Searching for sources of inefficiency in the German health care sector: demand-side, supply-side, and labour-force-status effects on health and health care utilisation

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Preface

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Chapter 1

Introduction

1.1 The Problem

In Germany, the average contribution rate to the social health insurance increased from 8.2 per cent in 1970 to 14.9 per cent in 2009 (IW, 2009). In a shorter period of time, the insurance contributions in the private system even tripled between 1985 and 2005 in nominal terms (Grabka, 2006). Likewise, Albrecht et al. (2010) report an average increase in health insurance contributions between 1997 and 2008 by 2.4 per cent per year in the social health insurance and by 3.9 per cent in the private health insurance. While the latter is not yet a public issue, the former induces a steady public concern. Since the contributions to the statutory health insurance are taken as a payroll tax, they impose a wedge between gross and net wage and have direct implications for the labour supply of individuals.

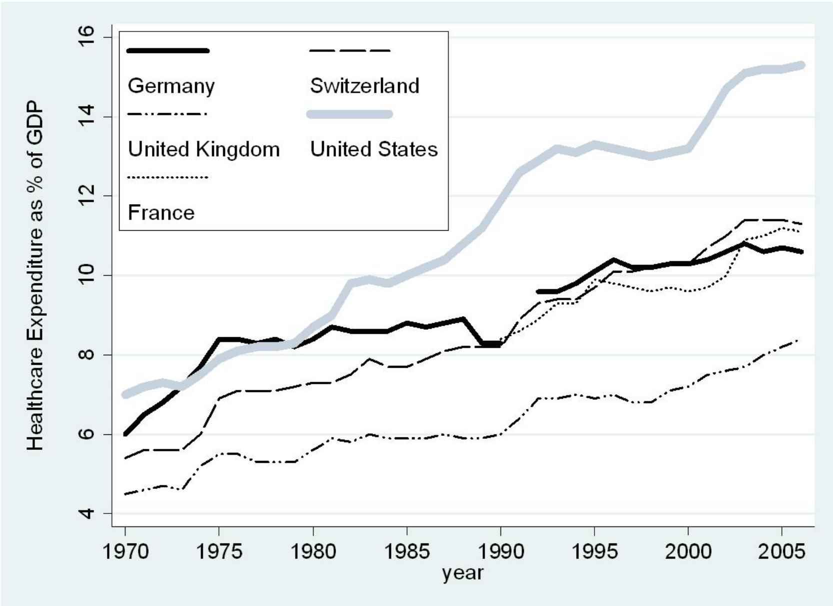
The reasons for this evolution are manifold and can be found on the revenue as well as on the expenditure side of the health insurances. Most important, the demographic change, i.e., the increasing average age of the German population, has consequences for both the revenues of the statutory health insurance and the health care expenditures. A decreasing absolute number of working individuals decreases the income of the health insurance companies *ceteris paribus* since the sum of contributions is directly linked to the payroll sum. At the same time, an ageing society needs more health care services and causes more expenditures.[1] A more important reason of higher expenditures is the technological progress. The development of new drugs and medical equipment helps patients to survive with chronic conditions which would have led to death some years ago. This highly welcome development, however, increases health care expenditures, and probably explains the major part of the overall increase in the last decades (see, e.g., Newhouse, 1992).

Concepts to improve or stabilise the revenues of the German health insurance are beyond the scope of this thesis which focuses on the expenditures side. In Germany, health care expenditures (HCE) as a share of the GDP have risen sharply between 1970 and 2006, from 6 per cent to more than 10 per cent (see Figure 1.1). However, while Germany was the country with the highest health care expenditures in the 1970s, this changed thereafter. Since the middle of the 1990s (until 2006) the ratio has been staying fairly stable while it increased substantially in most other industrialised countries. However, Germany still has the fourth highest health care expenditures of all OECD countries. The reason for the steadiness in Germany seems to be the introduction of sectoral budgets for hospital expenditures, ambulatory expenditures and pharmaceuticals in the early 1990s. These budgets are a means for the policy maker to contain costs; in most of the years they increased only by the rate of the payroll wage increase. However, while at first sight it seems pleasing, this comes at a cost: if the demand for health care constantly increases due to demographic change and technological progress but the expenditures are capped, this can only be resolved by a rationing of health care services. While officially there

[1] There is, however, a debate on this point. See, e.g., Zweifel et al. (1999), or Werblow et al. (2007).

is no rationing in the German health care system, there was a growing public debate about “hidden rationing” in the last years.

Figure 1.1: HCE as % of GDP of selected countries



Source: OECD Health Data 2008, own illustration

Since the demographic change and the technological progress will most likely not be stopped, the only possible solution to this dilemma - apart from rationing or prioritisation - is to detect and reduce inefficiencies in the health care sector. These inefficiencies arise, for instance, from information asymmetries between different agents in the health care market. One such information asymmetry is between insurance company and insuree. Since the insurance company cannot fully supervise the insured, it is difficult to sanction both an unhealthy behaviour and excess demand for trivial health care services. If individuals both exhibit unhealthy behaviour (the so-called ex-ante moral hazard) and/or increase health care utilisation due to their health insurance (ex-post moral hazard), a possible solution can be cost-sharing between health insurance and insuree in order to reduce overconsumption of medical services (Breyer et al., 2004). However, demand-side cost-sharing also has its limits. It imposes financial risk on the patients, thus, it induces a welfare loss in a world with risk-averse individuals and no moral hazard behaviour. Moreover, it might be in conflict with goals of universal access to health care, especially of poorer households (see Ellis and McGuire, 1993).

Another information asymmetry is the one between patient and physician. Since, in general, the physician has more information about the patient's health status than the patient himself, she can - at least to a certain degree - induce demand from the patient. This, of course, implies an inefficient use of resources in the health care system. Here, the remuneration system might be a good instrument to gouvern the behaviour of the physician if she is not fully altruistic but also cares for her own income. The physician remuneration system can - in the terminology of Ellis and McGuire (1993) - be classified into systems with high and low degrees of provider cost-sharing (or supply-side cost-sharing). Pure cost-reimbursement systems would be the extreme case of no supply-side cost-sharing while a pure prospective reimbursement system (with the full cost risk for the provider) would be the other extreme of complete supply-side cost-sharing.

Both levels of cost-sharing can be set independently. Ellis and McGuire (1990) set up a framework to simultaneously determine the optimal degree of demand-side and supply-side cost-sharing. Doing this, a first-best outcome is more often achieved than in models that only use one of the two instruments. Crucial in their model is that patients and providers may have different levels of demand and supply and bargain over the realised amount of medical services. The optimal mix of both is not clear without imposing assumptions, e.g. on the degree of the patient's risk aversion, the degree of moral hazard, the physician's objective function, or the bargaining power of both the patient and the physician. As a result, full cover insurance and partial supply-side cost-sharing can lead to a first best outcome even in the presence of moral hazard. This is the case when the physician is - due to his incentives implied by the reimbursement system - not willing to supply as much medical care as demanded by the fully insured individual. Thus, supply-side cost-sharing can be a means to reduce a too high level of medical care due to moral hazard to a socially optimal level without the need to increase the financial risk of the patient by introducing demand-side cost-sharing. Although this only holds in special circumstances[2] and less than full cover insurance combined with a mixed payment system is often optimal (although only second-best), Ellis and McGuire (1990) conclude that "supply-side policies are the preferred instruments for cost control".

Whether or not there is indeed moral hazard of individuals and whether or not physicians behave altruistically[3] are empirical questions. Answers to both are necessary in order to form an optimal insurance system and an optimal physician remuneration system. The second and the third chapter of this thesis analyse these two questions and shed some light on effects of demand-side cost-sharing and the reimbursement system on patient's and physician's behaviour.

While Chapters 2 and 3 can directly be seen as analyses of incentive effects of health insurance (for the insured and for the physician, since the patient's health insurance type determines the remuneration system), Chapters 4 and 5 might be more generally subsumed under the title "efficiency reserves in the health system". Chapter 4 takes a deeper look into one health care market - the market for private supplementary health insurance for hospital visits - and analyses information asymmetries between insurance company and insuree. This market becomes increasingly relevant since, first, more and more services are taken out of the standard benefit

[2]Specifically, this holds when individuals are risk averse, patients and physicians have the same bargaining power, and the physician does value the patient's health benefits, but not too much (see, Ellis and McGuire, 1990).

[3]Ellis and McGuire (1990) call this more generally the degree of agency of the physician, i.e., how much the physician values the patient's health benefits compared to his profits.

packages of the statutory health insurances and are left for private supplementary insurance and, second, this market has been strongly increasing in the last decades. Traditionally, the theoretical literature expects adverse selection in insurance markets with asymmetric information, see, e.g., Rothschild and Stiglitz (1976). However, there are recent theoretical models (de Meza and Webb, 2001) and empirical findings (Finkelstein and McGarry, 2006) that assume a more complex (i.e. multidimensional) information asymmetry which might lead to a positive, often called advantageous selection. Whether there is information asymmetry at all and if so, in what direction, will be analysed in that chapter.

Chapter 5 shades away from health insurances and looks at the causal effect of unemployment on health. Thus, it looks at efficiency reserves from another point of view. Health care expenditures rise if unemployment deteriorates individual health. However, whether this is indeed the case in Germany is not yet convincingly shown empirically. The answer to this question has important implications for the policy maker concerning her effort to help the unemployed finding their way back into the labour market.

All chapters in this thesis comprise empirical studies using microdata and microeconometric estimation methods. In the empirical work I considered two issues that are central in applied health econometrics. First, unobserved heterogeneity plays an important role. Individuals differ in important aspects which remain unobserved to the researcher, for instance parts of their health status. Models that take unobserved heterogeneity into account are therefore especially important in this field. Second, the dependent variable is often not a metric measure. Therefore, binary, count data or ordered models are often more appropriate than linear models. The specific estimation methods are explained in detail in each chapter.

The remainder of this introduction includes some descriptive statistics about health insurance and health care expenditures in Germany in the next section, an overview about data sets that are available to carry out health economic studies with microeconometric methods (Section 3), and a summary of all following chapters including their results (Section 4).

1.2 Some Descriptives

Germany has two independent health insurance systems, a public and a private one. Roughly 90 per cent of the population are insured by public health insurance (also called the statutory health insurance, SHI). It is statutory for all individuals with earnings below a certain income threshold (3,975 Euro per month in 2007) who are not civil servants or selfemployed and it is financed by payroll taxes. Individuals who earn more than the income threshold, the self-employed, and civil servants are allowed to opt out of the public insurance system and can instead buy private insurance (which accounts for the remaining 10 per cent of the German population, abstracting from a small group without any insurance coverage).

The average contribution rate to the statutory health insurance increased from 12.6 per cent in 1990 to 14.9 per cent in 2009. As can be seen in Figure 1.1, this is not mirrored by the same relative increase in health care expenditures. The reason for this increase mainly lies on the revenue side, namely that the payroll sum which determines the revenues of the statutory health insurance system rose by a lower rate than the GDP in the last decades. Moreover, there is a growing selection of good risks out of the SHI into the private insurance. These individuals pay on average higher insurance contributions before they leave and induce lower costs than those who remain in the public system.

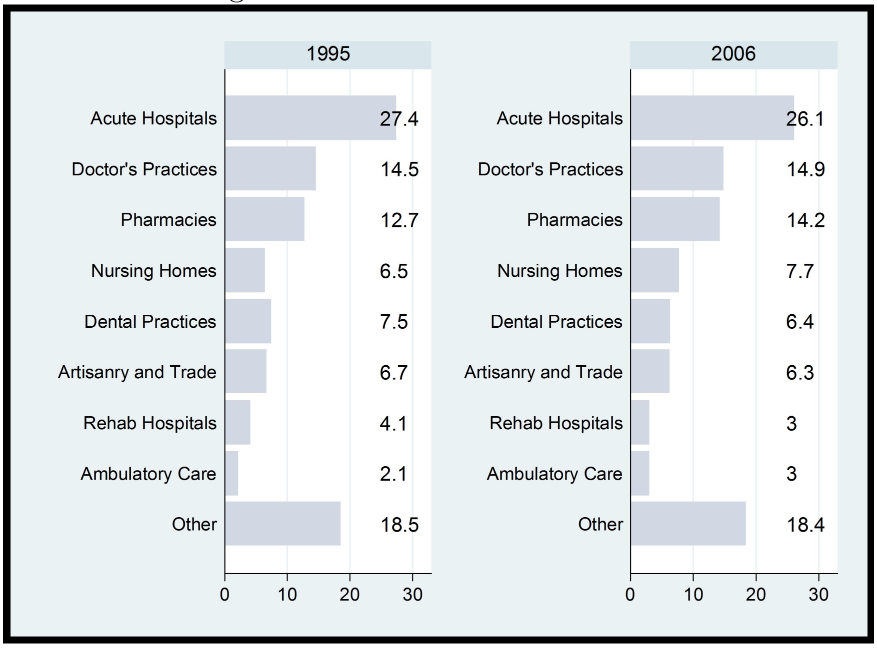
The private insurance sector has been steadily increasing over time. While there were 5.8 million individuals holding a private full cover insurance in 1970, 8.5 million held one in 2007 (IW, 2009). The market for private supplementary insurance exploded over time. While 2.5 million held at least one kind of private supplementary insurance in 1970, this number increased to 20.0 million in 2007 (IW, 2009). The major reason for the growing market for supplementary insurance seems to be the reduction of services in the standard benefit packages in the statutory health insurance system to curb the rise in contribution rates. However, contributions to the private health insurances also strongly increased in the last decades, even stronger than in the public system (Grabka, 2006). Therefore, efficiency concerns and ways to contain health care expenditures will be an important issue also in this system in the future.

In 2006, German health care expenditures amounted to 245 billion Euros. The major part was spent on hospital care (26.1 per cent) followed by the ambulatory care sector (14.9 per cent; without dentists) and pharmacies (14.2 per cent). Figure 1.2 shows the distribution of health care expenditures between 1995 and 2006. In this period, relative expenditures on acute hospitals, dentists, and rehab hospitals decreased by 1.1 to 1.3 percentage points, while relative expenditures on pharamacies, ambulatory and stationary care increased strongest. The expenditures on doctors' practices increased by 0.7 percentage points, implying an absolute increase by 9.4 billion Euros.

1.3 The Data

The data source used in this thesis is the German Socio-Economic Panel (SOEP) throughout all chapters. The SOEP turned out to be the best available data set for all the questions analysed in this thesis and, thus, was preferred to other possible

Figure 1.2: Distribution HCE in %



Source: Gesundheitsberichterstattung des Bundes, own illustration

data sets in every chapter. There are three major advantages in the SOEP to other data sets which will shortly be summarised below. First, it is a representative household data set and a long and still ongoing panel. In 2009, the 25th wave is getting available. Individuals are followed over a long period, many over almost 10 years, some even over 25 years. Since the questionnaire stayed consistent in most aspects over time, analyses over long time periods are possible (which is especially relevant for Chapters 3 and 5).

Second, it has a considerable sample size. While it started with 5,000 households and about 10,000 individuals in 1984, 12,000 households and about 24,000 individuals were sampled in 2008. With such a big amount, analyses of particular subgroups are often possible (important for Chapter 5).

Third, it contains a wide range of socio-economic variables (important for all chapters). Concerning relevant information for health economics, the SOEP includes a lot of information on health status (self-rated health, health satisfaction, limitations due to health problems, and, as of 2002, the body mass index and the SF12v2-questionnare), health care utilization (the number of doctor visits in the previous three months and the number of hospitalisations in the previous 12 months), health behaviour (smoking, drinking, doing sports), and the health insurance status (including supplementary health insurance).

The major drawbacks of the SOEP for the analyses in this thesis are the following ones: the information on the health status does not include chronic conditions, symptoms, or other objective measures. However, this was partly improved by including the SF12v2-questionnaire in 2002. Concerning the number of doctor visits, there is no distinction between visits to general practitioners and specialists. Moreover, one cannot infer the number of sickness spells from the information on the overall number of visits. Finally, and most important, all information in the SOEP is self-stated by the interviewed individuals. Answers to questions regarding the health status, health care utilisation, and income, for instance, are likely to contain considerable measurement error. If this is indeed the case, it might lead to downward biased estimation results. However, the SOEP is widely accepted as a high quality data set and it is the best available data set to analyse the research questions in this thesis.

In the following, I briefly describe other possible microdata sets that in principle allow the evaluation of health econometric research questions for Germany.

SHARE

The Survey of Health, Aging and Retirement in Europe (SHARE) is a large representative micro-data set containing information of about 30.000 individuals above the age of 50 from 13 European countries and Israel starting in 2004. The survey covers a wide range of topics, including physical health, health behaviour, socioeconomic status, and income. The major advantage of the SHARE is the detailed list of symptoms and chronic conditions and a more detailed information on health care utilisation. However, currently, there are only two waves available with about 1.900 individuals in Germany who participated in both waves. Research questions that either draw on a longer time horizon or a distinct subgroup can, therefore, not yet be answered with the SHARE.

Mikrozensus

The German Mikrozensus is an annual micro data survey which includes 1% of the population in Germany, i.e. about 830,000 observations per year. The scientific use file is a 70% subsample with about 530.000 observations. Randomly chosen interviewees are asked four years consecutively. Besides questions that are asked every year, there are supplementary questions that are asked only every four years. Since the health questions are in the supplementary part, the Mikrozensus cannot be used for panel analyses with health economic questions. Moreover, while the main part is compulsory, the health questions can be answered voluntarily which leads to a considerable non-response rate here.

GKV-Versichertenstichprobe

The GKV-Stichprobe is a 3% sample of all statutorily insured in Germany in 2002. In total, it includes 2.3 million individuals with detailed information on statutory and ambulatory treatments in 2001, with diagnosis and induced costs. Moreover, it has information on the consumption of prescription drugs and on work absenteeism. However, it is only a cross-section, it does not include the privately insured and information on the socio-economic background of the individuals is very limited.

EVS

The German Income and Expenditure Survey (Einkommens- und Verbrauchsstichprobe, EVS) samples about 75.000 households every five years (2008 is the most recent wave). Its main advantage is the very detailed information on income and expenditures of German household within a month. However, apart from not being a panel, the health information is very limited. The EVS includes the health insurance status (including contributions to health insurance), expenditures for tobacco, alcohol, drugs, pharmaceuticals, and expeditures for doctor, dentist, hospital visits and other health care utilisation.

Bundes-Gesundheitssurvey

This is a cross-section data set of 7.124 individuals between 18 and 79 years, asked in 1998. The questionnaire includes in total 637 variables, about health behaviour, health care utilisation and very detailed information about the the health status including objective measures like blood pressure, blood tests, and urine tests.

1.4 Overview and Summary of Findings

The second Chapter (**"More health care utilisation with more insurance coverage? - Evidence from a latent class model with German data"**) deals with effects of different types of health insurance on the behaviour of the demanders of health care. I analyse whether individuals with more health insurance coverage demand more health care services (measured as the number of visits to the doctor) than those with less coverage. Specifically, I examine whether privately insured with a deductible demand less doctor visits compared to those without a deductible, conditional on their health status and other socioeconomic variables. In the group of publicly (or statutorily) insured I analyse whether supplementary insurance increases the number of doctor visits. The contribution to the existing German literature is twofold. First, I use newly available data on the health status and self-stated individual risk aversion. Including more information is likely to reduce possible endogeneity problems of the insurance choice by a great deal (as long as they result from omitted variable bias). While the existing literature mostly neglects the endogeneity problem at all, I test for it. Second, I control for unobserved heterogeneity in a flexible way by estimating a latent class model. The model allows for different effects of health insurance and income on health care utilisation for different unobserved types of individuals.

I find that endogeneity is not a problem in the data set. The reasons might either be the improved quality of the data or/and the inflexible German insurance system that allows only a small fraction of individuals to choose between the two systems. Also, the effect of health insurance on utilisation is very small. I do not find any significant effects in the group of high users at all, neither in the private nor in the public insurance. It seems to be the case that these individuals need health care services anyway (for unobserved reasons) and do not care for their current health insurance status. However, I do find incentive effects in the group of low users. Those individuals who generally demand less doctor visits react on their insurance status and demand even less if they have less insurance coverage. Likewise, income only plays a role in the group of low users with richer individuals having more visits. The small magnitude of effects is only at first sight in contrast to the strong effects of demand-side cost-sharing found in the famous Rand Health Insurance Experiment (RHIE, see Manning et al., 1987). Note, that while the deductibles in the RHIE were mandatory, they are optional in the German system. Although individuals with less insurance coverage demand less health care in Germany, this is mostly due to a selection of good risks into contracts with less insurance coverage and not causally due the insurance status. Thus, I cannot conclude that demand-side cost-sharing as such does not have any effects but that optional deductibles rather lead to selection than to incentive effects in Germany.

The third Chapter (**"Practice budgets and the patient mix of physicians -**

Evaluating effects of remuneration system reforms on physician behaviour in Germany") takes a look at the supply side and analyses incentive effects of health insurance on physicians' behaviour through the remuneration system. In Germany, differently insured individuals imply different remunerations for physicians. For instance, doctors get more money for the same treatment of a privately insured compared to a publicly insured. This might impose an incentive to induce demand from privately insured patients. Due to the information asymmetry between doctor and patient this is possible to a certain degree. This chapter evaluates the effects of two major reforms of the remuneration system for publicly insured, the reforms of 1993 and 1997. In 1993, the payment system changed from a fee-for-service system, where the price of a treatment was known ex-ante, to a point system. From that time on, the overall budget for physician remunerations was fixed and doctors collected points for all treatments in a quarter. Ex-post, the point value and, thus, the income from treating publicly insured was calculated by dividing the money budget by the sum of all points collected by all physicians. The reform of 1997 introduced an individual practice budget for each physician, in addition. That is, the number of points that could be remunerated was capped. There was no comparable reform in the private health insurance.

Both reforms have not been evaluated before. I find that there are no effects of the reform of 1993 on the number of doctor visits but considerable effects after the introduction of individual budgets in 1997. Because I use a hurdle model, I can split up the number of doctor visits into two parts: the decision to see a doctor within a quarter (binary information, first part) and the number of recalls (integer value truncated at zero, second part). I can show that the total drop in the number of doctor visits of the publicly insured is entirely a result from the drop in the second step. Given the economic principal-agent model underlying the econometric hurdle model, this result can be interpreted as a mere supply-side effect. Moreover, there was not only a drop in the conditional number of visits of publicly insured but also a strong increase of conditional visits of the privately insured. This gives rise to the interpretation that physicians responded to the second reform by changing their patient mix, i.e., by substituting out the publicly insured for the privately insured. I argue that the estimate of the treatment-effect of a 10% drop in the number of doctor visits due to increased copayments for prescription drugs that is well established in the literature (Winkelmann, 2004a, 2004b, 2006) is biased and that it is more likely that the effect is due to the introduction of individual practice budgets.

The fourth Chapter (**"Risk aversion and advantageous selection in the German supplementary health insurance"**) takes a deeper look into the question of who buys private supplementary health insurance for hospital stays in Germany. It is well known that individuals who sign health insurance contracts have more information on their true risk type than insurance companies. This private information might lead to adverse selection. That is, since health insurance companies have to offer contracts with insurance premia that equal the average expected losses of all risk types, the good risks might leave the market if the insurance premium exceeds their expected loss. If there is an equilibrium at all, Rothschild and Stiglitz (1976) predict in their model that good risks buy less insurance than bad risks in this market with adverse selection. However, many recent studies find the opposite, namely that those individuals who hold more insurance are better risks and have a lower probability to actually need the insurance. This holds, for example, for life insurance, long-term care, and Medigap-markets (see Cutler et al., 2008 for an overview) and is often called advantageous selection. One explanation for this finding is that individuals do not only differ in their risk type as assumed by Rothschild and Stiglitz (1976) but also in their risk preferences. More risk averse individuals are more likely to hold insurance but also to invest in own health making less health care utilisation necessary.

In this chapter I analyse how holding supplementary insurance and the number of overnight hospital stays within six years after the interview are correlated with risk aversion concerning one's own health. Thereby I am the first one to use a directly stated degree of risk aversion and do not have to use proxy variables like smoking or drinking alcohol. I find that there is (overall) adverse selection in the German market for private supplementary health insurance for females only. For males, risk aversion increases the likelihood to buy private supplementary health insurance and decreases the expected number of hospital visits in the future. Thus, risk aversion is a source of advantageous selection here, possibly outweighing other sources of adverse selection.

The fifth Chapter (**"Why are the unemployed so ill - The causal effect of unemployment on health"**) analyses the impact of unemployment on health. Although there exists some German literature about this question, it has not yet been answered convincingly. It is well known that the stock of unemployed individuals exhibits a worse health status than the stock of working individuals. Furthermore, Arrow (1996) and Riphahn (1999) show that there is a selection of ill individuals into unemployment. The causal effect of unemployment on health, however, is not clear. The approach in this chapter both takes into account time-invariant unobserved individual effects that might be correlated with the health status and the likelihood to become unemployed, and reversed causality that goes from health to unemployment. The former is captured by estimating fixed-effects regressions, while the latter is done by only looking at individuals who lost their jobs due to plant closures and did not quit or get fired for other reasons. I use three different health measures: the health satisfaction as an overall health measure, a binary variable indicating a hospital visit within the four years after losing the job as a more objective variable, and a mental health score. The results confirm that, on average, unemployed individuals have a worse health status according to all measures. After controlling for fixed-effects and possible reversed causality the negative effect completely vanishes, however. I can show that without taking into account possible reversed causality (by using all unemployed) one would find a negative effect of unemployment on health. Because one cannot exclude reversed causality, this result is likely to be biased and shows the importance of relying on exogenous reasons of becoming unemployed.

Chapter 2

More health care utilisation with more insurance coverage? - Evidence from a latent class model with German data

2.1 Introduction

Rising health care expenditures have been an issue for several decades in most industrialised countries. While demographic change and technological progress can be seen as the main driving forces behind the increase, both factors can hardly be tackled in order to contain costs. Moreover, they might lead to even higher expenditures in the future. In order to lower costs, it seems more promising to detect and to reduce inefficiencies in the health care sector.

One inefficiency from the demand side is the problem of moral hazard induced by insurance, defined as "the change in health behavior and health care consumption caused by insurance" (Zweifel and Manning, 2000). If this leads to excess demand for trivial health services, efficiency could be gained by cost-sharing, e.g., deductibles or co-payments for doctor or hospital visits. However, the optimal amount of cost-sharing is a priori unclear. In a world with risk-averse individuals and without moral hazard, full cover insurance (i.e., no deductibles) is a first-best solution. Yet, when moral hazard is present, the first-best solution is no longer feasible and the introduction of mandatory deductibles can lead to a second-best solution (see Breyer et al., 2004).[1]

On the other hand, in particular European countries also follow the goal of universal coverage for health care services, available to all in need, independent of income (Wagstaff and Van Doorslaer, 2000). Cost-sharing might endanger access to health care services of poorer households and, thus, threaten equity goals in health care utilisation. Moreover, allowing for optional deductibles could lead to a decomposition of the risk pool as it can be assumed that good risks choose to buy less insurance to safe on insurance contributions whereas bad risks are left with higher premiums. Therefore, if optional deductibles do not increase efficiency due to a more cost conscious behaviour, this option might induce a welfare loss since, when again the risk aversion of individuals is considered, it lowers the expected utility behind the veil of ignorance.

Hence, finding out more about the price elasticity of demand for health services is an important empirical task in order to design an optimal health insurance system. This study analyses the price elasticity of demand for health services in Germany. In particular we examine whether (a) optional deductibles in private health insurance or (b) absence of private supplementary insurance cause a lower utilisation of health care services and, thus, are able to increase efficiency in the health care sector, or

[1] We therefore also have to assume absence of adverse selection. See below for the problem of separating moral hazard from adverse selection.

if they only lead to a decomposition of the risk pool. Second, we evaluate whether utilisation varies with income conditional on individual need.

The ideal way to determine causal effects of health insurance on the demand for medical services is a randomised experiment. Manning et al. (1987) report the results of the Rand Health Insurance Experiment, the only social experiment that exists in the health insurance literature. Between November 1974 and February 1977, 5809 individuals in six US-American cities were randomly assigned to different health insurance plans. The plans varied in the rate of coinsurance (from 0% to 95% of out-of-pocket expenditures) and upper limits of annual out-of-pocket payments (between 5 and 15% of annual family income, up to a maximum of 1000$). Furthermore, in some cases the co-payment differed for inpatient and outpatient services. The results concerning demand for medical services (number of treatments and expenditures caused by the treatments) show a clear incentive effect. Individuals without any coinsurance caused the highest expenditures due to the highest demand for health care. The demand decreased with higher amounts of co-payments. However, the differences between the insurance plans with co-payment are much less pronounced than the difference between users with coinsurance and those without. That is, the difference in doctor visits between individuals without coinsurance and those with a coinsurance rate of 25% is much higher than between individuals with coinsurance rates of 50% vs. 95%. These results generally hold for various subgroups (divided by income, age, health status).

The German experience with mandatory cost-sharing is mixed. Exploiting a natural experiment, Augurzky et al. (2006) and Schreyögg and Grabka (2010) evaluate a reform of 2004 that introduced a co-payment of 10 Euro for the first doctor visit in a quarter. The authors do not find reactions on the incentives to save costs induced by this reform. Farbmacher (2009), who also analyses the reform, finds a slight drop in the demand for doctor visits due to the reform. The most likely reason for these findings is that an amount of 10 Euro that only applies to the first visit in a quarter is too low to lead to significant effects. On the contrary, Winkelmann (2004a, 2004b, 2006) finds strong effects of increased co-payments for prescription drugs in 1997 on the demand for doctor visits. A moderate increase in co-payments led to a 10% drop in the number of doctor visits. However, see Chapter 3 of this thesis for an alternative explanation for the strong effects of this last reform.

The major difference of this study to the aforementioned ones is that it does not analyse effects of mandatory deductibles or co-payments but of optional ones. This has two important implications. First, the empirical challenge to find causal effects is much stronger because the selection of individuals into insurance plans has to be considered. Second, statements about the effects of deductibles can only be made for the group of individuals who actually hold contracts with deductibles. As regards deductibles in Germany this is important to note, since only a special group is allowed to choose deductibles. This group might not be representative for the whole German population (see Section 2.2 for a description of this group).

There are several studies that analyse the impact of the insurance status on the number of doctor visits or hospitalisations with German observational data (i.e., without exploiting either randomised or natural experiments), e.g., Pohlmeier and Ulrich (1995), Geil et al. (1997), and Riphahn et al. (2003). They compare the behaviour of individuals covered by private health insurance with those covered by public insurance. In the German system, the former can choose between having a deductible or not whereas there are basically no deductibles for the latter.[2] The results of these studies are mixed. Pohlmeier and Ulrich (1995) find that the probability of visiting a general practitioner (GP) is higher for the publicly insured, that

[2]A brief introduction to the German health insurance system will be given in Section 2.2.

is, implicitly, for those with more insurance coverage. Geil et al. (1997) show that, for females, being covered by public insurance has a positive (but moderate) effect on the number of hospitalisations, while no significant effect was found for males. Finally, Riphahn et al. (2003) find that private add-on insurance raises the number of hospitalisations of males while other variables that indicate the insurance status are not significant.

Felder and Werblow (2008) evaluate an experiment of one German sickness fund that allowed a subset of voluntarily insured to choose a contract with a deductible in 2003. They find that the deductible reduces the probability of visiting a specialist but not a GP. Furthermore the deductible reduced health care expenditures for acute care but not for preventive care. This result does not surprise, since the deductible in the experiment is much higher than, e.g., the co-payment of 10 Euros for only the first doctor visit in a quarter. However, the author's control for the notable selection of healthy individuals into the deductible program rests on strong assumptions (basically, the validity of exclusion restrictions).

The contribution of this study is threefold. First, we take unobserved heterogeneity into account in a fairly general way. By exploiting the panel structure of the data, Riphahn et al. (2003) and Geil et al. (1997) already account for unobserved individual effects that affect doctor visits, such as individual frailty, using a random effects model. Here, however, we estimate a finite mixture model that allows for different effects of insurance status and income for different latent classes. Latent class models where introduced to the health economics literature by Deb and Trivedi (1997) and have become very popular since then.[3] In the present study, we use the latent class hurdle model developed by Bago d'Uva (2006), which Bago d'Uva and Jones

[3]Recently they where used in different specifications by, e.g., Deb and Holmes (2000), Deb (2001), Gerdtham and Trivedi (2001), Deb and Trivedi (2002), Jimenez-Martin et al. (2002), Atella et al. (2004), Bago d'Uva (2005), Bago d'Uva (2006), Bago d'Uva and Jones (2009).

(2009) also apply, but which, so far, has not been estimated on German data.

Second, we control for the fact that privately and publicly insured individuals differ in many aspects, especially in their offered insurance contracts and their latent health status. Therefore, we run separate regressions for both groups.

Third, we use recent innovations in the German Socio-Economic Panel and include better health measures and a directly stated measure of risk aversion in the analysis. Doing this, we are much more likely to reduce possible omitted variable bias that renders the insurance choice an endogenous explanatory variable than those previous studies which do not exploit natural experiments.[4] Endogeneity of the insurance status might occur for two reasons: unobserved health status and unobserved risk aversion. When making a decision about the insurance plan, the individual takes into account her expected demand for health services in the future. A person who knows that she needs many visits to the doctor in the future due to health problems might not buy insurance with deductibles. Thus, the health status both affects the demand for doctor visits and the insurance type. An incompletely observed health status might lead to biased results. Similar reasoning applies to preferences like risk aversion. Risk-averse individuals tend to prefer full cover insurance and at the same time demand more doctor visits given a certain health status. However, note that, unconditionally, the effect might even be reversed with more demand for health insurance but less demand for health services, given that risk averse individuals are likely to be in better health due to higher preventive efforts, see de Meza and Webb (2001), Finkelstein and McGarry (2006), or Cutler et al. (2008).

We use the waves 2002, 2004, and 2006 of the German Socio-economic panel (SOEP) that include newly available health measures from a version of the SF12v2-questionnaire

[4]While Pohlmeier and Ulrich (1995) and Geil et al. (1997) do not discuss possible endogeneity of the insurance choice, Riphahn et al. (2003) do test for it and find that there is endogeneity of holding private supplementary insurance in the case of hospital visits.

and a directly stated degree of risk aversion to overcome this problem. It turns out that deductibles and private supplementary insurance affect the doctor visiting behaviour at least to a certain degree. Specifically, individuals who generally have a lower demand for doctor visits react on the imposed incentives given by the insurance status and demand less doctor visits with a deductible and more with private supplementary insurance. No or even reversed effects can be found in the group of high users, which however, is the smaller one compared to the group of low users. As regards income elasticity we find positive effects in the low user group and virtually no effects of high users. Thus, the results are in line with most other latent class analyses that find that especially the low users respond to insurance status and are also more price elastic than high users (see, e.g., Deb and Trivedi, 2002; Bago d'Uva, 2005; Bago d'Uva, 2006).

The paper is structured as follows. Section 2 gives a short introduction into the German health insurance system. Section 3 describes the data and the empirical strategy, while Section 4 presents the estimation results. Section 5 discusses some endogeneity concerns and Section 6 concludes.

2.2 Institutional Background

The German health insurance system consists of two parts. Roughly 88 per cent of the population are insured by public health insurance (also called the statutory health insurance, SHI). It is statutory for all individuals with earnings below a certain income threshold (3,975 Euro per month in 2007) and financed by payroll taxes. Therefore, it is basically independent of age, gender, or health status of the insured.[5] Since 1997, the publicly insured are allowed to choose between different

[5] It does, however, indirectly also depend on these factors since they affect the individual earnings.

insurance companies yet the benefit package is heavily regulated and does not vary much between companies.[6] The insurance companies are not allowed to reject applicants and non-working family members are covered without an extra premium. Until the most recent reform which took effect in April 2007, there were basically no (optional) deductibles in the public scheme. However, in 2004, a co-payment of 10 Euro for the first visit in a quarter was introduced.[7] Furthermore, there were low co-payments for hospitalisations (10 Euro per calendar day up to a maximum of 28 days) and for prescription drugs (Winkelmann, 2004b). The publicly insured can additionally purchase private supplementary insurance that either increases quality (e.g. double rooms in hospitals) or covers co-payments on dentures, corrective devices (like glasses) or other remedies.

Individuals who earn more than the income threshold, the self-employed, and civil servants are allowed to opt out of the public insurance system and can instead buy private insurance (which accounts for the remaining 12 per cent of the German population, abstracting from a small group without any insurance coverage). The private insurance premium does not depend on income but is a risk-equivalent contribution (depending on age, gender, and health status). Private insurance companies can reject bad risks. Furthermore, privately insured individuals have to pay higher premia in order to cover non-working family members. Thus, being a bad risk or having dependents might be two of the reasons for staying voluntarily in the public system for about 50 per cent of all the individuals who are allowed to opt out although private insurance often is perceived to lead to higher quality of health care. Private insurance companies usually offer a set of different contracts, including the choice of deductibles. Individuals who opt out of the public insurance system are in general

[6] About 95 per cent of the package that all insurers have to provide is stated in Social Code Book V (see Buchner and Wasem (2003)).

[7] No effects of this reform on the demand for doctor visits are found by Augurzky et al. (2006). Since they use the same data set as we do, we do not take this reform into account. Also, since our latest panel year is 2006, the reform of 2007 is of no importance for this study.

not allowed to re-enter later as long as they do not fall below the income threshold. Furthermore, contracts with a deductible cannot be transformed into full cover contracts without proof of good health. Hence, the decision to buy private insurance and about the deductible are practically lifetime decisions.

Because the insurance contracts differ so strongly between both the privately and the publicly insured, we analyse both groups separately. Besides income, the main focus will be on the effect of *more* insurance in the SHI system (private supplementary health insurance, also called add-on insurance) and *less* insurance in the private system (contracts with deductible) compared to individuals with standard contracts (no add-on, or no deductibles) in the respective system.

2.3 Empirical Model

2.3.1 Data and Variable Description

The database for the empirical analysis is the German Socioeconomic Panel (SOEP), which started in 1984 in West Germany and was extended to include East Germany in June 1990. There were several refreshments resulting in a sample size of more than 20,000 adult individuals living in more than 12,000 households that participated in the SOEP survey in 2006 (see, e.g., Wagner et al., 2007).[8] The SOEP includes questions about the number of doctor visits within the last three months prior to the interview and the number of hospital trips in the previous year.

The number of doctor visits depends, to a large extent, on the individual health sta-

[8]The data used in this paper were extracted using the Add-On package PanelWhiz v2.0 (Nov 2007) for Stata. PanelWhiz was written by Dr. John P. Haisken-DeNew (john@panelwhiz.eu). The PanelWhiz generated DO file to retrieve the SOEP data used here and any Panelwhiz Plugins are available upon request. Any data or computational errors in this paper are my own. Haisken-DeNew and Hahn (2006) describe PanelWhiz in detail.

tus. However, direct measurement of the health status is somewhat complicated and, especially in general surveys such as the SOEP, often restricted to the self-assessed health status (SAH, on a 5-point scale from very good to bad). As often argued, this measure is prone to measurement error and reporting bias, possibly leading to biased coefficient estimates (see, e.g., Bound, 1991 or Crossley and Kennedy, 2002). Furthermore, it is unlikely that a complete picture of individual health can be captured by the SAH. Two newly available and more objective measures are used here to alleviate this problem: the Physical Component Summary Scale (PCS), a measure of physical health, and the Mental Component Summary Scale (MCS) a measure of mental health (see Andersen et al. (2007) for a description). These measures are based on a variant of the SF12v2-questionnaire in the SOEP that includes several questions about health quality and satisfaction of the individuals.[9] Both variables are calculated using explorative factor analysis and lie between 0 and 100, with a higher value indicating a better health status. The mean value of the SOEP 2004 population is 50 points with a standard deviation of 10 points. Although both measures are also based on self-reported information, they give a much more detailed picture of the true health status.

As an objective measure of individual health we use the body mass index (BMI) and whether there was a hospital stay in the previous year. The three variables PCS, MCS, and BMI are only available in the waves 2002, 2004, and 2006, hence, these three waves of the SOEP are used for the analysis. It turns out that after controlling for these health measures, the self assessed health status still contains information to explain the number of doctor visits and should therefore not be left out.

One further variable that is newly available in the SOEP is a self-assessed attitude towards risk concerning health matters on an 11-point scale from 0 (very risk-averse)

[9]Questions concern, among others, bodily pain, stress, ability to carry out everyday tasks, phases of melancholy, etc.

to 10 (not at all risk-averse).[10] The risk-attitude is likely to have an important impact on the demand for health care. It was only asked in 2004. However, it seems reasonable to assume that this preference is constant over the span of five years. Although the attitude towards risks are self-assessed, Dohmen et al. (2010b) show in an experimental setting with a pre-test group of the SOEP that it is a fairly reliable measure. Another variable that reflects preferences of the individual is the assessment about worries concerning the own health (on a 3-point scale between 1= very concerned and 3= not at all concerned). On the one hand, it contains additional information on the true health status. On the other hand, it also reflects parts of the individual doctor visiting behaviour since there might be individuals who are more concerned about their health status and demand more doctor visits than others at a comparable true health status. Using these new variables (together with a set of other socio economic variables which are not the focus of this study), we can control for a lot of important heterogeneity that has an effect on the number of doctor visits and the insurance status at the same time and which remained unobserved in previous studies.

In our sample, only the privately insured are allowed to buy contracts with deductibles. In our dataset the information on deductibles of the privately insured is restricted to a binary variable indicating the existence or absence of a deductible. Although the publicly insured can indicate what kind of private add-on insurance they hold, we collapse these into a binary variable indicating whether or not an individual holds some kind of add-on insurance due to the low coverage of the publicly insured with add-on insurance (only about 14 per cent of all publicly insured hold some kind of private add-on insurance). However, add-on insurance that covers hospital stays or medical costs abroad are not included here. More precisely, the binary

[10] The question in the SOEP is: "How would you rate your willingness to take risks with your health?"

variable *Add-on* states whether an individual holds supplementary insurance that covers dentures, corrective devices, some kinds of therapeutic measures, or others. As an income measure we use the logarithm of the equivalised net household income.

We exclude civil servants from the sample due to their special insurance status. In general, the civil servant's employer covers 50 per cent (or more) of the health care costs while civil servants have to insure only the remaining 50 per cent, usually privately. Treating a civil servant with private insurance and deductible similar to other privately insured would possibly bias the results. All together, we use information from 20,751 individuals with 51,894 observations in person-year form after exclusion of observations with missing values in any of the variables used for the regression analysis.

Table 2.1: Doctor Visits of Subgroups

	Average # of visits	sd	Probability of one visit	sd	Average # of visits if > 0	sd	Number of obs.
Whole Sample	2.44	3.97	0.68	0.47	3.57	4.36	51894
Public Insurance	2.47	3.95	0.69	0.46	3.58	4.31	46319
– with add-on	2.56	4.16	0.71	0.45	3.58	4.53	3644
– without add-on	2.47	3.93	0.69	0.46	3.58	4.29	42675
Private Insurance	2.20	4.16	0.62	0.49	3.54	4.80	5551
– with deductible	1.96	3.86	0.58	0.49	3.36	4.57	3081
– without deductible	2.50	4.48	0.67	0.47	3.74	5.04	2470

Source: SOEP, pooled years 2002, 2004, 2006; no civil servants.

The number of doctor visits in the previous three months of several groups with different insurance statuses in the pooled sample is given in Table 2.1. The overall mean is 2.44, with 32 per cent of all the individuals not visiting any doctor. Conditional on having at least one visit, the average number of doctor visits in the whole sample is 3.57. The group of publicly insured exhibits a higher number of doctor visits than the group of privately insured (2.47 vs. 2.20). However, both groups are hardly comparable because, first of all, the group of privately insured consists of individuals with higher income and better education - characteristics that are known to be correlated with better health (see Table A3.2 in the appendix for means of the covariates for different subgroups). Furthermore, this group has the better risk-pool because bad risks are either rejected by private health insurance companies or would have to pay high contributions that preclude them from buying private health insurance. Therefore, and because, the insurance contracts of both groups differ substantially, we carry out separate regressions for both groups.

The comparison of the privately insured with and without deductible shows a clear picture: the privately insured with a deductible have much less doctor visits (1.96 vs. 2.50), a lower probability of visiting a doctor (58 per cent vs. 67 per cent), and even fewer doctor visits conditional on having at least one visit (3.36 vs. 3.74). Likewise, individuals with add-on insurance exhibit slightly higher numbers of doctor visits. However, these are only unconditional numbers which do not account for different health statuses across groups or different attitudes towards visiting a doctor. To find out if these differences in health care utilisation are only due to different risk pools or also due to different incentives given by the insurance statuses a more detailed analysis that controls for important observable and unobservable factors is necessary.

2.3.2 Latent Class Hurdle Model

It is often argued that the observed number of doctor visits is a result of two different (and probably independent) decision-making processes. First, the patient decides whether or not to see a doctor in case of an illness. Once a doctor is seen, however, the doctor determines the length of the treatment. Hence, a hurdle model seems to be the most appropriate formulation in order to explain the number of doctor visits (Mullahy, 1986; Pohlmeier and Ulrich, 1995). Let

$$g(y_{it}) = \begin{cases} f_1(0|x_{it}) & \text{if } y_{it} = 0 \\ (1 - f_1(0|x_{it}))f_2(y_{it}|x_{it}, y_{it} > 0) & \text{if } y_{it} > 0 \end{cases} \tag{1}$$

where $f_2(y_{it}|x_{it}, y_{it} > 0) = f_2(y_{it}|x_{it})[1-f_2(0|x_{it})]^{-1}$, y_{it} is the number of doctor visits of individual i at time t, and x_{it} is a vector of covariates. $f_1(0|x_{it}) = P(y_{it} = 0|x_{it})$ is a binary function that determines the probability of having no doctor visit at all in a given period. Given that the number of doctor visits exceeds zero (with probability $1 - f_1(0|x_{it})$), a truncated-at-zero function $f_2(y_{it}|x_{it}, y_{it} > 0)$ determines the exact number of visits.

Given that the dependent variable (number of doctor visits in the previous three months) is an integer, it is appropriate to use a count data model in order to specify the two underlying functions in the hurdle model. While the Poisson model is a good starting point for count data, it is often seen to be too restrictive due to its assumption of the equality of conditional mean and variance of the dependent variable, which is clearly not the case here (see Table 2.1). In order to allow for over-dispersion, one commonly introduces a gamma-distributed error term, ending up with the negative binomial distribution (see, e.g., Cameron and Trivedi, 2005, for a derivation) with the following probability density function:

$$f(y_{it}|\mu, \alpha) = \frac{\Gamma(\alpha^{-1} + y_{it})}{\Gamma(\alpha^{-1})(\Gamma(y_{it} + 1))} \left(\frac{\alpha^{-1}}{\alpha^{-1} + \mu}\right)^{\alpha^{-1}} \left(\frac{\mu}{\mu + \alpha^{-1}}\right)^{y_{it}} \tag{2}$$

where μ =exp$(x'_{it}\beta)$ and α is the over-dispersion parameter.

Combining the negative binomial distribution with the hurdle structure in (1), f_1

becomes

$$f_1(0|x_{it}) = P(y_{it} = 0|x_{it}, \beta_1) = (\mu_1 + 1)^{-1} \quad (3)$$

where μ_1 =exp($x'_{it}\beta_1$).[11] The truncated part in (1) becomes

$$f_2(y_{it}|x_{it}, \beta_2; y_{it} > 0) = \frac{\Gamma(\alpha^{-1} + y_{it})}{\Gamma(\alpha^{-1})(\Gamma(y_{it} + 1))((1 + \alpha\mu_2)^{\alpha^{-1}} - 1)} \left(\frac{\mu_2}{\mu_2 + \alpha^{-1}}\right)^{y_{it}} \quad (4)$$

where μ_2 =exp($x'_{it}\beta_2$).

While the x_{it}'s capture a lot of observable heterogeneity between individuals and across time (especially the health status, insurance status, age, sex, and education), there might still be great a deal of unobservable heterogeneity left. This could be general unmeasured frailty or preferences towards visiting a doctor. These factors clearly affect the demand for health care and can be considered time-invariant. In order to account for this unobserved heterogeneity, we use the latent class hurdle model derived by Bago d'Uva (2006) for panel count data in which the time-invariant individual effect follows a discrete distribution that takes on a small number of components. The latent class hurdle model combines the basic hurdle model (that groups individuals into "users" and "non-users") with a finite mixture model (where the latent classes can be given interpretations such as "high users" and "low users", see Deb and Trivedi, 2002).

As in Bago d'Uva (2006) or Clark and Etile (2006), the individual probability of belonging to one of C latent classes is specified as a multinomial logit.

$$\pi_{ij} = \frac{\exp(z'_i\gamma_j)}{\sum_{g=1}^{C}\exp(z'_i\gamma_g)}, j = 1, \ldots, C \quad (5)$$

[11]Here, α is set to one because it would not be identified from β_1 in this binary information case (see Deb and Trivedi (2002) or Bago d'Uva (2006))

This ensures that $0 < \pi_{ij} < 1$ and $\sum_{j=1}^{C} \pi_{ij} = 1$. In order to guarantee that each individual belongs to the same latent class over all time periods, we choose the z_i as time-invariant characteristics. We follow Bago d'Uva (2005) in using the individual averages of the x_{it}, defining $z_i = \bar{x}_i$. Note that this specification allows for correlation between the observable characteristics and the unobserved individual heterogeneity (that is, here, the latent class).

The likelihood function is finally given by

$$L = \prod_{i=1}^{N} \sum_{j=1}^{C} \pi_{ij} \prod_{t=1}^{T_i} g_j(y_{it}|x_{it}, \theta_j) \tag{6}$$

where $\theta_j = (\beta_{1j}, \beta_{2j}, \alpha_j)$ and equation (1), (3), (4) and (5) are plugged into equation (6). The most flexible formulation allows for different slope parameters in every latent class ($\beta_{1j} \neq \beta_{1k}$ and $\beta_{2j} \neq \beta_{2k}$ for $j \neq k$) and different parameters in the two hurdle parts ($\beta_{1j} \neq \beta_{2j}$). That is, belonging to a certain latent class does not only alter the intercept but is allowed to affect each slope parameter. This, however, requires estimation of very many parameters. For instance, a fully flexible hurdle model with three latent classes and, say, 30 regressors and a constant would include 251 coefficients[12] that have to be estimated. This flexible specification is very data-demanding. Since we only have three waves and, furthermore, carry out different regressions for publicly and privately insured, we restrict the model to the same slope parameters across latent classes and allow only for heterogeneity in the most interesting parts. These are: the intercept, the over-dispersion parameters and the two interesting insurance variables (deductible or add-on) plus the equivalised household income. This still requires the estimation of many parameters but much less than in the fully flexible specification. Apart from allowing for heterogeneous

[12]These are three classes times two sets of parameters for the first and second hurdle part times 31 parameters plus three different over-dispersion parameters plus two times 31 parameters for the probability of belonging to a certain latent class.

effects of insurance and income on the health care utilisation for different latent groups, there is also a statistical motivation for the use of a finite mixture model. This is the possibility to introduce a random-effect without imposing strong distributional assumptions. The likelihood function is maximized with respect to the vectors $\theta_1, \ldots, \theta_C, \gamma_1, \ldots, \gamma_{C-1}$ using the Broyden-Fletcher-Goldfarb-Shanno quasi-Newton algorithm.[13] Note, finally, that in this specification, the two parts of the hurdle model are not assumed to be independent. This restrictive assumption is relaxed due to the latent class specification.

2.4 Estimation Results

We carry out separate regressions for both groups of insured (SHI and private insurance) where the insurance variable in the case of SHI is the binary indicator of holding add-on insurance and in the case of private insurance it is the dummy for a deductible. The model selection, that is, the choice of the number of components is done by the Akaike criterion (AIC). Table 2.2 shows different values of AIC for different choices of components. In both cases the latent class panel model outperforms the standard hurdle model with a logit as the first part and a truncated negative binomial model as a second part (which is equivalent to the latent class model with one component). Hence, the fit is improved substantially by capturing unobserved individual effects. According to the AIC, the model with three components is preferred for the privately insured over the one with two components. In the SHI sample, the model with three components failed to converge. However, it is widely accepted that two components already capture a substantial part of individual unobserved heterogeneity. A model with four components did not converge

[13] We used the *ml* command in Stata and drawed on the code provided in Jones et al. (2007). Various starting values were used to rule out local maxima of the likelihood function.

in either case.

Table 2.2: Model selection

Components	Private AIC	SHI AIC
1	19,877	175,897
2	19,636	172,786
3	19,532	-

Source: own calculations

Based on the estimated parameters and the observed covariates we calculate both the individual predicted number of doctor visits (for all latent classes) and the individual likelihood of belonging to one of the three (or two in the case of SHI) latent classes using (5). Table 2.3 reports average probabilities for the different classes and average predicted values. Based on the predicted numbers of visits we call the classes the "low users", "medium users" and "high users".[14] Every group has a reasonably high share. The high users have a share of about one third whereas low users or low and medium users account for the remaining two thirds.

Table 2.3: Shares of individuals within each latent class and predicted numbers of physician visits

	Sample deductibles		Sample add-on	
	Share of individuals	Predicted mean	Share of individuals	Predicted mean
Low users	0.43	0.84	0.67	1.57
Medium users	0.25	2.19	-	-
High users	0.32	3.58	0.33	4.15

Predicted values based on regression results in Table A2.2. Predicted shares of latent classes are calculated based on the results in Table A2.3

Due to the nonlinearity of the model, the interpretation of the estimated parameters

[14] Another possibility would be to assign each individual the group she is most likely to be in according to her individual π_{ij} and then average the predicted values only over those appearing in the respective groups. This leads to very similar results.

is not straightforward. Here we focus on the calculated marginal effects of the most interesting variables in order to interpret the results (Table 2.4). The marginal effects are calculated in the following way: first we compute the marginal effect of each individual in both (or, in the private insurance case, three) latent classes. Then we average over all individual marginal effects within all latent classes. The full regression results can be found in Tables A2.2 and A2.3 in the appendix.

The effects of the insurance variables vary with the latent classes. Both low users and medium users in the private insurance have a lower likelihood of one doctor visit (in the first stage) in case of a contract with deductible. But only for the low users the effect is significant at the 5 per cent-level. Likewise, the effect is negative but insignificant for both low and medium users in the second stage. The same results hold for the low users in the SHI. Here, more insurance implies more doctor visits, also significant in both the first and the second stage. On the other hand, the insurance status does not significantly affect the behaviour of high users in the SHI sample and even has an unexpected effect in the privately insured sample. Here, less insurance seems to increase the demand for doctor visits (however, not significant). All together, especially low users appear to react on the incentives given by the insurance status. These results are in line with those found in Deb and Trivedi (1997) and Bago d'Uva (2006). However given an average probability of seing a doctor within three months of 68 per cent, a reduction by 7.1 percentage points (deductible/low users) or an increase by 5.8 percentage points appears to be rather small.

Concerning equivalised household income the results, again, differ by latent classes. Only small or insignificant effects can be found in both groups of high users. In the group of low and medium users, however, higher income is associated with more doctor visits. Again, this especially holds for a higher likelihood of one visit (i.e., the first stage). Thus, after controlling for the health status and other important factors like age and sex this result points into the direction of a pro-rich inequity. That is, richer individuals seem to have a better access to health care at least in the low users group. This is again in line with the results of previous international studies using finite mixture models. However, most other studies find a positive income effect for all latent classes but a stronger effect for the low users (e.g., Deb and Trivedi (2002),Bago d'Uva (2005), Bago d'Uva (2006)). We find this result only in the group of SHI insured, not in the private insurance. However, the results are not perfectly comparable, as we run different estimations for privately and publicly insured. It may well be that, taken both groups together, there is a positive effect in all latent classes.

Table 2.4: Marginal effects of insurance and income variables for all latent classes

Sample	Latent class	Deductible 1st stage	Deductible 2nd stage	Add-on 1st stage	Add-on 2nd stage	Income 1st stage	Income 2nd stage
Priv.	Low users	-0.071*	-0.358			0.082*	0.157
Ins.		(0.031)	(0.216)			(0.024)	(0.132)
	Medium users	-0.066	-1.175			0.125*	0.589
		(0.038)	(0.980)			(0.040)	(0.319)
	High users	0.075	1.623			-0.016	-0.140
		(0.039)	(1.058)			(0.034)	(0.277)
SHI	Low users			0.058*	0.549*	0.034*	0.119*
				(0.013)	(0.163)	(0.008)	(0.039)
	High users			-0.013	-0.413	0.026*	-0.105
				(0.016)	(0.475)	(0.011)	(0.108)

*$p<0.05$; Marginal effects based on regressions as reported in table A2.2. The marginal effects are calculated individually and over each of the components and then averaged over all individuals. The marginal effects of the continuous variables calculated numerically using the following formula: $ME_i = (p_{i,new} - p_i)/(Income_i/100)$, where p_i is the predicted value, $p_{i,new}$ is the predicted value assuming a household income of 1% more than the observed value.

We briefly summarise the effects of other important variables, particularly the newly available health measures. The estimated parameters of these variables are reported in Table A2.2. All three variables indicating the health status show a highly significant effect on the demand for health care. Both higher PCS and higher MCS (indicating a healthier individual) lead to less doctor visits, where the effect of physical health is stronger than the one of mental health. However, even when controlling for both more comprehensive health measures, self-assessed general health status remains significant in most equations. A very high body-mass index (> 30) significantly increases the number of visits for the publicly insured. Finally, having had a hospital visit in the last year or being handicapped furthermore increases the number of visits. Altogether, the health variables are highly significant and explain the demand for physician visits to a great amount.

Worries about the own health are associated with more doctor visits in the SHI sample. Thus, this variable might either capture even more information about the true health status or also behavioural differences between individuals with the same health status (or both). The self-assessed risk attitude towards health also has the expected sign (more risk-averse individuals have more doctor visits, conditional on the health status) and is significant in the first stage in the group of privately insured.

2.5 Endogeneity concerns

When analysing the impact of health insurance on the demand for health care services, there is possibly an endogeneity problem. As discussed in the introduction, this problem might stem from omitted variable bias due to unobserved health status and unobserved risk-preferences. However, including the new set of health and risk variables might reduce the endogeneity problem.

Given the principal-agent interpretation of the hurdle model, one can argue that possible endogeneity of the insurance choice should mainly play a role in the first stage, namely when the patient has full control. This is corroborated by the regression results that mainly showed significant effects of the insurance variables in the first and not the second stage (with one exception). Thus, as a robustness check, to determine if endogeneity is a problem, we focus on that stage and model the decision to see a doctor together with the decision about a certain insurance contract. Consider the following bivariate model

$$y_1 = 1[x_1\beta_1 + h\delta_1 + \alpha_1 y_2 + \mu_1 > 0] \quad (2)$$

$$y_2 = 1[x_2\beta_2 + h\delta_2 + \mu_2 > 0] \quad (3)$$

where y_1 is the binary decision to see a doctor and y_2 the binary decision about the insurance contract (e.g. deductible yes/no, or add-on insurance yes/no), h is the observed health status, and x_1 and x_2 are socio-economic variables like age, education, and income. Finally, μ_1 and μ_2 capture unobserved effects like unobserved health and preferences towards visiting a doctor. We assume that the number of doctor visits in one period depends on the insurance status whereas the insurance status does not depend on the number of doctor visits in that same period (given a certain health status). The correlation between μ_1 and μ_2, $\rho = corr(\mu_1, \mu_2)$, does not equal 0 if the unobserved effects that affect the number of doctor visits and the insurance choice are correlated. Assuming a bivariate normal distribution of the error terms, the parameters of this model (and the correlation ρ) can be estimated by a bivariate probit.

Again, we fit two different regressions, one where the insurance variable is *deductible* and one where it is *add-on*. In the first regression, ρ can be expected to be negative if the insured who expect to have fewer doctor visits in the future (for unobserved reasons) tend to buy insurance with a deductible. Unlike in the deductible case, add-on insurance leads to more insurance coverage, hence, a positive ρ can be expected in the second regression.

We add variables to x_2 that are assumed not to affect the decision to visit a doctor and are thus excluded from x_1. These are risk-aversion concerning financial matters and the general attitude towards co-payments for health care services.[15] The bivariate probit model allows for endogeneity of the insurance choice in the doctor-visits equation via correlation of the error terms. However, as Table 2.5 shows, in neither of the two cases is the estimated correlation coefficient significantly different from zero. We follow Knapp and Seaks (1998) in using a t-test on the correlation coefficient as a test for endogeneity of the dummy regressors. Here, the hypothesis of $\rho = 0$ (no endogeneity) cannot be rejected in either case.

Table 2.5: Estimated correlation in bivariate probit

Equation	$\hat{\rho}$	$\hat{se}(\hat{\rho})$	Observations
Deductible	0.022	(0.31)	5152
Add-on	0.090	(0.10)	43458

Standard errors clustered by individuals. Full estimation results in the appendix, see table A2.4

In the deductible equation the sign is different from what is expected, however, the value is very close to zero in both cases. It can be argued that capturing information from the new health variables and the degree of risk-aversion (plus health worries) together with all the other socio-economic variables reduces the endogeneity problem by a substantial amount. However, one can think of even more reasons originating in the insurance system that render endogeneity being much less of a problem than possibly expected. As discussed in Section 2, opting out of private

[15] Risk-aversion concerning financial matters is asked for in the same way as the risk-aversion concerning health. The question about the attitudes towards co-payments was asked only in 2002. As in the case of risk-aversion, we assume that this is a time-invariant preference.

insurance and deciding on deductibles are practically lifetime decisions. While the (partly unobserved) health status should have a high impact on the decision about health insurance in a given year, it only affects the number of doctor visits in the following years but not the decision about the insurance type. Moreover, Grabka (2006) gives another reason for the privately insured to switch to a contract with a deductible that is independent of changes in the health status of the insured. Unlike in the case of public insurance, cost containment and the stability of contribution rates have not been considered in the past decades in the private insurance system. This has led to a much higher proportional increase in costs than in the SHI and, thus, in steadily increasing contribution rates for the privately insured. One way for an insured to stop an increase in the contribution rate in a given year is to transform a contract without a deductible into one with a deductible to keep the basic insurance premium stable. In this case, the decision about buying insurance with a deductible is not affected by a change in the health status but by other reasons. Together with the performed statistical tests this supports the idea that the results of the previous section are not biased by endogeneity problems.

2.6 Conclusion

In this paper we analyse the income elasticity of German individuals with respect to the utilisation of health care services and the effect of health insurance coverage. We find that there are effects of health insurance on the demand for physician visits and that richer individuals tend to have more doctor visits conditional on their individual needs. Both effects, however, are only found in the group of low users, that is, those who have only a lower probability to see a doctor from the outset. With a flexible hurdle model that accounts for individual unobserved heterogeneity, we observe that especially the newly available health measures in the SOEP from the SF12v2-questionnaire, the BMI, and the measure of risk-aversion can partly explain the demand for physician visits (the latter only in the case of privately insured). Altogether, we find that possible endogeneity of the insurance status is not a big problem in the data set.

The results on the responsiveness of the group of German low users is in line with those in the recent literature for other countries that uses finite mixture models. The effects of the insurance status, however, are not straightforward to interpret. In the group of high users, deductibles do not seem to have an effect on cost-consciousness, nor does private supplementary insurance increase the demand for physician visits. Apparently, these individuals need physician visits anyway, irrespective of their insurance status. Therefore, in this group, optional deductibles rather seem to spur on the decomposition of the risk-pool instead of increasing efficiency. The findings for the group of low users, however, are more in line with those of Felder and Werblow (2008), who do find incentive effects of optional deductibles.

Although we find incentive effects that are - at least in some subgroups - statistically significant, they are not very strong in economic terms. While individuals who hold contracts with less insurance coverage have much less doctor visits, this difference is strongly reduced when we take observed and unobserved heterogeneity into account. Thus, the selection into different insurance schemes explains these observed differences in health care utilisation to a great deal. However, this does not mean that deductibles as such are not a good measure to control costs. The introduction of mandatory cost-sharing - as found in the Rand Health Insurance Experiment - might strongly affect the demand for health care. However, as the German experience shows, the amount of cost-sharing needs to be non-negligible in order to see some effects. Moreover, as the results of this paper imply, these effects might then be driven by the group of individuals who would not choose optional deductibles (either due to a bad health status, a high degree of risk-aversion or a strong preference for doctor visits). Since, on average, these are also financially less well-off, the increased efficiency due to mandatory cost-sharing might come at the cost of inequality in access to health care. Moreover, the finding of an already existing pro-rich inequity, although being small only, presents the possible problems of increased cost-sharing.

Therefore, one might think of supply-side mechanisms that reduce the realised consumption of health care. Ellis and McGuire (1990) argue that it can be optimal to leave the demand for health care on a too high level (i.e., higher than socially optimal) and not reducing it by demand-side cost-sharing. If the incentives of the physicians are set such that they do not satisfy this excess demand, the realised amount of health care services will be lower than the one preferred by the fully insured patients. This would be a possibility to achieve efficiency gains when individuals show moral hazard behaviour without the need to increase the financial risk of illness for the patients by cost-sharing. The next chapter provides some evidence that indeed physicians react on supply-side incentives imposed by the reimbursement system and, thus, can strongly influence the individual health care utilisation.

A qualification of this chapter concerns the separation of moral hazard and adverse selection. Both are problems that arise from the information asymmetry of insurer and insured and lead to a higher utilisation of health care of individuals with more insurance. Observing a higher number of doctor visits of those with more insurance need not necessarily be moral hazard. However, insurance contracts with deductibles as well as private supplementary insurance are offered by private insurance companies who are allowed to collect detailed information on the health status at the time the insurance contract is signed. Therefore, the degree of ex-ante information asymmetry which possibly leads to adverse selection is reduced due to the good information of the insurance companies. Indeed, although Schmitz (2009) finds evidence for adverse selection in one German market for private supplementary insurance (the one for hospital visits), it is only of a low degree. Nevertheless, the measured incentive effects should, therefore, be seen as an upper bound.

2.7 Appendix

Table A2.1: Sample means by subgroups

	SHI without add-on	SHI with add-on	Private with deductible	Private without deductible
Log. income	7.30	7.55	7.90	7.82
PCS	49.15	50.86	52.63	51.29
MCS	49.75	50.15	51.59	51.22
Self-assessed health	2.65	2.50	2.36	2.44
BMI high	0.36	0.34	0.37	0.34
BMI very high	0.15	0.15	0.10	0.10
Hospital stay prev. year	0.11	0.10	0.07	0.11
Handicapped	0.12	0.09	0.07	0.10
Smoker	0.30	0.31	0.28	0.23
Worries Health	2.10	2.21	2.32	2.27
Risk attitude health	2.82	3.20	3.49	3.10
Female	0.54	0.56	0.34	0.44
Age	48.0	44.90	47.29	49.13
Foreign	0.07	0.02	0.05	0.02
Married	0.62	0.63	0.64	0.64
Children in househ.	0.30	0.34	0.33	0.27
Full-time employed	0.36	0.49	0.62	0.34
Self-employed	0.02	0.07	0.37	0.18
Blue collar worker	0.18	0.15	0.01	0.01
White collar worker	0.28	0.46	0.31	0.23
Health job	0.03	0.05	0.07	0.05
Years of schooling	11.3	12.32	13.67	13.22
2002	0.34	0.25	0.31	0.34
2004	0.36	0.33	0.36	0.34
Risk attitude finance	2.24	2.76	3.45	2.89
Attitdue cost-sharing	3.34	3.10	2.56	2.82
Observations	42675	3644	3081	2470

Source: SOEP, pooled years 2002, 2004, 2006; no civil servants.

Table A2.2: Estimation results of latent class hurdle model

	Privately Insured				SHI Insured			
	$\hat{\beta}_{stage1}$		$\hat{\beta}_{stage2}$		$\hat{\beta}_{stage1}$		$\hat{\beta}_{stage2}$	
Deductible (LC 1)	-0.373*	(0.16)	0.156	(0.14)				
Deductible (LC 2)	-0.678	(0.40)	-0.371*	(0.09)				
Deductible (LC 3)	0.439*	(0.22)	-0.008	(0.09)				
Addon (LC 1)					0.312*	(0.07)	0.088*	(0.03)
Addon (LC 2)					-0.116	(0.14)	0.157*	(0.04)
Log. income (LC 1)	0.432*	(0.12)	0.171	(0.11)	0.182*	(0.04)	0.080*	(0.02)
Log. income (LC 2)	1.264*	(0.32)	0.342*	(0.09)	0.242*	(0.10)	-0.028	(0.03)
Log. income (LC 3)	-0.092	(0.20)	-0.039	(0.08)				
PCS	-0.053*	(0.01)	-0.024*	(0.01)	-0.045*	(0.00)	-0.029*	(0.00)
MCS	-0.022*	(0.01)	-0.016*	(0.00)	-0.013*	(0.00)	-0.012*	(0.00)
Self-assessed health	0.112	(0.10)	0.124*	(0.05)	0.209*	(0.03)	0.108*	(0.01)
BMI high	0.201	(0.12)	0.085	(0.06)	0.038	(0.03)	0.003	(0.02)
BMI very high	0.388	(0.24)	-0.108	(0.13)	0.105*	(0.05)	0.052*	(0.02)
Hospital stay prev. year	0.534*	(0.16)	0.244*	(0.06)	0.747*	(0.05)	0.316*	(0.02)
Handicapped	0.875*	(0.22)	0.236*	(0.09)	0.518*	(0.06)	0.059*	(0.02)
Smoker	-0.215	(0.14)	-0.003	(0.06)	-0.349*	(0.04)	-0.106*	(0.02)
Worries health	-0.108	(0.09)	-0.091	(0.05)	-0.304*	(0.02)	-0.145*	(0.01)
Risk attitude health	-0.088*	(0.03)	-0.017	(0.02)	-0.003	(0.01)	-0.003	(0.00)
Female	0.773*	(0.16)	0.235*	(0.07)	0.449*	(0.04)	0.045*	(0.02)
Age	-0.104*	(0.03)	-0.007	(0.01)	-0.064*	(0.01)	-0.014*	(0.00)
Age squared	0.001*	(0.00)	0.000	(0.00)	0.001*	(0.00)	0.000*	(0.00)
Foreign	-0.756*	(0.27)	-0.244	(0.16)	0.103	(0.06)	0.158*	(0.03)
Married	0.223	(0.17)	0.106	(0.07)	0.087*	(0.04)	0.067*	(0.02)
Children in househ.	-0.435*	(0.16)	-0.269*	(0.07)	-0.097*	(0.04)	-0.030	(0.02)
Full-time employed	0.008	(0.20)	0.043	(0.10)	-0.114*	(0.05)	-0.070*	(0.03)
Self-employed	0.075	(0.19)	-0.299*	(0.10)	-0.298*	(0.08)	-0.094	(0.05)
Blue collar worker	0.123	(0.37)	0.164	(0.17)	-0.170*	(0.05)	-0.090*	(0.03)
White collar worker	0.019	(0.22)	-0.171	(0.09)	-0.005	(0.05)	-0.079*	(0.03)
Health job	-0.518*	(0.24)	-0.303	(0.16)	-0.222*	(0.08)	0.034	(0.05)
Years of schooling	-0.018	(0.02)	-0.008	(0.01)	0.052*	(0.01)	0.017*	(0.00)
2002	0.080	(0.09)	-0.020	(0.05)	0.122*	(0.03)	0.093*	(0.01)
2004	0.089	(0.09)	-0.021	(0.05)	0.068*	(0.03)	-0.016	(0.01)
Constant (LC 1)	2.688*	(1.36)	1.064	(1.08)	2.408*	(0.42)	2.081*	(0.22)
Constant (LC 2)	-0.901	(2.79)	0.497	(0.85)	3.580*	(0.78)	3.625*	(0.27)
Constant (LC 3)	8.039*	(2.17)	3.913*	(0.81)				
Alpha (LC 1)			0.111	(0.10)			0.101*	(0.01)
Alpha (LC 2)			0.015	(0.07)			0.717*	(0.03)
Alpha (LC 3)			0.857*	(0.10)				
Log-pseudolikelihood	-9643.098				-86304.011			
Akaike	19532.196		.		172786.022		.	
Observations	5551				46319			

Standard errors in parentheses; * $p < 0.05$; The hypothesis of equality of all parameters across the two stages could be rejected in favor of the hurdle model. “LC” = latent class. Reference groups for dummies: BMI <= 25 (for BMI high and BMI very high), no full-time employment (for Full-time employed), unemployed or out-of-the labour force (for Self-employed, Blue collar worker, and White collar worker).

Table A2.3: Probabilities of latent class membership

	Privately Insured				SHI Insured	
	π_1		π_2		π	
	coefficient	std. error	coefficient	std. error	coefficient	std. error
Deductible	0.085	(0.32)	0.335	(0.36)		
Addon					-0.139	(0.15)
Log. income	0.664*	(0.29)	0.582	(0.31)	-0.149	(0.10)
PCS	-0.023	(0.03)	-0.037	(0.04)	-0.003	(0.01)
MCS	0.013	(0.02)	0.009	(0.02)	-0.004	(0.00)
Self-assessed health	-0.189	(0.32)	0.301	(0.37)	-0.192*	(0.08)
BMI high	0.317	(0.31)	-0.258	(0.31)	-0.057	(0.09)
BMI very high	1.502	(1.05)	1.533	(0.90)	0.127	(0.11)
Hospital stay prev. year	-0.996	(0.65)	0.293	(0.56)	-1.131*	(0.13)
Handicapped	0.837	(0.70)	0.702	(0.77)	-0.500*	(0.12)
Smoker	-0.195	(0.36)	-0.416	(0.32)	-0.181*	(0.09)
Worries health	0.770*	(0.31)	0.481	(0.32)	-0.126	(0.08)
Risk attitude health	-0.229*	(0.06)	-0.121*	(0.06)	0.006	(0.02)
Female	0.713	(0.38)	0.113	(0.34)	-0.250*	(0.10)
Age	-0.095	(0.06)	0.050	(0.06)	-0.015	(0.02)
Age squared	0.001	(0.00)	-0.001	(0.00)	0.000	(0.00)
Foreign	-1.193	(0.67)	-0.186	(0.72)	0.276	(0.15)
Married	-0.033	(0.44)	-0.452	(0.37)	0.203	(0.10)
Children in househ.	-0.758	(0.45)	0.421	(0.38)	-0.094	(0.10)
Full-time employed	1.852*	(0.59)	0.482	(0.55)	-0.011	(0.13)
Self-employed	0.355	(0.61)	0.617	(0.58)	-0.083	(0.23)
Blue collar worker	0.857	(1.30)	0.481	(1.22)	0.139	(0.15)
White collar worker	-1.451*	(0.59)	-0.731	(0.54)	-0.210	(0.14)
Health job	0.861	(0.89)	1.317	(0.95)	0.250	(0.21)
Years of schooling	-0.119*	(0.04)	-0.048	(0.04)	0.037*	(0.02)
2002	1.241*	(0.58)	1.857*	(0.66)	0.489*	(0.19)
2004	0.048	(0.55)	0.062	(0.64)	0.348*	(0.21)
Constant	-1.730	(3.99)	-5.389	(4.13)	2.609*	(1.07)

* $p<0.05$;

Coefficients have to be interpreted relative to the base category in the private insurance regressions.

Table A2.4: Bivariate probit results

	Privately Insured				SHI Insured			
	Doctor Binary		Deductible		Doctor Binary		Addon	
Deductible	-0.090	(0.51)						
Addon					-0.046	(0.20)		
Log. income	0.096*	(0.04)	0.019	(0.05)	0.131*	(0.02)	0.478*	(0.03)
PCS	-0.026*	(0.00)	0.005	(0.00)	-0.024*	(0.00)	-0.002	(0.00)
MCS	-0.011*	(0.00)	0.004	(0.00)	-0.007*	(0.00)	0.001	(0.00)
Self-assessed health	0.109*	(0.04)	0.070*	(0.04)	0.131*	(0.01)	-0.012	(0.02)
BMI high	0.004	(0.05)	-0.034	(0.05)	0.035*	(0.02)	0.014	(0.03)
BMI very high	0.038	(0.08)	-0.045	(0.08)	0.050*	(0.02)	0.061	(0.04)
Hospital stay prev. year	0.384*	(0.08)	-0.096	(0.06)	0.483*	(0.03)	0.084*	(0.03)
Handicapped	0.361*	(0.10)	-0.033	(0.09)	0.350*	(0.03)	0.028	(0.04)
Smoker	-0.094	(0.05)	-0.009	(0.05)	-0.170*	(0.02)	0.079*	(0.03)
Worries health	-0.157*	(0.04)	0.007	(0.04)	-0.168*	(0.01)	0.018	(0.02)
Risk attitude health	-0.007	(0.01)	0.003	(0.01)	-0.002	(0.00)	0.007	(0.01)
Female	0.245*	(0.05)	0.035	(0.06)	0.299*	(0.02)	0.110*	(0.03)
Age	-0.031*	(0.01)	0.042*	(0.01)	-0.031*	(0.00)	0.009	(0.01)
Age squared	0.000*	(0.00)	-0.000*	(0.00)	0.000*	(0.00)	-0.000*	(0.00)
Foreign	-0.153	(0.10)	0.146	(0.12)	0.018	(0.03)	-0.383*	(0.06)
Married	0.084	(0.06)	-0.119*	(0.06)	0.017	(0.02)	-0.009	(0.03)
Children in househ.	-0.012	(0.05)	0.055	(0.06)	-0.038	(0.02)	0.102*	(0.03)
Full-time employed	-0.291*	(0.11)	0.417*	(0.08)	-0.064*	(0.02)	0.022	(0.03)
Self-employed	0.006	(0.12)	0.455*	(0.09)	-0.161*	(0.04)	0.247*	(0.06)
Blue collar worker	-0.022	(0.16)	0.281	(0.16)	-0.117*	(0.03)	-0.000	(0.04)
White collar worker	0.264*	(0.09)	0.177*	(0.09)	0.031	(0.03)	0.152*	(0.04)
Health job	-0.347*	(0.10)	-0.331*	(0.10)	-0.178*	(0.04)	0.010	(0.06)
Years of schooling	0.014*	(0.01)	-0.014*	(0.01)	0.026*	(0.00)	0.015*	(0.01)
2002	0.023	(0.05)	-0.087*	(0.04)	0.056*	(0.02)	-0.324*	(0.02)
2004	0.039	(0.04)	-0.002	(0.03)	0.033*	(0.02)	-0.234*	(0.02)
Risk attitude finance			0.014	(0.01)			0.018*	(0.01)
Attitdue cost-sharing			-0.117*	(0.02)			-0.075*	(0.02)
Constant	2.123*	(0.55)	-1.494*	(0.56)	1.237*	(0.22)	-5.020*	(0.32)
ρ	0.022	(0.31)			0.090	(0.10)		
Log-pseudolikelihood	-6225.409				-33881.073			
Observations	5152				43458			

*p<0.05; Standard errors in parentheses

Estimations done by Stata program biprobit, standard errors clustered by individuals, less observations than in table A3.2 because of missing values in the instruments. Reference groups for dummies: BMI <= 25 (for BMI high and BMI very high), no full-time employment (for Full-time employed), unemployed or out-of-the labour force (for Self-employed, Blue collar worker, and White collar worker).

Chapter 3

Practice budgets and the patient mix of physicians - Evaluating effects of remuneration system reforms on physician behaviour in Germany

3.1 Introduction

Steadily increasing health care costs have been an issue in most industrialised countries for the last few decades. In Germany, however, health care expenditures as a fraction of the gross domestic product have been rather stable since the mid-1990s even though demographic change and technological progress have increased the pressure on the health care system. For instance, the share was 10.4% in 1996 and it stood at 10.6% by 2006. This fraction has risen by 12 to 20% in countries like Switzerland, France, the US, and the UK in the same period (OECD, 2008). A major reason for the stability in Germany could be the introduction of fixed budgets for various health care sectors (stationary, ambulatory, and pharmaceuticals) that increase only by a limited amount each year. These fixed budgets are in general a means to introduce rationing in the health care market.

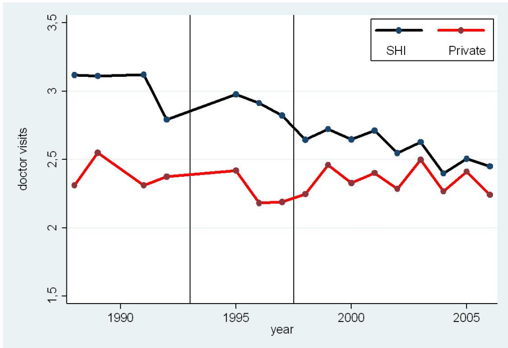
The budgets in the ambulatory sector directly affect the remuneration of physicians and, therefore, possibly also their behaviour. This study analyses the effect of the introduction of fixed budgets on physician's behaviour as measured by the length of treatment of patients. It uses a particularity of the German health insurance system, namely the existence of two different and independent insurance systems (private and statutory public insurance) that imply different remuneration systems for physicians. Until 1993, the remuneration for treating the statutorily insured was based on a fee-for-service (FFS) system. In 1993, the remuneration system was reformed with the introduction of a fixed budget and a point system for the statutorily insured. From then on, physicians got points for each treatment. The monetary value of each point was then calculated at the end of each quarter by dividing the total budget by the sum of all points collected by all physicians. In 1997, a further reform was introduced which capped the total points reported by doctors by a so-called "individual practice budget". Since no reform took place in the private sector in the period of analysis, this allows us to analyse the response of physicians to the change in financial incentives by using the reform as a source of exogenous variation.

This study contributes to a growing literature that analyses the effect of physician remuneration on the quantity of health care utilisation, typically measured by the number of physician visits. As found by Devlin and Sarma (2008), physicians conduct more patient visits under FFS than under any other remuneration system in Canada. Croxson et al. (2001) and Dusheiko et al. (2006) find effects of budgets on physician behaviour for the UK. Hennig-Schmidt et al. (2008) show in an experimental setting that physicians respond to incentives imposed by the reimbursement system and that they tend to overtreat patients under FFS and to undertreat them in a per-capita payment system. On the other hand, Madden et al. (2005) and Grytten and Sorensen (2001) do not find significant effects of the remuneration system on physician behaviour for Ireland and Norway. Implicitly, studies on the effects of the remuneration system on physicians' behaviour can be seen as analyses of supplier-induced demand. This is true at least for remuneration systems like the FFS-system, which provide incentives for physicians to conduct excess treatments.

To our knowledge, this is the first paper that evaluates the impact of the introduction of the fixed budget and the individual practice budget in Germany (see Wörz and Busse, 2005, who also note the absence of any scientific evaluation of these reforms). Up to now, German literature that analysed supplier-induced demand has concentrated on the effect of physician density on the number of doctor visits and findings show only weak and mixed evidence for Germany (Krämer, 1981; Breyer, 1984; Pohlmeier and Ulrich, 1995; Kopetsch, 2007). Only Jürges (2009) explicitly accounts for the differences in the remuneration system and finds that, in the year 2002, those who were privately insured had, on average, more doctor visits given that they had contacted a doctor. Moreover, while physician density increased the frequency of doctor visits for all patients, the effect was strongest for the privately insured.

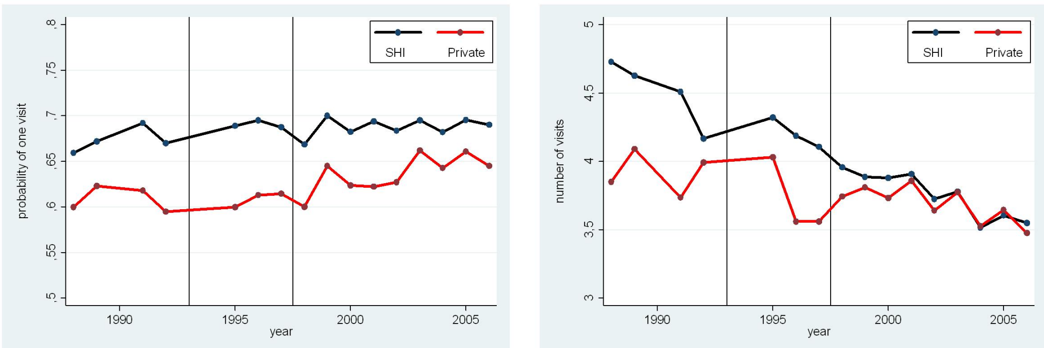
Figure 1 illustrates the evolution of the average number of doctor visits in the last three months prior to the interview using data from the German Socio-Economic Panel (SOEP) between 1988 and 2006. While the average number for the privately insured (around 10% of the population in 2006) stayed fairly constant around 2.3, the number for the publicly insured (the remaining 90%) steadily decreased from about 3.1 in 1988 to 2.5 in 2006. However, as Figure 2a shows, the probability of at least one visit slightly increased in the same period for both groups while the number of doctor visits for those individuals who had seen a doctor at least once in the previous three months (Figure 2b) decreased sharply for the publicly insured (from 4.7 to 3.5) and only slightly for the privately insured (from 3.8 to 3.5). Hence, the decline in the average number of doctor visits for the statutorily insured is almost exclusively a result of the decline in the number of visits for those individuals who had at least one doctor visit.

Figure 3.1: Average number of doctor visits in previous three months, overall



Source: SOEP, years 1988-2006. Vertical lines represent the years 1993 and 1997.

Figure 3.2: Average . . . in previous three months



(a) . . . probability of at least one doctor visit . . .

(b) . . . number of doctor visits, conditional on one visit

Source: SOEP, years 1988-2006. Vertical lines represent the years 1993 and 1997.

The slight increase in the probability of at least one doctor visit (Figure 2a) might reflect the growing importance of preventive doctor visits while the observed decline in the total number of doctor visits (Figure 1 and Figure 2b) may have several reasons. First, because this is a long panel, composition effects might play a role. That is, the panel might have changed in observables like age, education, or the health status of the respondents. All of these variables are important for the demand for doctor visits. Second, panel attrition could matter as well. It can be expected that unhealthy individuals (with a high demand for doctor visits) have a higher probability to drop out of the data set due to severe illness, death, or other reasons. Frijters et al. (2005) show that panel attrition of individuals with lower health satisfaction is an issue in the SOEP; Contoyannis et al. (2004) find similar problems in the British Household Panel Study (BHPS). These two points - compositional changes in observables and unobservables - could lead to a decline in the average number of doctor visits in the SOEP. However, because the SOEP is a representative panel that saw several refreshments in the observation period, both points should not be the only reasons for this picture.

Third, macroeconomic factors like the unemployment rate contribute to the demand for doctor visits. As is well established (e.g., Askildsen et al., 2005), work absenteeism is less frequent in recessions due to a greater fear of losing the job. Workers who are absent for more than three days, however, need a certificate from a doctor and, thus, a doctor visit. Hence, recessions could have decreased the number of doctor visits. Fourth, institutional changes concerning the supply side of health services (here, the physicians as providers of outpatient care) might have had an impact.

This study takes into account all four points but puts emphasis on the last one. It turns out that, after controlling for compositional effects, panel attrition, and economic conditions, the decline in the number of visits of the publicly insured is much less pronounced and can be attributed to the introduction of individual practice budgets in 1997. Moreover, not only did the number of doctor visits of the publicly insured decrease after the reform, it also increased for the privately insured. This gives rise to the interpretation that physicians responded to the reforms by changing their patient mix, i.e., by substituting out the publicly insured for the privately insured. The results are robust to different specifications. Moreover, several tests support the identifying assumptions, one of these being the test for the single spell assumption as derived by Santos Silva and Windmeijer (2001).

The remainder of this paper is structured as follows. Section 2 gives an overview of the institutional background and of the reforms that took place in the German health care system. Section 3 explains the data, Section 4 the empirical strategy. Section 5 presents the estimation results. Section 6 shows the robustness of the results and supports the identifying assumptions while Section 7 concludes.

3.2 Payment System and Major Reforms

The German health insurance system consists of two parts. About 90% of the population are insured by statutory health insurance (SHI; also called public insurance hereafter). It is compulsory for all individuals with earnings below a certain income threshold (3,975 Euro per month in 2007) and who are not civil servants or self-employed. It is financed by payroll taxes and non-working family members are covered without an extra premium. The benefit package is heavily regulated and does not vary much between insurance companies. Individuals who earn more than the income threshold, the self-employed, and civil servants are allowed to opt out of the statutory insurance system and instead buy private insurance. This group accounts for the remaining 10% of the German population. The private insurance premium does not depend on income but instead is a risk-equivalent contribution depending on age, gender, and health status when the contract is signed. Privately insured individuals have to pay higher premia in order to cover non-working family members. Thus, having many dependents is a reason for staying voluntarily in the public system for about 50% of all the individuals who are eligible to opt out.

Physicians are remunerated according to an FFS-system. Before 1993, the price for a treatment was fixed ex ante and depended on the complexity of the treatment. Treatments of the statutorily insured were (and still are) charged according to the EBM ("Einheitlicher Bewertungsmaßstab"), whereas treatments of privately insured were charged according to a different legal setting, namely the GOÄ ("Gebührenordnung für Ärzte"). The statutory health insurance is a full cover insurance (with some exceptions). The insurance company directly pays for the treatments and hence the patient does not see the costs she actually incurs. In such a system, neither the patient nor the doctor has an incentive to contain costs (Jürges, 2009). On the contrary, due to the well known information asymmetry between patient and physician, the physician can possibly induce demand to increase income. This incentive system might have contributed to an average number of doctor visits in Germany that is higher than in most other countries.

In reaction to the steadily increasing health care expenditures, the German government introduced a fixed budget for ambulatory fees for the statutorily insured in 1993. Under this system, doctors receive points for each treatment according to the severity of the case. At the end of each quarter, the monetary value for each point is calculated as the value of the total budget divided by the sum of all points collected by all doctors. The budgets and the sum of all points are determined regionally. Hence, the monetary value of one point varies by region and time.

The fixed budget was introduced in order to keep the overall costs stable for the social health insurance system. However, it cannot contribute to a reduction of medical services by physicians, as Benstetter and Wambach (2006) show theoretically. For instance, a single physician can still increase her income by increasing the duration of treatments. Given the fixed budget, however, and no coordination between physicians, this can only be at the cost of the point value. A decreasing point value again leads to increased activity of the physician, resulting in a further decrease in the point value. This is called the "treadmill effect". Indeed, the point value declined after 1993 (Benstetter and Wambach, 2006). This, however, was not due to an increasing number of physician visits but due to the fact that physicians charged much more services during a treatment, especially doctor's advice. The number of consultations even decreased by 6.5% but the number of charged points increased by more than 30% (Wittek, 1996).[1]

Although the increasing dispensation of doctor's advice was partly intended by the policy makers to strengthen the "speaking medicine", some groups of physicians were negatively affected by the reform while others benefited.[2] In order to stop the declining point value, individual practice budgets were introduced in addition to the overall fixed budget in July 1997. With this reform, each physician receives a maximum number of points she could reimburse for each quarter. Points exceeding the practice budget were reimbursed by a much lower point value. The reform was successful in stabilising the point value (see Benstetter and Wambach, 2006). Since 1999, the budget for ambulatory care may not rise faster than the payroll tax base in Germany. However, since 2000, the increase in the payroll tax base was lower than the inflation rate (see Table 3.1). Particularly in 2003, the budget did not increase at all. Altogether, this led to a reduction of the budget in real terms after 2000.

Given the incentives induced by the reform of 1993, a decrease in the number of doctor visits cannot be expected after the introduction of the fixed budget because,

[1] All numbers are only for the statutorily insured. Therefore, the decrease in doctor visits need not result from the reform but – due to the absence of a control group – can be due to a temporary shock. In fact, below, we evaluate the reform effects on the number of consultations including a group that was not directly affected, the privately insured.

[2] The net effect for all physicians was zero in monetary terms since the total budget was fixed at the expenditure level prior to the reform.

Table 3.1: Payroll tax base and CPI, normalised

Year	2000	2001	2002	2003	2004	2005	2006
Payroll tax base	100.0	102.1	102.8	102.8	103.4	103.2	104.7
CPI	100.0	102.0	103.4	104.5	106.2	108.3	110.1

Source: RWI Essen, base year is 2000.

as regards incentives for doctors, there is no strong difference between a system where physicians get money values or point values for a treatment. In contrast, the reform in July 1997 should have an effect on the physician's behaviour. If the physician incurs a cost for each treatment, she should have an incentive not to exceed the individual amount of points she can get reimbursed. If it was before the case that physician-induced demand (i.e., longer treatments than necessary) increased the income of (some) physicians, the incentive to carry out unnecessary treatments was reduced after the reform. Another possibility is to postpone recalls of publicly insured patients into the next quarter.

There might also be an incentive to substitute out the publicly for the privately insured. Reducing the number of recalls of the publicly insured leads to higher available capacities for physicians to spend on the privately insured. Since treatment of the privately insured does not affect the practice budget, physicians might have an incentive to fill the gap by boosting the recalls of the privately insured. The tightening of the budget in real terms after 2000 should have further strengthened these incentives.

The private insurance system has not seen any reform during the whole period. That is, while the reimbursement system for statutorily insured patients has substantially changed, it remained unchanged for privately insured patients. A first indicator of a positive effect of rationing in the SHI is the development of health insurance contributions in the public and the private system. Using data from the SOEP for the period 1984-2006, Grabka (2006) shows that, while the contributions nominally increased by about 130% in the SHI, they tripled in the private insurance system.

A major problem in identifying the reform effect is that, in this long time period, two other reforms affected the demand (i.e., patient) side of the market. This is especially important since the data we use here measure the number of doctor visits at the patient level. First, at the same time that the introduction of practice budgets became effective, there was a reform that increased the co-payments for prescription drugs by 6 DM (about 3 Euro).[3] Winkelmann (2004a, 2004b, 2006) finds remarkable negative effects of the reform on the demand for doctor visits using the same data as this paper. However, as we will argue later in Section 5, it is more likely that these effects are due to the introduction of practice budgets and not due to the increased co-payments for prescription drugs. Second, a co-payment of 10 Euro was introduced in 2004 for the first doctor visit in each quarter. While Augurzky et al. (2006) and Schreyögg and Grabka (2010) do find significant demand-side effects in the SOEP, Farbmacher (2009) finds a reduction in the probability of visiting a physician which is small but significant.

3.3 Data

Our empirical analysis is based on the German Socio-Economic Panel (SOEP), a survey which started in 1984 with more than 12,000 individuals in West Germany and was extended to include East Germany in June 1990. There were several refreshments resulting in a sample size of more than 20,000 adult individuals living in about 13,000 households that participated in the SOEP survey in 2006 (see, e.g.,

[3]The reform was partly abolished in 1999.

Wagner et al., 2007).[4] The SOEP includes questions about the total number of doctor visits within the last three months prior to the interview in all years except for 1990 and 1993. In the years 1984 to 1987 and 1994, the SOEP does not ask for the total number but differentiates between general practitioners (GPs) and various kinds of specialists. Because these questions differ from the one in all other years, we drop the years 1984-1987 and 1994 from the analysis.[5]

In order to not confound our results with the possible effects of the co-payment reform in 2004, we also disregard information from the years after 2003. Finally, we drop all individuals of the year 1997 that were interviewed after June. Because the question refers to the number of doctor visits in the previous three months, one cannot see if the stated number of visits falls into the period before or after the reform for these observations. Since most of the interviews take place in the first months of each year we do not lose many observations.

The main drawback of the SOEP for this kind of analysis is that it only provides the total number of doctor visits per respondent in the last three months. There are no information on the number and duration of illness episodes that are captured by this value. Hence, the first observed count within the quarter might be the continuation of a previous illness episode instead of being the initiation of a new spell. Furthermore, several visits in a quarter might either result from one longer illness episode or from multiple short ones. Considering the identification strategy outlined in the next section, the latter issue might be particularly problematic.

[4]The data used in this paper were extracted using the Add-On package PanelWhiz v2.0 (Nov 2007) for Stata. PanelWhiz was written by Dr. John P. Haisken-DeNew (john@panelwhiz.eu). The PanelWhiz generated DO file to retrieve the SOEP data used here and any Panelwhiz Plugins are available upon request. Any data or computational errors in this paper are my own. Haisken-DeNew and Hahn (2006) describe PanelWhiz in detail.

[5]Including these years does not affect the results at all. Because only the total number of visits is asked in all remaining years, we cannot distinguish effects between general practitioners and specialists because, apart from the mentioned years, the type of physician is not specified in the data set. Since the reforms affected all types of physicians we consider this a minor problem.

Pohlmeier and Ulrich (1995) argue that the period of three months in the SOEP is a good compromise in reducing the former problem (that gets smaller with longer periods) without letting the latter problem get too large (which is reduced in shorter periods). In the following, we assume that the observed number of doctor visits results from only one sickness spell that starts at the beginning of the quarter to which the question refers. Santos Silva and Windmeijer (2001) call this the "single spell assumption" and argue that the validity of this assumption critically depends on the length of the observation period. In fact, this assumption can be tested. In Section 6.2 we outline the test for the single spell assumption that was derived by Santos Silva and Windmeijer (2001) and report the results.

The SOEP has several important virtues and it is probably the only available data set to answer the questions above. First, it covers a long period that starts well before the first reform became effective and continues to go on. It includes many variables that affect the individual demand for health care like the health status and many other socio-economic variables. Due to its panel nature, unobserved heterogeneity of the individuals can also be taken into account. Finally, and most importantly, it includes a group (the privately insured) that was not directly affected by either of the reforms. Although the groups of privately and statutorily insured are somewhat different, assuming that the group differences would have stayed stable over time without the reforms makes it possible to identify the reform effect on physician behaviour.

Several variables are included to control for the differences between the privately and the publicly insured. The main variables are those which control for the observable health status. These are satisfaction with own health (on an 11-point scale), the number of hospital stays in the previous year, age, and if the respondent is handicapped. *Sport* controls for different health behaviour. The average unemployment rate and a dummy for job absenteeism for more than three days in the last year captures macroeconomic aspects that affect the demand for doctor visits. A dummy for West Germany captures different regional behaviour as well as different infrastructure. Furthermore, two variables are included to capture the effects of population density. Finally, a few other socio-economic control variables are included. Since males and females exhibit considerable differences in their doctor visiting behaviour, we carry out separate regressions for both groups. Table A3.2 in the appendix explains the variables and reports sample means.

3.4 Empirical Strategy

3.4.1 Count Data Hurdle Model

The dependent variable (number of doctor visits) takes on only non-negative integer values. Therefore, it seems reasonable to use a count data model such as the negative binomial model that has the following probability density function (see, e.g., Cameron and Trivedi, 2005):

$$f(y_{it}|\mu,\alpha) = \frac{\Gamma(\alpha^{-1}+y_{it})}{\Gamma(\alpha^{-1})(\Gamma(y_{it}+1))}\left(\frac{\alpha^{-1}}{\alpha^{-1}+\mu}\right)^{\alpha^{-1}}\left(\frac{\mu}{\mu+\alpha^{-1}}\right)^{y_{it}}$$

where μ =exp($x'_{it}\beta$) and α is the over-dispersion parameter.

It is often argued that the observed number of doctor visits is a result of two different (and maybe independent) decision-making processes. In case of an illness, the patient decides whether or not to see a physician (1st stage). Once a doctor is seen, however, the doctor – maybe together with the patient – determines the length of the treatment (2nd stage). Hence, a hurdle model (also called a two-part model) seems to be the most appropriate formulation in order to explain the observed number of doctor visits (Mullahy, 1986; Pohlmeier and Ulrich, 1995). The underlying economic structure is that of a principal-agent model. That is, the first stage should mainly capture demand-side effects, while the second stage should also capture supply-side effects.

The availability of panel data allows to account for unobserved heterogeneity. We follow Bago d'Uva (2006) and add a time-invariant random effect that affects both stages. This random effect is supposed to follow a discrete distribution that takes on a small number of mass points. There is an economic and a statistical motivation for this resulting finite mixture model. The economic motivation classifies individuals into a small number of latent classes, e.g., two, the "high users" and the "low users", with different effects of covariates on the outcome variable. There is a debate in the literature on whether the standard hurdle model (with the differentiation between "users" and "non-users") or the finite mixture negative binomial model (with the less restrictive differentiation between "high users" and "low users") is better able to explain data on doctor visits (see, e.g., Deb and Trivedi (1997), Deb and Trivedi (2002), Jimenez-Martin et al. (2002)). The advantage of the model derived by Bago d'Uva (2006), which is also used by Bago d'Uva and Jones (2009), is to combine both previous models and to allow for latent classes but, at the same time, to maintain the principal-agent structure of the model.

The latent class hurdle model that, in its most general form, allows for slope heterogeneity, however, has very many parameters to estimate and is very data-demanding. Here, we keep the model parsimonious and restrict it to intercept heterogeneity and to different over-dispersion parameters (i.e., different values of α) for the different latent classes. Thus, the motivation for the resulting finite mixture model comes more from a statistical side, that is, the possibility to introduce a time-invariant individual effect θ_i^m without imposing too many distributional assumptions on the effect, except for some general random-effects assumptions

$$E[\theta_i^m|x_{it}] = 0; E(\theta_i) = \sum_{m=1}^{M} P(\theta_i^m)\theta_i^m = 0; \sum_{m=1}^{M} P(\theta_i^m) = 1, \forall m(m = 1, \ldots, M)$$

where M is the total number of mass points and $P(\theta_i^m)$ is the probability of mass point θ_i^m. The density of the observed data is given by

$$g_m(y|x, \theta_i^m) = \begin{cases} f_{1m}(0|x, \theta_i^m) & \text{if } y = 0 \\ \\ (1 - f_{1m}(0|x, \theta_i^m))f_{2m}(y|x, \theta_i^m, y > 0) & \text{if } y > 0 \end{cases}$$

where

$$f_{1m}(0|x_{it}, \theta_i^m) = P(y_{it} = 0|x_{it}, \theta_i^m, \beta_1) = (\mu_{1m} + 1)^{-1}$$

and

$$f_{2m}(y_{it}|x_{it}, \theta_i^m, \beta_2; y_{it} > 0) = \frac{\Gamma(\alpha_m^{-1} + y_{it})}{\Gamma(\alpha_m^{-1})(\Gamma(y_{it} + 1))((1 + \alpha_m\mu_{2m})^{\alpha_m^{-1}} - 1)} \left(\frac{\mu_{2m}}{\mu_{2m} + \alpha_m^{-1}}\right)^{y_{it}}$$

and μ_{1m} =exp($x'_{it}\beta_1 + \theta_i^m$), μ_{2m} =exp($x'_{it}\beta_2 + \theta_i^m$).

The parameter vectors β_1, β_2, the heterogeneity terms α_m, and the locations and probabilities of the mass points are estimated jointly by maximizing the following likelihood function, where T_i denotes the number of years individual i is observed in the data set:[6]

$$L = \prod_{i=1}^{N} \sum_{m=1}^{M} P(\theta_i^m) \prod_{t=1}^{T_i} g_m(y_{it}|x_{it}, \theta_i^m).$$

3.4.2 Estimation Strategy

In this study, we estimate the effects of two reforms on the patient mix of physicians. The analysis is complicated by the fact that the data are on the patient and not on the physician level. Given the principal-agent structure of the estimation model outlined in the previous section, we measure the physicians' behaviour in the second stage of the hurdle model using data of patients. The fact that the reform directly affected only the treatment of the publicly and not the privately insured gives reason to evaluate the effect of introducing fixed budgets for doctors on the number of doctor visits of the publicly insured using the group of the privately insured as a control group. However, this would mean assuming the absence of any general equilibrium effects, i.e., of any effects the reform might have had on the private sector. The theoretical discussion in Section 2 and the raw numbers in Figures 1 and 2b support the notion that the privately insured are also (indirectly) affected by the reform, because it might be that doctors substitute out the publicly for the privately insured. Therefore, the privately insured are not well-suited as a control group and the estimated treatment effect of a difference-in-differences analysis is likely be biased.

Nevertheless, in the estimation, we include a dummy for being publicly insured, time dummies, and interactions between time dummies and the indicator for public insurance (see Table 3.2 for the structure of the time dummies). We do this to

[6] We use the *ml* command in Stata and the Broyden-Fletcher-Goldfarb-Shanno quasi-Newton algorithm. We draw from the code provided in Jones et al. (2007). Different starting values are used to rule out local maxima of the likelihood function.

compare the expected number of doctor visits of the publicly insured with the one of the privately insured before and after the reform, keeping in mind that the difference does not necessarily measure the effect of introducing budgets on the number of doctor visits of the publicly insured.

Table 3.2: Covariates

Year	Regulations	T_{93-97}	T_{98-99}	T_{00-03}
1988 - 1992	Base system	0	0	0
1993 - 1997	Fixed budget	1	0	0
1998 - 1999	Individual budget	0	1	0
2000 - 2003	Stricter individual budget	0	0	1

By doing this, we can analyse whether or not physicians changed their patient mix after the reform. Ideally, one would want to compare the patient mix of doctors who were affected by the reform with the one of doctors who were not affected. Since virtually all physicians treat both privately and publicly insured, there is no control group of physicians available and therefore no true difference-in-differences analysis possible.[7] Our analysis is therefore a before-after evaluation. In order to identify a causal reform effect, we do not only have to assume absence of unobserved effects that differ for treatment and control groups (like in a difference-in-differences analysis) but absence of any unobserved effects that change the patient mix of doctors over time. This, however, does not seem to be a very restrictive assumption in our case. Assuming that there are no exogenous shocks that affect the *absolute number* of doctor visits might be too strong (think of flu epidemics, for example), making a reform evaluation without a control group unfeasible when the patient behaviour is the object of interest. However, we see no reason why unobserved effects should alter the *relative number* of visits, i.e., why doctors should change their patient mix for

[7]There is a very small group of physicians that specialise on treating privately insured patients only (less than 1% of all physicians). However, this group is not only small but usually specialises in fields like psychotherapy and is therefore not well-suited as a control group.

reasons other than the reform (i.e., privately and publicly insured are not affected differently by flu epidemics).

For the analysis to be valid, we furthermore have to assume common trends for publicly and privately insured. That is, we assume that the existing differences in the expected number of doctor visits between both groups would have stayed stable without the reform. Obviously, this counterfactual situation is not testable but we come back to this point in Section 6.1 (Robustness Checks). We also assume that individuals do not self-select into public or private insurance due to the reform. One might argue that publicly insured individuals who need many doctor visits realise that they would get shorter treatments due to the reform and therefore change into the private system. Because these would be the sicker individuals, the average number of doctor visits in the SHI would decrease and the number in the private system increase. This is very unlikely to be the case in Germany. First, only a small number of individuals (about 20%) can actually choose between statutory and private insurance. The remaining 80% cannot change even if they want to. Second, sick individuals in particular would decide to stay in the statutory system because private insurers are allowed to use the health status in calculating insurance premia. Thus, the contribution is higher in the private system than in the public system for sick individuals or, even worse, some services might be excluded from the private benefit package at the outset. However, as a robustness check, we carry out a regression including only those individuals who never switched health insurances after 1996.

3.4.3 Control for Panel Attrition

Although there were several refreshments in the SOEP, panel attrition might be a problem in such a long period of analysis (Frijters et al., 2005). It might be the case that unhealthy individuals with a high demand for doctor visits have a higher probability to drop out of the data set due to severe illness, death, or other reasons. This is a problem because it can be expected that the statutory health insurance covers a higher share of unhealthy people compared to the private insurance because of the worse risk-pool. We follow Freund et al. (1999) and include an inverse Mills ratio to control for possible panel attrition. A natural problem with panel attrition is that no individual characteristics can be observed when a person has already left the panel. Only the information of not being in the panel can be observed for attritors. Therefore, the inverse Mills ratio is constructed using the estimates of a probit regression of an indicator to be in the panel in the next period on all current control variables that appear later in the regression model. Variables that are assumed to have an effect on panel attrition but not on the number of doctor visits (conditional on the other control variables) are also included here, namely the degree of life satisfaction, a dummy for oral interview (instead of written), and the duration of the interview. Life satisfaction should not affect the demand for doctor visits once health satisfaction is controlled for but is likely to affect the general likelihood of participating in a panel study.[8] Likewise, if the interview was oral and shorter, the probability of staying in the panel is assumed to increase, whereas these characteristics should not affect the number of physician visits.

3.5 Estimation Results

Following the Akaike information criterion (AIC), a mixture model with four mass points outperformed models with three and two mass points, and the basic model without control for unobserved heterogeneity. The main results, however, are similar to all other models with a smaller number of mass points. A model with five mass

[8]See Frijters et al. (2004) who find panel attrition in the SOEP that is negatively linked to life satisfaction.

points failed to converge. The regression results are reported in Table 3.3.

Due to the non-linearity of the model, the estimated coefficients cannot be interpreted as marginal effects. However, since the underlying models are a logit and a negative binomial model, signs and significance can be directly interpreted. Looking at the differences between publicly and privately insured over time (i.e., looking at the coefficients of $SHI \times T_{93-97}$, $SHI \times T_{98-99}$, and $SHI \times T_{00-03}$), we find that the basic effects do not differ strongly between males and females. The coefficients in the first stage are always insignificant for both men and women. The effect of the introduction of a fixed budget in 1993 is also insignificant in the second stage, thus confirming the expected result of no effects of this reform on the number of doctor visits. However, there are highly significant negative coefficients of the 1997 reform in the second stage. The period of increased tightness of the national budget (after 2000) is again associated with a negative coefficient for the publicly insured which, however, is not statistically different from the coefficients of the 1997 reform. Therefore, these results provide some evidence that the effect of the 1997 reform was not only a short-term one.

To get a better idea of the effects, we use the regression results and look at the time trends of the number of doctor visits when compositional differences and macroeconomic conditions are controlled for. Figures 3.3 and 3.4 show predicted values for the first and the second stages, holding all characteristics fixed at the sample averages and varying only the time and the insurance group indicators. Furthermore, the predicted values are averaged over the four latent types, weighted by their respective probabilities. That is, we compare two hypothetical average individuals that only differ in their insurance status (and not, for instance, in their health status). No real variation over time can be found in the first stage (Figures 3a and

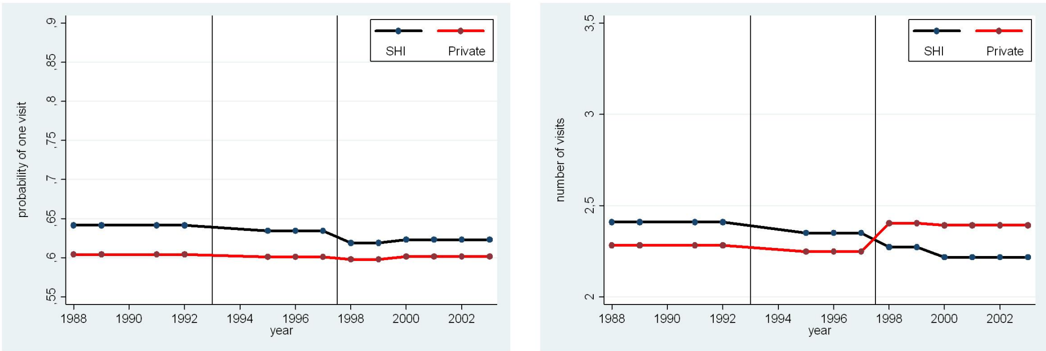
Table 3.3: Estimated coefficients of the finite mixture hurdle model

	Males				Females			
	Stage 1		Stage 2		Stage 1		Stage 2	
$SHI \times T_{93-97}$	-0.022	(0.10)	-0.015	(0.06)	-0.031	(0.13)	-0.057	(0.06)
$SHI \times T_{98-99}$	-0.085	(0.10)	-0.192*	(0.06)	-0.043	(0.14)	-0.158*	(0.06)
$SHI \times T_{00-03}$	-0.082	(0.08)	-0.229*	(0.05)	0.118	(0.11)	-0.175*	(0.05)
T_{93-97}	-0.015	(0.10)	-0.028	(0.06)	0.103	(0.13)	0.043	(0.06)
T_{98-99}	-0.032	(0.10)	0.091	(0.06)	0.033	(0.14)	0.095	(0.06)
T_{00-03}	-0.014	(0.08)	0.082	(0.05)	-0.164	(0.11)	0.071	(0.05)
SHI	0.193*	(0.07)	0.095*	(0.04)	0.067	(0.10)	0.113*	(0.04)
Health Satisfaction	-0.269*	(0.01)	-0.166*	(0.00)	-0.252*	(0.01)	-0.143*	(0.00)
Disabled	0.855*	(0.04)	0.302*	(0.02)	0.787*	(0.06)	0.290*	(0.01)
Hospital visit previous year	0.049*	(0.01)	0.010*	(0.00)	0.038*	(0.01)	0.012*	(0.00)
Sport	0.094*	(0.01)	0.006	(0.01)	0.076*	(0.01)	0.003	(0.00)
Unemployment rate	-0.004	(0.01)	0.008	(0.01)	-0.025*	(0.01)	0.002	(0.00)
Absent $>$ 3 days	0.847*	(0.02)	0.329*	(0.01)	0.745*	(0.03)	0.300*	(0.01)
Age	-0.044*	(0.01)	0.009*	(0.00)	-0.048*	(0.01)	0.005	(0.00)
Age Squared	0.001*	(0.00)	-0.000	(0.00)	0.001*	(0.00)	-0.000	(0.00)
Married	0.209*	(0.03)	0.018	(0.02)	0.173*	(0.03)	0.061*	(0.01)
Children under 16	-0.151*	(0.03)	-0.060*	(0.02)	-0.083*	(0.03)	-0.054*	(0.01)
Small town	-0.067*	(0.03)	-0.038*	(0.02)	-0.038	(0.03)	-0.037*	(0.01)
Large town	0.011	(0.03)	0.061*	(0.02)	0.060	(0.03)	0.032*	(0.01)
Years of schooling	0.035*	(0.01)	-0.008*	(0.00)	0.042*	(0.01)	0.007*	(0.00)
Full-time employed	-0.159*	(0.06)	-0.081*	(0.04)	-0.304*	(0.05)	-0.153*	(0.02)
Part-time employed	-0.036	(0.10)	0.058	(0.05)	-0.246*	(0.05)	-0.121*	(0.02)
Unemployed	-0.304*	(0.05)	0.021	(0.02)	-0.205*	(0.04)	-0.008	(0.02)
Blue collar worker	-0.380*	(0.06)	-0.151*	(0.04)	-0.267*	(0.05)	-0.105*	(0.02)
White collar worker	-0.262*	(0.06)	-0.189*	(0.04)	-0.023	(0.05)	-0.121*	(0.02)
Self-employed	-0.513*	(0.07)	-0.259*	(0.04)	-0.322*	(0.07)	-0.157*	(0.03)
Health job	-0.135	(0.11)	-0.143	(0.08)	-0.177*	(0.05)	-0.037	(0.03)
Civil	-0.055	(0.08)	-0.090	(0.05)	0.134	(0.10)	-0.087*	(0.04)
Net-household inc./1000	0.011	(0.01)	-0.004	(0.00)	-0.003	(0.01)	-0.014*	(0.00)
West Germany	-0.163	(0.09)	0.215*	(0.05)	-0.358*	(0.09)	0.101*	(0.04)
Inverse Mills ratio	-0.670	(0.36)	0.684*	(0.20)	-1.198*	(0.43)	0.650*	(0.17)
α_1			5.062*	(0.672)			0.827*	(0.023)
α_2			0.418*	(0.015)			0.091*	(0.012)
α_3			0.213*	(0.012)			0.285*	(0.010)
α_4			10.208*	(0.462)			13.002*	(0.520)
θ^1	0.523*		(0.092)		1.069*		(0.019)	
θ^2	0.990*		(0.022)		-0.047*		(0.019)	
θ^3	0.246*		(0.020)		0.531*		(0.016)	
θ^4	- 1.366*		(0.018)		1.131*		(0.016)	
$P(\theta^1, \alpha_1)$	0.095*		(0.01)		0.145*		(0.01)	
$P(\theta^2, \alpha_2)$	0.227*		(0.01)		0.267*		(0.01)	
$P(\theta^3, \alpha_3)$	0.386*		(0.01)		0.367*		(0.01)	
$P(\theta^4, \alpha_4)$	0.291*		(0.01)		0.221*		(0.01)	
Log-pseudolikelihood	-147254.915				-182720.336			
Akaike	294657.830				365588.672			
Observations	82,621				88,575			

$*$ indicates significance at the 5% level

4a) for males or for females.[9] Conditional on observed characteristics like the health status, the publicly insured have a slightly higher likelihood of one visit throughout the entire observation period. This might reflect incentive effects of the insured due to the absence of co-payments in the public system.

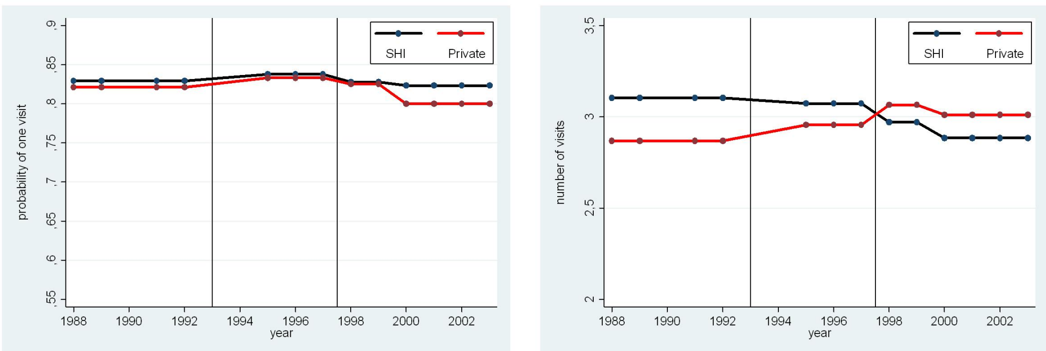
Figure 3.3: Predicted values males



(a) 1st stage (b) 2nd stage

Predicted values for average individuals in the sample, based on regression results in Table 3.3.

Figure 3.4: Predicted values females



(a) 1st stage (b) 2nd stage

Predicted values for average individuals in the sample, based on regression results in Table 3.3.

However, there are remarkable evolutions in the second stage. Note that Figures 3b and 4b do not show the clear decreasing time trend that was found in the unconditional numbers in Figures 1 and 2. Until 1997, the publicly insured had more conditional doctor visits than comparable privately insured. Not only did their num-

[9]Note that the slight drop of the SHI-group after 1997 in Figure 3a is not significant.

ber of visits decrease after 1997, the number of visits in the privately insured group also increased. Since there is no reason to assume that there was an unobserved shock that affected only the privately insured and shifted their number of visits to a higher level, this evolution seems to be a result of a change in the patient mix by the physicians. This important identifying assumption is also supported by the evolution after 1997. While before the reform the difference between the privately and the publicly insured is stable (more visits of the publicly insured in the second stage), it is also stable afterwards (more visits of the privately insured). The change after 1997 is a long-term change which is unlikely to result from time-varying unobserved effects. Looking at these figures, the assumption of no general equilibrium effects becomes very unlikely to hold true.

In a series of articles, Winkelmann (2004a, 2004b, 2006) also uses the SOEP to estimate the effect of increased co-payments for prescription drugs on the number of doctor visits. In July 1997, the co-payments for prescription drugs were increased for the publicly insured by 6 DM (about 3 Euro) which, depending on the package size was an increase of about 86% (from 7 DM to 13 DM for large sizes) to 200% (from 3 DM to 9 DM for small sizes). The privately insured were not affected by the reform. Winkelmann uses them as a control group and finds a reduction in the expected number of doctor visits by about 10% between 1995 and 1999. In order to make our results comparable to the ones of Winkelmann, we use the results of our regression and calculate a "treatment effect", the derivation of which is explained in the appendix.[10]

The first column of Table 3.4 reports the average treatment effect on the treated. Although it is only unbiased if one is willing to assume that there are no general

[10] We are aware of the problem with this terminology. Although it is not clear, what this quantity really measures in this case, we call this a "treatment effect". We do this to keep things simple because our specification is similar to a difference-in-differences estimation.

equilibrium effects, we report it as a benchmark to compare it to Winkelmann's results. Signs, significance, and relative sizes of the coefficients to each other do not differ to the results in Table 3.3. However, here, the sizes can be directly quantified. The estimated treatment effect for the reform of 1993 is zero. The one for the reform of 1997, however, is strong and significant. The values of about 0.22 for males and 0.32 for females mean an approximate drop of 10% in the number of doctor visits due to the reform. Thus, assuming that the privately insured are a proper control group, we can replicate Winkelmann's finding. The question is, whether this effect results from the remuneration reform or the co-payment reform.

Our hurdle model specification allows us not only to calculate the overall effect but to decompose it into parts that are due to changes in the first stage and those due to changes in the second stage. The third and the fifth columns in Table 3.4 report these effects. The effects in the first stage are very small and insignificant for all three reforms. Those in the second stage for the reform of 1997 and the time thereafter, however, are quite large and statistically significant. Thus, the negative overall treatment effect is exclusively the result of a negative effect in the second stage. The motivation of the hurdle model as a model with two different decision-making processes – the patient has full control in the first stage but the physician takes over in the second stage – supports the interpretation that the reform did not affect the patient's but only the physician's behaviour.

Winkelmann argues that the co-payment reform might have lowered the probability of doctor visits, because individuals either do not go to the doctor anymore because they fear getting a prescription, or that they have less visits than before, because they demand prescriptions for larger package sizes. The first argument seems disputable since this is a very indirect effect and it is hard to imagine that this would reduce the total number of doctor visits by such a great amount. While the co-payment increase

Table 3.4: Estimated Treatment Effects

		Overall	s.e.	1st stage	s.e.	2nd stage	s.e.
Males	Reform 1993	-0.024	(0.071)	-0.004	(0.019)	-0.021	(0.082)
	Reform 1997	-0.221*	(0.084)	-0.016	(0.020)	-0.270*	(0.098)
	After 2000	-0.249*	(0.067)	-0.016	(0.015)	-0.313*	(0.078)
Females	Reform 1993	-0.123	(0.120)	-0.004	(0.015)	-0.129	(0.128)
	Reform 1997	-0.322*	(0.141)	-0.005	(0.017)	-0.355*	(0.150)
	After 2000	-0.284*	(0.108)	0.015	(0.014)	-0.381*	(0.114)

Treatment effects calculated according to equation (2) in the appendix based on regression results in Table 3.3. All control variables (except for the interesting reform dummies) set to represent the average individual in the data set. $*$ indicates significance at the 5% level

is high in relative terms, it is rather low (about 3 Euro) in absolute terms. Other studies show that even more direct and somewhat stronger demand side incentives have much smaller effects. In an evaluation of the introduction of a 10 Euro co-payment for doctor visits, Farbmacher (2009) finds a reduction in the probability of one doctor visit (i.e., in the first stage) of up to 3.4% while there are no effects in the positive part of the distribution (the second stage). Moreover, apart from the co-payments for doctor visits, also in 2004, there was another increase in co-payments for prescription drugs. By attributing the entire reduction to the co-payments for doctor visits, Farbmacher (2009) implicitly finds no effects of increased co-payments for prescription drugs here. The second of Winkelmann's arguments (patients demand higher package sizes to reduce the number of doctor visits) is more likely to be true. However, because co-payments for large package sizes are still about 50% higher than for small sizes, the incentives to switch from smaller to larger package sizes might not be very strong. All in all, the incentives imposed by the prescription-drug reform are quite low and such a strong reaction – a 10% drop in the number of doctor visits – seems to be surprising.

More importantly, the increased co-payments should lead to a demand-side effect only. Turning back to Table 3.4 and Figures 3 and 4, the results for the first stage are practically zero. The decreasing expected number of doctor visits only result from the changes in the second stage.[11] Even though it is possible that the patient also has some level of control in the second stage apart from the physician, the findings from the first stage indicate that it is unlikely that the second-stage results are driven by demand-side effects.

Having said this, looking back at the evolution in Figures 3 and 4, we directly see that the estimated "treatment effect" of a decrease in the number of doctor visits by 10% is upward biased. This strong result does not only follow from a drop in the visits of the publicly insured but to a great deal also from an increase in the visits of the "control group". Therefore, no difference-in-differences analysis is possible here when the effect of reforms on the *absolute* number of doctor visits is to be evaluated. However, we can say that physicians changed their patient-mix. While until 1997, the publicly insured had more conditional doctor visits than the comparable privately insured, this picture turned around immediately after the reform took effect.

Other Covariates

Not surprisingly, given the high number of observations, most of the other control

[11] Winkelmann (2004b) also uses a two-part model to distinguish the effects on the first and the second stage. Contrary to our study, he finds significant effects in both stages, and even a bigger one in the first stage. His study, however, does not use a control group but is just a before-after comparison. Without a control group, however, one has to impose the rather strict assumption of absence of exogenous temporary shocks that affect the number of doctor visits. Turning back to Figure 2a, we can see a drop in the probability of one doctor visit between 1996 and 1998. This, however, applies to both statutorily and privately insured. Winkelmann (2004a) uses a control group, but does not present results of the hurdle model, hence one cannot separate a demand from a supply-side effect. Winkelmann (2006) uses the method of quantile regressions for count data and the years 1996 and 1998 only. Using a control group here, he finds that the reform effect was larger in the lower part of the distribution (the "low-users") than in the upper part (the "high-users"). This strategy allows to analyse different behavioural responses of heterogenous individuals. However, arguably, the two-part model might be better suited to separate demand from supply-side effects than a quantile regression model.

variables are highly significant (Table 3.3). Here, we only discuss the most interesting ones. The variables that capture the observed health status (health satisfaction, hospital stays in the last year, at least three days of absence, handicap) are all highly significant in both stages and have the expected signs. Individuals who frequently do sports have a higher probability of visiting a doctor. Conditional on the health status this can be interpreted as a higher concern for own health of these individuals. This interpretation is supported by the finding that *sport* is not significant in the second stage. West Germans have a lower probability of visiting a doctor than East Germans. This remarkable difference can be interpreted by preventive doctor visits that used to have a much higher importance in the former GDR and still have in East Germany. However, the conditional number of visits is much higher in West Germany.[12] Individuals who live in large towns have more doctors visits. This might be explained by better access to services in larger towns (should affect the first stage) but also increased competition between doctors in urban regions, resulting in a higher degree of physician-induced demand here (second stage). The unemployment rate also shows the expected sign, although it is only significant for females. A higher unemployment rate is associated with a lower number of doctor visits (this effect is partly also captured by the absenteeism indicator which, however, also captures the individual health status). Note, however, that it only affects the first stage, thus being in line with the interpretation of individuals deciding not to see a doctor in times of recessions.

[12]This explains the drop in doctor visits after 1991 in Figures 1 and 2b. This is a compositional effect because before 1992 there are only West Germans in the sample. Note the absence of this drop in Figures 3 and 4 after conditioning on the region.

3.6 Robustness Checks

3.6.1 Subsamples and Specifications

The identification of the reform effect on the patient-mix rests on some assumptions. The first one is that the trends in doctor visiting behaviour would have stayed stable without the reform in 1997. This cannot be tested directly but a test used by Galiani et al. (2005) could give a notion of how credible this assumption is. Basically, this test consists of using only the pre-reform years, running placebo-reforms and testing whether one finds differences between control and treatment groups before. Since we already included an interaction-dummy for the period of 1993 to 1997 in the regression ($SHI \times T_{93-97}$) which is not significant, we directly infer that the assumption of parallel trends is supported.

As discussed earlier, switching insurance from public to private or vice versa is not possible for the majority of individuals. Moreover, switching to private insurance as a reaction to the reforms seems unlikely because it is especially the sicker individuals who would be punished by paying a higher insurance premium in the private sector. Nevertheless, as a second robustness check, to completely rule out endogeneity problems of the treatment, we dropped all individuals from the sample who switched at least once their insurance status after 1996. The results for these two subsamples are reported in Table 3.5 in Columns 1 and 2. Neither in the first nor in the second stage do we see important differences in the estimated effects.

We experimented with several other specifications and subsamples to check the robustness of the results. As pointed out in the text, apart from a baseline model without a random effect, we also estimated models with two and three mass points. The model with four mass points is preferred for statistical reasons but does not lead to results different to those of the other models. As shown by Bertrand et al. (2004),

Table 3.5: Non-changers

	Males		Females	
	Stage 1	Stage 2	Stage 1	Stage 2
$SHI \times T_{93-97}$	-0.010	-0.030	0.022	-0.004
	(0.11)	(0.07)	(0.15)	(0.07)
$SHI \times T_{98-99}$	-0.055	-0.283*	0.095	-0.167*
	(0.12)	(0.07)	(0.17)	(0.08)
$SHI \times T_{00-03}$	-0.046	-0.276*	0.084	-0.201*
	(0.09)	(0.05)	(0.13)	(0.06)
T_{93-97}	-0.045	-0.015	0.034	-0.012
	(0.11)	(0.07)	(0.15)	(0.07)
T_{98-99}	-0.065	0.182*	-0.117	0.107
	(0.12)	(0.07)	(0.18)	(0.08)
T_{00-03}	-0.056	0.132*	-0.141	0.098
	(0.09)	(0.05)	(0.13)	(0.06)
SHI	0.161	0.113*	0.012	0.111*
	(0.08)	(0.05)	(0.12)	(0.05)
Observations	74,942		82,728	

Asterisk indicates significance at the 5% level. Coefficients of all other covariates not shown here.

difference-in-differences estimations tend to underestimate the standard errors of the treatment effect as the included number of years after the treatment increases. This does not seem to be a problem here since for the most interesting treatment effect (the reform of 1997), only two post-reform years are used and moreover, the estimated z-statistics are rather high. Nevertheless, we re-estimated the same model using only the years 1996 (pre-reform) and 1998 (post-reform).[13] The results do not change qualitatively (i.e., no significant effect in stage 1 but a significant reduction in stage 2).

3.6.2 Test for Single Spell Assumption

As mentioned above, the analysis using the hurdle model rests on the assumption that the observed number of doctor visits in a quarter results from one sickness spell

[13]Estimation results of the robustness checks are not presented here but available upon request.

only. This "single spell assumption" can be verified by a test which was derived by Santos Silva and Windmeijer (2001). We only briefly outline the procedure here and refer to Santos Silva and Windmeijer (2001) for a more detailed derivation and description. The notation here also follows Santos Silva and Windmeijer (2001). Let V be the number of doctor visits in the previous three months. If S denotes the number of illness spells and R_j the number of recalls in spell j, V can be expressed as $V = \sum_{j=1}^{S} R_j = S + \sum_{j=1}^{S} R_j^*$, where $R_j^* = R_j - 1$. Assuming that S and R_j are conditionally independent and with $E(R_j|x,\gamma) = E(R|x,\gamma), j = 1,\ldots,S$, one can show that

$$E(V|x,\beta,\gamma) = E(S|x,\beta)E(R|x,\gamma).$$

We only observe V but want to identify the parameters β and γ of $E(S|x,\beta)$ and $E(R|x,\gamma)$. This is only possible if S does not exceed 1, that is, if $S = d = min\{V,1\}$. If this is true, the following moment condition holds:

$$E[d - E(S|,x,\beta)] = 0. \tag{1}$$

This condition can be used to estimate β. Likewise, since for the positive counts

$$E(V|V>0, S \leq 1, x,\gamma) = E(R|x,\gamma),$$

the following moment condition can be used to estimate γ:

$$E\{[V - E(R|x,\gamma)]|V>0\} = 0. \tag{2}$$

The idea of the test for the single spell hypothesis is to estimate β and γ by GMM using the moment conditions (1) and (2) and specification of the first moments and then to test whether the following expression significantly differs from zero when the estimated parameters are plugged in, i.e., to test if the following holds:

$$E[m(V, x, \beta, \gamma)] = E[V - E(S|x, \beta)E(R|x, \gamma)] = 0.$$

This type of conditional moments test is described in, e.g., Cameron and Trivedi (2005). Under the null hypothesis of $E[m(V, x, \beta, \gamma)] = 0$, the test statistic M follows a chi-square distribution with h degrees of freedom where h is the number of moment conditions, i.e., the number of included variables in x:

$$M = \hat{m}(\hat{\beta}, \hat{\gamma})'[\hat{V}\{\hat{m}(\hat{\beta}, \hat{\gamma})\}]^{-1}\hat{m}(\hat{\beta}, \hat{\gamma}) \sim \chi^2(h)$$

Here, in order to stay consistent with the negative binomial model in section 4.1, we specify

$$E(S|x, \beta) = 1 - exp(-exp(x'\beta))$$

and

$$E(R|x, \gamma) = \frac{exp(x'\gamma)}{1 - exp(-exp(x'\gamma))}.$$

We bootstrap the estimate of the variance-covariance matrix of m with 500 replications. A test statistic above the critical value leads to a rejection of the null. Consequently, failure to reject the null hypothesis would then be an evidence in favour of the single spell assumption. Since the test by Santos Silva and Windmeijer (2001) is derived for cross-sections, we carry out separate tests for every year. We include the same covariates as in the regression analysis but have to leave out all those which do not vary between individuals within a given year (essentially, these are the time and treatment effect dummies).

Table 3.6 reports the results of the single spell test. The test statistic is below the critical value of the chi-square distribution with a significance level of 5% for all cases. Thus, we cannot reject the single spell hypothesis.

Table 3.6: Test for single spell assumption

Year	Test statistic		Critical value
	Males	Females	
1988	24.909	12.899	36.415
1989	21.562	28.765	36.415
1991	24.737	28.796	36.415
1992	29.391	36.639	37.652
1995	25.026	32.811	37.652
1996	33.872	22.739	37.652
1997	27.474	30.590	37.652
1998	25.635	36.362	37.652
1999	36.069	23.129	37.652
2000	15.322	34.767	37.652
2001	27.273	33.080	37.652
2002	30.449	29.758	37.652
2003	27.084	31.638	37.652

Critical values are of chi-square distribution with 24 or 25 degrees of freedom and 5% significance level. 25 degrees of freedom after 1991 because a dummy for West Germany is included only thereafter.

To our knowledge, we are the first to apply this test apart from Santos Silva and Windmeijer (2001) who used it with data from 1985 of the SOEP.[14] Therefore, no evidence from other data sets is available with which to compare this result. However, we think that the short period of three months within which the number of visits to the doctor is measured leads to this finding. While in most other household panel data sets, the period is one year, three months seem to be short enough to justify the single spell assumption. Based on this result, we argue that differentiation between first and second stage, which is essential for our identification strategy, is possible given the data at hand.

[14]The authors reject the assumption for specialist visits but fail to reject it for GPs.

3.7 Conclusion

Two major reforms affected the supply-side of ambulatory care in Germany in the last 15 years, namely the introduction of a fixed budget in 1993 and of individual practice budgets for physicians in 1997. With data from the German Socio-Economic Panel that cover the period 1988-2003, we find no effect of the introduction of a fixed budget but a strong effect of the individual practice budgets on the number of doctor visits of both publicly and privately insured.

The results show that the behaviour of patients has not changed due to the reforms since the likelihood of one visit to the doctor within a period of three months remained stable. However, the number of recalls changed gradually. While until 1997, publicly insured patients had more doctor visits than comparable privately insured individuals, given they had seen a doctor at least once, this picture turned around after the second reform became effective. After the reform, privately insured patients have more visits when characteristics like the health status are controlled for. The results hold for both males and females and are robust to several subsamples.

The results are in line with a general notion in the German public of the privately insured getting preferential treatment from physicians at the cost of the publicly insured (see Lungen et al. (2008) for an experimental study on the access to physicians of the privately and publicly insured). This study gives evidence on how the remuneration system can directly affect physician behaviour. Since we control for patient characteristics like the health status, education or income, the strong variation in the number of recalls displays the physicians' influence in controlling the demand for treatments. Therefore, this finding is also a hint at the existence of supplier-induced demand. Since we did not evaluate health changes due to the reforms, we cannot say if physicians reduced excess and supposedly trivial treatments in the the group of publicly insured after 1997 or if they also reduced the number of necessary treatments. The increased number of visits among privately insured which cannot be attributed to a shock like a sudden drop of health status, however, points to an increased number of unnecessary treatments in this group. A full evaluation of the health effects of these reforms is left for future research.

Previous German studies only find weak or no effects of demand-side incentives on cost-saving behaviour of the insured (see, e.g., Pohlmeier and Ulrich, 1995, Riphahn et al., 2003, Augurzky et al., 2006, Schreyögg and Grabka, 2010, Farbmacher, 2009; however also see Felder and Werblow, 2008). The findings of the current study suggest that doctors react strongly to incentives. Therefore, in general, supply-side regulations might be a much better instrument to manage health care expenditures than adjusting incentives on the demand side in Germany. Using the physician remuneration system to reduce the inefficiency implied by the information asymmetry between doctor and patient seems to be a good starting point in order to maintain universal access to health care at a reasonable cost.

3.8 Appendix

Derivation of the Treatment Effect with Nonlinear Difference-in-Differences

Let Y be the observed outcome variable (number of doctor visits), G an indicator of the treatment group, and T a dummy variable for the post-treatment time.[15] Then the treatment effect on the treated is defined as

$$\tau(T=1, G=1, X) = E[Y^1|T=1, G=1, X] - E[Y^0|T=1, G=1, X]$$

where Y^1 is the observed outcome and Y^0 is the potential outcome in the absence of the treatment (the contrafactual situation). With a control group and the identifying assumption that the differences between treatment and control group would have been stable over time, this expression reduces to the parameter of the interaction term $T \times G$ in a linear regression of Y on T, G, $T \times G$, and other control variables. This, however, is not possible in a nonlinear model. Let the conditional expectation of Y depend on a possibly nonlinear function g

$$E[Y|T, G, X] = g(\alpha T + \beta G + \gamma TG + X\theta)$$

Then the treatment effect is given as (see Puhani, 2008)

$$\tau(T=1, G=1, X) = g(\alpha + \beta + \gamma + X\theta) - g(\alpha + \beta + X\theta)$$

The expectation in the hurdle specification is given by the probability of a positive count (1st stage; call this function ψ_1) times the expectation conditional on positive

[15] The notation and the general notion of nonlinear difference-in-differences closely follow Puhani (2008).

counts (2nd stage; call this function ψ_2)[16]:

$$
\begin{aligned}
E[Y|T,G,X] &= P(Y>0|T,G,X) * E[Y|Y>0,T,G,X] \\
&= \psi_1(\alpha_1 T + \beta_1 G + \gamma_1 TG + X\theta_1) * \psi_2(\alpha_2 T + \beta_2 G + \gamma_2 TG + X\theta_2)
\end{aligned}
\tag{3.1}
$$

Then, the treatment effect on the treated, together with the hurdle specification, is given by

$$
\begin{aligned}
\tau(T=1,G=1,X) = {} & \psi_1(\alpha_1+\beta_1+\gamma_1+X\theta_1) * \psi_2(\alpha_2+\beta_2+\gamma_2+X\theta_2) \\
& - \psi_1(\alpha_1+\beta_1+X\theta_1) * \psi_2(\alpha_2+\beta_2+X\theta_2)
\end{aligned}
\tag{3.2}
$$

Unlike in the linear case, the treatment effect is not constant over all individuals. Here, it is calculated according to (2) where all the control variables (except for the interesting reform dummies) are set to represent the average individual in the dataset.

[16]Note that both functions in the hurdle model are strictly monotonic transformations of the linear index and, thus, also the product of the two.

Table A3.1: Probability of staying in the sample

	Coefficient	Std. Error	Coefficient	Std. Error
$SHI \times T_{93-97}$	-0.106	(0.067)	-0.116	(0.073)
$SHI \times T_{98-99}$	0.057	(0.069)	-0.297*	(0.089)
$SHI \times T_{00-03}$	-0.011	(0.050)	-0.281*	(0.058)
T_{93-97}	0.188*	(0.065)	0.163*	(0.072)
T_{98-99}	0.108	(0.067)	0.457*	(0.088)
T_{00-03}	0.169*	(0.049)	0.432*	(0.057)
SHI	0.022	(0.046)	0.359*	(0.050)
Health Satisfaction	0.016*	(0.004)	0.021*	(0.003)
Disabled	0.009	(0.021)	0.074*	(0.024)
Hospital Stays prev. year	-0.004*	(0.002)	-0.006*	(0.002)
Sport	0.010	(0.006)	0.005	(0.006)
Unemployment rate	0.021*	(0.006)	0.022*	(0.006)
Absent > 3 days	0.046*	(0.015)	0.046*	(0.017)
Age	0.040*	(0.003)	0.043*	(0.002)
Age Squared	-0.000*	(0.000)	-0.000*	(0.000)
Married	0.115*	(0.018)	0.055*	(0.016)
Children under 16	-0.012	(0.016)	0.002	(0.016)
Small town	-0.004	(0.017)	0.010	(0.016)
Large town	-0.011	(0.017)	-0.021	(0.017)
Years of schooling	0.010*	(0.003)	0.012*	(0.003)
Full-time employed	-0.103*	(0.041)	-0.044	(0.035)
Part-time employed	-0.131*	(0.065)	-0.020	(0.037)
Unemployed	-0.067*	(0.029)	-0.033	(0.028)
Blue collar worker	0.107*	(0.041)	0.033	(0.036)
White collar worker	0.117*	(0.043)	0.025	(0.035)
Self-employed	-0.031	(0.046)	-0.065	(0.046)
Health job	-0.063	(0.080)	0.108*	(0.038)
Civil servant	-0.055	(0.055)	0.075	(0.061)
Net-household inc./1000	0.009	(0.005)	-0.004	(0.004)
West Germany	0.092	(0.057)	0.110*	(0.055)
Life Satisfaction	0.033*	(0.004)	0.027*	(0.004)
Oral Interview	0.132*	(0.013)	0.129*	(0.013)
Long Interview	-0.103*	(0.013)	-0.093*	(0.013)
Constant	-0.507*	(0.129)	-0.852*	(0.127)
Log-pseudolikelihood	-23727.056		-23994.092	
Observations	82,621		88,575	

Star indicates significance at the 5% level; Standard errors in parentheses

Table A3.2: Variable description and sample means

Variable	Mean Males	Mean Females	Description
SHI	0.88	0.93	Dummy for statutory health insurance
Health Satisfaction	6.78	6.58	Self-assessed health satisfaction betw. 0 (very bad) and 10 (very good)
Hospital Stays prev. Year	0.33	0.37	Number of hospital stays in the previous year
Disabled	0.13	0.09	Dummy for disability
Sport	2.16	2.00	Doing sports betw. 1 (almost never) and 4 (at least once a week)
Unemployment Rate	10.85	10.87	Average national unemployment rate per year
Absent $>$ 3 days	0.32	0.25	Dummy for being absent for more than 3 days
Age	45.06	46.30	Age
Married	0.67	0.63	Dummy for being married
Children under 16	0.34	0.35	Number of children under 16
Small town	0.42	0.41	Dummy for living in a town with less than 20.000 inhabitants
Large town	0.32	0.33	Dummy for living in a town with more than 100.000 inhabitants
West Germany	0.77	0.77	Dummy for West Germany
Years of schooling	11.71	11.26	Years of Schooling
Full-time employed	0.63	0.28	Full-time employed
Part-time employed	0.01	0.15	Part-time employed
Unemployed	0.07	0.07	Unemployed
Blue collar worker	0.31	0.12	Blue collar worker
White collar worker	0.22	0.29	White collar worker
Self-employed	0.07	0.03	Self-employed
Health job	0.01	0.03	Health job
Civil Servant	0.05	0.02	Civil Servant
Net-household inc./1000	2.55	2.38	Net-household inc./1000
Life Satisfaction	7.00	7.00	Self-assessed life satisfaction between 0 (very bad) and 10 (very good)
Oral Interview	0.45	0.48	Oral Interview
Long Interview	0.52	0.50	Long Interview
Number of observations	82,621	88,575	

Chapter 4

Risk aversion and advantageous selection in the German supplementary health insurance

4.1 Introduction

Standard insurance models with asymmetric information like the Rothschild-Stiglitz-model (1976) predict a positive correlation between insurance cover and the occurrence of the insured risk conditional on the information of the insurance provider. That is, individuals who are bad risks choose insurance from a set of offered contracts that has a higher coverage than do good risks. Private individual information on the true risk type prevents insurance companies from perfectly calculating proper insurance premia for all risk types which might drive the good risks out of the market, that is, there is adverse selection in the insurance market. An empirical test on the positive correlation between insurance cover and the occurrence of the risk (the "positive correlation test") conditional on all the information that is observed by the insurance company can be seen as one possible test on the presence of asymmetric information in an insurance market (Chiappori and Salanie, 2001). Rejecting the hypothesis of no asymmetric information in favour of a positive correlation is then evidence for either moral hazard, adverse selection, or both together which are usually difficult to separate.

However, many empirical applications find no evidence for adverse selection in different insurance markets like markets for long-term care, Medigap insurance or life-insurance (see Cutler et al., 2008, for an overview). In some markets, there can even be found a negative correlation between insurance cover and experience of risks. One explanation for this finding is that individuals do not only have private information on their risk type (possibly leading to adverse selection) but that also preferences like risk aversion shape their demand for insurance and the probability experiencing the insured risk. A more risk-averse individual might demand more insurance and at the same time try to minimise the probability of occurrence of the risk. Risk aversion is also unobserved by the insurer but in this case this information asymmetry does not lead to adverse selection, it is rather a source of the opposite, namely advantageous selection. Because the private information about the risk type and risk preferences is not one-dimensional but multi-dimensional, potential sources of adverse and advantageous selection may net out and the overall effect is not clear a priori.

Therefore, the positive correlation test as a test on asymmetric information is invalid (Finkelstein and Poterba, 2006). Failure to reject the hypothesis of no asymmetric information can arise if there are sources of adverse and advantageous selection that partly cancel each other out. Finkelstein and Poterba (2006) thus propose another test on information asymmetry, the "unused observables test". The existence of only one variable that is not used by the insurer to calculate the risk classification of the insured, but which is correlated with both the insurance choice and the risk of the insured loss (conditional on the observed variables) is evidence for asymmetric information. They find for the UK that the place of residence is correlated with both purchasing annuities and the annuitant's mortality but not being used for insurer's risk calculation, thus leading to adverse selection.

In this study, we analyse the existence and direction of information asymmetries in the German market for private supplementary insurance for hospital stays. In Germany, about 90 per cent of the population are covered by public insurance that generally pays for all the expenditures for hospital visits (except for a small amount of co-payments). However, additional to this basic insurance, individuals can buy private supplementary insurance that enables a better treatment during the hospital stay, namely a double room and treatment by a chief physician. When buying this private supplementary insurance, individuals have to give a detailed statement about their age, sex, occupation and current health status. Hence, all these variables are known by the insurance companies and can thus be used to calculate the monthly insurance premium. However, several important variables are unobserved by the insurer. Not only might the true health status still be unknown (possibly being a source of adverse selection), but so is also the degree of risk aversion (possibly being a source of advantageous selection). Risk aversion is usually not observed by the econometrician either, but the individual health behaviour is often used as a proxy.

The contribution of this study is twofold. First, in this study, we do not rely on proxy variables for risk tolerance but use a measure of risk aversion concerning health matters that is directly obtained by the survey instrument. This measure is the self-stated degree of risk aversion concerning one's own health. Similar measures have recently become increasingly popular, see, e.g., Dohmen et al. (2010a), Dohmen et al. (2010b), or Jaeger et al. (2010). As Dohmen et al. (2010b) show, the individual degree of risk aversion may differ depending on the context. Although they are highly correlated, individuals assess their risk aversion with respect to, say, car driving differently to the one concerning financial matters. In particular, the stated risk aversion concerning health predicts actual health behaviour more accurately than risk aversion concerning other domains. Thus, in a health context, the risk aversion concerning health is strongly preferable over risk aversion measured by, e.g., standard hypothetical financial lotteries which are - if at all - usually used as direct measures of risk aversion (Dohmen et al., 2010b).

Second, it is the first study to analyse the degree of information asymmetries in the German market for private supplementary insurance. As the bulk of evidence about adverse selection in health insurance uses data from the US - with a healthcare system that, according to Buchmueller et al. (2008), is an outlier in the industrialised world - the present paper provides evidence about a type of health insurance that is common in many other industrialised countries.

Our results show that for women, conditional on observed variables, there is a positive correlation of holding insurance and having a hospital stay, which indicates the presence of adverse selection. However, the degree of adverse selection is rather small. For males, we do not find a significant positive correlation. Moreover, we find that risk averse males have a higher likelihood to buy supplementary health insurance while they have, on average, less hospital visits within a period of six years after buying the insurance. Thus, risk aversion is a source of advantageous selection that potentially nets out other sources of adverse selection and possibly leads to the result of no overall adverse selection effect.

This paper is organised as follows. The next section contains a theoretical discussion and previous empirical results. Section 3 gives a brief overview of the German health insurance system with its private supplementary insurance. The data are presented in Section 4. Section 5 explains estimation and testing methods, Section 6 reports the results, while Section 7 concludes.

4.2 Previous theoretical and empirical literature

One standard insurance model incorporating asymmetric information was developed by Rothschild and Stiglitz (1976). In their model individuals have private information about their risk type, i.e., their propensity to suffer from the loss they seek to insure against. Individuals can only hold one insurance contract at a time and there is perfect competition among insurance providers. Rothschild and Stiglitz (1976) show that, depending on the share of good risks, there is either no equilibrium at all or an equilibrium in which the good risks buy less insurance than the bad risks. Private individual information on the true risk type prevents insurance companies from perfectly calculating proper insurance premia for all risk types which might drive the good risks out of the market, i.e., there is adverse selection in the insurance market. While Rothschild and Stiglitz (1976) assume that individuals only differ in their risk type, de Meza and Webb (2001) also allow for differences in risk preferences among the individuals. In their model, individuals do not only have private information about their risk type but also on their degree of risk aversion. Risk aversion, however, affects both the insurance coverage and the risk type. Risk-averse individuals (named the "timid" by de Meza and Webb, 2001) demand more insurance and lower their risks by preventive behaviour, whereas the "bold" care less for insurance and prevention, thus increasing their risk. While the Rothschild-Stiglitz-model predicts a positive correlation of risk type and insurance coverage, de Meza and Webb (2001) allow for equilibria that exhibit a negative correlation between risk and insurance coverage.

Cutler et al. (2008) review the empirical literature and find that whether there is more or less insurance of high-risk individuals depends on the type of insurance market. While in acute care and annuity markets high-risk individuals usually buy more insurance (as the standard model predicts; see Cutler and Zeckhauser, 2000 or Finkelstein and Poterba, 2004), the opposite is true in the case of life insurance, long-term care, and Medigap-markets (Cawley and Philipson, 1999, Finkelstein and McGarry, 2006, and Fang et al., 2008). Doiron et al. (2008) and Buchmueller et al. (2008) also find evidence for advantageous selection for private supplementary health insurance in Australia, which, however, is in contrast to the previous findings in the American market for acute health insurance.

Cutler et al. (2008) furthermore find that risky behaviour like smoking, alcohol abuse, not doing preventive care, not using a seat belt or holding a risky job is negatively correlated with holding insurance in all the mentioned markets. However, these risk tolerant individuals have higher claims for long-term care insurance and life insurance and lower claims for annuities. In contrast, the results for Medigap and acute health insurance are mixed. Fang et al. (2008) show that not only attitudes towards risks as measured by these proxies may be important but that there are also other possible variables that correlate with insurance and experience of risk. They find that cognitive ability and wealth are sources of advantageous selection in the Medigap market.

4.3 Institutional background

The insurance market we analyse in this paper is the German market for private supplementary health insurance. In Germany, about 90 per cent of all individuals are covered by public health insurance (called the "statutory health insurance"). In general, this is a full cover insurance. Except for small co-payments for doctor visits, hospital visits, and prescription drugs it pays for all health care expenditures caused by the insured. As regards hospital visits, the statutory health insurance is basic in the sense that it does not cover stays in a double room and treatments by the chief physician. However, statutorily insured can buy private supplementary insurance that bears the additional costs for this improved quality. While the general public insurance premium does not depend on the risk type but is funded by payroll taxes, the premium for the private supplementary insurance is risk-adjusted. When buying supplementary insurance, individuals have to give a detailed statement about their age, sex, occupation and health status. The basic private insurance premium depends on the age and sex of the insured individual. Health problems increase it. Finally, insurance companies can further raise the insurance premium of those applicants whose jobs are deemed risky.

There are two main advantages in analysing the market for private supplementary insurance for hospital visits in Germany. First, while in general the German health insurance system is strictly regulated, this is a market in the health care sector that exhibits competition. By law, the vast majority of the publicly insured cannot opt out of the system or personalise their degree of insurance coverage. The only free choice they have is whether or not to buy private supplementary insurance.[1] The second advantage is that the reality in Germany allows for the identification of truly adverse-selection effects without being contaminated with moral-hazard issues. Moral hazard also results from the information asymmetry between the insured and her insurance provider that leads to a positive correlation between insurance and the consumption of health care services which one could observe in the presence of adverse selection. This is because insurance coverage effectively reduces the price of such services and, *ceteris paribus*, the quantity demanded would therefore increase. If the consumption of services and the experience of risk is measured in the same unit (e.g. health care expenditures, number of doctor visits, number of hospitalisations), it is difficult to disentangle moral hazard from adverse selection when a positive correlation is found. However, as regards hospital visits, the price elasticity is found

[1]There is also competition on the supply side with currently about 50 private insurance companies being on the market, according to "Gesundheitsberichterstattung des Bundes", see http://www.gbe-bund.de.

to be very low, at least in Germany (Geil et al., 1997). After all, it is difficult to imagine a person increasing her demand for hospital visits simply because its price has been effectively lowered. We therefore assume that moral hazard does not play a role in the case of hospital visits and that a positive correlation can solely be attributed to adverse selection.[2] Note, however, that the problem of separating adverse selection from moral hazard only applies to the positive correlation test. The "unused observables test" can clearly assign problems of information asymmetry to selection effects (Finkelstein and Poterba, 2006), making the assumptions of no moral hazard in the German hospital sector less restrictive.

4.4 Data

We use data from the German Socio-Economic Panel (SOEP), which is a representative large-scale panel data set.[3] The SOEP includes information about the health insurance status (public or exclusively private) of the individual. In case of public insurance, it also asks whether or not an individual holds private supplementary insurance of several different types. One of these includes supplementary insurance for hospital visits. In 2002, 7.8% of all males and 8.7% of all females in the sample hold this type of insurance.

Since 2002, the SOEP contains detailed - although self-stated - measures of individual health. Most important are the Physical Component Summary Scale (PCS), a measure of physical health which is formed by the SF12-questionnaire (see Andersen et al., 2007, for a description), and the BMI as an objective measure of health. Since

[2] Following the same idea, we assume that there is also no ex-ante moral hazard, i.e., individuals do not change their behaviour and, thus, their risk type due to the supplementary insurance.

[3] The data used in this paper were extracted using the Add-On package PanelWhiz v2.0 (Nov 2007) for Stata. PanelWhiz was written by Dr. John P. Haisken-DeNew (john@panelwhiz.eu). The PanelWhiz generated DO file to retrieve the SOEP data used here and any Panelwhiz Plugins are available upon request. Any data or computational errors in this paper are my own. Haisken-DeNew and Hahn (2006) describe PanelWhiz in detail.

the health status at the time of signing the insurance contract is relevant for the insurance premium, we focus on these individuals who newly bought supplementary health insurance in 2002. Thus, we do not consider the complete stock of individuals who hold supplementary insurance in 2002 because we do not know their detailed health status when they signed the insurance contract - possibly decades ago. We also drop individuals above the age of 65 from the sample because only very few enter supplementary health insurance at this age. Our final estimation sample consists of 7,618 individuals out of which about 1.5% purchased supplementary health insurance in 2002; the rest remained without supplementary insurance in 2002.

We measure the occurrence of the insured risk by the number of overnight hospital visits (outpatient interventions in hospitals are therefore not included in this variable). The SOEP contains information about hospital visits in the previous twelve months. However, as a measure of risk occurrence we are not interested in past hospital stays but in the future probability of entering a hospital, after having signed the supplementary insurance contract. Because a hospital visit is usually a rare event, we count all hospital visits in the six years after the wave 2002, thus using as much information from the SOEP as currently possible. The number of overnight hospital stays ranges from 0 to 59 in the sample with a mean value of 0.82.

The SOEP includes several variables that are not observed by insurance companies and that are likely to affect both the insurance choice and the likelihood of entering a hospital. Apart from several variables that could be seen as proxies for risk aversion concerning health matters (like smoking, drinking alcohol, following a healthy diet, frequently exercising), it also directly asks the individual to state its willingness to take risks regarding health matters. The question in the SOEP is: "How would you rate your willingness to take risks with your health?". It is measured on an 11-point scale from 0 (completely ready to take risks) to 10 (not willing to take

Table 4.1: Distribution of Risk Aversion

	Category	Males Obs.	Females Obs.
Not risk averse	0	23	15
	1	38	26
↓	2	129	80
	3	209	163
	4	243	174
	5	561	571
	6	345	359
	7	457	528
↑	8	531	664
	9	311	539
Very risk averse	10	628	1,024

Source: SOEP

risks).[4] Although this variable is self-assessed, Dohmen et al. (2010b) show in an experimental setting with a pre-test group of the SOEP households that a similar question on general risk attitudes is a fairly reliable measure of the revealed true risk aversion. Likewise, the individual assessment of risk aversion concerning health matters is a good predictor of actually observed health behaviour.[5] Therefore, we are confident that this variable measures the true degree of risk aversion regarding health more accurately and in a more general way than, e.g., if the individual smokes or drinks alcohol. Table 4.1 reports the distribution of risk aversion in the estimation sample for males and females. Most of the individuals in the sample report a fairly high degree of risk aversion concerning health matters. As is also found for other dimensions of risk aversion (Dohmen et al., 2010b), men report a slightly lower degree (median of 7) than women (median of 8).

[4]We recoded the original variable in order to let a higher value represent a higher degree of risk aversion.

[5]These self-assessed variables of risk aversion are widely used in other contexts in the recent literature, see, e.g., Dohmen et al. (2010a), Jaeger et al. (2010), or Caliendo et al. (2009).

4.5 Methods

As a first step to test for information asymmetries in the German market for private supplementary health insurance, we perform one version of the positive correlation test as proposed by, e.g., Chiappori and Salanie (2001). Although we are aware of the objections of Finkelstein and Poterba (2006) to the test, we believe it is helpful to use it as a first benchmark to which the subsequent estimations can be compared. In our case, the test employs a seemingly unrelated regression model with the choice to buy supplementary insurance and the number of hospital stays in the following six years as the two left hand side variables. On the right hand side appear only the variables which are observed by the insurance companies: health status, age, sex, and job risk, all included in the vector X in equations (1) and (2). Age-effects are captured by dummy variables for 10-year age-groups. To allow for a flexible estimation and to control for further non-linearities we include interactions between the health status variables.

$$HOSPITALSTAYS = X\beta_1 + \mu_1 \tag{1}$$

$$INSURANCE = X\delta_1 + \mu_2 \tag{2}$$

The error terms μ_1 and μ_2 capture effects that are not observed by the insurance company but affect the number of hospital stays or the likelihood to buy insurance. The sign and significance of ρ (which is the estimated correlation between μ_1 and μ_2) informs about asymmetric information and whether there is adverse or advantageous selection. For instance, a significantly positive correlation coefficient implies that there are unobserved factors that both lead to higher utilisation of health care and to an increased demand for insurance. Note that - as Finkelstein and Poterba (2006) emphasise - a significant correlation in the unobserved part is a sufficient but not a necessary condition for the existence of information asymmetries. Note further that the estimated coefficients in this model do not measure causal effects of the included variables on the probability of buying insurance and entering a hospital because they are likely to suffer from omitted variable bias. In any case, the aim of this test is not to find causal effects of the included variables but to estimate the correlation in the unobserved part that remains when all the information observable to the insurance company is controlled for.

However, ρ just measures the overall degree of asymmetric information and there might still be sources of adverse and advantageous selection that cancel out in the market for private supplementary insurance. Focussing on the model of de Meza and Webb (2001), we are mostly interested in the effect of risk aversion on both the demand for insurance and the occurrence of the insured risk as an unobserved factor. Finding that risk aversion both affects the likelihood to buy insurance (positively) and future hospital visits (negatively) would be a sign for risk aversion being a single source of advantageous selection, possibly outweighing other sources of adverse selection. This "unused observable" would, thus, be another source of information asymmetry.[6]

The individual degree of risk aversion can be seen as an exogenous personality trait. For instance, it is one of the prime examples used to motivate fixed-effects estimation methods by the idea to remove the important but time-constant individual risk-aversion. Basically, we want to use this notion of an exogenously given characteristic here when we compare the demand for health insurance and hospital visits for more and less risk averse. However, as was shown in the literature (Dohmen et al., 2010b),

[6]Finkelstein and Poterba (2006) use the term "unused observable" for variables that are, in principle, known by the insurer but not used to calculate the insurance premium. Here, the degree of risk aversion is not known by the insurer and "observable" only by the researcher. The basic idea of the test is, however, unchanged.

risk aversion changes in age, sex, income, and other characteristics.[7] Especially important for the degree of risk aversion concerning health matters is certainly the own health status. It is easy to imagine an individual with a chronic illness who cares much more for his own health than a healthy individual. This first individual would most likely also report a higher degree of risk aversion concerning his own health. Thus, if we want to find the effect of, say, a baseline level of exogenously given risk aversion on the two outcome variables, we have to control for the health status which affects the stated degree of risk aversion.

The approach here is to match individuals who have a comparable health status, age, income and other important characteristics but differ in their risk aversion. To this end we use matching techniques as known from the treatment evaluation literature. The "treatment" here is to be risk averse or not. Matching individuals on observables should account for differences in the stated degree of risk aversion that are due to differences in health, age, etc. The idea (and the identifying assumption) is that, conditional on the observable variables, the personality trait "risk averse" is randomly assigned to individuals by nature. As could be seen in Table 4.1, only very few individuals consider themselves risk loving (i.e., they state a very low level to characterise their risk tolerance). Moreover, the possible answers to the question do not allow for a clear cut between risk averse, risk neutral and risk loving individuals.[8] Therefore, we classify the individuals into more risk averse and less risk averse using the the median value of the reported degree of risk aversion as a threshold. A value of at least 8 on the scale indicates more risk averse individuals, while less risk aversion is denoted by values of up to 7. For simplicity, we call the first group the

[7] While high income might also result from a low degree of risk aversion, i.e., there is reversed causality, age and sex are sufficiently exogenous. Thus, this shows that risk aversion is both time-varying and it partly depends on other factors.

[8] Dohmen et al. (2010b) actually can do so, although with respect to the domain "financial matters". They find that 78% in their sample are risk averse, 13% are risk neutral, while 9% might be classified as risk loving.

"risk averse" and the second group the "not risk averse", keeping in mind that the "not risk averse" are mostly just less risk averse than the first group.[9]

Matching on observables can be a challenge because, in general, very many characteristics are required to match observations on in order to find valid comparison observations for which the conditional independence assumption holds. In order to circumvent this so called "curse of dimensionality" we use a propensity score matching, as proposed by Rosenbaum and Rubin (1983, 1985). We estimate the propensity score by using a probit regression of being risk averse on a set of covariates that potentially affect the degree of stated risk aversion.[10] These covariates are the health status (PCS, BMI, self-rated health, disability degree, worries about own health status, hospital visits in the previous year), age, income, homeownership, education, and the labour-force status. Because the matching results are often sensitive to the specific matching technique we use several different ones. These are kernel matching (with a bandwidth of 0.06), caliper matching and nearest neighbor matching (both with a caliper of width 0.02). Moreover, we also estimate the effect of being risk averse on the demand for insurance and hospital visits by simple OLS regressions to check for the robustness of the results.

4.6 Results

The results of the SUR for the positive correlation test are reported in Table 4.2. Since the regression was carried out in order to perform the positive correlation test and the coefficients do not have a causal interpretation, we restrict the presentation to the estimated correlation of the error terms. Full estimation results can be found in Table A4.1 in the Appendix.

[9] The results are unchanged if we set the threshold to 7 instead of 8.

[10] The Stata routine "psmatch2" (Leuven and Sianesi, 2008) was used for the matching.

Table 4.2: Positive Correlation Test

	Males	Females
ρ	0.0217	0.0267*
Observations	3,671	4,392

* $p < 0.1$. Full estimation results in Table A4.1.

Table 4.3: Sample Means by Risk Aversion

		Means	
		Risk averse	Not risk averse
Males	Private suppl. Insurance	0.016	0.010
	Hospital visits	0.807	0.759
	Observations	1,470	2,005
Females	Private suppl. Insurance	0.016	0.016
	Hospital visits	0.919	0.799
	Observations	2,227	1,916

We find that - for women - there is a small but significantly positive correlation between insurance coverage and hospital visits conditional on the information known by the insurer (estimated $\rho = 0.0267$ with the Breusch-Pagan-test indicating significance at the 10%-level), thus indicating adverse selection. That is, there are unobserved factors (unobserved by the insurance company) that both increase the probability of a hospital visit and the likelihood of holding supplementary insurance. The correlation in the male equation is slightly smaller and not significant. As noted in the beginning, this is only the aggregate degree of information asymmetry and there might be single sources of advantageous and adverse selection that cancel each other out. That is, not rejecting the hypothesis that ρ differs from zero in the males regression does not necessarily imply the absence of information asymmetry.

Table 5.1 show the means of hospital visits within six years after 2002 and the decision to buy supplementary health insurance in 2002 for the risk averse and the less risk averse individuals. While men who consider themselves risk averse buy private insurance more often (1.6% vs. 1.0%) this is not the case for women where both types have a likelihood of 1.6%. Interestingly, the risk averse have more hospital visits in the period of six years after the interview than the "control group" of the less risk averse, which is not surprising given the discussion in the previous section. Risk aversion concerning health is more common among already ill individuals because they have to care more for their own health. These differences should therefore not be mistaken for negative causal effects of risk aversion on the health status (which in turn affects the hospital visits).

The last argument is supported by the probit regressions of being risk averse on the control variables used to estimate the propensity score, the results of which are reported in Table 4.4. We see that reporting to be risk averse mainly varies with the self-rated health status, the education level and income: healthy, better educated and wealthier individuals are less likely to report a high degree of risk aversion regarding their health status.

Hence, the matching approach that directly compares similar individuals that do not differ in their health status (among others) but only in their degree of risk aversion is necessary. Table 4.5 shows that the results change notably when matching is used - at least for males. For Kernel and Radius matching we find that risk averse males are significantly more likely to buy private health insurance and also spend less stays in a hospital within the six years after purchasing the insurance. In absolute terms, the difference in the likelihood to buy supplementary insurance is small with 0.7 percentage points. However, given the low overall likelihood to buy supplementary insurance in a certain year, this effect is very high in relative terms with an almost 50% increase. The nearest neighbor matching supports the results regarding the choice to buy insurance but not regarding the number of hospital stays. Here, we

Table 4.4: Probit Estimates for the Propensity Scores

	Males	Females
SAH very good	0.076*	0.114***
	(0.039)	(0.037)
SAH good	0.044*	0.066***
	(0.023)	(0.022)
SAH poor or bad	0.031	-0.003
	(0.034)	(0.030)
PCS	-1.790	-0.734
	(1.300)	(1.196)
PCS squared	1.346	0.424
	(0.968)	(0.896)
PCS x Age	0.004	0.001
	(0.009)	(0.009)
PCS x BMI	0.014	0.006
	(0.021)	(0.017)
Body-Mass-Index	-0.012	-0.003
	(0.010)	(0.009)
Disabled	-0.024	0.013
	(0.062)	(0.073)
Degree Disability	0.002*	0.001
	(0.001)	(0.001)
Hospital visits last year	0.004	0.016
	(0.021)	(0.017)
Years of schooling	-0.007**	-0.011***
	(0.003)	(0.003)
Worries health	-0.009	-0.012
	(0.015)	(0.014)
Age	0.004	0.006
	(0.008)	(0.007)
Age squared	0.000	-0.000
	(0.000)	(0.000)
Homeowner	0.008	0.004
	(0.017)	(0.017)
Log. equiv. HH-income	-0.080***	-0.033*
	(0.022)	(0.019)
Foreign	0.045	0.012
	(0.029)	(0.028)
Full-time employed	0.007	-0.066***
	(0.023)	(0.019)
White collar worker	-0.042**	0.011
	(0.021)	(0.019)
Constant	1.436***	0.935**
	(0.518)	(0.463)
Observations	3475	4143

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

also find a negative effect of risk aversion on the number of hospital visits which is, however, small and not significant. The results are supported by OLS regressions of insurance and hospital visits on the risk aversion dummy and the same covariates as in the propensity score regressions, with almost the same effects of risk aversion as in the Kernel and the Radius matching.[11] Therefore, we conclude that there is (weak) evidence of risk aversion concerning health as one source of advantageous selection, at least for males, since it is an unobserved factor that is associated with more insurance coverage and less hospital visits. This can also explain why we do not find evidence for adverse selection for this group with the positive correlation test shown in Table 4.2. The private information on the health status which is a source of adverse selection is partly outweighed by the private information on risk aversion which is a source of advantageous selection. Note again, that, although the positive correlation test does not find evidence for information asymmetries, the unused observables test does so.

No such effects can be found for females. Neither is the likelihood to buy supplementary health insurance different between the risk averse and the not risk averse, nor do the two groups differ significantly in their number of hospital stays. Risk aversion does not seem to be a source of advantageous selection for females. This could explain why we find a significant, albeit small, degree of overall adverse selection here which is not outweighed by this potential source of advantageous selection.

One explanation for risk aversion being not as important for females as for males could be that the mostly male breadwinner gets more information about potential private insurance or is more likely to be the target of the insurance companies. He might either decide to buy insurance for himself and his partner or induce her to also choose private insurance. In this case, the women's risk aversion is not decisive for

[11] Estimation results of the OLS regression are reported in Table A4.2 in the Appendix

Table 4.5: Effects of Risk Aversion

	Effects of risk aversion		
	Kernel	NN	Radius
	(1)	(2)	(3)
Males			
Private suppl. Insurance	0.007*	0.012**	0.007*
	(0.004)	(0.005)	(0.004)
Hospital visits	-0.114*	-0.027	-0.126**
	(0.063)	(0.083)	(0.064)
Females			
Private suppl. Insurance	0.002	0.001	0.002
	(0.004)	(0.005)	(0.004)
Hospital visits	0.042	0.028	0.029
	(0.051)	(0.067)	(0.052)

*p<0.1, **p<0.05. Standard errors in parentheses.
A few observations that are outside the common support
where removed from the matching estimations.

her likelihood to hold more insurance.[12] Also, women usually have routine visits at the gynaecologist anyway, making the individual degree of risk aversion concerning health not as decisive for prevention and, thus, for later incidence of illnesses as in the case of males. Unfortunately, the already small number of observations makes a subsample analysis to find out the reasons underlying this result unfeasible.

4.7 Conclusion

Using data from the SOEP we find evidence for information asymmetries in the German market for private supplementary insurance for hospital stays. A positive correlation test shows that the overall degree of adverse selection is very small and only significant for women. For males there is no significant adverse selection. However, the unused observables test shows that the individual degree of risk aversion

[12]Certainly, risk averse women might be more likely to also have risk averse partners. This indirect effect of the women's risk aversion via her spouse's risk aversion is likely to be too small to lead to significant results.

is a source of advantageous selection which possibly outweighs sources of adverse selection. That is, risk averse men have a higher likelihood to buy private supplementary insurance and, at the same time, a smaller number of hospital stays within a period of six years after purchasing the insurance. For males, this finding matches the prediction of de Meza and Webb (2001) who theoretically show that risk aversion can be a source of advantageous selection which outweighs sources of adverse selection.

This is the first study that uses a direct measure of risk aversion regarding health when testing for information asymmetries in insurance markets and does not rely on, e.g., reported health behaviour as a proxy for risk aversion. However, small sample sizes and the low incidence of supplementary insurance allow only for weak evidence due to large standard errors.

What does the finding of several sources of adverse and advantageous selection imply? Were the insurance companies able to observe all the variables, they could calculate proper insurance premia for all individuals with their different characteristics. However, as they are not, the information asymmetry prevents a first-best outcome of the market equilibrium. Even if there is one-dimensional private information (e.g., on the true health status), the market outcome is inefficient. The inefficiency even increases if private information is multi-dimensional as is the case here due to the importance of risk aversion (see Finkelstein and McGarry, 2006, for a discussion on list last point). This problem becomes increasingly relevant when more services that insurers have to provide in the statutory health insurance are removed from the benefit packages and have to be covered by private supplementary insurance, as is currently debated.

4.8 Appendix

Table A4.1: Positive Correlation Test - Full Results

	Males		Females	
	Insurance	Hospital Visits	Insurance	Hospital Visits
26 $<=$ Age $<$ 36	0.014	-0.059	-0.002	0.040
	(0.010)	(0.142)	(0.009)	(0.133)
36 $<=$ Age $<$ 46	0.010	0.009	-0.015	-0.229
	(0.014)	(0.207)	(0.014)	(0.194)
46 $<=$ Age $<$ 56	0.009	0.065	-0.014	-0.217
	(0.019)	(0.287)	(0.019)	(0.268)
56 $<=$ Age $<$ 66	0.019	0.172	-0.014	-0.034
	(0.024)	(0.360)	(0.024)	(0.340)
PCS	0.073	-3.415	0.098	-10.250***
	(0.252)	(3.735)	(0.246)	(3.516)
PCS squared	0.033	2.947	-0.046	6.509**
	(0.188)	(2.787)	(0.190)	(2.717)
PCS x Age	-0.001	0.008	0.001	-0.004
	(0.001)	(0.017)	(0.001)	(0.016)
PCS x BMI	-0.002	-0.132*	-0.004	0.041
	(0.005)	(0.069)	(0.004)	(0.060)
BMI	0.002	0.081**	0.001	-0.010
	(0.002)	(0.034)	(0.002)	(0.030)
Disabled	-0.001	0.097	-0.014	0.110
	(0.014)	(0.210)	(0.018)	(0.257)
Degree Disability	-0.000	0.006*	0.000	0.011***
	(0.000)	(0.004)	(0.000)	(0.004)
Risky Job	-0.014	0.076	-0.018	-0.184
	(0.016)	(0.235)	(0.023)	(0.334)
Constant	-0.048	1.093	-0.016	4.197***
	(0.085)	(1.261)	(0.080)	(1.140)
ρ	0.0217		0.0267*	
Observations	3671		4392	

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4.2: OLS Results

	Males		Females	
	Insurance	Hospital Visits	Insurance	Hospital Visits
Risk averse	0.007*	-0.103*	0.000	0.048
	(0.004)	(0.059)	(0.004)	(0.055)
SAH very good	0.007	0.018	-0.002	-0.018
	(0.009)	(0.136)	(0.009)	(0.130)
SAH good	0.010*	0.002	-0.001	-0.088
	(0.005)	(0.081)	(0.006)	(0.077)
SAH poor or bad	-0.001	0.317***	-0.008	0.206*
	(0.008)	(0.118)	(0.008)	(0.105)
PCS	0.017	3.412	0.029	2.381
	(0.304)	(4.510)	(0.306)	(4.231)
PCS squared	-0.011	-1.444	-0.001	-0.911
	(0.226)	(3.358)	(0.229)	(3.170)
PCS x Age	0.000	0.013	0.001	-0.071**
	(0.002)	(0.032)	(0.002)	(0.030)
PCS x BMI	-0.001	-0.164**	-0.004	0.022
	(0.005)	(0.072)	(0.004)	(0.061)
Body-Mass-Index	0.002	0.098***	0.001	-0.001
	(0.002)	(0.036)	(0.002)	(0.030)
Disabled	0.000	0.082	-0.012	-0.074
	(0.015)	(0.216)	(0.019)	(0.260)
Degree Disability	-0.000	0.005	0.000	0.011***
	(0.000)	(0.004)	(0.000)	(0.004)
Hospital visits last year	-0.003	0.630***	0.005	0.932***
	(0.005)	(0.072)	(0.004)	(0.059)
Worries health	0.003	-0.163***	0.006*	-0.173***
	(0.004)	(0.053)	(0.004)	(0.051)
Years of schooling	-0.000	0.004	0.002**	0.016
	(0.001)	(0.011)	(0.001)	(0.011)
Age	0.000	-0.007	-0.001	-0.008
	(0.002)	(0.029)	(0.002)	(0.026)
Age squared	-0.000	0.000	0.000	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Homeowner	0.004	-0.040	0.005	-0.110*
	(0.004)	(0.060)	(0.004)	(0.059)
Log. equvi. hh-income	0.002	-0.011	0.009*	-0.014
	(0.005)	(0.076)	(0.005)	(0.068)
Foreign	-0.010	-0.034	-0.004	-0.174*
	(0.007)	(0.101)	(0.007)	(0.099)
Full-time employed	-0.003	-0.048	-0.005	0.043
	(0.005)	(0.081)	(0.005)	(0.068)
White collar worker	0.005	-0.075	-0.005	-0.025
	(0.005)	(0.074)	(0.005)	(0.068)
Constant	-0.046	-0.845	-0.070	0.659
	(0.121)	(1.799)	(0.119)	(1.639)
Observations	3475	3475	4143	4143

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Chapter 5

Why are the unemployed so ill? - The causal effect of unemployment on health

5.1 Introduction

The association between unemployment and health is well documented in the empirical literature. Various studies report a strong negative correlation between individual health and the experience of unemployment, or, more general, between health and low income (see, e.g., Adams et al., 2003). However, the direction of causality is not yet well understood. There are at least three pathways that can lead to the observation of a less healthy stock of unemployed compared to the stock of employed. First, there is a selection of ill workers from work into unemployment. García-Gómez et al. (2010), Arrow (1996), Riphahn (1999), and Lindholm et al. (2001) provide evidence that the likelihood of becoming unemployed is higher for ill workers. Second, poor health causes longer unemployment spells, as shown by Stewart (2001). Both points - selection of ill workers into unemployment and selection of healthy workers out of unemployment - increase the probability of observing an ill individual in the stock of unemployed and, thus, lead to a lower average health status of the stock of unemployed.

Third, unemployment itself might lead to a deterioration in health. The causal effect of unemployment on health is probably the most difficult of the three to show. There are most likely individual unobservable effects that both affect health and the probability of becoming unemployed, for instance a general frailty or other genetic factors. Usually, panel data help to control for this unobserved heterogeneity. Moreover, the health-related selection into unemployment needs to be considered. Since there might be reversed causality (e.g., a health shock that both decreases health and leads to unemployment), a causal effect can only be established if this selection effect is controlled for.

If unemployment indeed deteriorates health, the individual and social costs of unemployment are higher than usually assumed and policymakers should try even harder to get the unemployed back into the labour market. An additional motivation to examine this third point is to find out more about the nature of unemployment. The life satisfaction literature concludes that unemployment is involuntary if it causally reduces life satisfaction (Winkelmann and Winkelmann, 1998). Similar arguments hold for health. It can be assumed that unemployment negatively affects health especially if it is involuntary.[1]

There are only a few studies that analyse the causal effect of unemployment on health with German data. The most recent one, Romeu Gordo (2006), finds a negative effect of short-term unemployment on health satisfaction for men but no effect for women with SOEP data from 1984-2001. Moreover, long-term unemployment decreases health satisfaction of both men and women. However, although Romeu Gordo (2006) uses panel data and can therefore control for unobserved heterogeneity, the author cannot exclude that reversed causality may have biased the result.

[1]This is true at least in countries where unemployment does not imply the loss of health insurance and leads to only a moderate drop in income, like in Germany.

This paper extends Romeu Gordo (2006) in several ways. First, it does not only use health satisfaction (or the self-rated health status which is highly correlated with health satisfaction) as outcome variable but also the probability of a hospital visit within four years after the interview as a more objective health measure, and a measure of mental health. To our knowledge, this is the first study to analyse mental health effects of unemployment with German panel data. Second, it uses the appropriate econometric methods by accounting for the ordered nature of the health satisfaction variable. Third, and most importantly, it accounts for the possible endogeneity of the entry into unemployment. In principle, this can be done by estimating a simultaneous equations model with health and the labour market status as endogenous variables (see Cai, 2010). Here, we rely on an alternative approach by only using plant closures as an exogenous reason for unemployment. Doing this, reversed causality (from bad health to unemployment) is ruled out.

We find that using only exogenous unemployment entries has a strong impact on the results. Using data from the German Socio-Economic Panel for the years 1991-2008 and including *all* unemployed in the analysis, we find that unemployed are less healthy than employed according to all health measures. However, this is not causally due to unemployment, since this effect disappears if unobserved heterogeneity is controlled for and only exogenously unemployed are considered. In this latter group, unemployment does not deteriorate health, neither in the short- nor in the long-run. Thus, the worse health status of the unemployed (and especially the long-term unemployed) is only a selection effect into unemployment.

The results are in line with several international studies in the recent literature that either use plant closures or mass lay-offs to rule out a health-driven selection into unemployment. Browning et al. (2006) find no causal effect of job loss on the probability of entering a hospital due to symptoms caused by mental stress four years after with Danish register data. Salm (2009) finds no effect on several subjective and objective health measures with data from the HRS. Kuhn et al. (2009) do not find short-run effects of job loss on public health costs associated with health care utilisation. However, they do find that job loss increases hospitalisations for mental health reasons and prescriptions for antidepressants (both for males only). There are also studies that do find strong effects of involuntary job loss on subsequent mortality (e.g., Sullivan and Wachter, 2009; Eliason and Storrie, 2009). The difference in our study, however, is that we analyse the effect of actually being unemployed instead of the mere job loss on health. Also doing this, Böckerman and Ilmakunnas (2009) do not find negative effects of unemployment on self-assessed health for Finland, although they do not restrict their analysis to mass lay-offs or plant closures as reasons for unemployment.

The next section presents the data used in the analysis. Section 3 explains the econometric strategy while Section 4 reports the regression results. Section 5 concludes.

5.2 Data

The database for the empirical analysis is the German Socio-Economic Panel (SOEP), which started in 1984 with more than 12,000 individuals in West Germany and was extended to East Germany in June 1990. There were several refreshments resulting in a sample size of more than 20,000 adult individuals living in about 13,000 households that participated in the SOEP survey in 2006 (see, e.g., Wagner et al., 2007).[2]

[2]The data used in this paper were extracted using the Add-On package PanelWhiz v3.0 (Jul 2008) for Stata. PanelWhiz was written by Dr. John P. Haisken-DeNew (john@panelwhiz.eu).

The PanelWhiz generated DO file to retrieve the SOEP data used here and any Panelwhiz Plugins are available upon request. Any data or computational errors in this paper are my own. Haisken-DeNew and Hahn (2006) describe PanelWhiz in detail.

The SOEP contains information on the current labour force status in each year of the panel membership. We collapse full-time and part-time employment to the category "working". Individuals who are either in some kind of education or out of the labour force during the entire observation period are dropped from the sample. In order to analyse the long-term effects of unemployment, we also interact unemployment with the current duration of the spell in months at the interview date. Therefore, we use the retrospective monthly calendar information on the labour force status in the previous year which provides the exact unemployment duration in months.[3] The advantage of the calendar information is that a person who is unemployed in two consecutive years at the time of the interview is not necessarily long-term unemployed. With the calendar information we can - as an example - identify persons who were unemployed at the time of both interviews but working in the mean time. These would falsely be classified as long-term unemployed without the calendar information.[4]

In case of a job termination, the SOEP asks for the reason. Possible reasons include own resignation, dismissal, plant closure, and end of a temporary job. Because the question differed somewhat before 1991 and we rely on the reason of the job loss later in the econometric analysis, we only use data from 1991 to 2008 in the analysis. We exclude all individuals above the age of 58 because of special regulations that allowed for possible voluntary unemployment in combination with early retirement at around this age in the past. Over the whole sample period we get a panel of up to 180,562 observations in person-year form, resulting from 23,734 individuals (see

[3]Note that there is some recall error in that variable, see Jürges (2007). However, since we mainly rely on the labour market status at the interview date, this retrospective error is arguably not a big problem here.

[4]Instead of the interaction of unemployment and duration of the current spell in months, we also included a dummy variable indicating long-term unemployment which takes on the value one in case of a duration of more than twelve months and zero otherwise. The results with this specification did not differ in qualitative terms from the one with the interaction of unemployment and months of unemployment.

Table 5.1 below).

We use three different health measures. The first one, satisfaction with health, is a self-stated measure on an 11-point scale, ranging from 0 (totally unhappy) to 10 (totally happy). Although it is a subjective measure, it has been shown to have high predictive power for morbidity and subsequent mortality (see, e.g., Idler and Benyamini, 1997, for a review of studies which use self-rated health which is highly correlated with health satisfaction). Furthermore, it gives a more complete picture of overall health than many single objective measures can do. The second measure, the Mental Component Summary Scale, is a measure of mental health. It is based on the SF12-questionnaire in the SOEP that includes several questions about health quality and health satisfaction of individuals. The exact questions which include questions about phases of melancholy, emotional problems or social limitations due to mental health problems are given in Table A5.7 in the appendix. The Mental Component Summary Scale is provided by the SOEP-group and calculated using explorative factor analysis. It ranges from 0 to 100, with a higher value indicating a better health status. The mean value of the SOEP 2004 population is 50 points with a standard deviation of 10 points (see Andersen et al., 2007, for a description).

The third measure is a more objective indicator of individual health. It is the binary variable for having at least one overnight hospital stay in the next four years after the interview. In order to make it comparable to the other two measures with a higher value meaning a better health status, we define the variable as *no* hospital visit. The SOEP asks about the hospital visits in the *last* twelve months, therefore we use information from the four future waves after the current interview. Hospital visits are a fairly crude measure of health, but also used in the literature. However, in contrast to Browning et al. (2006) we do not know the reason of the hospital visit and can, therefore, not distinguish between hospital visits due to symptoms that arise from mental stress and other reasons.

While health satisfaction is available for all waves between 1991 and 2008, the question about the hospital visits was not asked in 1993. Therefore, and because of the prospective nature of the variable, we cannot use the years 1991-1992 and 2005-2008 for this measure. The mental health score is only available for the years 2002, 2004, 2006, and 2008. Hence, more observations for health satisfaction than for the other two indicators can be used. The three measures reflect different aspects of the individual health status. While health satisfaction is an overall measure, the Mental Component Summary Scale only represents mental health. The hospital visits are objective but can be seen as an indicator of bad health only. For instance, this measure does not discriminate between forms of very good and good health if for both types no hospital visit at all is necessary. No measure can thus *per se* be preferred to the other ones and a complete picture of the effects of unemployment on health can be achieved when all indicators are used together in the analysis. The three variables are significantly and positively correlated in the data with health satisfaction and mental health showing the strongest correlation (0.32). Since (no) hospital visits are given as a binary variable only, the correlation coefficient of this one and the other two measures is naturally smaller (0.14 with health satisfaction and 0.07 with mental health).

Table 5.1 reports the means of the three health measures for all working individuals, unemployed and long-term unemployed (more than 12 months). According to all measures, the stock of unemployed consists of less healthy individuals than the stock of employed. Long-term unemployed are even less healthy than the whole group of unemployed, although only slightly. However, these are only raw means without control for observable and unobservable individual effects that may be correlated with both health and the labour market status. Furthermore, it does not control for selection into unemployment due to bad health. Hence, a causal relationship between health and unemployment cannot be found in the table. We try to answer this question with a regression analysis, the strategy of which is outlined in the next section.

Table 5.1: Means of the health measures

Means	Health satisfaction	No hospital visit	Mental health
Working	6.96	68.41%	49.40
Unemployed	6.37	63.32%	47.36
Long-term unemployed	6.00	62.70%	46.98
Individuals	23,734	16,525	16,085
Person-year observations	180,562	95,217	44,504

Source: SOEP 1991-2008

5.3 Empirical Strategy

Health satisfaction is an ordinal measure, hence ordered logit or ordered probit seem to be the appropriate estimation methods instead of ordinary least squares which assumes cardinality of the outcome variable. When estimating the relationship between health and unemployment it is essential to control for other factors that affect both health and the likelihood of becoming (and staying) unemployed. Although we include several variables to control for observed heterogeneity, a great deal of unobservable heterogeneity is likely to remain. One might think of genetic factors, but also risk aversion, or time preferences. As most of this unobservable heterogeneity can be assumed to be time invariant - at least over a limited period of time -, fixed-effects methods are capable of solving this problem. Ferrer-i-Carbonell and Frijters (2004) develop a fixed-effects ordered logit estimator which collapses the ordered variable into a binary one with the thresholds that determine whether the original ordered variable is transformed to a one or a zero being individual-specific. Since implementation of this estimator is not trivial and convergence time is very long, the authors point to an easily implementable approximation of their estimator which works pretty well and is widely used in the recent literature.[5] In the approximation, the information on health satisfaction is collapsed into a binary variable that takes on the values 1 if the health satisfaction exceeds the within-individual average over time, and 0 if it is below. The model is then estimated as a conditional logit model (Chamberlain, 1980). As usual, only individuals can be included in the conditional logit regression who change their health status at least once in the observed period. This, however, is the case for most of the individuals in the sample. A draw-back of this estimator (as well as with the normal fixed-effects logit estimator) is that one cannot calculate marginal effects without implying further assumptions on the fixed effect. We therefore show parameter estimates only.

Since the mental health score lies between 0 and 100, we use the linear fixed effects model here, while the conditional logit model is used for the binary hospital visit variable. The fixed-effects estimation also removes the possible problem of selection of healthy workers out of unemployment because we only consider the within differences. Therefore, if we assume that the *changes* in health due to unemployment are the same for healthy and ill unemployed, it does not bias the analysis if healthy individuals are more likely to find their way back into the labour market. However, if there are negative effects of unemployment and they are stronger for already ill individuals, we might overestimate negative effects of unemployment even with the use of fixed-effects estimation.

Although fixed-effects methods remove a lot of unobserved heterogeneity that might be correlated with both health and unemployment, they cannot solve the potential

[5] See e.g. Böckerman and Ilmakunnas (2009), Kassenböhmer and Haisken-DeNew (2009), or Brenner (2007). Jones and Schurer (2010) report that the differences in the estimates between the original estimator and the approximation are negligible.

problem of reversed causality or endogenous unemployment. It may well be that we observe a working individual in good health in one year in the sample and in bad health and unemployment in the following one. Because we only have the information on the health status at two points in time (before and after the day of the job loss) we cannot exclude the case that the individual first became ill and then lost her job (or quit) due to bad health. In order to identify the causal effect of unemployment on health we need to find an exogenous reason for unemployment, especially one that is not related to the individual health status.

In this study we rely on plant closures as an exogenous reason for unemployment (see Salm, 2009, Browning et al., 2006, or Kuhn et al., 2009 for a similar argumentation).[6] Table 5.2 reports the number of observations for the different health measures. Only about 5 per cent of all the unemployed (in person-year observations) are unemployed due to plant closure. This means a reduction of identifying observations for the estimation of the effect of unemployment on health. However, we still have enough individuals for a reasonable analysis.

Table 5.2: Number of observations

Observations	Health satisfaction	No hospital visit	Mental health
All	180,562	95,217	44,504
Unemployed	15,114	7,982	3,736
Unemployed Plant Closure	876	522	194

Source: SOEP 1991-2008

Because, in general, blue collar workers are more likely to lose their job due to plant closures than white collar workers and they are - on average - less healthy than the latter, we also control for job characteristics (blue collar, white collar, self-employed, civil servant, other position). Since there is no information on the job characteristics

[6] "Plant closure" does *not* include job loss of selfemployed who had to close down their own business. The questionnaire explicitly allows for this reason as well, allowing us to distinguish between both reasons and use only the category "plant closure" in the SOEP.

of unemployed, we include the job position prior to unemployment for this group. Furthermore, we include a full-set of dummies for the type of industry (2 digit NACE codes).

5.4 Results

Table 5.3 reports estimation results for the three health measures when pooled estimation models without fixed effects are used and all reasons for unemployment are considered. Since the estimated coefficients of the unemployment dummies are likely to be biased, they should not be interpreted as causal effects. Table 5.3 rather serves as a first benchmark and descriptive analysis. According to all health measures, unemployed are less healthy than working individuals. The coefficients are all highly significant. Considering unemployment duration the results are less clear. While long-term unemployed have an even lower health satisfaction than the short-term unemployed, no duration effect can be found for the mental health measure and the hospital visits.

Although unemployment is the most interesting variable in this study, we briefly discuss the results of the other covariates. Males, foreigners, and individuals who have a higher income or frequently do sports report a better health status than the respective base categories. West Germans have a better self-stated health but more hospital visits than East Germans, broadly the same picture holds for younger individuals. Note, however, that individuals above the age of 58 are excluded from the sample. Private health insurance is associated with better health, the same holds for more education and children in the household. Although no coefficient has a causal interpretation, they have in general the expected signs.

Table 5.4 reports the results when fixed effects are taken into account but still all reasons for unemployment are taken together. Here, the time-invariant variables

Table 5.3: Pooled models

	Health Satisfaction (Ordered logit)		No Hospital Visit (Logit)		Mental Health Score (OLS)	
Unemployed	-0.225*	(0.027)	-0.222*	(0.040)	-1.410*	(0.259)
Months of unemployment	-0.005*	(0.001)	-0.000	(0.002)	0.002	(0.010)
28 <= Age <= 32	-0.363*	(0.023)	-0.058	(0.042)	-1.023*	(0.215)
33 <= Age <= 37	-0.642*	(0.028)	0.207*	(0.048)	-1.274*	(0.237)
38 <= Age <= 42	-0.916*	(0.030)	0.313*	(0.050)	-1.452*	(0.240)
43 <= Age <= 47	-1.093*	(0.031)	0.204*	(0.050)	-1.154*	(0.240)
48 <= Age <= 52	-1.243*	(0.032)	0.071	(0.052)	-0.925*	(0.256)
53 <= Age <= 58	-1.419*	(0.033)	-0.060	(0.052)	-0.344	(0.258)
Male	0.057*	(0.024)	0.582*	(0.035)	1.595*	(0.161)
Foreign	0.180*	(0.032)	0.044	(0.045)	0.480	(0.248)
West	0.129*	(0.023)	-0.057	(0.034)	0.537*	(0.157)
Years of education	0.012*	(0.004)	0.006	(0.006)	-0.025	(0.031)
log. equiv. HH-income	0.271*	(0.021)	0.067*	(0.032)	1.967*	(0.154)
Frequency of sports	0.111*	(0.007)	0.032*	(0.010)	0.147*	(0.047)
Married	0.003	(0.022)	-0.125*	(0.033)	1.162*	(0.160)
Children in household	0.190*	(0.019)	0.101*	(0.030)	0.260	(0.148)
Private insurance	0.080*	(0.032)	0.091	(0.053)	0.267	(0.229)
Education/Vocational training	-0.064*	(0.028)	-0.170*	(0.043)	-0.625*	(0.253)
Out of labor force	-0.250*	(0.029)	-0.175*	(0.039)	-1.579*	(0.235)
Blue collar	-0.133*	(0.026)	0.010	(0.040)	-0.404*	(0.193)
Selfemployed	-0.020	(0.035)	0.165*	(0.058)	-0.380	(0.250)
Civil Servant	-0.094	(0.054)	-0.304*	(0.082)	-1.061*	(0.387)
Other Position	0.093*	(0.038)	0.193*	(0.059)	0.469	(0.318)
Quarter of Interview = 2	0.005	(0.014)	-0.002	(0.024)	-0.475*	(0.126)
Quarter of Interview = 3	0.048*	(0.024)	0.103*	(0.043)	-0.249	(0.212)
Quarter of Interview = 4	0.038	(0.071)	0.049	(0.108)	-1.519	(1.106)
Constant			-0.067	(0.245)	34.150*	(1.197)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	180,562		95,217		44,504	

Standard errors in parentheses; * $p < 0.05$
Cut-off points for the ordered logit model not presented here.
White collar workers are the reference group for job position.

(male, foreigner, west) cannot be included. Note here the difference between the conditional logit and the fixed-effects ordered logit. Because only individuals who change their health outcome at least once in the observed time period can be used in the analysis, there is a high loss of information in the hospital equation. That is, many individuals have either no hospital visit at all in the entire period or regularly enter a hospital. The loss is much smaller in the health satisfaction equation where only individuals are dropped who never change their health satisfaction on an 11-point scale, which is rarely the case.

Table 5.4: Fixed effects models

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	-0.063*	(0.026)	0.097	(0.051)	-0.681*	(0.226)
Months of unemployment	-0.003*	(0.001)	0.001	(0.003)	-0.006	(0.009)
28 <= Age <= 32	0.054	(0.029)	-0.021	(0.059)	-0.398	(0.267)
33 <= Age <= 37	0.064	(0.042)	0.392*	(0.087)	-0.565	(0.384)
38 <= Age <= 42	0.065	(0.055)	0.530*	(0.115)	-0.826	(0.482)
43 <= Age <= 47	0.063	(0.069)	0.425*	(0.144)	-0.717	(0.578)
48 <= Age <= 52	0.085	(0.084)	0.298	(0.174)	-0.527	(0.677)
53 <= Age <= 58	-0.000	(0.099)	0.105	(0.205)	-0.514	(0.785)
Years of education	0.005	(0.006)	-0.037*	(0.013)	0.012	(0.060)
log. equiv. HH-income	0.083*	(0.021)	-0.139*	(0.046)	0.915*	(0.175)
Frequency of sports	0.063*	(0.006)	0.006	(0.013)	0.108*	(0.050)
Married	-0.024	(0.024)	-0.231*	(0.050)	1.109*	(0.216)
Children in household	0.041*	(0.019)	0.418*	(0.040)	0.068	(0.167)
Private insurance	0.066	(0.036)	-0.323*	(0.081)	0.128	(0.316)
Education/Vocational training	0.018	(0.028)	0.221*	(0.055)	0.256	(0.241)
Out of labor force	0.002	(0.024)	0.582*	(0.048)	-0.319	(0.204)
Blue collar	-0.068*	(0.026)	0.084	(0.055)	0.023	(0.217)
Selfemployed	-0.024	(0.039)	0.291*	(0.088)	-0.557	(0.334)
Civil Servant	-0.073	(0.075)	-0.246	(0.161)	-1.623*	(0.690)
Other Position	-0.100*	(0.038)	0.073	(0.082)	-0.145	(0.335)
Quarter of Interview = 2	0.008	(0.014)	0.024	(0.033)	-0.244*	(0.122)
Quarter of Interview = 3	0.030	(0.026)	0.043	(0.063)	-0.415	(0.220)
Quarter of Interview = 4	0.035	(0.078)	0.388*	(0.159)	-1.828	(1.232)
Constant					42.748*	(1.694)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	174,893		45,764		44,504	

Standard errors in parentheses; * $p < 0.05$

The size of the coefficients markedly decreases after controlling for fixed effects.

That means that individual unobserved effects determine the likelihood of becoming (and staying) unemployed and, at the same time, being in bad health to a great deal. Although the coefficients of the unemployment variable decrease they are still highly significant, except for the hospital visits equation where they turn positive but also insignificant. The size of the other estimated coefficients also decreases. For instance, the effect of household income on all health measures becomes smaller and even negative for hospital visits when fixed effects are controlled for.

However, as discussed in the previous section, fixed-effects estimations do not provide consistent estimates of the effect of unemployment on health if reversed causality can be expected. If we include all unemployed in the sample we cannot rule out that there are individuals that endogenously became unemployed (i.e., they quit their job or lost it due to health problems). Only if we use the group of exogenously unemployed (those who became unemployed due to plant closure) we can establish causality. Table 5.5 shows the results of the fixed-effects methods with exogenous unemployment.[7] The estimated parameters turn insignificant in all cases. It almost reaches zero for the Mental Health Score and it is even positive (though insignificant) for health satisfaction and hospital visits. The results indicate that endogenous unemployment might have biased the estimates in Table 5.4 and that there is no causal effect of unemployment (and even long-term unemployment) on health according to all three measures.

We find no significant effect of unemployment on health for the entire sample. However, it may well be that some groups suffer differently from unemployment than others. To check the robustness of the results we split up the sample into subgroups and again carry out the fixed-effects estimations with unemployed due to

[7]Here, individuals who became unemployed for other reasons are dropped from the sample. In a different specification, we left them in the sample and included a dummy variable for unemployment due to plant closure and one for unemployment due to other reasons. This did not lead to different conclusions.

Table 5.5: Fixed Effects models with plant closure

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	0.145	(0.107)	0.083	(0.204)	-0.084	(0.894)
Months of unemployment	-0.001	(0.005)	-0.015	(0.011)	0.006	(0.042)
28 <= Age <= 32	0.046	(0.030)	0.004	(0.063)	-0.340	(0.277)
33 <= Age <= 37	0.064	(0.044)	0.467*	(0.092)	-0.501	(0.398)
38 <= Age <= 42	0.085	(0.058)	0.628*	(0.122)	-0.782	(0.499)
43 <= Age <= 47	0.076	(0.073)	0.474*	(0.153)	-0.832	(0.599)
48 <= Age <= 52	0.111	(0.088)	0.405*	(0.184)	-0.606	(0.701)
53 <= Age <= 58	0.028	(0.104)	0.195	(0.218)	-0.585	(0.813)
Years of education	0.003	(0.006)	-0.033*	(0.014)	0.012	(0.060)
log. equiv. HH-income	0.077*	(0.023)	-0.132*	(0.050)	0.669*	(0.189)
Frequency of sports	0.062*	(0.007)	0.003	(0.014)	0.114*	(0.052)
Married	-0.027	(0.025)	-0.266*	(0.053)	1.033*	(0.225)
Children in household	0.040*	(0.020)	0.418*	(0.043)	0.013	(0.174)
Private insurance	0.071	(0.036)	-0.287*	(0.084)	-0.051	(0.320)
Education/Vocational training	0.024	(0.029)	0.258*	(0.059)	0.383	(0.254)
Out of labor force	-0.008	(0.025)	0.604*	(0.051)	-0.228	(0.213)
Blue collar	-0.073*	(0.027)	0.077	(0.059)	0.042	(0.229)
Selfemployed	-0.031	(0.041)	0.276*	(0.094)	-0.318	(0.355)
Civil Servant	-0.088	(0.076)	-0.301	(0.164)	-1.371*	(0.689)
Other Position	-0.117*	(0.042)	-0.011	(0.091)	-0.433	(0.368)
Quarter of Interview = 2	0.004	(0.015)	0.024	(0.034)	-0.304*	(0.125)
Quarter of Interview = 3	0.034	(0.027)	0.044	(0.065)	-0.310	(0.226)
Quarter of Interview = 4	0.053	(0.081)	0.412*	(0.167)	-2.220	(1.288)
Constant					44.985*	(1.808)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	159563		40988		40962	

Standard errors in parentheses; * $p < 0.05$

plant closures. Table 5.6 reports the results of the different regressions. Here, only the two most interesting coefficients (unemployment and unemployment duration) are shown. The full estimation results can be found in Tables A5.2 - A5.6 in the appendix. The results indicate that there is no negative effect of both short-term and long-term unemployment for males, females and West Germans according to all measures. In East Germany this also holds for health satisfaction and mental health. However, for those individuals we find a marginally significant effect of unemployment-duration on hospital visits.

Table 5.6: Fixed Effects models with plant closure - Subsamples

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Males						
Unemployed	0.066	(0.144)	-0.003	(0.301)	-0.362	(1.068)
Months of unemployment	0.004	(0.007)	-0.001	(0.015)	0.040	(0.048)
Observations	82,961		18,181		20,736	
Females						
Unemployed	0.253	(0.160)	0.234	(0.289)	0.489	(1.570)
Months of unemployment	-0.009	(0.008)	-0.028	(0.018)	-0.064	(0.079)
Observations	76,602		22,807		20,226	
West						
Unemployed	0.166	(0.144)	0.270	(0.273)	-0.113	(1.060)
Months of unemployment	-0.000	(0.006)	0.001	(0.014)	0.001	(0.048)
Observations	120,636		29,563		31,543	
East						
Unemployed	0.086	(0.164)	0.107	(0.326)	0.225	(1.705)
Months of unemployment	-0.003	(0.009)	-0.041*	(0.019)	0.019	(0.089)
Observations	38,282		11,090		9,419	
Over 50 years						
Unemployed	-0.164	(0.197)	-0.440	(0.408)	1.337	(1.900)
Months of unemployment	0.004	(0.008)	-0.017	(0.015)	0.025	(0.083)
Observations	28,418		6,236		9,157	

* $p < 0.05$; Standard errors in parentheses; Full estimation results in tables A5.2 - A5.6

Unemployed individuals above the age of 50 (males, females, east and west taken together) have a lower health satisfaction and more hospital visits. Surprisingly, they also have a much better mental health status even after controlling for exogenous entry into unemployment. However, although the coefficients are rather high they are not precisely estimated (probably due to the very small numbers of identifying observations in this subgroup). Overall, the effects are almost always very small and insignificant, even in the subgroups.

Table 5.7: Fixed Effects models with plant closure - Other health measures

	Hospital visit previous year (FE Logit)		# Hospital visits next 4 years (FE Neg. Binomial)		# Doctor visits (FE Neg. Binomial)		Body mass index (Linear FE)	
Unemployed	-0.210	(0.213)	0.018	(0.114)	0.018	(0.057)	-0.082	(0.179)
Months of unemployment	0.009	(0.010)	0.008	(0.006)	0.001	(0.003)	-0.008	(0.008)
28 <= Age <= 32	-0.145*	(0.054)	-0.098*	(0.029)	-0.015	(0.014)	0.205*	(0.055)
33 <= Age <= 37	-0.435*	(0.079)	-0.425*	(0.038)	-0.022	(0.016)	0.207*	(0.079)
38 <= Age <= 42	-0.609*	(0.106)	-0.559*	(0.043)	-0.045*	(0.018)	0.125	(0.099)
43 <= Age <= 47	-0.450*	(0.134)	-0.565*	(0.047)	0.009	(0.019)	0.132	(0.119)
48 <= Age <= 52	-0.337*	(0.162)	-0.538*	(0.051)	0.099*	(0.021)	0.022	(0.139)
53 <= Age <= 58	-0.203	(0.192)	-0.387*	(0.057)	0.238*	(0.023)	-0.071	(0.161)
Years of education	0.054*	(0.012)	0.009	(0.006)	0.021*	(0.002)	0.058*	(0.012)
log. equiv. HH-income	-0.101*	(0.040)	0.132*	(0.026)	0.017	(0.011)	-0.042	(0.037)
Frequency of sports	-0.032*	(0.012)	0.016*	(0.007)	0.023*	(0.003)	-0.044*	(0.010)
Married	0.248*	(0.044)	0.125*	(0.025)	0.095*	(0.011)	0.382*	(0.045)
Children in household	0.194*	(0.036)	-0.202*	(0.023)	-0.053*	(0.010)	0.026	(0.035)
Private insurance	-0.092	(0.073)	0.209*	(0.044)	-0.072*	(0.018)	-0.099	(0.064)
Education/Vocational training	0.948*	(0.048)	-0.128*	(0.031)	0.118*	(0.015)	0.033	(0.050)
Out of labor force	1.043*	(0.039)	-0.307*	(0.026)	0.150*	(0.012)	0.223*	(0.042)
Blue collar	-0.092	(0.050)	-0.006	(0.031)	-0.057*	(0.013)	0.018	(0.045)
Selfemployed	-0.134	(0.077)	-0.208*	(0.050)	-0.214*	(0.020)	-0.036	(0.070)
Civil Servant	0.471*	(0.147)	0.159*	(0.073)	0.039	(0.030)	-0.061	(0.137)
Other Position	-0.729*	(0.077)	0.083	(0.047)	-0.018	(0.021)	-0.126	(0.073)
Quarter of Interview = 2	-0.066*	(0.028)	0.051*	(0.019)	0.002	(0.008)	0.015	(0.025)
Quarter of Interview = 3	-0.040	(0.051)	0.019	(0.035)	-0.044*	(0.015)	0.016	(0.045)
Quarter of Interview = 4	-0.039	(0.151)	-0.110	(0.093)	-0.056	(0.044)	-0.051	(0.253)
Constant			-0.421*	(0.212)	0.244*	(0.088)	25.204*	(0.369)
Year dummies	yes		yes		yes		yes	
Industry dummies	yes		yes		yes		yes	
Observations	76,591		43,653		150,385		41,843	

Standard errors in parentheses; * $p < 0.05$

As another robustness check we change the dependent variable of the more objective measure, the hospital visit. First, we use the original variable in the SOEP, a hospital visit within the previous twelve months. This variable has the drawback, that hospital visits might possibly be counted before the unemployment spell started. Therefore, this variable is not the preferred one and is only used for a robustness check. Second, we use the total number of hospital stays within the next four years instead of the binary variable to use all the available information. Third, we use the number of doctor visits in the previous three months as a health measure. On the one hand, the latter might be better than the hospital visits as a health measure because it does not only capture severe health problems (as the hospital visit mainly does) but also smaller ones. On the other hand, while, in general, a hospital visit can be assumed to be involuntary for the unemployed, this is not necessarily the case for a doctor visit. As the opportunity costs decrease with unemployment, more doctor visits than before need not necessarily reflect a worse health status but maybe just more available time. Finally, we use another objective health measure, the body-mass index, as an outcome variable. This variable is available for the years 2002, 2004, 2006, and 2008 in the data set.

Since the number of hospital visits and the number of doctor visits are count variables, we use a fixed-effects negative binomial model as presented in, e.g., Cameron and Trivedi (2005) for this estimation.[8] Table 5.7 reports the results when the other health measures are used. In contrast to the health measures before, higher values of these variables indicate a worse health status. That is, a positive coefficient implies a negative impact on health. However, again the coefficients of unemployment are small, insignificant and not of the expected sign in the binary hospital equations. The coefficient in the doctor visits equation is about zero. Likewise, the body-mass index remains largely unchanged due to unemployment. Apparently, the results are not sensitive to these health measures.

5.5 Conclusion

We estimate the causal effect of unemployment on health using data from the German Socio-Economic Panel for 1991-2008. With fixed-effects methods and an exogenous entry into unemployment we do not find a causal effect of neither short-term nor long-term unemployment on health. These results hold for various health measures and across several subgroups.

This is the first study that analyses the effect of unemployment on mental health for Germany. This is especially interesting since it can be assumed that unemployment

[8]The stata command *xtnbreg* was used for the estimation.

first reduces mental health before it deteriorates the overall health status. However, we do not find evidence for a negative effect of unemployment on mental health.

The results are not in line with an earlier study by Romeu Gordo (2006) who finds negative effects of unemployment using the same data set. Our results indicate that the major reason for the difference is that we consider only truly exogenously unemployed individuals in the preferred specification while the former study does not make this distinction.[9] We argue that it is crucial to take the possible endogeneity of unemployment (and, thus, reversed causality) into account to get consistent estimates.

One potential shortcoming of our study might be the health measures. Although satisfaction with health is likely to be the most interesting outcome variable in terms of a utility measure it might be prone to measurement error. Especially in the health-and-retirement literature it is often argued that self-stated health indicators might suffer from a justification bias. Transferred to our study this means that unemployed feel uncomfortable with telling the interviewer about not yet having found a job and state a lower health status as an excuse. It is debatable if this is the case in Germany where unemployment is more widely perceived as bad luck than as one's own fault. Even if this were the case and the health satisfaction variable suffered from a justification bias, the negative effects of unemployment on health would be overestimated - yet, we do not find negative effects. Moreover, the results also hold when more objective health measures are used.

Our results indicate that the selection of ill workers into unemployment and healthy workers out of unemployment lead to the observation that the stock of unemployed has on average a worse health status than the stock of employed but that there is no causal effect of unemployment on health. These results are also in line with

[9] Moreover, the use of calendar information might lead to different effects of long-term unemployment even when we consider all reasons of unemployment.

those found in the recent international health economic literature (Browning et al., 2006; Salm, 2009; Böckerman and Ilmakunnas, 2009). One reason for the absence of negative effects on health in Germany (as well as in the Scandinavian countries cited above) might be the following. First, before the most recent labour market reform, Germany had an unemployment insurance system that was characterised by generous insurance benefits and especially by long entitlement durations of unemployment benefits. Therefore, the income loss in case of unemployment was (and still is) usually moderate. Moreover, Frijters et al. (2005) find only a very small causal effect of income on health in Germany. Second, job loss never causes the loss of health insurance in Germany. Health care utilisation should, therefore, not be affected by financial constraints due to unemployment. This could explain why unemployment does not lead to adverse health outcomes in Germany compared to, e.g., the US, where some authors do find negative health effects of unemployment.

5.6 Appendix

Table A5.1: Sample Means

Variable	Mean	S.D.	Min	Max
Unemployed	0.084	0.277	0	1
Months of unemployment	1.019	6.338	0	178
28 <= Age <= 32	0.132	0.338	0	1
33 <= Age <= 37	0.147	0.354	0	1
38 <= Age <= 42	0.149	0.356	0	1
43 <= Age <= 47	0.140	0.347	0	1
48 <= Age <= 52	0.123	0.329	0	1
53 <= Age <= 58	0.129	0.335	0	1
Male	0.519	0.500	0	1
Foreign	0.121	0.326	0	1
West	0.741	0.438	0	1
Years of education	11.941	2.755	0	18
log. equiv. HH-income	7.286	0.470	3.034	11.166
Frequency of sports	2.338	1.264	1	4
Married	0.623	0.485	0	1
Children in household	0.440	0.496	0	1
Private insurance	0.110	0.313	0	1
Education/Vocational training	0.071	0.256	0	1
Out of labor force	0.088	0.284	0	1
Blue collar	0.354	0.478	0	1
Selfemployed	0.078	0.268	0	1
Civil Servant	0.058	0.233	0	1
Other Position	0.060	0.238	0	1
Quarter of Interview = 2	0.233	0.423	0	1
Quarter of Interview = 3	0.057	0.232	0	1
Quarter of Interview = 4	0.005	0.069	0	1
Observations		180,562		

Table A5.2: Fixed Effects models with plant closure- Males

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	0.066	(0.144)	-0.003	(0.301)	-0.362	(1.068)
Months of unemployment	0.004	(0.007)	-0.001	(0.015)	0.040	(0.048)
28 <= Age <= 32	0.102*	(0.043)	0.046	(0.104)	-0.526	(0.398)
33 <= Age <= 37	0.122*	(0.062)	0.282	(0.146)	-0.305	(0.556)
38 <= Age <= 42	0.171*	(0.082)	0.417*	(0.190)	-0.708	(0.690)
43 <= Age <= 47	0.190	(0.102)	0.034	(0.235)	-0.818	(0.821)
48 <= Age <= 52	0.252*	(0.124)	-0.060	(0.281)	-0.739	(0.958)
53 <= Age <= 58	0.199	(0.146)	-0.216	(0.329)	-0.866	(1.104)
Years of education	-0.017	(0.009)	0.028	(0.022)	-0.004	(0.089)
log. equiv. HH-income	0.081*	(0.033)	-0.165*	(0.081)	0.937*	(0.266)
Frequency of sports	0.073*	(0.010)	-0.022	(0.022)	0.046	(0.073)
Married	-0.041	(0.037)	-0.143	(0.088)	1.021*	(0.317)
Children in household	0.032	(0.028)	-0.068	(0.065)	-0.163	(0.237)
Private insurance	0.123*	(0.047)	0.037	(0.115)	-0.421	(0.388)
Education/Vocational training	-0.069	(0.057)	0.316*	(0.149)	0.422	(0.458)
Out of labor force	-0.135*	(0.047)	0.475*	(0.110)	-0.534	(0.417)
Blue collar	-0.073	(0.039)	0.025	(0.090)	-0.177	(0.315)
Selfemployed	-0.143*	(0.056)	-0.159	(0.142)	-0.203	(0.475)
Civil Servant	-0.036	(0.104)	-0.409	(0.246)	0.705	(0.906)
Other Position	-0.039	(0.068)	-0.634*	(0.177)	-0.445	(0.559)
Quarter of Interview = 2	0.004	(0.021)	-0.019	(0.052)	-0.411*	(0.169)
Quarter of Interview = 3	0.032	(0.038)	0.052	(0.099)	-0.473	(0.306)
Quarter of Interview = 4	0.063	(0.113)	0.520*	(0.256)	-1.867	(1.805)
Constant					45.562*	(2.746)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	82,961		18,181		20,736	

Standard errors in parentheses; * $p < 0.05$

Table A5.3: Fixed Effects models with plant closure- Females

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	0.253	(0.160)	0.234	(0.289)	0.489	(1.570)
Months of unemployment	-0.009	(0.008)	-0.028	(0.018)	-0.064	(0.079)
28 <= Age <= 32	-0.025	(0.043)	-0.105	(0.081)	-0.240	(0.392)
33 <= Age <= 37	-0.005	(0.063)	0.535*	(0.122)	-0.783	(0.576)
38 <= Age <= 42	-0.010	(0.083)	0.800*	(0.164)	-0.925	(0.727)
43 <= Age <= 47	-0.039	(0.104)	0.921*	(0.207)	-0.927	(0.877)
48 <= Age <= 52	-0.030	(0.126)	0.925*	(0.251)	-0.549	(1.030)
53 <= Age <= 58	-0.136	(0.150)	0.749*	(0.298)	-0.405	(1.199)
Years of education	0.018*	(0.008)	-0.071*	(0.019)	0.034	(0.084)
log. equiv. HH-income	0.073*	(0.031)	-0.142*	(0.066)	0.385	(0.271)
Frequency of sports	0.047*	(0.009)	0.009	(0.019)	0.168*	(0.074)
Married	-0.019	(0.035)	-0.292*	(0.069)	1.075*	(0.324)
Children in household	0.046	(0.029)	0.826*	(0.059)	0.182	(0.260)
Private insurance	0.036	(0.060)	-0.587*	(0.128)	0.587	(0.552)
Education/Vocational training	0.051	(0.035)	0.259*	(0.066)	0.275	(0.316)
Out of labor force	0.035	(0.030)	0.553*	(0.059)	-0.212	(0.259)
Blue collar	-0.067	(0.039)	-0.018	(0.082)	0.289	(0.344)
Selfemployed	0.113	(0.060)	0.572*	(0.131)	-0.456	(0.537)
Civil Servant	-0.168	(0.115)	-0.146	(0.232)	-3.787*	(1.056)
Other Position	-0.137*	(0.055)	0.341*	(0.113)	-0.535	(0.514)
Quarter of Interview = 2	0.002	(0.021)	0.056	(0.047)	-0.163	(0.186)
Quarter of Interview = 3	0.036	(0.039)	0.028	(0.089)	-0.147	(0.335)
Quarter of Interview = 4	0.044	(0.117)	0.319	(0.227)	-2.512	(1.839)
Constant					44.944*	(2.443)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	76,602		22,807		20,226	

Standard errors in parentheses; * $p < 0.05$

Table A5.4: Fixed Effects models with plant closure- West

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	0.166	(0.144)	0.270	(0.273)	-0.113	(1.060)
Months of unemployment	-0.000	(0.006)	0.001	(0.014)	0.001	(0.048)
28 <= Age <= 32	0.036	(0.035)	0.032	(0.073)	-0.342	(0.328)
33 <= Age <= 37	0.056	(0.050)	0.591*	(0.107)	-0.478	(0.464)
38 <= Age <= 42	0.102	(0.067)	0.709*	(0.143)	-0.716	(0.578)
43 <= Age <= 47	0.129	(0.084)	0.649*	(0.181)	-0.612	(0.692)
48 <= Age <= 52	0.141	(0.101)	0.516*	(0.218)	-0.335	(0.810)
53 <= Age <= 58	0.054	(0.120)	0.368	(0.257)	-0.299	(0.939)
Years of education	-0.002	(0.007)	-0.033*	(0.017)	0.005	(0.074)
log. equiv. HH-income	0.083*	(0.026)	-0.143*	(0.058)	0.389	(0.217)
Frequency of sports	0.064*	(0.008)	-0.004	(0.016)	0.061	(0.060)
Married	-0.019	(0.028)	-0.347*	(0.061)	1.020*	(0.255)
Children in household	0.037	(0.024)	0.416*	(0.051)	-0.206	(0.209)
Private insurance	0.094*	(0.041)	-0.333*	(0.095)	0.002	(0.361)
Education/Vocational training	0.013	(0.032)	0.215*	(0.066)	0.323	(0.282)
Out of labor force	-0.025	(0.028)	0.585*	(0.057)	-0.503*	(0.241)
Blue collar	-0.079*	(0.032)	0.094	(0.070)	-0.005	(0.269)
Selfemployed	-0.012	(0.047)	0.258*	(0.110)	-0.093	(0.410)
Civil Servant	-0.014	(0.091)	-0.417*	(0.200)	-1.412	(0.802)
Other Position	-0.066	(0.049)	-0.059	(0.110)	-0.342	(0.434)
Quarter of Interview = 2	-0.005	(0.016)	0.051	(0.038)	-0.332*	(0.142)
Quarter of Interview = 3	0.024	(0.028)	0.063	(0.069)	-0.357	(0.244)
Quarter of Interview = 4	0.027	(0.082)	0.437*	(0.168)	-2.244	(1.342)
Constant					47.520*	(2.116)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	120,636		29,563		31,543	

Standard errors in parentheses; * $p < 0.05$

Table A5.5: Fixed Effects models with plant closure- East

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	0.086	(0.164)	0.107	(0.326)	0.225	(1.705)
Months of unemployment	-0.003	(0.009)	-0.041*	(0.019)	0.019	(0.089)
28 <= Age <= 32	0.081	(0.066)	-0.028	(0.130)	-0.620	(0.545)
33 <= Age <= 37	0.098	(0.095)	0.095	(0.190)	-1.113	(0.807)
38 <= Age <= 42	0.030	(0.123)	0.429	(0.245)	-1.618	(1.035)
43 <= Age <= 47	-0.086	(0.153)	0.141	(0.302)	-2.186	(1.237)
48 <= Age <= 52	-0.006	(0.185)	0.289	(0.363)	-2.336	(1.440)
53 <= Age <= 58	-0.084	(0.218)	-0.047	(0.427)	-2.473	(1.664)
Years of education	0.006	(0.012)	-0.029	(0.027)	0.074	(0.114)
log. equiv. HH-income	0.091	(0.049)	-0.157	(0.105)	1.673*	(0.399)
Frequency of sports	0.060*	(0.014)	0.027	(0.029)	0.296*	(0.109)
Married	-0.066	(0.059)	-0.024	(0.116)	1.417*	(0.508)
Children in household	0.017	(0.040)	0.481*	(0.082)	0.467	(0.327)
Private insurance	-0.029	(0.086)	-0.152	(0.194)	-0.169	(0.736)
Education/Vocational training	0.069	(0.072)	0.449*	(0.134)	0.265	(0.629)
Out of labor force	0.067	(0.059)	0.803*	(0.115)	0.773	(0.474)
Blue collar	-0.066	(0.054)	-0.079	(0.115)	0.098	(0.449)
Selfemployed	-0.103	(0.089)	0.366	(0.196)	-0.834	(0.751)
Civil Servant	-0.249	(0.151)	-0.128	(0.310)	-1.163	(1.504)
Other Position	-0.232*	(0.090)	-0.069	(0.176)	-0.143	(0.760)
Quarter of Interview = 2	0.029	(0.038)	-0.098	(0.093)	-0.122	(0.283)
Quarter of Interview = 3	0.089	(0.091)	0.096	(0.241)	0.516	(0.688)
Quarter of Interview = 4	1.169	(0.667)			-2.491	(5.429)
Constant					36.131*	(3.681)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	38,282		11,090		9,419	

Standard errors in parentheses; * $p < 0.05$

Table A5.6: Fixed Effects models with plant closure- Above 50

	Health Satisfaction (FE Ordered logit)		No Hospital Visit (FE Logit)		Mental Health Score (Linear FE)	
Unemployed	-0.164	(0.197)	-0.440	(0.408)	1.337	(1.900)
Months of unemployment	0.004	(0.008)	-0.017	(0.015)	0.025	(0.083)
53 <= Age <= 58	-0.031	(0.045)	0.116	(0.100)	-0.278	(0.316)
Years of education	0.061	(0.039)	0.015	(0.066)	-1.254	(2.284)
log. equiv. HH-income	0.147*	(0.064)	-0.006	(0.154)	1.515*	(0.491)
Frequency of sports	0.098*	(0.019)	-0.042	(0.042)	0.174	(0.131)
Married	-0.273*	(0.106)	-0.900*	(0.263)	1.680*	(0.826)
Children in household	0.068	(0.073)	-0.418*	(0.166)	-0.551	(0.540)
Private insurance	-0.093	(0.113)	-0.236	(0.259)	-0.919	(0.941)
Education/Vocational training	-0.184	(0.111)	0.315	(0.293)	0.914	(0.792)
Out of labor force	-0.073	(0.073)	0.827*	(0.159)	-0.925	(0.650)
Blue collar	-0.068	(0.084)	-0.097	(0.195)	0.297	(0.675)
Selfemployed	0.100	(0.131)	0.169	(0.327)	1.688	(1.025)
Civil Servant	-0.229	(0.285)	-0.181	(0.654)	-2.608	(2.561)
Other Position	-0.915	(0.664)	-0.665	(0.932)	-4.777	(6.621)
Quarter of Interview = 2	-0.016	(0.037)	0.235*	(0.092)	-0.468	(0.311)
Quarter of Interview = 3	0.116	(0.071)	0.093	(0.181)	-0.599	(0.590)
Quarter of Interview = 4	0.403	(0.223)	0.672	(0.468)	-3.407	(3.299)
Constant					52.576	(28.986)
Year dummies	yes		yes		yes	
Industry dummies	yes		yes		yes	
Observations	28,418		6,236		9,157	

Standard errors in parentheses; * $p < 0.05$

Table A5.7: SF-12v2 questionnaire in the SOEP

Please think about the last four weeks. How often did it occur within this period of time, ...	Always	Often	Some- times	Almost never	Never
⋄ that you felt rushed or pressed for time?					
⋄ that you felt run-down and melancholy?					
⋄ that you felt relaxed and well-balanced?					
⋄ that you used up a lot of energy?					
⋄ that you had strong physical pains?					
⋄ that due to physical health problems					
... you achieved less than you wanted to at work or in everyday tasks?					
... you were limited in some form at work or in everyday tasks?					
⋄ that due to mental health or emotional problems					
... you achieved less than you wanted to at work or in everyday tasks?					
... you carried out your work or everyday tasks less thoroughly than usual?					
⋄ that due to physical or mental health problems you were limited socially, i.e. in contact with friends, acquaintances or relatives?					

Chapter 6

Concluding Discussions

The constantly increasing pressure on the health care system in Germany due to phenomena like the technical progress or the demographic change call for solutions that are capable of containing costs. These might either be rationing of health care services or reducing inefficiencies in the health care market, or both. Since there is a trade-off between efficiency and other goals like universal access and equity in health care utilisation, it is worthwhile to empirically assess the potential gains of increasing incentives to contain costs because they often go together with utility loss for the consumers. E.g., demand-side cost-sharing might lead to a more cost-conscious behaviour of the patient. However, it also reduces the protection of individuals against the financial risk of illness which is welfare enhancing if individuals are risk-averse.

The results of the first three chapters of this thesis show that there are indeed considerable inefficiencies that could be reduced. However, the findings imply different magnitudes of inefficiencies. I find that the, so far, existing solutions that address the demand side of health care, that is, the patients, do not show strong effects when it comes to containing health care costs. Specifically, optional deductibles or supplementary health insurance do have some impact on health care utilisation - however, only a moderate one. The reason for this is that healthy individuals - who know to need less health care services - self-select into contracts with less insurance coverage because they have the opportunity to save on the insurance premia. Therefore, the mere possibility to choose a lower insurance coverage does not seem to contribute to cost consciousness of individuals.

Moreover, Chapter 4 highlights another problem that occurs, when trying to contain costs induced by demanders of health care which is, so far, not discussed in the public. This arises if benefit packages of statutory health insurance are reduced and left for supplementary insurance on the private market. Not only are information asymmetries between health insurance company and insured individual likely to lead to moral hazard. There is also adverse or advantageous selection (sources of both partly at the same time) that lead to inefficient market outcomes in private insurance markets.

However, the results do not imply that cost-sharing as such is not suitable to contain health care costs in Germany in general. First, especially individuals with a high income are allowed to choose a deductible in the German system. It is by all means possible that the price elasticity in this special group is not representative for all households in the population. The stronger impact of private supplementary insurance on the number of doctor visits - as found in Chapter 2 - is an indication of this point because this insurance type can be purchased by the whole population. Second, and most important, deductibles which are not voluntary but mandatory (as, e.g., in the Swiss health system) might have a strong impact on the demand for health care, even in Germany.[1] Therefore, the result in this thesis is not directly comparable to the result of the famous Rand Health Insurance Experiment, which showed considerable effects of mandatory deductibles on the demand for health care

[1]Although the weak effects of the co-payment reform in 2004 imply that mandatory co-payments might only change the doctor visiting behaviour of the German population if they are not too small, see Augurzky et al. (2006); Schreyögg and Grabka (2010); Farbmacher (2009).

(Manning et al., 1987).

According to the results in Chapter 3, the supply side (e.g., the physicians) seems to bear much more possibilities to reduce inefficiencies. In Germany, there are considerable differences in doctor visiting behaviour of individuals which can be attributed to physician behaviour. Therefore, setting the right incentives for physicians seems to be a promising way to save costs while keeping quality on a high level. This should be done by a remuneration system that prevents physicians from inducing their own demand. Moreover, setting the right incentives for physicians can also be able to reduce the problem of excess-demand of patients due to moral hazard, without the need to introduce demand-side cost-sharing (with its mentioned limits). This is the case if physicians have incentives to provide less health care services than demanded by the full-cover insured patients (Ellis and McGuire, 1990).

Future work should draw on better data sets to improve the quality of the results. Administrative data sets or data from health insurances clearly have the advantage of not being reported by the patients and are, thus, less likely to suffer from measurement error. Moreover, they should be more detailed as regards health care utilisation than survey data. Strongly increased sample sizes of administrative data or data from insurance companies also make subsample analyses more feasible. However, these data sets often lack good health measures and, moreover, usually do not contain both privately and publicly insured at the same time.

Another way to improve the analysis might be to reduce the parametric assumptions in the empirical specifications. I reacted on the non-linear nature of the dependent variables by fully specifying a density function up to some parameters which were estimated by maximum-likelihood (an exception being the matching approach in Chapter 4). Although the flexible specification I used in most chapters - e.g., unobserved heterogeneity that follows an arbitrary discrete distribution or inclusion of a full set of dummy variables to account for non-linearities in exogenous variables instead of polynomials - is sometimes called “semi-parametric” in the literature, the estimation results are inconsistent if the distributional assumptions (which cannot be tested) are wrong. However, again, fully non-parametric regressions often do not seem to be feasible given the data base used in this thesis.

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