenous or endogenous variables.

Dependent Variables

The SOEP contains various questions about the usage of health services and the health insurance. Our main dependent variable *Rehab* measures whether the respondent went to a health spa or underwent an inpatient medical rehabilitation therapy in the previous year. It takes on the value one if that was the case and zero if not. This variable has been generated from the following question that was continuously asked from 1995 to 1999:*"Were you admitted to a health spa or an inpatient medical rehabilitation therapy last year?"* In German, this question becomes even clearer because of the well known umbrella term *Kur* and the therapy-related change of location which minimizes measurement errors. That we do not know the exact time period of the therapy does not severely hamper the analysis, even more as such treatments are usually not carried out over Chrismas and New Year's Day. Hence, there should be no doubt whether the therapy was in 1996 or 1997.

By combining our main dependent variable with further questions, we generate additional dependent variables which we employ in refined model specifications. We call the first of the two *Rehab due to illness*. It takes on the value one for respondents who have a one on their *Rehab* variable and who claimed that the SPI funded their therapy.[4] As we know that the SPI only funds rehabilitation therapies to prevent work incapacities, we thus capture employees who underwent such a rehabilitation therapy.

Two dependent variables are generated to measure the incidence rate of medical rehabilitation therapies to recover from a work accidenct (*Rehab due to accident*). Respondents were asked whether they were admitted to a hospital or whether they received medical treatment because of a work related accident in the previous year.[5] Hence, the third dependent variable has a one for those employees who had a work related accident that required a hospital stay and whose *Rehab* variable takes on the value one. Analogously, a variable with a one for respondents who claimed to having needed medical treatment due to a work related accident and with a one on their *Rehab* variable is generated (see Table 2). In both cases it is very likely that those individuals unterwent the medical rehabilitation therapy due to the work related accident. Even small measurement errors would not be a big issue as long as the reforms had no influence on the measurement errors which should not be the case.

Control Variables and Treatment Dummies

In our econometric specifications, we make use of various control variables. These control variables capture personal and family related characteristics such as *age*, *female*, *immigrant*, *partner*, or *children*. Moreover, we control for educational characteristics by using data on the highest school degree obtained. An important determinant of the demand for health spa and rehabilitation treatments is the health status of the respondents which we control for. We also include covariates that measure whether the person was

[4] The exact SOEP question reads: *"Who paid the greater part of the costs? a.) Statutory Pension Insurance b.) Statutory Health Insurance c.) other organization ."* As this question was only asked until 1998, we cannot employ the 1999-wave in the models where this variable serves as dependent variable.

[5] The exact SOEP question reads: *"Were you in the hospital or did you receive medical treatment last year because of a work related accident? a.) yes, received medical treatment b.) yes, went to the hospital c.) no"*

Table 2: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Rehab	0.039	0.194	0	1	65486
Rehab due to illness	0.012	0.107	0	1	63650
Rehab due to accident:					
with hospital stay	0.00123	0.035	0	1	34073
w/o hospital stay	0.00173	0.042	0	1	34090
Personal characteristics					
Female	0.519	0.5	0	1	65486
Age	44.234	16.633	18	99	65486
Age squared	2233	1621	324	9801	65486
Immigrant	0.181	0.385	0	1	65486
East Germany	0.275	0.447	0	1	65486
Partner	0.722	0.448	0	1	65486
Children	0.405	0.491	0	1	65486
Educational characteristics					
Drop out	0.062	0.241	0	1	65486
Degree after 8 years' schooling	0.39	0.488	0	1	65486
Degree after 10 years' schooling	0.288	0.453	0	1	65486
Degree after 12 years' schooling	0.03	0.17	0	1	65486
Degree after 13 years' schooling	0.131	0.337	0	1	65486
Other degree	0.088	0.283	0	1	65486
Job characteristics					
Full-time employed	0.455	0.498	0	1	65486
Part-time employed	0.077	0.266	0	1	65486
Marginally employed	0.02	0.141	0	1	65486
Gross wage per month	1163	1301	0	51129	65486
Regional unemployment rate	12.016	3.9	7	21.7	65486

full-time, part-time, or non-employed and additionally control for the gross income per month. A list of all covariates, their means and standard deviations can be found in Table 2.

According to Table 1, we define three treatment dummies that mirror whether a person belongs to Treatment Group 1, 2, 3, or to a control group. *Treatment Group 1* takes on the value 1 if the person belongs to Treatment Group 1 and zero if the person is in the control group. *Treatment Group 2* also has a zero for those in the control group and a one for belonging to Treatment Group 2 whereas *Treatment Group 3* takes on the value one when respondents are in Treatment Group 3 and zero when they are in Treatment Group 2. To estimate the causal reform effects, each of these treatment dummies enters the model specification alone and in interaction with a reform year dummy as will be explained in the next subsection.

4 Estimation Strategy

Difference-in-Differences

We would like to measure how the different reform elements affected the decision to go to a health spa or to particpate in a medical rehabilitation program. Thinking of the policy intervention as a treatment, we fit a probit model of the form:

$$P[y_i = 1|\boldsymbol{X}] = \Phi(\beta_0 + \beta_1 \text{post97} + \beta_2 \text{D} + \delta \underbrace{(\text{post97*D})}_{\text{DiD}} + \boldsymbol{\psi'}\beta) \quad (1)$$

where *post97* is a dummy that takes on the value one for the post-reform years and *D* is the treatment dummy. The interaction term between both dummies gives us the DiD estimator with δ being the causal effect of the policy intervention. To evaluate how the reform affected our outcome variable, henceforth, we always compute and display the marginal effect of the interaction term $\frac{\Delta\Phi(.)}{\Delta(\text{p97*D})}$.[6] $\Phi(.)$ is the cumulative distribution function for the standard normal distribution and $\boldsymbol{\psi'}$ is a vector including all other covariates.

Identification

Our main identification strategy relies on DiD estimation and hence on the assumption of a common trend for the treatment and control group in the absence of the treatment. As we control for various socio-economic characteristics, including the health status, and as it is difficult to think of unobservables that might have influenced the dynamic of the outcome differently for treated and controls, we argue that this assumption is likely to hold.

In recent years, there has been an extensive debate about drawbacks and limitations of DiD estimation. A special concern is the underestimation of OLS standard errors due to serial correlation in case of long time horizons and unobserved (treatment and control) group effects when the number of groups is small. We focus on short (and varying) time horizons. As Bertrand et al. (2004) have shown, the main reason for understating the standard errors stem from serial correlation of the outcome and intervention variable which is basically eliminated when focussing on less than 5 periods. We also use robust standard errors and correct for clustering at the individual level.[7]

A crucial issue in most studies that try to evaluate policy reforms is, beside the absence of a control group, selection in or out of the treatment. We are in the fortunate position to rely on a framework were two almost independent health care systems exist side by side as explained in Section 2. On the one hand, this grants a well defined control group and on the other hand, we do not need to fear that reform induced selection distorted the results as there is virtually no switching between the SHI and the PHI and as all SHI insured are covered by universal health plans. Since a switch to the PHI system can be seen as a lifetime decision, it is very unlikely that SHI insured changed to the PHI system in reaction to the reforms. Even theoretically, a switch to the PHI was only possible for a small fraction of optionally SHI insured which we are able to identify and to exclude when running robustness checks.

Furthermore, we need to consider the possibility of pull-forward effects. A health spa or rehabilitation therapy is usually planned a couple of months, or even years, in advance. Certainly, health spa stays are easier to schedule than rehabilitation therapies. Since the first policy reform plans were made public at the end of 1995 (Handelsblatt, 1995), it may be that a significant portion of SHI insured realized their health spa stays or medical rehabilitation programs in 1996 instead of 1997. Consequently, we take care of this

[6] Puhani (2008) has shown that the advice of Ai and Norton (2004) to compute the discrete double difference $\frac{\Delta^2\Phi(.)}{\Delta\text{p97}\Delta\text{D}}$ is not of relevance in nonlinear models when the interest lies in the estimation of a treatment effect.

[7] As Imbens and Wooldridge (2007) note, the two-step estimation approach proposed by Donald and Lang (2007) has several shortcomings and can not be applied in the case of only one treatment and one control group. Imbens and Wooldridge (2007) show that for the two group case, Donald and Lang's criticism is equivalent to the fundamental question in any DiD analysis on whether the observed effect goes entirely back to the policy change or not.

by including and excluding the year 1996 in our analysis.

To be able to fully attribute changes in the incidence rates to changes in the demand for health spa and medical rehabilitation programs, supply side effects should not play a role. We have not found indications for supply side constraints. Contrarily, there were reports about the deepest crisis in the market for convalescent care since the end of the Second World War (Handelsblatt, 1998). Dozens of medical facilities in health resorts had to close and, hence, there is strong evidence that there was excess of supply.

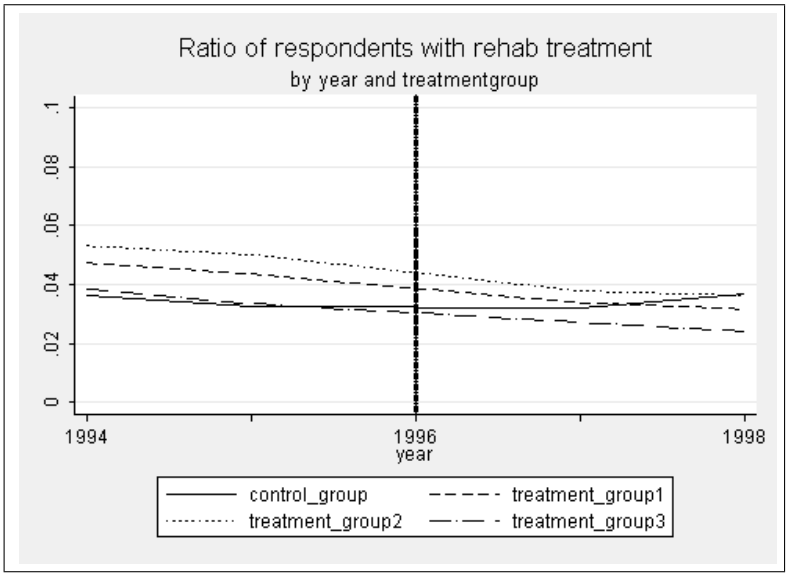
Admittedly, it may have been the case that, due to awareness rising and political pressure, the SHI and the PHI were more restrictive in the authorization of therapy programs during the time when the reforms were in the political discussion. We take care of this possibility by excluding 1996 in serveral model specifications.

A hampering point in our identifiaction strategy goes back to the design of the data. As the respondents were not directly asked about the medical reasons for the therapy, we can only indirectly identify specific therapy forms. As described in Section 3, we combine the general question about a convalescent care therapy with the more specific question of the funder of the therapy. Thanks to the general and clear principle in the German Social legislation that the SPI only funds medical rehabilitation therapies that are necessary to prevent work incapacity, we are able to identify such therapy forms. While the identification of this therapy form is rather distinct, we need one assumption to identify another specific rehab therapy form: we plausibly assume that respondents who had a work related accident that required medical treatment and who were in the same year prescribed a convalescent care therapy, administered the therapy due to the accident. In this context, we have to point out that the number of respondents who belong to the control group and who underwent a medical rehabilitation therapy due to a work related accident is so small that we should not build our analysis upon this group. Hence, we discard the controls in these subspecifications and rely our analysis on all respondents who faced a copayment increase, e.g. subsamples (1)-(5), to obtain a sufficiently large sample size. For both specific therapy forms, rehabilitation therapies due to illness as well as due to work related accidents, the daily copayments were unambiguously increased. Hence, we can accurately attribute post-reform changes in demand to reform induced exogenous price variations.

Due to various methodological issues, never before was an elasticity estimate for preventive care computed. As health spa stays can be categorized as some form of preventive care, we make the first try to compute an elasticity estimate relying thereby on the results for the other specifications. Unfortunately, we cannot directly identify such therapy forms. However, we know how the total demand for convalescent care reacted to the copayment increases and know from official data that health spa therapies accounted for 15.13 percent of all convalescent care therapies. As we also know the fraction of medical rehabilitation therapies and can directly identify and calculated the reform effects for rehab therapies to prevent work incapacity, we can derived and indirectly calculate price elasticities for health spa stays. More details about the assumptions that lead to the upper and lower bound elasticity estimates can be found in Section 5.

As a last point we should emphasize that our identification strategy relies on different model specifications according to our main outcome measures: convalescent care treatments, rehab treatments due to illness, rehab treatments due to work accidents that required a hospital stay, and rehab treatments due to work accidents that required medical treatment but no hospital stay. Additionally, the first two of these model specifications are estimated by including alternatively three different treatment dummies represent-

Figure 1: Unconditional Incidence Rates of Rehab Stays over time (without 1996)



ing different subsamples which were differently affected by the reforms. This whole setup guarantees the results to be based upon various pillars with each underlying exogenous price variations. Hence, the identification of the reform effects is backed up by a broad strategy. By crosschecking, the plausibility and sensitivity of our results is automatically verified.

5 Results

Figure 1 displays the average incidence rates for health spa and medical rehabilitation programs from 1994 to 1998. It is easy to see that the level was generally higher for Treatment Group 1 as compared to the control group. Already before the reforms became effective, the average rate for Treatment Group 1 decreased from 4.7 percent in 1994 to 4.2 percent in 1996 and then fell sharply to 3.4 percent in 1997 and 3.2 percent in 1998. As for the control group, we also observed a decline from 1994 to 1996 but instead of a further drop, in 1997 and 1998 the demand for rehabilitation therapies rose again. In column six, we averaged the incidence rates for the pre- and the post-treatment periods and both groups and took the difference. The unconditional DiD estimate represents a decline of -1.52 percentage points which reflects an overall reform induced decrease of about 34 percent as compared to the average post-reform incidence rate (column seven).

All regression results in Table 3 were estimated with a pooled sample according to equation 1. Columns one to three incorporate the years 1994 to 1998, whereas in columns four to six the pre-treatment year 1996 is ommited because of potential pull-forward effects. Column one shows estimation results when we include Treatment Group 1 as treatment dummy and thus contrasts everybody who was affected by the increase in copayments with those who were entirely unaffected by the reforms. We find that the reforms led to a significant decrease in the incidence rate of convalescent care treatments of around 1.2 percentage points. The absolut decrease is a little bit higher (-1.3 percentage points) when we compare SHI insured who were *only* exposed to the increase in copayments with the control group (column two). On the other hand, the specification in column three, which compares those who were affected by all policy changes

with those who were only affected by the increase in copayments, does not provide evidence that the cut in sick pay and vacation days had a significant effect. Columns four to six reinforce the previous results: The increase in copayments has induced a significant decrease of the incidence rate of health spa and medical rehabilitation treatments of around 1 percentage point. However, we find no evidence that the cut in sick pay and paid vacation led to a decrease. Interestingly, if we do no consider the pre-treatment year 1996, the magnitude of the decrease is approximately 0.25 percentage points, or around 20 percent, lower which suggests that pull forward effects may have occured.

To investigate whether a change in the composition of the sample size may have confounded our results, we balance the sample and restrict it to respondents who were successfully interviewed in all years and have valid information on their variables. The same picture as before appears (not displayed). We find that the reforms caused a significant decrease of 1.22 percentage points when all those who were exposed to the increase in copayments are compared to the control group. The decrease shrinks about 0.20 percentage points when we do not consider 1996 (-1.05). This is also true when we contrast those who were only affected by the copayment increase with those who were totally unaffected (-1.45 vs. -1.3 percentage points). Again, we find no evidence that the cut in sick pay and the deduction of vacation days had any effect.

We now want to estimate how the reforms affected medical rehabilitation programs for working people who run the risk of becoming incapacitated for work without treatment. The SPI funds these programs and we have information about the funder of the therapy for the years 1994 to 1997. Hence, columns one to three in Table 4 display regressions results when we analyze respondents who indicated that the therapy was paid by the SPI. We find marginally significant but substantially lower effects of -0.28 and -0.24 percentage points when Treatment Group 1 and 2 are considered. Again, we find no effect for Treatment Group 3.

It should be noted that the power of the statistical tests clearly decreases due to smaller sample sizes for the refined specifications. However, in contrast to column three, the coefficients in column one and two show p-values of 0.11 and 0.21. We take this as evidence that it is solely the sample size that caused the p-values to decrease in comparison to Table 3.

As we also have information whether the employee had a work related accident in the corresponding year, we consider people who answered the *Rehab* and the *Work Accident* question with "yes." By this means, we estimate how the reforms affected medical rehabilitation treatments due to work accidents. Even more, we further differentiate whether to work related accident required a hospital stay or only outpatient medical treatment. This comes at the cost of loosing the control group as too few repondents in these subsamples meet these criteria. As most of the employees with work accidents and rehab stays are in subsample (1), we use Treatment Group 1 for these specifications. As we have already seen that the results are more or less identical irrespective of whether we use Treatment Group 1 or 2, the focus on Treatment Group 1 should not make a big difference. Column four gives us a marginally significant coefficient of -0.038 percentage points and according to column five, the reform induced a significant decrease of -0.07 percentage points. Note that the strength of the decrease is not comparable between the specifications as the average incidence levels differ between the specifications. For example, the overall incidence rate for rehab therapies due to work accidents with hospital stays for Treatment Group 1 and the pre-treatment period was 0.148 (see Table 2 for average incidence rates of all dependent variables) leading to a reform induced decrease in demand of around 25 percent.

Table 3: Difference-in-Differences Estimation With Pooled Sample

	Years: 1994-1998			**Years: 1994/1995 vs. 1997/1998**		
Variable	**Treatment Group 1**	**Treatment Group 2**	**Treatment Group 3**	**Treatment Group 1**	**Treatment Group 2**	**Treatment Group 3**
DiD (d)	-0.0118**	-0.0130**	0.0020	-0.0092*	-0.0106*	0.0008
	(0.005)	(0.005)	(0.003)	(0.005)	(0.006)	(0.003)
Treatment Group (d)	0.0068**	0.0062	0.0011	0.0041	0.0032	0.0028
	(0.003)	(0.004)	(0.002)	(0.004)	(0.005)	(0.003)
Educational characteristics	yes	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
R-squared	0.0966	0.0903	0.0957	0.0953	0.0891	0.0946
χ^2	1560	995	1459	1289.7952	827.5800	1202.4146
N	65486	41960	61229	52875	33928	49385

(d) for discrete change of dummy variable from 0 to 1

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: dummy that is 1 if respondent went to health spa or rehab

Standard errors in parentheses are adjusted for clustering on person id

Table 4: Working population: Medical Rehabilitation Due To Illness vs. Medical Rehabilitation Due To Accident

	Medical Rehabilitation: Illness			**Medical Rehabilitation: Accident**	
Variable	**Treatment Group 1**	**Treatment Group 2**	**Treatment Group 3**	**with hospital stay**	**w/o hospital stay**
DiD (d)	-0.0028	-0.0024	-0.0005	-0.0004*	-0.0007*
	(0.002)	(0.002)	(0.001)	(0.0002)	(0.0004)
	[0.111]	[0.211]	[0.677]	[0.097]	[0.083]
Treatment Group (d)	-0.0107***	-0.0147***	0.0021**		
	(0.003)	(0.004)	(0.001)		
Educational characteristics	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes
R-squared	0.1272	0.1190	0.1298	0.084	0.0725
χ^2	773	425	683	100	88
N	50670	32219	47322	34073	34090

(d) for discrete change of dummy variable from 0 to 1

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable in first three columns: dummy that is 1 if respondent had rehab stay which was paid by SPI

Dependent variable in column four: dummy that is 1 if respondent had rehab stay and work accident that required a hospital stay

Dependent variable in column five: dummy that is 1 if respondent had rehab stay and work accident that required medical treatment but no hospital stay

Column four and five: only subsamples (1)-(5) are used

Standard errors in parentheses are adjusted for clustering on person id

P-values in sqared brackets

Table 5: Treatment Group 2: Various Model Specifications

Variable	**1996 vs. 1997**	**w/o optional insured**	**hospital < 7 nights**	**hospital > 7 nights**	**funded by SHI**	**West Germany**
DiD (d)	-0.0170*	-0.0120**	-0.0094**	-0.0052	-0.0111**	-0.0143**
	(0.009)	(0.005)	(0.004)	(0.003)	(0.0049)	(0.051)
Treatment Group (d)	0.0108**	0.0059	0.0047*	0.0013	0.0065*	0.0044
	(0.005)	(0.004)	(0.003)	(0.002)	(0.0031)	0.0039
Educational characteristics	yes	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
R-squared	0.0946	0.0908	0.0766	0.0861	0.1135	0.0934
χ^2	486	926	574	691	635	680
N	16913	38903	38364	40933	32751	29728

(d) for discrete change of dummy variable from 0 to 1

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: dummy that is 1 if respondent went to health spa or rehab

Standard errors in parentheses are adjusted for clustering on person id

In Table 5 we present additional model specifications to prove the consistency and robustness of our results. Therefore, we limit the analysis to Treatment Group 2, e.g. the "pure" copayment effect. Column one gives the esimation result when the period under consideration is shortened to two years and thus represents the short-run impact of the reform, full-forward effects included. Consequently, the reform effect is higher and lies around -1.7 percentage points. To definitely exclude the possibility that selection in or out of the treatment distorted our results, we drop the only group that had the option to change the health insurance system, namely optionally insured. We see that the estimates almost perfectly fit to our basic results as the reform effect of the copayment increase is estimated to be -1.2 percentage points. We also try to differentiate by severity of illness by dropping respondents who were more and less than seven nights in hospital in the corresponding year, respectively. We find that the reform effect is almost twice as large for those who stayed less than one week in hospital (-0.94 vs. -0.52 [p-value: 0.119]). Although we cannot identify which specific convalescent care programs were funded by the SHI, we find a plausible 1.11 percentage point decrease (base incidence level: 2.9 percent) for these programs. Finally, restricting the sample to West Germans leads to a significant 1.4 percentage points reform induced decline. All in all, these robustness checks are insofar remarkable as every single of these specifications fits to our main results and is in line with the literature.

As a final check to corroborate the common time trend assumption, we also estimated placebo regressions for the years 1995 and 1996. That is, we pretend as if the reform became effective in 1995 or 1996. If any significant reform effects came up, the assumption of a common time trend for treatment and control group in the absence of the treatment would be seriously challenged. However, this is not the case (results not shown).

Price Elasticities of the Demand for Convalescent Care

Using the estimates from the previous subsection, we calculate arc price elasticities of demand for various convalescent care programs. To compute the change in demand we rely on the estimates for Treatment Group 2 in Tables 3 and 4.

Another, so far neglected, reform element is likely to have partially driven the decrease in the incidence rates. Given that no severe medical reasons speak against it, the qualifying period between two therapies to be eligible for cost coverage again, was reduced from three to four years in the course of the reforms. This change applied for all forms of convalescent care programs. Under the assumption that a fraction of insured is well-informed and takes advantage of the legal possibility to undergo a therapy regularly, we would overestimate the copayment induced decrease in demand and thereby the elasticities. Looking at the data reveals that indeed around 20 percent of the average program users are prescribed such therapies regularly according to the legally defined minimum time intervals. For example, an SHI insured who was prescribed a therapy in 1990 is eligible for another therapy not until at least three years have passed in case of no urgent medical reason. Taking all SHI insured, i.e. subsamples (1) to (5), 17.8 percent of those who were presribed a convalescent care treatment in 1990, underwent such a therapy again in 1993 (or 1994) but not in 1991, 1994 (1993), or 1995.[8] These number are confirmed if we take those who were in therapy in 1991 and again in 1994 or 1995 without being in therapy in-between. In total, 17.8 and 16.9 percent of those who were in therapy in 1990 and 1991, respectively, underwent a therapy after

[8] We do not have data for the year 1992. To obain the numbers, we condition on those respondents you answered the questionnaire in the years 1991, 1992, 1994, 1995, 1996.

approximately three years again. This also means that their illness was not severe enough for a freely chosen physician to certify that urgent medical reasons would justify another therapy before the expiring of the waiting time. Hence we take these numbers as evidence that around 20 percent of all administered convalescent care therapies go back to patients who know their rights very well and who participate in convalescent care programs on a regular basis. Given that we have information about two post-reform years, the prolongation of the qualifying period prevented one year-fraction of well informed to attend a therapy in these two post-reform years. Hence, when calculating the copayment induced demand decrease, we subtract 10 percent of the average incidence rate from the estimated percentage point decrease.

Before the reforms became effective, experts claimed that around a quarter of all health spa and rehabilitation therapies were unnecessary (Schmitz, 1996; Sauga, 1996). Moral hazard was one central argument for politicians to increase the copayments. Remember that according to our estimates, around 20 percent of the overall demand decrease is attributable to pull-forward effects. These numbers fits to the estimated proportion of well-informed regularly convalescent care program users which are very likely to be responsable for the bulk of the anticipation effect. The plausibility and complementarity of these numbers are additional pieces of evidence for the consistency of our estimation and identification strategy.

Table 6 emerges. We differentiate between estimates were all years from 1994 to 1998 are considered and estimates without the year 1996 to make allowance for pull-forward effects. Additionally, row three shows estimates for West Germany which might be more precise as we do not have to weight the price increases according to the overall East/West incidence rate of convalescent care programs. Another argument for separate West elasticities would be that East Germans might have reacted differently to the reforms only 7 years after the reunification. On the other hand, the precision of the estimates decrease due to a smaller sample size.

All elasticities are of a reasonable and overlapping range and show the same general pattern. We always find that the computed elasticities without considering 1996 are lower which implies strong evidence for anticipation effects. Moreover, the estimated elasticities for West Germany are of the same range as the overall elasticities but are a little bit higher in absolut values throughout all specifications.

As already mentioned in Section 4, we can only indirectly calculate the price elasticities for health spa programs and need to make further assumptions. Hence we present broad upper and lower bound estimates which we obtain under the following assumptions. We rely our calculations on data from the German Federal Statistical Office (2008) stating that between 1994 and 1998, health spa programs accounted for 15.13 percent of all convalescent care programs. Outpatient health spa programs, for which no copayments were raised, made up 8.14 percent. Back-of-the-envelope calculations under consideration of the decrease in total demand for convalescent care and the decrease in demand for rehab programs to prevent work incapacits results in a reform induced decrease in total demand for health spa programs of 67.9 percent. If we fully attribute this decrease in demand to the copayment increase for inpatient health spa programs which made up 46.2 percent of all health spa programs (German Federal Statistical Office, 2008), we end up with an overall elasticity of about -2. For the reasons mentioned above and as a high degree of nescience among the insured can be assumed, this should be regarded as an upper bound estimate. The German media transmitted the impression that the increase in copayments was valid for all *Kuren* and did not make a distinction by explaining that outpatient health spa therapies were not affected. Hence, we compute a lower bound elasticity for health spa therapies attributing the total decrease in demand to all health spa programs ending up with an estimate of around -1. Hence, it is reasonable to conclude that in contrast to rehabilitation programs, the price elasticity for health spa therapies, which can be classified as preventive health care, is elastic.

Table 6: Price Elasticity Estimates for Different Convalescent Care Programs

	Rehabilitation *and* health spa programs	**Rehab due to illness** (to avoid work incapacity)	**Rehab due to work accident: hospital**	**Rehab due to work accident: no hospital**	**Health spa therapies**
1994-1998	-0.28 up to -0.43	-0.15 up to -0.27	-0.31 up to -0.42	-0.45 up to -0.55	-0.98 up to -2.12
1996 excluded	-0.21 up to -0.35	-0.09 up to -0.22	-0.22 up to -0.34	-0.20 up to -0.32	-0.81 up to -1.75
West Germany: 1996 excluded	-0.32 up to -0.44	-0.13 up to -0.27	-0.32 up to -0.45	-0.29 up to -0.42	-1.31 up to -2.83

Arc Price Elasticities are displayed which were calculated according to $\varepsilon_{q,p} = \frac{(q_1 - q_0)/\bar{q}}{(p_1 - p_0)/\bar{p}}$

Under consideration that copayments were increased by (25-12/(25+12)/2=) 70.27 percent in West and (20-8/(20+8)/2=) 85.71 percent in East Germany and by assuming that 18.8 percent of all therapies were undertaken by East Germans between 1994 and 1998 (not needed for row three) (German Statutory Pension Insurance, 2008).

Under consideration that 15.13 percent of all programs were health spa programs from 1994 until 1998 (German Federal Statistical Office, 2008).

Under consideration that 8.14 percent of all programs were outpatient health spa programs from 1994 until 1998 (German Federal Statistical Office, 2008).

6 Discussion

In the previous sections, we have shown by means of various model specifications and robustness checks that the German health reforms of 1996 decreased the demand for health spa and medical rehabilitation programs of around one percentage point throughout Germany. This corresponds to an overall decrease in demand of approximately 20 percent.

At first sight, it may be surprising that the increase in copayments had such a large effect and that we have not found evidence for an impact of the cut in sick pay and the new possibility for an employer to deduct two days of paid vacation for every five days of therapy. We have several explanations for this. First, according to unionist functionaries, around half of the German employees had not been affected by the laws that specified the sick pay decrease due to collective wage agreements between employers and unions. We are not able to identify employees who underlay such agreements. However, even if ony half of the respondents were affected, one would expect to measure an effect. This may not be the case as, second, the tightening of the conditions were no binding restriction for most of the eligibles. It could have been irrelevent, as the majority might have faced a decision between going to rehab or staying home to recover from illness or accident. In any case, the patient would have been on sick leave. If necessary, physicians usually recommend an inpatient therapy but if patients prefer to stay at home on sick leave, their will is mostly respected. Third, the cut in sick pay or in vacation days may have been no binding constraint as many employees take their paid vacation or part of it to go to a health resort or to rehab, anyway. Although being entitled to take paid leave *in addition* to their paid vacation, many employees fear negative job consequences, especially when unemployment rates are high. The only population groups that are either autonomous or in a secure job are self-employed and public sector workers, both of whom are not in Treatmentgroup 3 which is used to estimate the effect. Finally, the only group that was *only* affected by the cut in sick pay were PHI insured private sector employees. We defined them as Treatment Group 4 and contrasted them to the control group by estimating the same econometric specifications as for the other treatment groups (not shown).We were unable to find any significant reform effects which confirms on the one hand our previous results. On the other hand, this may go back to the small sample size as this group is made up of only about 140 observations per year.

Although measurement errors might have slightly biased our results, all our estimates are in line with the previous literature. Though estimated with less precision, we find significant negative reform effects for rehab treatments due to severe illnesses and work accidents. Despite the usage of different model specifications and different subsamples to derive these effects, the calculated elasticities are remarkably similar which strengthens the credibility and identification of the reform effects. In all our specifications, we consistently found that pull-foward effects were of importance. Moreover, these anticipation effects are of the same size throughout all models and fit to the estimated fraction of well-informed SHI insured who take regular advantage of the legal possibilities to go for convalescent care. An additional finding that confirms previous results is that the effect for respondents who stayed less than one week in hospital as compared to those who stayed more than one week is about double the size. Though only indirectly calculated under several assumptions, we are the first to present elasticity estimates for preventive care. Although the offered computation range is broad, it is narrow enough to state that the price elasticity for health spa stays is very likely to be elastic which is again in line with the literature. All in all, our setup produces various price elasticity estimates that all stem from distinct exogenous price variations of a single cost-sharing parameter without having the issue of treatment related selection. The crosschecking

of our results by means of three main different outcome measures, combined with different treatment and control groups, and various robustness checks produces remarkably robust and plausible price elasticity estimates. Hence, we are confident having obtained, for the first time, reliable price elasticity estimates for convalescent care treatments.

7 Conclusion

The question of the price elasticity of demand for health services has kept health economists busy ever since the beginning of the existence of the profession. The RAND health insurance experiment, which was conducted in the 70s, has led to tremendous advancements in that field, particularly as it relied on a sound methodology as compared to former studies.

This study evaluates how several German policy reforms – an increase in copayments, a cut in sick pay, and the possibility for an employer to cut paid vacation – affected the demand for medical rehabilitation therapies and health spa programs. We separetely assess the impact of these reform elements on the demand for different forms of convaslescent care programs. First, we estimate the overall reform induced change in demand for convalescent care therapies. Second, we calculate the demand effects for rehabilitation programs to recover from severe illnesses which threaten the patient's work capacity. Third, we present estimates showing how the demand for rehabilitation therapies due to work accidents reacted, and finally, how the demand for health spa programs changed. To our knowledge, this is the first time that price elasticities for these health services are computed. Due to the two-track German health care system, all estimates are calculated under consideration of a sound control group that did not underlay the policy changes. Thanks to the panel structure of the data, we also take the sample composition into account. Moreover, selection in or out of the treatment is no issue due to the strict legislative regulations in Germany.

We show that the increase in copayments from DM 12 (€ 6.14) to DM 25 (€ 12.78) per day, together with a cut in sick pay and the possibility for employers to shorten paid vacation, has led to an overall decrease in the demand for convaslescent care of around 1.3 percentage points. This corresponds to a decline in total demand of approximately 25 percent. The data suggest that about a fifth of the decrease can be explained by pull-forward effects such that the pure reform effect lies around 20 percent. According to our calculations, the price elasticity for rehabilitation therapies that avert imminent total or partial occupational invalidities amounts to around -0.15. The elasticity for rehabilitation therapies that help to overcome the aftermaths of a work accident lies around -0.25. Contrarily, the price elasticity for health spa treatments that can be categorized as preventive care is elastic and lies between -1 and -2. All estimates are in line with the literature. Moreover, they are remarkably plausible and insensitive to alternate model specifications, the incorporation of different treatment and control groups, and several robustness checks. Hence, we are confident of having obtained, for the first time at all, reliable estimates for various forms of convalescent care programs including the first elasticity estimate for preventive care.

The question to which degree such policy reforms reduce primarily moral hazard or lead to adverse health outcomes is difficult to quantify and is beyond the scope of this paper. However, it should be subject of further research.

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