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In loving memory of

Dr. Maximilian Rüger

with deep gratitude for the gift of your love,

your patience and

your wonderful sense of humour.

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Chapter 1

Introduction

Health is commonly perceived as the most valuable or even invaluable asset in life. Statements like “Health is the greatest wealth” (European Commission 2007), “Work is important. Success has benefits; but health is everything” (WSO 2013), or “The right to health is the most valuable and important human right” (EESC 2003) emphasize the fundamental importance of health in everyday life. Indeed, to be in good health is one of the most important prerequisites of social participation nowadays, to choose between opportunities and thereby to take advantage of modern life. Nevertheless, health is not a static endowment which is equally distributed among the population and which remains constant over time. In contrast, health is constantly subject to impairment and shocks during the course of one’s entire life span, it depends on randomly distributed and uncertain factors as well as on individual behavior. Medical science has made enormous progress in acquiring knowledge regarding the prevention and treatment of diseases over the last few decades. As a result, average life expectancy has increased dramatically. Moreover, the quality of life with regard to health conditions has substantially improved.

The ability to benefit from these achievements depends essentially on one’s access to health care. However, access to health care and provision of health care is often contingent upon the financial power of the health care system and the financial endowment of health care users. This underscores the fact that although health is perceived as lifes most valuable and priceless asset, health care, which is necessary to preserve and improve health, happens to be a commodity available only for a certain price.

Grossman (1972) developed a model for the demand of health care (as a derived demand for the commodity “good health”) in which he accounted for the depreciating nature of health. He attributes two targets to health. First, health is part of the overall utility of a person and second, health (in terms of healthy days and associated income) determines the opportunities for consumption which in turn become part of the overall utility. Therefore, he interprets the consumption of health care as an investment in health that can be optimized with respect to the maximization of overall utility. Because overall utility does not depend exclusively on an individual’s health condition but also on the consumption of other goods, the optimal health stock is not necessarily equal to the maximum health stock. Hence, the optimal amount of investment (demand for health care) can be derived from this model: the optimal investment in health reflects the trade-off between health and the consumption of other goods in the overall utility.

A broader interpretation of consumption leads to what can be observed in everyday life. Although health is perceived as the most valuable asset, individuals do not decide their actions with exclusive respect to health. In contrast, decisions are based on consideration of the trade-off between health and other goods. Smoking and drinking are leading examples of utility increasing behaviors that are perils to one's own and others' health. Running red lights exemplarily reflects the time-consuming and therefore costly nature of health protective behaviors. This and many such examples emphasize that health-protective measures are indeed not priceless and individuals balance costs for health care by their respective benefits.

In general, health care services and products can be exclusive and rival in consumption and this defines them as private and scarce commodities which are traded on markets. Nevertheless, the consumption of health care can give rise to external effects (e.g. prevention of communicable diseases) and turn the particular purpose of a health care measure into a public good. If markets were perfectly competitive, the price mechanism could efficiently equalize supply and demand for health care. But, certain characteristics of the commodity health care can render the market equilibrium inefficient. These characteristics which are typical for but not confined to health care include uncertainty, externalities, and information asymmetries. Most actions in the health care market are affected by at least one of these characteristics but often a combination of them determines the agents' behavior and can lead to substantial market failures.

In addition, the overall health care market is different from other markets as it is closely interrelated with the health insurance market. In contrast to other insurances, health insurance does not directly compensate for a financial loss in the asset. The monetary value of health is not clearly identifiable since no markets and hence prices for this asset per se exist. Instead, health insurances pay for the consumption of health care products and health care services to either restore, preserve, or improve health. Health insurance leads to consumer prices for health care products and services which are below the prices that the suppliers receive. This differential in prices for a commodity creates incentives for inefficient market behavior both for consumers and suppliers.

In addition, health insurance providers interfere with the health care market itself. Depending on the institutional design of a particular health insurance, strong incentives for risk selection may appear. Systematic selection of risk types influences the amount and allocation of health care benefits. Furthermore, risk selection can – in its extreme form – lead to a collapse of the health insurance market, thereby affecting the accessibility of health care. Adverse selection, ex-ante and ex-post moral hazard and supplier-induced demand are keywords for distorted competition in health care and health insurance markets.

Without regulation, the above-mentioned features of health care render market equilibria (if existent at all) and the corresponding allocation of health care different from Pareto-optimal allocations. Applying the First Theorem of Welfare Economics, external effects of the consumption of health care (and the associated public good nature of health care) as well as the distortive competition in health insurance markets justify governmental interventions from an economic perspective. Today's extent of governmental regulation in health care markets around the world is hardly found in other markets.

Finally, technological change plays a particular role in the health care market. The demographic structure of population is continually changing (although not exclusively) due to technological progress in health care. In turn, demographic changes influence the way health insurance and therefore health care is financed. The optimal allocation of the corresponding changes in costs and benefits among the population raises important issues regarding equity and efficiency.

The uniqueness and complexity of topics and issues in health care markets make it important to analyze them discretely within economics.

Williams (1987) outlined the discipline of health economics in his famous "plumbing diagram". The established sub-disciplines in the field were discussed, amongst others, by

Maynard and Kanavos (2000) with regard to the developments in health economics until the end of the last millennium. On the basis of their definitions and following the slightly edited structure of the "plumbing diagram" by Culyer and Newhouse (2000), the scope of health economics can be summed up in eight main topics. The central four topics (A1-A4) outline the "engine room" of health economics. The other four topics (B1-B4) represent the "periphery" of health economics for which the "engine room" exists. The health economics "engine room" consists of:

A1. *Health.*

In this topic, the conceptual framework of health is outlined. This includes attributes of health, indexes of health status, the measurement of health, and the valuation and utility of health. In this field, for example, quality-of-life measures are developed which are increasingly used for the economic evaluation and prioritization of health care measures.

A2. *Determinants of health.*

This topic is concerned with the determinants of health other than health care. Analyzed influencing factors include, for example, the level and distribution of income and education, work activity, consumption patterns, and nutrition. In this field, health is analyzed as a capital stock that depreciates and which can be invested in.

A3. *Demand for health care.*

This topic concerns the demand for health care which is derived from the demand for health. The demand for health care is influenced by the first two topics as they differentiate the demand across different groups in the population (e.g. depending on socio-economic characteristics). But the demand for health care is also influenced by barriers to the access to health care such as prices, institutions, the definition of "needed health care", time costs, and psychological hurdles.

Positive and negative externalities as well as information asymmetries in the principal-agent relationships between patients and health professionals play an important role in the amount of health care that is demanded.

A4. *Supply of health care.*

This topic is broad and research questions in this field refer, for instance, to production technologies and costs of the production of health care, the markets for health care labor and capital, and the market for pharmaceuticals. Another strand of literature within this field analyzes the role of incentives in the supply of health care. Incentives for efficient health care supply generated by paying schemes for hospitals and physicians are broadly discussed in this field.

Culyer and Newhouse (2010) emphasize that the "engine room" does not exist uniquely for its own sake but that the findings of these fields deliver the necessary insights to deal with the issues of the four "peripheral" topics. These topics are:

B1. *Market analysis.*

This topic addresses how the desired equilibria defined by policy objectives in the markets described above can be attained. This includes the analysis of market mechanisms. Monetary prices, time prices, waiting lists and non-price rationing systems are analyzed as tools to balance needs on the demand side and capacities on the supply side. Direct and indirect influences of rationing devices are objects of investigation in this field. Health insurance and institutions providing health care play an important role. The results in this field are mainly positive but also normative based on the evaluation of the performance of markets and tools.

B2. *Microeconomic appraisal.*

This topic attends to the microeconomic evaluation at the treatment level. This involves applied cost-effectiveness, cost-benefit, and cost-utility analyses of alternative ways of delivering care at all phases of the health care process. Mode, place, and timing of detection, diagnosis, treatment, and aftercare are evaluated in this field. Analyses in this field are often normative as they specifically evaluate technologies and mechanisms under certain concepts of utility and value judgment.

B3. *Planning, budgeting, regulation and monitoring mechanisms.*

This topic is concerned with the evaluation of the effectiveness of available instruments for optimizing the health care system. The performance of budgetary controls, manpower allocation, norms and regulations, and the incentive structures generated by these instruments form part of this field. Explicit policy goals are required for the evaluation of the efficiency and effectiveness of planning, budgeting and monitoring mechanisms so that analyses in this field mainly concern particular health care systems and their respective institutional structures.

B4. *Evaluation at the whole system level.*

Within this topic, the performance of the outcomes of B1 and B2 on system objectives such as equity and allocative efficiency is analyzed. Further topics include differences in expenditure rates, mechanisms and outcomes across systems and countries. Not only explanations for the observed differences but the comparability of different systems and what and how to learn from other systems are discussed in this field.

In this dissertation three different topics in health economics are analyzed and discussed. These topics either contribute to the "engine room" or provide applications of central topics and contribute to the "periphery" of health economics. The core of each empirical study provided in this dissertation is a positive analysis to improve the understanding within the respective subfield. In addition, normative discussions of the results and insights derived from the positive analyses are provided for each topic in order to contribute to the discussion of current and challenging issues that health care systems are actually facing today.

In the following, I briefly outline the topics of the remaining chapters in this dissertation and their contributions to the respective subfields in health economics.

Reference-Dependent Effects of Unemployment on Mental Well-Being

Levels of health not only impact economic performances and create costs and benefits, but conversely, economic circumstances also have an effect on health conditions and create costs and benefits through the channel of health. Owing to this circular relationship between economic factors and health, it is of interest for economists, and in particular health economists, to understand how economic circumstances determine health and how health conditions affect economic performances. In Chapter 2, we focus on the employment status as a determinant for mental well-being and analyze the effect of unemployment on mental well-being. In particular, we analyze how the effect of unemployment on mental well-being is affected by expectations with regard to employment.

Contributions in this field have analyzed the relationship between health and economic factors at the aggregated as well as at the individual level. In his seminal contributions, Ruhm (2000, 2003, and 2005 as examples) provides analyses of the relationship between economic cycles and health, and shows how economic circumstances at the aggregated level determine health behavior at the individual level. His general finding is that during economic downturns health improves because during such times individuals have more time to spend on health increasing activities, whereas during cyclical upturns individuals tend to riskier health behavior. Another strand of literature examines, at the individual level, how employment status (in particular unemployment) affects health. In this literature, the outcome variable health is separated into physical and mental health. Ruhm (2003) shows that mental health (in terms of suicide rates) is affected conversely to physical health when it comes to economic downturns. He finds that mental health deteriorates during recessions. This result emphasizes the need to examine the impact of economic determinants on mental health and associated mental well-being separately from physical health.

In the literature, mental well-being is commonly used as a proxy for mental health for two reasons. Firstly, the analysis of a distinctive measure for mental health requires a clear definition of mental health and the provision of appropriate data for an empirical analysis. Mental health (illness) is defined as a diagnosable illness such as depression, anxiety, or schizophrenia which significantly interferes with an individual's cognitive, emotional or social abilities (NHSinform 2014). Alternatively, with regard to physical health, one can think about an overall mental health index, which, in turn, is closely related to the more general concept of mental well-being. A statement by the WHO (2014) underlines the generality of the concept of mental well-being: "Mental health is defined as a state of well-being in which every individual realizes his or her own potential, can cope with the normal stresses of life, can work productively and fruitfully, and is able to make a contribution to her or his community." Definitions of mental well-being generally include areas such as life satisfaction, optimism, self-esteem, mastery and feeling in control, having a purpose in life, or a sense of belonging and support (NHSinform 2014). Secondly, from an economic perspective, mental well-being can also be interpreted as overall utility, thereby providing a close connection to theoretical models which are aimed at maximizing overall utility.

The general findings in the literature on unemployment and mental well-being at the individual level state that unemployment negatively impacts mental well-being. The discussion of the role of unemployment rates in the perception of unemployment has created much controversy. The general finding here is that the severity of the negative effect of unemployment on mental well-being (the perception of unemployment) depends on the rate of unemployment. Nevertheless, contradictory statements in the empirical literature such as whether unemployment rates affect the perception of unemployment positively or negatively reveal that the mechanism behind this relationship is yet to be clarified. Furthermore, the available empirical literature on unemployment and mental well-being is not based on theoretical economic models which can explain the mechanism of this relationship and forecast the sign of the effect.

Chapter 2 of this dissertation aims to fill this gap by providing a theory-based empirical analysis of the effect of unemployment on mental well-being. The specification of our econometric model is derived from an underlying theoretical equation for mental well-being which is based on models for reference-dependence utility with endogenous reference points. Reference points in our theoretical model reflect the individual expectations about future employment status. We assume unemployment rates as an information on which the formation of the reference point is based, i.e. expectations about future employment status are derived from unemployment rates in the individually relevant labor market. The reference point defines a status quo for future employment status. The individual valuation of the actual employment status depends on deviations from the reference point. In particular, we hypothesize that the negative effect of unemployment on mental well-being is more severe in the case of unexpected unemployment. The results of the estimation confirm the standard finding that unemployment has a negative impact on mental well-being. As the central outcome, the econometric analysis yields empirical evidence for a reference-dependent relationship between unemployment and mental well-being. The results suggest that becoming unemployed unexpectedly hurts individuals harder than when unemployment was expected. The additional negative effect that is due to the unexpectedness of unemployment is not only statistically significant but also substantial in its estimated size which is equal to one standard deviation of mental well-being in the sample and about three-fourths of the mean difference in mental well-being between the employed and the unemployed.

Chapter 2 of this dissertation contributes to the literature in a twofold way. Firstly, the empirical analysis that we apply in order to test hypotheses derived from a theoretical model with endogenous reference points provides an explanation of how unemployment rates affect the perception of unemployment. Our findings improve the understanding of unemployment as a determinant for mental well-being. Furthermore, by applying a theoretical model which has its foundation in the emerging field of behavioral economics, we expand the hitherto available approaches to this topic. Secondly, the results of our analysis support theoretical models with endogenous reference points by providing empirical evidence. Although health economics is usually seen as an applied field of economics that uses toolkits from more traditional fields, the findings and insights derived from health economics also contribute to the progress in other fields in economics.

Public and Private Health Insurance in Germany: The Ignored Risk Selection Problem

Chapter 3 of this dissertation provides an analysis of risk selection problems in the German health insurance market with dual sources of financing. This chapter is based on joint work with Robert Nuscheler. Theoretical models for health insurance predict certain market outcomes and failures, depending on the distribution of risks and the observability of risk types by the insurance companies. If the individual risk is observable, the insurer can offer actuarially fair premiums to the insured (i.e. premiums which are equal to the expected health care costs), which would lead to an efficient market outcome. With asymmetric information about the risk types, full coverage with actuarially fair premiums can no longer be offered for all risk types. Problems of adverse selection and moral hazard can appear as long as high-risk individuals have incentives to mimic low-risk individuals and in order to aquire insurance coverage at low-risk premiums.

Moreover, if the risk is observable but the insurer cannot use this information to discriminate between risk types, the same problems can arise than when risk is unobservable. This is the case in many health insurance markets. Although the risk type might be common information, health insurers are obliged to provide community rating and open enrollment. These institutional characteristics can result from governmental and social objectives of equity but also from potential adverse selection and crowding out of high risks in the case of unobservable risk types. Community rating and open enrollment in turn induce incentives for insurance providers to engage in risk selection. With premiums that are unrelated to the individual risk, insurance providers have strong incentives to select low-risk individuals with lower expected health care expenditures compared to high-risk individuals in order to maximize profits. With successful risk selection, insurance providers may gain a competitive advantage over other insurance companies which does not stem from superior efficiency.

This market failure justifies public policy interventions targeted at reducing incentives for risk selection. A common regulatory measure is risk adjustment. Within risk adjustment schemes, insurance companies are compensated for different structures of their risk pool in order to reduce competitive advantages that only stem from a better risk structure.

Risk selection within public health insurance systems has received considerable attention. Although in many countries there are not only pure public or private sectors but a mixture of both types, risk selection between systems has largely been ignored so far.

Institutional differences between public and private systems can induce strong incentives for risk selection between both systems. This is the case for the German health insurance system where there are two branches of health care financing. Several institutional differences between public and private health insurance provide strong incentives for risk selection by the insurance providers and for an elaborate selection of an insurer on part of the insured. The main institutional difference that affects this self-selection is the calculation of premiums. While in the public system premiums are based on income and are unrelated to the individual risk, premiums in the private system are risk-based and unrelated to income. This induces strong incentives for low-risk-high-income-earners to select themselves into the private system where they can benefit from risk-related premiums that are unaffected by their relatively high income. Incentives for risk selection on the supplier side in the private system are straightforward. As soon as claimed health care expenditures impend to exceed the expected expenditures which are covered by the risk-rated premium, the private insurers have strong incentives to crowd the high risks out. Due to compulsory health insurance and open enrollment, high-risk individuals are consequently dumped into the public system. As there is no risk adjustment between both systems, private insurers do not have to compensate public insurers for this shift in the risk structure. The empirical analysis in Chapter 3 clearly shows that the incentives due to institutional differences between the public and the private health insurance systems in Germany do indeed lead to observable risk selection between both systems.

Within the "engine room" of health economics, Chapter 3 contributes to the topic A3. Due to the systematic selection of risk types in the public and private systems, the risk-type-corresponding-demand for health care is inefficiently shifted between both systems. Access to health care and benefits are different between the public and private system. Consequently, the individual demand for health care is affected by the analyzed risk selection. Nevertheless, the main contribution of Chapter 3 is to the "peripheral" topic B1. Health insurance providers as the financial link between providers and consumers in the market for health care play a crucial intermediary role in equalizing the demand and supply of health care. Therefore, a stabilized health insurance market with fair competition is essential for balancing the health care market as a whole. We identify risk selection as a competition-distorting factor in the German health insurance market. Derived from our empirical findings, we propose the integration of the private health insurance market into the risk adjustment scheme that already exists within the public system, in order to minimize incentives for risk selection and to increase fairness in the competition between the two systems.

The Effectiveness of Breast Cancer Screening in Germany: Evidence from a Natural Experiment

Chapter 4 of this dissertation analyzes the effectiveness of the organized breast cancer screening in Germany. Several approaches have been proposed in the literature to evaluate the effectiveness of health care procedures (e.g., cost-effectiveness-analysis, cost-utility-analysis, cost-benefit-analysis). The need for the evaluation of health care measures is derived directly from the question of how to allocate the scarce commodity health care. The evaluation of the effectiveness of an action is a classical approach in economics, and the evaluation of the effectiveness of prevention measures is a central topic in health economics. Prevention methods have been originally classified into three levels by Caplan (1964, p.16) and can be summarized as follows:

1. Primary prevention aims to prevent diseases from occurring.

2. Secondary prevention focusses on early detection and treatment of a disease which has already developed in a person but does not yet show any clinical symptoms, e.g., before the patient notices any indications of the disease.

3. Tertiary prevention is applied when the disease already shows symptoms. It aims to influence the course of disease in order to prevent damages, pain, and complications, to give better care to the patients, and to cure from the disease or slow down its course.

Organized screening programs for breast cancer are part of secondary prevention. The current state of medicine cannot prevent the development of breast cancer. But the treatment of this disease has considerably improved over the last few decades, and survival rates promise increasing chances of non-lethal courses of breast cancer. Nevertheless, survival rates strongly correlate with the stage at which breast cancer is treated. The earlier breast cancer is treated, the higher is the statistical chance of survival. The 5-year relative survival rate for breast cancer detected at stages 0 and I is 100%, 93% at stage II, 72% at stage III, and only 22% at stage IV (SEER 2013). Therefore, early detection of breast cancer is the most important first step in the prevention of this disease today.

Organized mammography screening, e.g., mass screening of clinically asymptomatic women, has been implemented in many countries around the world to detect breast cancer as early as possible in as many women as possible. However, while mammography is accepted as a proven diagnosis tool for breast cancer, the discussion in the literature of the effectiveness of organized screening programs has attracted some controversy. This includes not only the effectiveness regarding the detection rates but also the impact of the side effects that are induced by such mass screening programs. Leading examples of such side effects include false-positive test results and overdiagnosis with subsequent overtreatment. This means that patients with no cancer or patients with cancer that during the patient's lifetime would never have caused any symptoms are unnecessarily treated. Side effects not only increase the costs but also lower the benefit of the screening program. The primary goal of organized mammography screening programs is the reduction of breast cancer mortality. Thus, a reduction in breast cancer mortality provides a crucial argument for the effectiveness of organized mammography screening programs. The costs would not only include direct costs but also indirect costs (e.g. psychological costs) of the screening.

For Germany, where an organized mammography screening program has gradually been implemented nationwide since 2001, no analysis of the effectiveness of this program has been attempted so far. In Chapter 4 of this dissertation, which is based on joint work with Salmai Qari, an empirical analysis of the effectiveness of the German mammography screening program is provided and discussed. We exploit the step-by-step implementation of the program at the regional level to analyze the effectiveness of the organized screening program in the setting of a natural experiment. Our results suggest no effect of the organized screening program on breast cancer mortality.

The evaluation of the German mammography screening program in Chapter 4 contributes to the "peripheral" field B2 and to the heated international debate on the impact of organized screening programs. Although normative analyses with certain assumptions about utility measures and value judgments are common in this field, our analysis only exploits the effectiveness of the screening program with regard to mortality without any judgments on the value of life and health. This becomes necessary if the effectiveness of the program was weighted against the costs of the program. Data to quantify the costs of the German mammography screening program is not yet available, and therefore, we only concentrate on the effectiveness of the program as a first step in the evaluation process. Yet, our results underscore the importance of further and more detailed investigations in order to reveal the factors that render the program ineffective. Nevertheless, such analyses require the provision of more detailed data which are currently unavailable for common scientific use.

Chapter 2

Reference-Dependent Effects of Unemployment on Mental Well-Being

2.1 Introduction

The relationship of unemployment and health was, amongst others, discussed in a series of papers by Ruhm (e.g. 2000, 2003, 2005), who found for the US that unemployment rates are negatively correlated with mortality rates, health care utilization and chronic conditions. Interpreting mortality rates as a proxy for health, he concludes that with decreasing macro-economic circumstances health increases. For Germany, this finding was confirmed by Neumayer (2004). Ruhm reasons that people have more time for health increasing activities during recessions and tend to more risky health behavior during economic upturns (smoking, drinking, etc.). When he analyzes the effect of unemployment rates on case-specific mortality rates and specific chronic diseases, he finds that only the variation in suicides and mental illness is procyclical in macro-economic conditions, i.e. suicide rates and the number of mental health problems increase when unemployment rates increase. He concludes that mental health and mental well-being behave in sharp contrast to physical well-being (Ruhm 2003, p. 655). Therefore, the relationship between mental well-being and economic conditions should be analyzed separately from physical health conditions.

In addition, the number of reported mental health problems has been steadily increasing in recent years. Health care expenditures caused by mental illnesses are increasing above average compared to expenditures for physical health problems (Destatis 2010). As it seems that mental well-being is affected differently by (macro-) economic circumstances than physical health, it is of particular importance for health economists to understand what determines mental well-being.

On the individual level, Clark and Oswald (1994) established the general result that subjective well-being is negatively affected by unemployment. Winkelmann and Winkelmann (1998) disentangled the negative effect of unemployment on life satisfaction into a pecuniary and a non-pecuniary effect. The non-pecuniary effect is the psychological burden of unemployment that arises in addition to the loss of income which characterizes the economic burden of unemployment. They found that the non-pecuniary effect is much larger than the effect that stems from the loss of income that is associated with unemployment.

Other studies (e.g. Stutzer and Lalive 2004, Shields et al. 2008, Clark et al. 2009) find that the negative effect of unemployment on mental well-being itself is related to the regional unemployment rate. This result is usually discussed in the context of social norms. The studies' general findings state that being unemployed in a region with a high unemployment rate has a smaller negative effect on mental well-being than being unemployed in a region with low unemployment rates. In high-unemployment-rate regions, being unemployed means to be conform to the social norm of unemployment. The results suggest that not deviating from the social norm lowers the psychological burden of unemployment. In contrast, Vatter (2012) found that subjective well-being in East Germany where unemployment rates are considerably higher than in West Germany is more affected by unemployment. He argues that lower job prospects in high-unemployment-rate regions increase the negative effect of unemployment. Clark et al. (2010), and Knabe and Rätzel (2011) provide empirical evidence for this relationship, and show that negative expectations about becoming re-employed in the future additionally reduce subjective well-being among the unemployed. These studies conclude that the negative effect of unemployment on mental well-being is heterogenous among individuals. Furthermore, the size of the negative effect depends on unemployment rates and future job prospects. Nevertheless, contradictory statements in the economic literature regarding in which direction unemployment rates affect the perception of unemployment reveal that the mechanism behind this relationship is yet to be clarified.

De Witte (1999) provides a review of the psychological literature on the relationship between perceived job insecurity and psychological well-being. He summarizes from the literature that job insecurity significantly reduces well-being in different psychological domains. Furthermore, he analyzes the question of how important the factor job insecurity is within the effect overall of unemployment. His empirical findings suggest that the anticipation of unemployment has the same impact as unemployment on psychological well-being. His results confirm a statement which Lazarus already made in 1966, that "the anticipation of harm can have effects as potent as experiencing the harm itself" (quoted by Roskies et al. 1993, p.619).

Dekker and Schaufeli (1995, p.58) state that in the psychological literature it has become apparent that the phase of job insecurity, in which termination is more or less anticipated, may very well be the most stressful aspect of the whole unemployment process. They compare two groups of employees in a large Australian public transport organization, who at the same time faced uncertainties about whether or not they will become unemployed due to organizational changes. They find that the psychological well-being of those who became unemployed in the next period improved compared to those who were still uncertain at this point. This result indicates that uncertainty about the future employment status not only affects mental well-being directly but also the perception of the unemployment status. Although one group of employees was finally made redundant, they experienced an increase in their psychological well-being. They felt relieved from their uncertainty because they became unemployed according to their expectation.

Green et al. (2000) on the other hand, analyzed which factors determine perceived job insecurity. They find that for the employed higher levels of unemployment rates increase perceived job insecurity, and higher levels and increases in unemployment rates also increase perceived difficulties of re-employment for the unemployed.

In this chapter, we bring together findings from the economic and psychologic literature on unemployment and mental well-being and provide an explanation of the underlying mechanism of how unemployment rates and the anticipation of unemployment affect the perception of unemployment. Furthermore, our analysis is based on economic theory. The theoretical foundation for the econometric analysis comes from models with reference-dependent preferences with endogenous reference points, developed in the behavioral economics literature. These models formalize the effect of the anticipation of an event as well as the effect of a deviation from an individual's expectation about the outcome for this event.

Our analysis differs from previous studies in two more points. First, we analyze changes rather than levels of the employment status. Second, we account not only for the influence of current unemployment rates or job prospects on current mental well-being, but we also account for the effect of expectations about future employment status and deviations from the expected employment status on the perception of unemployment. From a prospect-theoretical point of view, it seems more plausible that changes in the employment status rather than the absolute status influence mental well-being, and that the valuation of unemployment depends on a certain reference point. Kahneman and Tversky (1979, p.277) state: "...the carriers of value are changes in wealth or welfare, rather than final states. This assumption is compatible with basic principles of perception and judgment. Our perceptual apparatus is attuned to the evaluation of changes or differences rather than to the evaluation of absolute magnitudes. When we respond to attributes such as brightness, loudness, or temperature, the past and present context of experience defines an adaptation level, or reference point, and stimuli are perceived in relation to this reference point." Therefore, differences in the perception of unemployment regarding mental well-being are probably not only the result of social norms that are somehow derived from unemployment rates, but of potential deviations of the individual employment status from what an individual had expected, i.e. his reference point.

The literature on reference-dependence provides a discussion of the determination of reference points and mainly distinguishes exogenous and endogenous reference points. For our analysis, the concept of endogenous expectation-based reference points introduced by Kőszegi and Rabin (2006, 2007, 2009) is applied. They propose that the individual's reference-point is determined by lagged expectations about outcomes rather than the status quo. Several recent studies focussed on the empirical evidence of reference points that are determined by expectations. Abeler et al. (2011) show in a real-effort laboratory experiment that labor supply is in line with the predictions of models with reference dependent preferences where reference points are formed by expectations. Crawford and Meng (2011) re-analyze the labor supply of New York City cab drivers and find empirical evidence for reference-dependent preferences with expectation-based reference points. Card and Dahl (2011) analyze violent behavior dependent on outcomes of football games. They find that for unexpected losses of the home team domestic violence significantly increases whereas expected losses of the football team have no significant effect on domestic violence.

In our context, unemployment rates serve as an information that determines reference points of the individuals, and the magnitude of changes in mental well-being is related to the deviation of this reference point. More precisely, we assume that individuals observe relevant unemployment rates (e.g. industrial sector specific or regional unemployment rates) and that this information is used to build expectations about the future employment status. These current expectations serve as the reference point for future employment status.[1] Finally, the individuals compare the actual outcome of their employment status with their expected outcome. If the actual employment status deviates from the expected employment status, we expect a larger effect from this outcome compared to the effect that arises when the actual employment status was already expected. More precisely for unemployment, we hypothesize that becoming unemployed has a more severe effect on mental well-being when unemployment hits the individual by surprise rather than having been anticipated.

To test this hypothesis empirically, it is essential to control for any unobserved individual level heterogeneity in mental well-being. As we focus on becoming unemployed rather than being unemployed, this naturally leads to a fixed effects estimator. We use the waves from 1998 to 2009 provided by the German Socio Economic Panel (SOEP) that provides all relevant information for our analysis.

In Section 2.2, we develop a simple theoretical model which motivates our empirical analysis. In Section 2.3, we explain the regression model and the estimation strategy. Section 2.4 provides detailed information on the data set and variables used for the estimation. In Section 2.5, we show and interpret the estimated effects. Finally, Section 2.6 concludes.

2.2 Theoretical Framework

Theoretical models for reference-dependent preferences with endogenous reference points based on expectations from the behavioral economics literature deliver the theoretical back-

[1] We do not explicitly model the reference points formation process neither in the theoretical model nor in the empirical models. We can refrain from this as variables that explicitly measure the individual reference points are available for our empirical analysis.

ground to our problem. These models support the idea that an individual is more affected by an outcome of an event that was not expected than when the same outcome was expected (see Section 2.1).

To motivate and structure the empirical analysis of reference-dependent effects of unemployment on mental well-being, we borrow the formal structure of these theoretical models and substitute utility with the state of mental well-being. We can then formalize the following theoretical model:

$$M_{it}(x_{it}, x_{it-1}, q_{it}, q_{it-1}) = u(x_{it}, x_{it-1}) + v(q_{it}, q_{it-1}) + \mu\left((1 - x_{it}) - q_{it-1}\right) \tag{2.1}$$

$$\text{with} \quad x_{it} = \begin{cases} 1 & \text{if } i \text{ unemployed in } t \\ 0 & \text{if } i \text{ employed in } t \end{cases}$$

$$\text{and} \quad q_{it} = \begin{cases} 1 & \text{if } i \text{ has positive expectations in } t \\ 0 & \text{if } i \text{ has negative expectations in } t \end{cases}$$

Overall mental well-being $M_{it}(\cdot)$ for individual i at time t depends on the employment status x in t and $t-1$, $u(\cdot)$; on expectations about the future employment status q in t and $t-1$, $v(\cdot)$; and from a deviation of the current employment status in t from the expected employment status for t, $\mu(\cdot)$.

x_{it} describes the current employment status in t and takes the value 1 if the individual is unemployed in t and 0 if he is employed in t.

Expectations q are defined to be positive if an individual expects to be employed, and to be negative if the individual expects to be unemployed in the future. For simplicity, we assume a binary outcome for expectations and q_{it} equals 1 for positive expectations in t about the employment status in $t+1$, and q_{it} equals 0 for negative expectations in t about the employment status in $t+1$.

As $M_{it}(\cdot)$ depends on employment status at two different points in time, t and $t-1$, we can distinguish four different cases of employment histories:

(1) i is employed in t and $t-1$

(2) i is unemployed in t and employed in $t-1$

(3) i is employed in t and unemployed in $t-1$

(4) i is unemployed in t and $t-1$.

Table 2.1 summarizes the four different cases.

Table 2.1: Employment histories

x_{it-1} \ x_{it}	0	1
0	(00)	(01)
1	(10)	(11)

Moreover, $M_{it}(\cdot)$ depends on expectations in t and $t-1$. Similarly to the employment histories, we can distinguish four different cases for the expectations histories:

(i) negative expectations in $t-1$ and t

(ii) negative expectations in $t-1$ and positive expectations in t

(iii) positive expectations in $t-1$ and negative expectations in t

(iv) positive expectations in $t-1$ and t

The four cases of expectations histories can appear in each of the four cases of employment histories. Therefore, we can finally distinguish 16 different types of individuals regarding their unemployment status and expectations over two periods in time. Table 2.2 shows the different combinations of expectations and employment histories.

Table 2.2 can be summarized in a compact employment-expectations matrix form with j rows and k columns.

Table 2.2: Expectations and employment histories

	x_{it-1}	0		1	
q_{it-1}	x_{it} / q_{it}	0	1	0	1
0	0	(0000)	(0100)	(1000)	(1100)
	1	(0001)	(0101)	(1001)	(1101)
1	0	(0010)	(0110)	(1010)	(1110)
	1	(0011)	(0111)	(1011)	(1111)

$$Z = \begin{pmatrix} 0000 & 0100 & 1000 & 1100 \\ 0001 & 0101 & 1001 & 1101 \\ 0010 & 0110 & 1010 & 1110 \\ 0011 & 0111 & 1011 & 1111 \end{pmatrix}$$

Each element z_{jk} of the matrix contains the following information:

$$z_{jk} = (x_{it-1} \quad x_{it} \quad q_{it-1} \quad q_{it}) \text{ with } j = 1, ..., 4 \text{ and } k = 1, ..., 4. \tag{2.2}$$

All individuals in the first row of the employment-expectations matrix where employed in $t-1$ and t. All individuals in the second row where employed in $t-1$ and unemployed in t. All individuals in the third row where unemployed in $t-1$ and employed in t. All individuals in the last row where unemployed in $t-1$ and t. All individuals in the first column had negative expectations in $t-1$ and t. All individuals in the second column had negative expectations in $t-1$ and positive expectations in t. All individuals in the third column had positive expectations in $t-1$ and negative expectations in t. All individuals in the last column had positive expectations in $t-1$ and t.

For example, the individual denoted (0000) was employed in $t-1$ and t and had negative expectations in $t-1$ and t, whereas the individual (0101) was employed in $t-1$, unemployed in t, had negative expectations in $t-1$ and positive expectations in t. Therefore, individuals

(0110) and (0111) were employed in $t-1$ but became unemployed in t, although they had positive expectations about their employment status in $t-1$. Thus, these individuals became unemployed unexpectedly. Respectively, individuals (1110) and (1111) remained unemployed unexpectedly.

Based on the current empirical literature on unemployment and mental well-being and the theoretical literature on reference-dependence, the following two hypothesis on the relationship between unemployment and mental well-being can be formulated:

1. In the case of becoming unemployed, mental well-being deteriorates and in the case of becoming employed, mental well-being increases.

2. If an individual expected his current employment status, his mental well-being is less affected by the outcome of his actual employment status than when he did not have expect his current employment status. More precisely, in the case of unemployment if an individual expected to become unemployed then the negative effect of becoming unemployed on mental well-being is less pronounced than when he did not have expect to become unemployed.

In Section 2.3, both the structure of the theoretical model and the consequential types of individuals are used to develop an econometric model which allows us to test these hypotheses empirically.

2.3 Empirical Strategy

2.3.1 Empirical Model

In order to identify reference-dependent effects of unemployment on mental well-being empirically, we translate Equation 2.1 into two different econometric models. Firstly, a dummy variable model for all possible combinations of employment status and expectations in both periods. Secondly, a model with pairwise interactions for employment status and expectations in both periods.

The dummy variable model follows straightforward from the theoretical model, where 16 different cases of employment and expectations histories were distinguished. A dummy variable d is used for each of the cases. For simplicity, we preliminarily abstract from any additional influencing factors as well as from unobserved heterogeneity (both will be introduced in the second regression model). We can write the following compact form of a linear regression model with y_{it} measuring mental well-being of individual i at time t:

$$y_{it} = \pi_0 + \sum_{j=1}^{4}\sum_{k=1}^{4} \pi_{jk} d_{jkit} - \pi_{11} d_{11it} + \epsilon_{it} \tag{2.3}$$

$$\text{with} \quad d_{jkit} = \begin{cases} 1 & \text{if } (x_{it-1} \;\; x_{it} \;\; q_{it-1} \;\; q_{it}) = z_{jk} \\ 0 & \text{otherwise} \end{cases}$$

Expanding Equation 2.3 yields the following dummy variable model:

$$\begin{aligned} y_{it} = \pi_0 &+ \pi_{12}(0100)_{it} + \pi_{13}(1000)_{it} + \pi_{14}(1100)_{it} \\ &+ \pi_{21}(0001)_{it} + \pi_{22}(0101)_{it} + \pi_{23}(1001)_{it} + \pi_{24}(1101)_{it} \\ &+ \pi_{31}(0010)_{it} + \pi_{32}(0110)_{it} + \pi_{33}(1010)_{it} + \pi_{34}(1110)_{it} \\ &+ \pi_{41}(0011)_{it} + \pi_{42}(0111)_{it} + \pi_{43}(1011)_{it} + \pi_{44}(1111)_{it} \\ &+ \epsilon_{it} \end{aligned} \tag{2.4}$$

Individual (0000) is arbitrarily chosen as the reference category. This model allows an immediate comparison of the mental well-being of different individuals. For example, π_{31} reflects the difference in mental well-being of an individual who in $t-1$ expected to stay employed in t and the reference individual who in $t-1$ did not expect to stay employed in t, all else equal. In spite of its attractiveness for an easy comparison of individuals, this model does not allow for a non-ambiguous identification of reference-dependent effects of unemployment on mental well-being. Suppose we were interested in the effect of becoming unemployed unexpectedly. As already shown in the theoretical model in Section 2.2, this situation is given in two cases. In the dummy variable model, the effect of unexpected unemployment is captured by the coefficients of all individuals who where employed in $t-1$ and are unemployed in t and had positive expectations in $t-1$. In this example, these are the coefficients π_{32} and π_{42} (for individuals (0110) and (0111)). Both coefficients contain the effect of a deviation of the current employment status in t from the expected employment status for t. But, these two individuals differ in their current expectations in t about their future employment status in $t+1$. This difference is also captured by the coefficients π_{32} and π_{42}. Therefore, such a dummy variable model does not allow the unique identification of reference-dependent effects of unemployment. However, the structure of this model supports the later interpretation of the following econometric model with pairwise interactions of unemployment and expectations.

So far, we have not explicitly distinguished between different expectations about future employment status of the employed and the unemployed. The employed individuals build expectations about becoming unemployed or staying employed in the future. In contrast, the unemployed individuals build expectations about becoming re-employed or staying unemployed in the future. In the pairwise interacted model, we will differentiate between the expectations of the employed and the unemployed (as it is also done in the data, see Section 2.4). For the employed individual i, the expectation in t about his employment status in $t+1$ is denoted by $\overline{q}_{it}$. The expectation of an unemployed individual i in t about his employment status in $t+1$ is denoted by $\underline{q}_{it}$. The outcomes of both variables are defined analogous to the general expectation q_{it} in Equation 2.1:

$$\overline{q}_{it} = \begin{cases} 1 & \text{if the employed } i \text{ in } t \text{ expects to stay employed in } t+1 \\ 0 & \text{if the employed } i \text{ in } t \text{ expects to become unemployed in } t+1 \end{cases}$$

$$\underline{q}_{it} = \begin{cases} 1 & \text{if the unemployed } i \text{ in } t \text{ expects to become re-employed in } t+1 \\ 0 & \text{if the unemployed } i \text{ in } t \text{ expects to stay unemployed in } t+1 \end{cases}$$

In order to keep the notation easy, we formerly abstracted from the distinction between expectations of employed and unemployed. Nevertheless, because expectations $\overline{q}_{it}$ and $\underline{q}_{it}$ are mutually exclusive for individual i in t, the distinction between $\overline{q}_{it}$ and $\underline{q}_{it}$ was implicitly done before in the theoretical and the dummy variable model.

With $\overline{q}_{it}$ and $\underline{q}_{it}$, the following pairwise interacted model that corresponds to Equations 2.1 and 2.3 can be obtained:

$$\begin{aligned} y_{ist} = \beta_0 &+ \beta_1 x_{ist} + \beta_2 x_{ist-1} \\ &+ \beta_3 \overline{q}_{ist} + \beta_4 \underline{q}_{ist} \\ &+ \beta_5 \overline{q}_{ist-1} + \beta_6 \underline{q}_{ist-1} \\ &+ \beta_7 \left(x_{ist} \times x_{ist-1}\right) \\ &+ \beta_8 \left(\overline{q}_{ist} \times \overline{q}_{ist-1}\right) + \beta_9 \left(\overline{q}_{ist} \times \underline{q}_{ist-1}\right) \\ &+ \beta_{10} \left(\underline{q}_{ist} \times \overline{q}_{ist-1}\right) + \beta_{11} \left(\underline{q}_{ist} \times \underline{q}_{ist-1}\right) \\ &+ \beta_{12} \left(\overline{q}_{ist-1} \times x_{ist}\right) + \beta_{13} \left(\underline{q}_{ist-1} \times x_{ist}\right) \\ &+ \beta_{14} \left(\overline{q}_{ist} \times x_{ist-1}\right) + \beta_{15} \left(\underline{q}_{ist} \times x_{ist-1}\right) \\ &+ w_{ist}\boldsymbol{\beta} + \alpha_i + \delta_s + \lambda_t + \left(\delta_s \times \lambda_t\right) + \varepsilon_{ist} \end{aligned} \tag{2.5}$$

y_{ist} measuring mental well-being of individual i in federal state s at time t. As aforementioned, it is only assumed that people use the information of a certain unemployment rate to build expectations about their own employment status (see Section 2.1). In the empirical analysis, we focus on unemployment rates at the federal state level.[2] In order to control for possible correlation between individuals at this level, the federal state in which each individual lives is is incorporated through the subscript s.

As before, x_{ist} takes the value 1 if the individual i in federal state s is unemployed in t. $\overline{q}_{ist}$ and $\underline{q}_{ist}$ take the value 1 for positive expectations in t about the future employment status in $t+1$ of the employed and the unemployed in federal state s, respectively.

To measure causal effects of unemployment on mental well-being, it is necessary to control for any other factors that influence mental well-being as well as unemployment. w_{ist} is a vector of control variables at the individual level. α_i captures all time-invariant unobserved individual heterogeneity. δ_s captures all time-invariant unobserved heterogeneity at the federal state level, and λ_t captures time-fixed effects. The interaction of δ_s and λ_t controls for all federal state specific effects that vary over time. This includes, for example, unemployment rates at the federal state level, but also, more generally, all time-variant unobserved heterogeneity between federal states. Modeling explicitly federal state specific time-variant heterogeneity, captures all possible correlation between individuals in the same federal state. Instead of interactions, clustered standard errors at the federal states level could have been used to allow for correlation between individuals in the same federal states. But, clustered standard errors at this level impose problems regarding those individuals who move between federal states. Clustering in this case would need additional correction of degrees of freedom in the model. We also avoid the alternative to just exclude all individuals who moved between federal states as this would not only impose a general loss of information, but could also lead to biased estimates due to selection if individuals, who move between federal states, systematically differ in their characteristics from individuals who do not move. ε_{ist} is the

[2]However, as a robustness check, we have run the same regression models with data on the industrial sector level (2-digit NACE code) rather than on the federal state level. The regression with industrial sector fixed effects yields estimates very similar to the estimates of the regression with federal state fixed effects. Therefore, the results of the regression with industrial sector fixed effects are relegated to the Appendix of this chapter and are not further discussed (compare Table 2.8 in Section 2.5.3 and Table 2.21 in the Appendix.)

usual idiosyncratic error term.

The coefficients β_1 to β_6 measure direct effects from current and past unemployment and current and past positive expectations. Therefore, the linear coefficients β_1 to β_6 reflect the functions $u(\cdot)$ and $v(\cdot)$ in Equation 2.1 if we assume linearity for $u(\cdot)$ and $v(\cdot)$. The coefficients β_7 to β_{15} measure the effects of all possible pairwise interactions of the employment status and expectations in two subsequent periods of time. Although β_{12} and β_{13} are effects of unexpected unemployment, we cannot derive the function $\mu(\cdot)$ in Equation 2.1 straightforward from this model. Only certain linear combinations of coefficients allow the interpretation of effects as a reflection of $\mu(\cdot)$. When interpreting the estimated effects in Section 2.5, this will be explained in detail. Table 2.3 provides a detailed interpretation of those coefficients in the model that are related to the employment status and expectations.

Table 2.3: Variables, coefficients, and corresponding measured effects

Variable	Coefficient	Effect of
x_{ist}	β_1	current unemployment
x_{ist-1}	β_2	past unemployment
$\overline{q}_{ist}$	β_3	current positive expectations of the currently employed
$\underline{q}_{ist}$	β_4	current positive expectations of the currently unemployed
$\overline{q}_{ist-1}$	β_5	past positive expectations of the previously employed
$\underline{q}_{ist-1}$	β_6	previous positive expectations of the previously unemployed
$x_{ist} \times x_{ist-1}$	β_7	continued unemployment
$\overline{q}_{ist} \times \overline{q}_{ist-1}$	β_8	continued positive expectations of the continuously employed
$\overline{q}_{ist} \times \underline{q}_{ist-1}$	β_9	continued positive expectations of the previously unemployed and currently employed
$\underline{q}_{ist} \times \overline{q}_{ist-1}$	β_{10}	continued positive expectations of the previously employed and the currently unemployed
$\underline{q}_{ist} \times \underline{q}_{ist-1}$	β_{11}	continued positive expectations of the continuously unemployed
$\overline{q}_{ist-1} \times x_{ist}$	β_{12}	past positive expectations of the previously employed and current unemployment
$\underline{q}_{ist-1} \times x_{ist}$	β_{13}	past positive expectations of the previously unemployed and continued unemployment
$\overline{q}_{ist} \times x_{ist-1}$	β_{14}	current positive expectations of the currently employed and the previous unemployment
$\underline{q}_{ist} \times x_{ist-1}$	β_{15}	current positive expectations of the currently unemployed and the previous unemployment

In order to find the effects that uniquely identify reference-dependent effects of becoming or staying unemployed, we can link the pairwise interacted model to the dummy variable model. Table 2.4 shows the relevant coefficients for each of the 16 cases. As shown in Section 2.2, the individuals (0110) and (0111) are those of interest as they became unemployed unexpectedly in t. The only difference between these two are their expectations in t. From Table 2.4 we can see that individual (0111) differs from individual (0110) in the coefficients β_4 and β_{10}. Both effects stem from the positive expectations that individual (0110) has in t compared to individual (0111). The coefficient that is unique for both individuals is β_{12}. This effect stems from the combination of positive expectations in $t-1$, employment in $t-1$ and unemployment in t, i.e. unexpected unemployment. Analogous, for individuals (1110) and (1111), we find β_{13} to be the coefficient that identifies the effect of remaining unemployed unexpectedly as β_{13} stems from the combination of being unemployed in $t-1$ and t but having positive expectations in $t-1$. Therefore, for becoming or staying unemployed, the coefficients β_{12} and β_{13} uniquely identify reference-dependent effects from unemployment on mental well-being, respectively. However, in order to have a meaningful comparison of individuals, it will be necessary to compare certain linear combinations of coefficients. To test the hypothesis that an individual who became unemployed unexpectedly suffers more from becoming unemployed than an individual who already expected unemployment the linear combination of β_5 and β_{12} (and additionally β_{10} in the case of positive expectations in t) is tested whether it is different from zero. The prediction is that this linear combination is negative, reflecting the additional negative effect that stems from the deviation of the expected employment status (i.e. 'employed in t') from the actual employment status ('unemployed in t'). The detailed outline for the interpretation of the results is given in Section 2.5.2.

2.3.2 Estimation strategy

In order to identify a causal effect of unemployment on mental well-being we need to control for any heterogeneity that influences both mental well-being and unemployment. Panel data allow to account for any observed and unobserved determinants that are invariant over time or invariant over individuals or both. With interactions between federal states

Table 2.4: Interpretation of coefficients

	x_{it-1}	0		1	
q_{it-1}	q_{it} \ x_{it}	0	1	0	1
0	0	(0000) β_0	(0100) $\beta_0 + \beta_1$	(1000) $\beta_0 + \beta_2$	(1100) $\beta_0 + \beta_1 + \beta_2 + \beta_7$
	1	(0001) $\beta_0 + \beta_3$	(0101) $\beta_0 + \beta_1 + \beta_4$	(1001) $\beta_0 + \beta_2 + \beta_3 + \beta_{14}$	(1101) $\beta_0 + \beta_1 + \beta_2 + \beta_4$ $+\beta_7 + \beta_{15}$
1	0	(0010) $\beta_0 + \beta_5$	(0110) $\beta_0 + \beta_1 + \beta_5 + \beta_{12}$	(1010) $\beta_0 + \beta_2 + \beta_6$	(1110) $\beta_0 + \beta_1 + \beta_2 + \beta_6 + \beta_7$ $+\beta_{13}$
	1	(0011) $\beta_0 + \beta_3 + \beta_5 + \beta_8$	(0111) $\beta_0 + \beta_1 + \beta_4 + \beta_5$ $+\beta_{10} + \beta_{12}$	(1011) $\beta_0 + \beta_2 + \beta_3 + \beta_6$ $+\beta_9 + \beta_{14}$	(1111) $\beta_0 + \beta_1 + \beta_2 + \beta_4 + \beta_6$ $+\beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$

Note: $q_{it} = 1$ if expectations are positive, $x_{it} = 1$ if unemployed in t

and time, the model additionally controls for any federal state specific factors that vary over time (see Section 2.3.1). However, correlation over time within individuals which is not accounted for by any of the effects described above can still exist, e.g., unobserved factors at the individual level that evolve over time, like life experience, perception of the relationship status, etc., which probably lead to a trend in mental well-being. This is reflected in an autocorrelated structure of the error term. Not accounting for such autocorrelation would lead to biased estimates of the standard errors of the coefficients, and consequently to biased statistical tests. Therefore, we estimate heteroskedastic and autocorrelation consistent (HAC) standard errors by clustering at the individual level.

The dependent variable is subjective life satisfaction, with outcomes on a scale from 0 (low) to 10 (high). Thus, the dependent variable can be assumed to be cardinal or ordinal. Depending on the assumptions, the regression can be performed using a linear estimator (e.g. ordinary least squares (OLS)) or a non-linear ordered latent response estimator (e.g. ordered probit or logit), respectively. Ferrer-i-Carbonell and Frijters (2004) provide an analysis of differences in estimated life satisfaction depending on the estimator. They show that using linear OLS and non-linear ordered response estimators essentially yield the same results for life satisfaction. They emphasize that controlling for time-invariant unobserved factors (individual fixed effects) matters to the estimates but not assumptions on cardinality or ordinality of life satisfaction. Therefore, we estimate mental well-being with OLS and control for time-invariant unobserved heterogeneity.

Fixed and Random Effects Estimators

In general, there are two different estimators that allow to control for unobserved individual specific heterogeneity: the fixed effects and the random effects estimator. The two estimators differ in the assumptions on the individual effects. The fixed effects estimator explicitly models time-invariant individual effects as a determinant of the dependent variable. By averaging the data over time, the fixed estimator controls for all constant individual heterogeneity but inherently removes variation from the covariates. Identification relies on variation within individuals. The random effects estimator is based on the assumption that the time-invariant individual effects are random and uncorrelated with all other explanatory variables and are modeled as part of a composed error term. Identification with the random effects estimator relies on variation within and between individuals. Therefore, if the assumption of randomness of the time-invariant individual effects holds, the random effect estimator is more efficient than the fixed effects estimator and should generally be preferred.

Because the random and the fixed effects estimators differ in the source of identification, one should be aware of the exact question that is to be answered in the analysis. Whereas the coefficient of unemployment estimated with the random effects estimator can be interpreted as the effect of being unemployed on mental well-being, the coefficient of unemployment estimated with the fixed estimator reflects the effect of becoming unemployed on mental well-being. In this chapter, we analyze the effect of unexpected changes in employment status rather than levels of the employment status on mental well-being. This leads directly to the fixed effects estimator. Nevertheless, in our context, the random effects estimator could still deliver reasonable interpretation of the coefficients when changes rather than levels of the variables are used in the empirical model. However, it is essential to check whether the crucial assumption that there is no correlation between unobserved heterogeneity and the observables holds. This can be tested by conducting a variable addition test (VAT), where the dependent variable is regressed on the regressor matrices X and $\overline{X}$ (X averaged over time by individuals).[3] The null hypothesis that the coefficients of $\overline{X}$ are zero is tested with the classical F-Test.[4]

Estimation of Effects for Different Parts of the Population

The basic model is estimated for all individuals in the analysis data set (see Section 2.4). We are also interested to see whether certain groups in the population are affected differently by reference-dependent effects of unemployment on mental well-being, and if the results of the basic estimation are robust for different parts of the population. We focus on differences between gender and age groups. In order to keep the interpretation of results manageable, the basic pairwise interacted model (Equation 2.5) is estimated for various stratifications of the data rather than adding interactions terms for different groups in the population. Furthermore, in the model with individual fixed effects (see Section 2.3.2), only stratifying by gender allows the examination of gender specific differences. With individual fixed effects, any time constant variables such as gender become zero when averaging the data over time. Therefore, a gender effect cannot be estimated with a fixed effects estimator.

2.4 Data

2.4.1 Sample

For the empirical analysis, we use the waves from 1998 to 2009 from the German Socio-Economic Panel (SOEP). The SOEP started in 1984 with approximately 12,000 individuals in 6,000 households in West Germany and was extended to East Germany in 1990. After various sample refreshments, the SOEP included more than 22,000 adult respondents

[3]The standard Hausman test in this case is problematic as it does not allow to perform the test with heteroskedastic and autocorrelation robust standard errors. Arellano (1993) developed a generalized VAT that is robust to autocorrelation and heteroscedasticity of arbitrary forms.

[4]See Baltagi (1998) and Arellano (1993) for further details and properties of this test.

in approximately 12,500 households in 2006 (Wagner et al., 2007). The data set contains information about the current employment status and expectations about the future employment status in each year. Unemployed individuals are asked for the reason of their job termination. The SOEP also provides various measures for mental well-being (see below for further details) and socio-economic characteristics of the individuals.

In order to analyze the relevant part of the population, we reduce the observations for the final analysis data set. Only those individuals who are part of the economically active population are included. Therefore, the sample is restricted to individuals who are of age 30 to 55. The lower limit is to avoid the part of the population that is most probably still in education, or in a precarious and instable job situation. The upper limit excludes those individuals who already could face special incentives for job termination due to proximity to retirement, e.g. special regulations for early retirement. Among the employed individuals, only those who are in full-time employment enter the sample. The restrictions imposed on the unemployed in the sample are in order to make these individuals most comparable to the employed. Therefore, only legally registered unemployed individuals who intend immediate full-time re-employment are kept. As the dependent variable in the model is a measure of mental well-being, most probably reverse causality between mental well-being and unemployment would appear. People with mental health problems plausibly have a higher probability of becoming unemployed due to less productivity. Without further restrictions, the estimation could suffer from an endogeneity problem. Following Kassenboehmer and Haisken-DeNew (2009)[5] and Schmitz (2011)[6], we concentrate on those individuals with exogenous entries into unemployment due to plant closures in order to minimize the potential bias in the estimation of the effect of unemployment on mental well-being due to the endogeneity of unemployment.

As a proxy for mental well-being, we use life satisfaction which is self rated on a scale

[5]Kassenboehmer and Haisken-DeNew (2009) show that only controlling for exogenous unemployment entries allows the identification of a causal effect of unemployment on life satisfaction. They find a significant and substantial negative effect of unemployment for women and men and an additional negative effect of exogenous unemployment entries due to plant closures for women.

[6]Schmitz (2011) shows that the general finding of a negative effect of unemployment on health is likely to be the result of biased estimates and does not reflect a causal relationship. With only plant closures as exogenous entries into unemployment, he does not find any effect of unemployment on various health measures.

of 0 (low) to 10 (high).[7] The SOEP data also allows to analyze the relationship between unemployment and mental health rather than mental well-being as it provides a measure for mental health, the Mental Component Summary Scale (MCS)[8]. But, MCS is provided only every two years. As our model requires observations of two consecutive periods, we lose too many observations with MCS. Therefore, we concentrate on life satisfaction as a proxy for mental well-being. Table 2.5 shows correlation coefficients and p-values of life satisfaction and MCS with particular self-reported mental-health measures provided biannually in the SOEP.

Table 2.5: Life satisfaction and mental health measures

Variables	Life Satisfaction		MCS	
MCS	0.3940	(0.000)		
pressed	-0.1937	(0.000)	-0.3567	(0.000)
melancholy	-0.4158	(0.000)	-0.6645	(0.000)
balanced	0.3644	(0.000)	0.6011	(0.000)
energy	0.3534	(0.000)	0.5376	(0.000)
acclessmental	-0.3717	(0.000)	-0.6737	(0.000)
lesscarementaI	-0.3325	(0.000)	-0.6438	(0.000)
N				23485

Note: p-values in parenthesis

Not surprisingly, the correlations between life satisfaction and certain mental-health measures are less pronounced than the correlations between MCS and the same mental-health measures as MCS is calculated on the basis of these variables. However, all correlations show the same sign as with MCS and are highly statistically significant. The strength of correlation varies more between MCS and mental-health measures than for life satisfaction. The correlation of life satisfaction and MCS is 0.39 and highly statistically significant. Therefore, life satisfaction can be interpreted as a proxy for mental health as well as for mental well-being. Furthermore, we will interpret general life satisfaction as a measure of mental

[7]For a detailed analysis of the relationship between mental health and life satisfaction see Layard et al. (2013). Using three different panel surveys from Great Britain, Germany, Australia they show the strong correlation between these two factors.

[8]MCS is scale score that is calculated using explorative factor analysis with various self-reported measures of mental health in the SOEP (see Andersen et al. (2007) for further information on the algorithm).

well-being as this interpretation seems to be more adequate in the context of utility which is the dependent variable in the theoretical models for reference-dependence.

2.4.2 Variables and Descriptive Statistics

Table 2.6 reports summary statistics for the key variables of the analysis. The total number of observations in the analysis sample is 62,135. The share of legally registered unemployed individuals is only 0.74%. Rather than reflecting the true population unemployment rate in Germany, this low share of unemployed is caused by the selection process of observations described above. The choice of including only exogenous entries into unemployment is very restrictive and a significant number of unemployed individuals do not enter the sample.

Table 2.6: Summary statistics of life satisfaction, employment status, and expectations

<table>
<tr><td colspan="11">Employed</td><td colspan="11">Unemployed</td></tr>
<tr><td colspan="11">$N = 61\ 678$
99.26%</td><td colspan="11">$N = 457$
0.74%</td></tr>
<tr><td colspan="22">$N = 62\ 135$</td></tr>
<tr><td colspan="22">Employment Expectations</td></tr>
<tr><td colspan="4">very</td><td colspan="3">somewhat</td><td colspan="4">not at all</td><td colspan="4">impossible</td><td colspan="4">difficult</td><td colspan="3">easy</td></tr>
<tr><td colspan="4">15.4%</td><td colspan="3">44.3%</td><td colspan="4">40.3%</td><td colspan="4">21.2%</td><td colspan="4">74.3%</td><td colspan="3">4.5%</td></tr>
<tr><td colspan="22">Life Satisfaction</td></tr>
<tr><td colspan="7">low</td><td>∅</td><td></td><td colspan="2">high</td><td colspan="5">low</td><td>∅</td><td colspan="3"></td><td colspan="2">high</td></tr>
<tr><td>0</td><td>1</td><td>2</td><td>3</td><td>4</td><td>5</td><td>6</td><td>7</td><td>8</td><td>9</td><td>10</td><td>0</td><td>1</td><td>2</td><td>3</td><td>4</td><td>5</td><td>6</td><td>7</td><td>8</td><td>9</td><td>10</td></tr>
<tr><td>0.2</td><td>0.2</td><td>0.9</td><td>2.1</td><td>3.1</td><td>10.3</td><td>11.6</td><td>25.2</td><td>32.8</td><td>10.8</td><td>2.8</td><td>1.1</td><td>2.0</td><td>5.0</td><td>9.6</td><td>9.4</td><td>25.2</td><td>14.2</td><td>16.4</td><td>13.6</td><td>2.6</td><td>0.9</td></tr>
</table>

Expectations about the future employment status are different for the employed and the unemployed (see also Section 2.3.1). In the SOEP questionnaires, the employed individuals are asked about their concerns about their job security and can choose between three possible answers: very concerned, somewhat concerned, and not concerned at all. The unemployed are asked about their perceived difficulties to find an appropriate position and can choose between the categories: easy, difficult, and almost impossible. Comparing the distribution of answers over the three ordered categories of expectations shows very distinctive patterns for the employed and unemployed. About 40% of the employed are not concerned at all about their job security. But only 4.5% of the unemployed believe that it will be easy for them to find a new job. 15.4% of the employed are very concerned about their job security and 21.2% of the unemployed expect that it will be almost impossible to find a new job. Whereas 44.4% of the employed report to be somewhat concerned about their job security, 74.3% of the unemployed expect to have difficulties to find a new job. The descriptive statistics suggest that the unemployed tend to be more pessimistic about their employment future than the employed. In order to keep the interpretation of the estimated effects manageable, we collapse the expectations into binary variables. Therefore, according to the theoretical model in Section 2.2, we will interpret the effect of positive expectations with reference to negative expectations. The response categories deliver a natural cut-off between negative and positive expectations (only the categories 'not at all' and 'easy' have a non-negative comprehension). Therefore, a dummy variable for positive expectations for the employed is defined as taking the value 1 for individuals who are 'not concerned at all' about their job security (corresponding to $\overline{q}_{it}$ in Section 2.3.1). The choice of the cut-off between categories for the expectations of the unemployed is unfortunately not that clear-cut. The category 'easy' would be the natural outcome to define positive expectation. However, it cannot solely be used because of the low share of respondents in this category. With only 4.5% of the unemployed in this category, there would not be reasonable enough variation in the binary variable for positive expectations. Therefore, the cut-off is chosen between 'impossible' and 'difficult' and a dummy variable for positive expectations for the unemployed is defined as taking the value 1 if for individuals who expect that finding a new job will be 'easy' or 'difficult' but not 'impossible' (corresponding to $\underline{q}_{it}$ in Section 2.3.1).

The dependent variable in the model is life satisfaction as a proxy for mental well-being. Individuals are asked to rate their overall life satisfaction on a scale from 0 (low) to 10 (high). The distribution of answers on this scale, again, is different for the employed and the unemployed. Whereas about 90% of the employed rate their life satisfaction between 5 and 9 with a peak in 8, the variance of life satisfaction is higher for the unemployed. The average life satisfaction for the employed is 7.1 and for the unemployed 5.5 (see also Table 2.7).

The standard errors for life satisfaction for the employed and unemployed are 1.59 and 2, respectively. Without controlling for any additional factors, the average difference in life satisfaction between the employed and the unemployed is about 1.6 points.

Table 2.7: Summary statistics or dependent variable and controls

	All		Unemployed		Employed	
Variable	Mean	SD	Mean	SD	Mean	SD
Life Satisfaction	7.05	1.60	5.46	2.00	7.06	1.59
Age	42.72	7.00	42.45	6.96	42.72	7.00
Years of Education	12.59	2.70	11.68	2.43	12.60	2.70
Married	0.71	0.45	0.58	0.49	0.71	0.45
Number of Children in Household	0.79	0.97	0.81	1.02	0.79	0.97
Net Income	1675.50	992.77	1.77	37.90	1687.90	985.89
Foreign	0.07	0.26	0.08	0.27	0.07	0.26
Private Insurance	0.13	0.34	0.01	0.11	0.13	0.34
Blue Collar	0.33	0.47	-	-	0.33	0.47
Self Assessed Health	3.55	0.81	3.48	0.91	3.55	0.81
N	62135		457		61678	

Table 2.7 additionally reports summary statistics for the control variables by employment status. We control for age, years of education, marital status (binary), number of children living in the same household, monthly net income (excluding transfer payments), citizenship (binary), private health insurance (binary), blue-collar employment (binary), and self assessed health (scale from 1 (low) to 5 (high)). The employed and unemployed are, on average, very similar in the control factors, except for net income and private health insurance. The fact that some unemployed individuals have a positive net income at all (on average 1.77 Euro per month) is because the unemployed are allowed to earn a certain amount of money without having their legal unemployment status and their unemployment benefits affected. Only 1% of the unemployed are privately insured, compared to 13% of the employed. This difference can be explained by the German institutions for health insurance. In general, only high income earners, self-employed, and civil servants are allowed to opt out of the public health insurance. When becoming registered as unemployed, the privately insured typically have to switch back into the public system. However, there are some exceptions from this

and under certain circumstances the unemployed are allowed to stay in the private system (mainly at their own expenses).

2.5 Results

2.5.1 Variable Addition Test for Unobserved Heterogeneity

In Section 2.3.2, the importance of testing for correlation between unobserved heterogeneity and the observed variables included in the model in order to decide whether the random effects estimator is applicable to our analysis was emphasized. We performed a VAT (see Section 2.3.2) following Arellano (1993). The usual F-Test rejects the joint null hypothesis that all coefficients of the averaged explanatory variables are zero at the 0% significance level for all models (including all stratifications). This means, it is rejected that none of the unobserved time-invariant heterogeneity captured by means over time is uncorrelated with the observed explanatory variables. Consequently, the random effects estimator is not applicable in our case as its crucial assumption of independence of the unobserved heterogeneity is rejected. Therefore, we rely the interpretation of the estimated effects of on the results from the fixed effects estimation.

2.5.2 Interpretation Strategy of the Results

For the interpretation of effects, the results are examined in three steps following the structure of the two empirical models that were introduced in Section 2.3.1.

First, individuals that are employed and unemployed in t, each with the same expectations history and the same employment status in $t-1$, are compared pairwise. In particular, we compare the following pairs that were employed in $t-1$: (0000) and (0100), (0001) and (0101), (0010) and (0110), and (0011) and (0111). We compare the following pairs that were unemployed in $t-1$: (1000) and (1100), (1001) and (1101), (1010) and (1110), and (1011) and (1111). Applying hypothesis tests for multiple coefficients and calculating linear combinations of coefficients, this kind of comparison allows us to analyze whether or not comparable employed and unemployed individuals differ significantly in their mental well-being at all, and to quantify the magnitude of such a difference.

In the second step, individuals that were employed in $t-1$ and became unemployed in t, but, who have different expectations regarding their employment status in t, are compared. In particular, we compare individuals (0101) and (0111), and (0100) and (0101). It is tested whether individuals who became unemployed unexpectedly differ from individuals who expected their unemployment.

Finally, the coefficient that uniquely measures the effect that originates from the unexpectedness of unemployment on mental well-being is interpreted, in order to quantify the reference-dependent effect of unemployment on mental well-being.

The interpretation of the results follows the same three step structure for all stratifications.

2.5.3 Results from Fixed Effects Estimation for All Individuals

Table 2.8 shows the estimated OLS coefficients for the pairwise interacted fixed effects model applied to the whole sample. The estimates correspond to the coefficients in Equation 2.5 of the theoretical regression model introduced in Section 2.3.2.

Differences between employed and unemployed

As explained above, we first concentrate on the difference in mental well-being between employed and unemployed individuals. Table 2.9 shows the results of calculated and F-tested linear combinations of estimated coefficients that reflect the differences in mental well-being between comparable pairs of employed and unemployed individuals. The second and the third columns show comparisons of currently employed and unemployed individuals. While in the second column both individuals were employed in $t-1$, individuals compared in the third column were both unemployed in $t-1$. In the rows, the pairs of currently employed and unemployed individuals are distinguished by their histories of expectation.

Table 2.8: Fixed effects estimates for life satisfaction

Variable		All Coefficient	HAC SE
x_{ist}	β_1	-0.3032	0.2314
x_{ist-1}	β_2	-0.1258	0.1371
$\overline{q}_{ist}$	β_3	0.2147***	0.0195
$\underline{q}_{ist}$	β_4	-0.4892*	0.2593
$\overline{q}_{ist-1}$	β_5	0.0642***	0.0190
$\underline{q}_{ist-1}$	β_6	0.0399	0.1459
$x_{ist} \times x_{ist-1}$	β_7	-0.6005	0.5949
$\overline{q}_{ist} \times \overline{q}_{ist-1}$	β_8	-0.0198	0.0264
$\overline{q}_{ist} \times \underline{q}_{ist-1}$	β_9	-0.6130*	0.3709
$\underline{q}_{ist} \times \overline{q}_{ist-1}$	β_{10}	1.4960*	0.8750
$\underline{q}_{ist} \times \underline{q}_{ist-1}$	β_{11}	1.2091	0.8602
$\overline{q}_{ist-1} \times x_{ist}$	β_{12}	-1.6021**	0.8098
$\underline{q}_{ist-1} \times x_{ist}$	β_{13}	-0.8131	0.6450
$\overline{q}_{ist} \times x_{ist-1}$	β_{14}	0.6851*	0.3513
$\underline{q}_{ist} \times x_{ist-1}$	β_{15}	0.0329	0.7730
Age		-0.0298*	0.0137
Years of Education		-0.0185	0.0174
Married		0.1265**	0.0361
Children in household		0.0189	0.0140
Net Income		0.0001**	0.0000
Foreign		0.1573	0.1233
Private insurance		0.0440	0.0463
Blue collar		-0.0359	0.0311
Self assessed health		0.4582**	0.0107
Constant		6.6014**	0.6638
α_i		yes	
δ_s		yes	
λ_t		yes	
$\delta_s \times \lambda_t$		yes	
N		62135	

Note: *p<0.1, **p<0.05, ***p<0.01

Table 2.9: Estimated differences in LS: employed and unemployed – All

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/0) vs (0/1)	(1/0) vs (1/1)
(0/0)	H_0: $\beta_1 = 0$ p-value = 0.1902 β_1 = -0.3032	H_0: $\beta_1 + \beta_7 = 0$ p-value = 0.1005 $\beta_1 + \beta_7$ = -0.9037
(0/1)	H_0: $\beta_3 = \beta_1 + \beta_4$ p-value = 0.0000 $(\beta_1 + \beta_4) - (\beta_3)$ = -1.0071	H_0: $\beta_3 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{15}$ p-value = 0.0021 $(\beta_1 + \beta_4 + \beta_7 + \beta_{15})$ $-(\beta_3 + \beta_{14})$ = -2.2599
(1/0)	H_0: $\beta_1 + \beta_{12} = 0$ p-value = 0.0142 $\beta_1 + \beta_{12}$ = -1.9053	H_0: $\beta_1 + \beta_7 + \beta_{13} = 0$ p-value = 0.0000 $\beta_1 + \beta_7 + \beta_{13}$ = -1.7168
(1/1)	H_0: $\beta_3 + \beta_8$ $= \beta_1 + \beta_4 + \beta_{10} + \beta_{12}$ p-value = 0.0005 $(\beta_1 + \beta_4 + \beta_{10} + \beta_{12})$ $-(\beta_3 + \beta_8)$ = -1.0934	H_0: $\beta_3 + \beta_9 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$ p-value = 0.0000 $(\beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15})$ $-(\beta_3 + \beta_9 + \beta_{14})$ = -1.2509

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

The first cell shows the difference in mental well-being of currently employed and currently unemployed individuals, where both individuals were employed in $t-1$ and both had negative expectations in $t-1$ and in t, and all else equal. The mental well-being of these pairs differs in the coefficient β_1, and is on average 0.3032 points lower for those that are unemployed compared to the mental well-being of those employed. The null hypothesis that β_1 equals zero cannot be rejected at a significance level lower than 19.02%. Therefore, we do not find a significant difference in the mental well-being of employed and unemployed individuals with currently negative expectations when both were employed and had negative expectations in the past period.

Comparing currently employed and unemployed individuals with negative expectations in both periods but with unemployment in $t-1$, the linear combination of β_1 and β_7 is not statistically significant different from zero just at the 10% significance level. Thus, independent from the past employment status, we find no statistically significant difference in the mental well-being of employed and unemployed individuals if negative expectations are present in t and $t-1$. The effects of negative expectations in two consecutive periods seem to dominate any difference in mental well-being between employed and unemployed individuals which stems from the difference in the employment status.

Mental well-being of currently employed and unemployed individuals differs statistically highly significant for all other combinations of expectations and employment histories. The highest difference in mental well-being appears between those employed and unemployed who had negative expectations in $t-1$, but positive expectations in t (between (1001) and (1101)). In this case, we observe the difference in mental well-being of an individual who became employed unexpectedly in t (a positive deviation from the reference point) and with positive expectations in t, and an individual who remained unemployed expectedly (no deviation from the reference-point), also with positive expectations in t. This finding may be interpreted as a first empirical hint to reference-dependence in the context of employment and unemployment. Also, the average difference of 1.91 and 1.72 points in mental well-being of the employed and unemployed with positive expectations in $t-1$ and negative expectations in t, given past employment and unemployment respectively, is not only statistically significant but substantial. In both cases, we observe individuals who became unemployed unexpectedly and adjusted their expectations in t downwards. Thus, the comparison of employed and unemployed individuals already shows evidence for reference-dependent effects of the employment status on mental well-being, because the biggest differences in mental-well being can be found in those cases, where a change in employment status was unexpected.

Differences between expected and unexpected unemployment and the reference-dependent effect

In the following, we concentrate on those individuals who became unemployed unexpectedly. Becoming unemployed unexpectedly is defined by employment and positive expectations in $t-1$ and unemployment in t. Therefore, the individuals of interest are (0110) and (0111). Both were employed in $t-1$, are unemployed in t, and had positive expectations in $t-1$. The only difference between both individuals lies in their expectations in t. Individual

(0110) has negative expectations in t about becoming re-employed in $t+1$, whereas, individual (0111) has positive expectations in t. As we are interested in the effect of unexpected unemployment on mental well-being, we compare the two types of unexpected unemployed individuals to those unemployed individuals who expected their unemployment but have the same expectations in t, and all else equal. In this sense, the compared individuals have the same employment histories and the same expectations in t but differ in their expectations in $t-1$. This makes two comparable pairs: individuals (0100) versus (0110), and (0101) versus (0111). Both pairs were employed in t and unemployed in $t-1$. Within both pairs, the individuals differ in their expectations in $t-1$ but agree in their expectations in t. Between pairs, the difference lies in their expectations in t, where the first pair has negative expectations and the latter pair positive expectations.

Table 2.10 shows the results of calculated and F-tested linear combinations of estimated coefficients that reflect the differences in mental well-being between comparable pairs of individuals who became expectedly and unexpectedly unemployed.

Table 2.10: Estimated differences in LS: expected and unexpected unemployment – All

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/1)
(0/0) vs (1/0)	H_0: $\beta_5 + \beta_{12} = 0$ p-value = 0.0575 $(\beta_5 + \beta_{12})$ = -1.5379
(0/1) vs (1/1)	H_0: $\beta_5 + \beta_{10} + \beta_{12} = 0$ p-value = 0.9008 $(\beta_5 + \beta_{10} + \beta_{12})$ = -0.0419

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

The first cell shows the estimated average difference in mental well-being between unexpected and expected unemployed with negative expectations in t for both. The difference in mental well-being between these two individuals is reflected by the linear combination of β_5 and β_{12}. The estimated difference in life satisfaction is 1.54 points. The F-test rejects the null hypothesis that the linear combination of β_5 and β_{12} equals zero at an acceptable 5.75% significance level. Therefore, an individual that did not expect to become unemployed has, on average, a life satisfaction that is 1.5 points lower compared to an individual who expected unemployment, all else equal. This applies for unexpected unemployment when expectations about future employment are adjusted downwards in the period of becoming unemployed. In contrast, we find no statistically significant difference in mental well-being between unexpected and expected unemployed when expectations remain or become positive in t, respectively. The distinguishing linear combination of coefficients in this case is $\beta_5 + \beta_{10} + \beta_{12}$. The estimated coefficient of this linear combination is -0.04 points in life satisfaction and is statistically not significant. Thus, individuals who became unemployed unexpectedly but who still have positive expectations about their future employment status, are not different from those unemployed who expected to become unemployed but also have positive expectations about their future employment. Hence, depending on the expectations in t, we find a reference-dependent effect of unemployment on mental well-being. While individuals with current positive expectations seem not to be affected by the fact that their unemployment was not expected, we find a clear negative effect for those individuals who are pessimistic about their future employment status which stems from the unexpectedness of their unemployment. A detailed look at the estimated coefficients in the particular linear combinations reveals the mechanism behind this difference.

Again, Table 2.8 shows in particular the estimated coefficients that contribute to the calculation of the linear combinations above. First of all, the coefficient of the variable $(\overline{q}_{ist-1})(x_{ist})$, β_{12} is the one which uniquely measures the reference-dependent effect of becoming unemployed unexpectedly. For both types of individuals who became unexpectedly unemployed, this coefficient is part of the linear combinations of coefficients that distinguish them from the expectedly unemployed. The estimate is -1.6 and is statistically significant at the 5% level. The estimated size of the reference-dependent effect equals one standard deviation of overall life satisfaction and about three-fourths of the mean difference in life satisfaction between the employed and the unemployed (see Tables 2.6 and 2.7). For both types of unexpectedly unemployed individuals, this result shows an average drop in life satisfaction of almost 2 points. Again, this negative effect only stems from the unexpectedness of their unemployment. However, both types benefit from their positive expectations in $t-1$. This effect is reflected in the coefficient of the variable $\overline{q}_{ist-1}$, β_5. The estimate of this coefficient is 0.06 and statistically significant at a level lower than 1%. However, compared to individuals who expected their unemployment, all else equal, this positive effect is not able to outweigh the negative effect from the unexpected unemployment. Therefore, the results suggest that the unexpected incidence of unemployment worsens the situation for the unemployed. Focusing on those unexpectedly unemployed with ongoing positive expectations, the coefficient of the variable $(\underline{q}_{ist})(\overline{q}_{ist-1})$, β_{10} is of additional relevance. This coefficient captures the effect of continued positive expectations in the case of becoming unemployed in t. The estimate is 1.5 and is statistically significant at the 10% level. In absolute values, the estimate of β_{10} is close to the estimate of β_{12}. The fact that these individuals, in spite of their unexpected unemployment, go on with positive expectations makes them statistically not distinguishable from individuals who expected their unemployment. The positive effect from ongoing positive expectations outweighs the negative effect from unexpected unemployment.

In summary, the results show a general reference-dependent negative effect for all individuals that became unemployed unexpectedly. This effect stems from the unexpectedness of unemployment, i.e. a negative deviation from the reference point. Individuals who have negative expectations about their job future after they became unemployed unexpectedly, i.e. individuals who adjusted their expectations downwards after becoming unemployed unexpectedly, directly suffer from the negative deviation of their employment status from their reference point. Their positive expectations in the period prior to their unemployment cannot outweigh the negative effect from the unexpected unemployment. In contrast, individuals who became unemployed unexpectedly but with unaffected positive expectations about their future employment are statistically not different from those who became unemployed expectedly. This similarity is owed to the fact that in this case the positive effect from ongoing positive expectations outweighs the negative effect from unexpected unemployment.

2.5.4 Results from Fixed Effects Estimation by Age Groups

In order to estimate different slopes of the regression line for different ages, the data set is divided into two age groups (similar to using interaction terms). The number of only two sub-samples is mainly driven by the limited number of observed unemployed individuals. The first sub-sample includes individuals aged 30 to 40 (24,731 observations) and the second sub-sample includes individuals aged 41 to 55 (37,404 observations). Table 2.22 in the Appendix shows the distribution of life satisfaction and expectations over years of age by employment status. Average life satisfaction in the older age groups (6.99 for the employed and 5.31 for the unemployed individuals) is slightly lower than in the younger group (7.16 for the employed and 5.18 for the unemployed individuals). However, the average share of unemployed with positive expectations in the older age group is about 12% points lower than in the younger age group (83.4% and 71.7%, respectively). There is no such clear difference in average expectations between younger and older individuals who are employed (40.7% and 40.1%, respectively).

The estimated coefficients for the younger age group mainly confirm the findings from the basic estimation, see Table 2.11.

Table 2.12 shows the results for the estimated differences in life satisfaction between employed and unemployed young individuals.

Again, the highest differences in life satisfaction between employed and unemployed individuals is found for unexpected outcomes in the employment status with adjusted expectations in the next period. The estimated difference between individuals who became unexpectedly employed and individuals who expectedly remained unemployed with an upward adjustment of expectations, (1010) and (1110), is 3.87 points in life satisfaction (0.2% significance level). Also, individuals who became unexpectedly unemployed followed by downward adjusted expectations have a life satisfaction that is 2.8 points lower than comparable employed individuals (0.0% significance level). In both cases, the difference in life satisfaction exceeds the overall difference in life satisfaction between employed and unemployed individuals, see Section 2.4.2.

Table 2.11: Fixed effects estimates for life satisfaction

Variable		30 - 40		41 - 55	
		Coefficient	HAC SE	Coefficient	HAC SE
x_{ist}	β_1	-0.9223*	0.5290	-0.2328	0.2476
x_{ist-1}	β_2	-0.8241***	0.2829	0.1703	0.1578
$\overline{q}_{ist}$	β_3	0.2397***	0.0310	0.2118***	0.0257
$\underline{q}_{ist}$	β_4	-0.0409	0.5864	-0.4071	0.2838
$\overline{q}_{ist-1}$	β_5	0.0983***	0.0298	0.0556**	0.0259
$\underline{q}_{ist-1}$	β_6	0.8115***	0.2905	-0.2497	0.1747
$x_{ist} \times x_{ist-1}$	β_7	-0.8023	1.4722	-0.3582	0.5604
$\overline{q}_{ist} \times \overline{q}_{ist-1}$	β_8	-0.0961**	0.0408	0.0115	0.0361
$\overline{q}_{ist} \times \underline{q}_{ist-1}$	β_9	-2.0815***	0.7744	0.0005	0.4393
$\underline{q}_{ist} \times \overline{q}_{ist-1}$	β_{10}	2.3309***	0.8276	0.6217	1.1881
$\underline{q}_{ist} \times \underline{q}_{ist-1}$	β_{11}	0.8471	1.5446	1.0205	0.9507
$\overline{q}_{ist-1} \times x_{ist}$	β_{12}	-1.9170***	0.7040	-1.1909	1.0895
$\underline{q}_{ist-1} \times x_{ist}$	β_{13}	-0.0606	1.1869	-1.2872*	0.6900
$\overline{q}_{ist} \times x_{ist-1}$	β_{14}	1.9672***	0.7555	0.1033	0.3981
$\underline{q}_{ist} \times x_{ist-1}$	β_{15}	0.1054	1.6763	0.1925	0.8119
Age		-0.0467*	0.0278	-0.0147	0.0169
Years of Education		0.0003	0.0236	-0.0183	0.0280
Married		0.2077***	0.0449	0.0406	0.0659
Children in household		0.0263	0.0239	0.0277	0.0209
Net Income		0.0001***	0.0000	0.0001***	0.0000
Foreign		-0.0952	0.1671	-0.2523	0.2067
Private insurance		-0.0752	0.0653	0.1354*	0.0698
Blue collar		-0.0774*	0.0469	0.0095	0.0425
Self assessed health		0.4119***	0.0178	0.4640***	0.0136
Constant		6.7249***	1.0561	5.9936***	0.9546
α_i		yes		yes	
δ_s		yes		yes	
λ_t		yes		yes	
$\delta_s \times \lambda_t$		yes		yes	
N		24731		37404	

Note: *p<0.1, **p<0.05, ***p<0.01

Table 2.12: Estimated differences in LS: employed and unemployed – 30 - 40

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/0) vs (0/1)	(1/0) vs (1/1)
(0/0)	H_0: $\beta_1 = 0$ p-value = 0.0813 β_1 = -0.9223	H_0: $\beta_1 + \beta_7 = 0$ p-value = 0.1677 $\beta_1 + \beta_7$ = -1.7246
(0/1)	H_0: $\beta_3 = \beta_1 + \beta_4$ p-value = 0.0000 $(\beta_1 + \beta_4) - (\beta_3)$ = -1.2028	H_0: $\beta_3 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{15}$ p-value = 0.0022 $(\beta_1 + \beta_4 + \beta_7 + \beta_{15})$ $-(\beta_3 + \beta_{14})$ = -3.8670
(1/0)	H_0: $\beta_1 + \beta_{12} = 0$ p-value = 0.0000 $\beta_1 + \beta_{12}$ = -2.8392	H_0: $\beta_1 + \beta_7 + \beta_{13} = 0$ p-value = 0.0000 $\beta_1 + \beta_7 + \beta_{13}$ = -1.7852
(1/1)	H_0: $\beta_3 + \beta_8$ $= \beta_1 + \beta_4 + \beta_{10} + \beta_{12}$ p-value = 0.0584 $(\beta_1 + \beta_4 + \beta_{10} + \beta_{12})$ $-(\beta_3 + \beta_8)$ = -0.6928	H_0: $\beta_3 + \beta_9 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$ p-value = 0.0059 $(\beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15})$ $-(\beta_3 + \beta_9 + \beta_{14})$ = -0.9989

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

For the older age group, the results suggest only minor differences in life satisfaction between employed and unemployed individuals, see Table 2.13. Moreover, the unexpectedness of the employment status outcome in cases with adjusted expectations in the next period seems not to play a role. Only in the case with unexpected ongoing unemployment and downward adjusted expectations ((1010) versus (1110)), the estimated difference in life satisfaction of 1.88 points is significant at the 0.0% level.

These findings for both age groups are also reflected in the estimated differences in life satisfaction between expectedly and unexpectedly unemployed individuals, see Table 2.14 and Table 2.15.

For the younger age group, the reference-dependent effect (β_{12}) is -1.9 and highly significant. This effect is only slightly lowered by the highly significant effect of previous positive expectations (β_5), 0.1. Therefore, the overall reduction in life satisfaction that occurs because the unemployment was not expected is estimated to be 1.8 points (0.1% significance level). In the case where positive expectations are not affected by unemployment, a significant

Table 2.13: Estimated differences in LS: employed and unemployed – 41 - 55

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/0) vs (0/1)	(1/0) vs (1/1)
(0/0)	H_0: $\beta_1 = 0$ p-value = 0.3472 β_1 = -0.2328	H_0: $\beta_1 + \beta_7 = 0$ p-value = 0.2494 $\beta_1 + \beta_7$ = -0.5910
(0/1)	H_0: $\beta_3 = \beta_1 + \beta_4$ p-value = 0.0000 $(\beta_1 + \beta_4) - (\beta_3)$ = -0.8517	H_0: $\beta_3 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{15}$ p-value = 0.2085 $(\beta_1 + \beta_4 + \beta_7 + \beta_{15})$ $-(\beta_3 + \beta_{14})$ = -1.1207
(1/0)	H_0: $\beta_1 + \beta_{12} = 0$ p-value = 0.1787 $\beta_1 + \beta_{12}$ = -1.4238	H_0: $\beta_1 + \beta_7 + \beta_{13} = 0$ p-value = 0.0000 $\beta_1 + \beta_7 + \beta_{13}$ = -1.8782
(1/1)	H_0: $\beta_3 + \beta_8$ $= \beta_1 + \beta_4 + \beta_{10} + \beta_{12}$ p-value = 0.0019 $(\beta_1 + \beta_4 + \beta_{10} + \beta_{12})$ $-(\beta_3 + \beta_8)$ = -1.4324	H_0: $\beta_3 + \beta_9 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$ p-value = 0.0000 $(\beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15})$ $-(\beta_3 + \beta_9 + \beta_{14})$ = -1.3879

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

Table 2.14: Estimated differences in LS: expected and unexpected unemployment – 30 - 40

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/1)
(0/0) vs (1/0)	H_0: $\beta_5 + \beta_{12} = 0$ p-value = 0.0098 $(\beta_5 + \beta_{12})$ = -1.8187
(0/1) vs (1/1)	H_0: $\beta_5 + \beta_{10} + \beta_{12} = 0$ p-value = 0.2477 $(\beta_5 + \beta_{10} + \beta_{12})$ = 0.5123

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

reference-dependent effect does not appear.

Table 2.15: Estimated differences in LS: expected and unexpected unemployment – 41 - 55

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/1)
(0/0) vs (1/0)	H_0: $\beta_5 + \beta_{12} = 0$ p-value = 0.2972 $(\beta_5 + \beta_{12})$ = -1.1354
(0/1) vs (1/1)	H_0: $\beta_5 + \beta_{10} + \beta_{12} = 0$ p-value = 0.2820 $(\beta_5 + \beta_{10} + \beta_{12})$ = -0.5137

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

As expected from the comparison of life satisfaction levels between employed and unemployed individuals in the older age group, the results suggest no empirical evidence for reference-dependent effects of unemployment for this part of the population.

A generally higher fluctuation in the job market for younger individuals and thus a higher variation in the employment status in the data set could make up for a possible explanation for this result. Table 2.23 in the Appendix shows the number of observations for all appearing counts of total unemployment periods per individual. The distribution of total counts is almost the same for the younger and older age group. Hence, a higher volatility for younger individuals between employment and unemployment periods seems not to be the reason for our findings.

Another explanation could be that younger individuals tend to be less risk averse than older individuals. Therefore, they might choose jobs with generally lower job security, such as, in young and developing startup companies with a higher probability to become unemployed than more risk averse older individuals. However, estimation of the same regression model but with industrial fixed effects instead of federal state fixed effects yields similar results (see Table 2.21 in the Appendix).

We tend towards the level of expertise in the job market as the most plausible explanation for the difference between younger and older individuals. While older individuals might be more experienced in the evaluation of information regarding their future employment status, younger individuals seem to be less able to anticipate potential unemployment. The difference in the ability to foresee unemployment between younger and older individuals is supported by the data, see Table 2.24 in the Appendix. 19.1% of the younger individuals who became unemployed did not expect their unemployment, whereas, only 5.2% of the older age group became unemployed without expecting it. Not such a clear but similar pattern can be found for those individuals who stayed unemployed. 75.8% of the younger individuals who were unemployed in $t-1$ and who stayed unemployed in t had positive expectations for t, whereas, the share amongst the older unemployed is 70%. These numbers suggest that too few individuals in the older age group did not expect to become unemployed to show a statistically significant reference-dependent effect of unemployment on mental well-being among this group.

2.5.5 Results from Fixed Effects Estimation by Gender

The estimated coefficients for the stratified data by gender mainly confirm the findings from the basic estimation, see Table 2.16.

For males, we find a statistically significant lower life satisfaction by 1.3 points on average for unemployed individuals even with negative expectations in both $t-1$ and t and unemployment in $t-1$, see Table 2.17. However, there is no significant difference between employed and unemployed males when both were employed in $t-1$, unemployment was not expected and expectations adjusted downwards in t ((0010) versus (0110)).

This result is also reflected in Table 2.18. While the results of the basic estimation suggest a statistically significant difference in life satisfaction of expectedly and unexpectedly unemployed individuals with downward adjusted expectations in t, this is not the case for males. The linear combination of β_5 and β_{12} shows a lower life satisfaction, by 1.3 points, for unexpectedly unemployed males but the difference is not statistically significant. For a deeper insight, we estimate a further stratification for males by age and find that only for males aged 41 to 55 no reference-dependent effect appears. For males aged 30 to 40, we find a drop in life satisfaction by 2.3 points on average (0.3% significance level) caused by the

Table 2.16: Fixed effects estimates for life satisfaction – Gender

Variable		Male		Female	
		Coefficient	HAC SE	Coefficient	HAC SE
x_{ist}	β_1	-0.1930	0.3101	-0.5517*	0.3116
x_{ist-1}	β_2	-0.0874	0.1951	-0.1488	0.1895
$\overline{q}_{ist}$	β_3	0.2008***	0.0259	0.2332***	0.0296
$\underline{q}_{ist}$	β_4	-0.8133**	0.3451	0.1529	0.3543
$\overline{q}_{ist-1}$	β_5	0.0541**	0.0240	0.0755**	0.0306
$\underline{q}_{ist-1}$	β_6	-0.0079	0.2042	0.0880	0.2070
$x_{ist} \times x_{ist-1}$	β_7	-1.0904*	0.6440	0.0749	0.9737
$\overline{q}_{ist} \times \overline{q}_{ist-1}$	β_8	0.0130	0.0339	-0.0615	0.0419
$\overline{q}_{ist} \times \underline{q}_{ist-1}$	β_9	-1.3243***	0.4752	-0.1012	0.5319
$\underline{q}_{ist} \times \overline{q}_{ist-1}$	β_{10}	1.4514	1.1996	1.9160**	0.8826
$\underline{q}_{ist} \times \underline{q}_{ist-1}$	β_{11}	1.0028	0.9886	1.6252	1.2343
$\overline{q}_{ist-1} \times x_{ist}$	β_{12}	-1.3649	1.1375	-2.1862***	0.7146
$\underline{q}_{ist-1} \times x_{ist}$	β_{13}	-0.4965	0.7184	-1.2393	1.0928
$\overline{q}_{ist} \times x_{ist-1}$	β_{14}	1.1417**	0.4468	0.3780	0.5049
$\underline{q}_{ist} \times x_{ist-1}$	β_{15}	0.7222	0.8475	-1.4823	0.9835
Age		-0.0365**	0.0179	-0.0247	0.0216
Years of Education		-0.0185	0.0218	-0.0190	0.0289
Married		0.1350***	0.0440	0.1056*	0.0610
Children in household		0.0255	0.0168	0.0000	0.0251
Net Income		0.0001***	0.0000	0.0001**	0.0000
Foreign		-0.1410	0.1461	-0.2112	0.2319
Private insurance		0.0543	0.0524	0.0087	0.0930
Blue collar		-0.0537	0.0387	-0.0136	0.0516
Self assessed health		0.4600***	0.0144	0.4530***	0.0160
Constant		6.7223***	0.8834	6.4740***	1.0288
α_i		yes		yes	
δ_s		yes		yes	
λ_t		yes		yes	
$\delta_s \times \lambda_t$		yes		yes	
N		34608		27527	

Note: *p<0.1, **p<0.05, ***p<0.01

Table 2.17: Estimated differences in LS: employed and unemployed – Male

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/0) vs (0/1)	(1/0) vs (1/1)
(0/0)	H_0: $\beta_1 = 0$ p-value = 0.5337 β_1 = -0.1930	H_0: $\beta_1 + \beta_7 = 0$ p-value = 0.0192 $\beta_1 + \beta_7$ = -1.2834
(0/1)	H_0: $\beta_3 = \beta_1 + \beta_4$ p-value = 0.0000 $(\beta_1 + \beta_4) - (\beta_3)$ = -1.2070	H_0: $\beta_3 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{15}$ p-value = 0.0023 $(\beta_1 + \beta_4 + \beta_7 + \beta_{15})$ $-(\beta_3 + \beta_{14})$ = -2.7169
(1/0)	H_0: $\beta_1 + \beta_{12} = 0$ p-value = 0.1544 $\beta_1 + \beta_{12}$ = -1.5579	H_0: $\beta_1 + \beta_7 + \beta_{13} = 0$ p-value = 0.0002 $\beta_1 + \beta_7 + \beta_{13}$ = -1.7800
(1/1)	H_0: $\beta_3 + \beta_8$ $= \beta_1 + \beta_4 + \beta_{10} + \beta_{12}$ p-value = 0.0014 $(\beta_1 + \beta_4 + \beta_{10} + \beta_{12})$ $-(\beta_3 + \beta_8)$ = -1.1335	H_0: $\beta_3 + \beta_9 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$ p-value = 0.0020 $(\beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15})$ $-(\beta_3 + \beta_9 + \beta_{14})$ = -0.8862

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

unexpectedness of unemployment when expectations are adjusted downwards. Again, this result reflects the differences between age groups as discussed in Section 2.5.4. Interestingly, when expectations of younger men are adjusted upwards or remain positive after becoming unemployed, the effect of ongoing expectations not only outweighs the negative effect from unexpected unemployment but even exceeds it ($\beta_{10} = 3.27$ at 0.1% significance level).

Table 2.18: Estimated differences in LS: expected and unexpected unemployment – Male

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/1)
(0/0) vs (1/0)	H_0: $\beta_5 + \beta_{12} = 0$ p-value = 0.2490 $(\beta_5 + \beta_{12})$ = -1.3108
(0/1) vs (1/1)	H_0: $\beta_5 + \beta_{10} + \beta_{12} = 0$ p-value = 0.7138 $(\beta_5 + \beta_{10} + \beta_{12})$ = 0.1406

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

For females, we find similar results as in the basic estimation both in the comparison of employed and unemployed (see Table 2.19) and in the comparison of expected and unexpected unemployment (see Table 2.18). In the case of downward adjusted expectations after becoming unemployed unexpectedly, the average reference-dependent effect is -2.11 points in life satisfaction. For females with constant expectations, we find no reference-dependent effect for females. The results of the regression for the further stratified female sub-sample into age groups suggest no differences between older and younger women regarding reference-dependent effects of unemployment on mental well-being.

Table 2.19: Estimated differences in LS: employed and unemployed – Female

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/0) vs (0/1)	(1/0) vs (1/1)
(0/0)	H_0: $\beta_1 = 0$ p-value = 0.0767 β_1 = -0.5517	H_0: $\beta_1 + \beta_7 = 0$ p-value = 0.6326 $\beta_1 + \beta_7$ = -0.4768
(0/1)	H_0: $\beta_3 = \beta_1 + \beta_4$ p-value = 0.0009 $(\beta_1 + \beta_4) - (\beta_3)$ = -0.6321	H_0: $\beta_3 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{15}$ p-value = 0.0041 $(\beta_1 + \beta_4 + \beta_7 + \beta_{15})$ $-(\beta_3 + \beta_{14})$ = -2.4174
(1/0)	H_0: $\beta_1 + \beta_{12} = 0$ p-value = 0.0000 $\beta_1 + \beta_{12}$ = -2.7379	H_0: $\beta_1 + \beta_7 + \beta_{13} = 0$ p-value = 0.0003 $\beta_1 + \beta_7 + \beta_{13}$ = -1.7161
(1/1)	H_0: $\beta_3 + \beta_8$ $= \beta_1 + \beta_4 + \beta_{10} + \beta_{12}$ p-value = 0.0870 $(\beta_1 + \beta_4 + \beta_{10} + \beta_{12})$ $-(\beta_3 + \beta_8)$ = -0.8410	H_0: $\beta_3 + \beta_9 + \beta_{14}$ $= \beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15}$ p-value = 0.0003 $(\beta_1 + \beta_4 + \beta_7 + \beta_{11} + \beta_{13} + \beta_{15})$ $-(\beta_3 + \beta_9 + \beta_{14})$ = -1.9303

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

Summarizing this subsection, we find empirical evidence for reference-dependent effects of unemployment on mental well-being for females and younger males.[9] Only for older men the results suggest no evidence for reference-dependence in the context of unemployment.

[9]Following the reasoning by Kassenboehmer and Haisken-DeNew (2009) this gender difference can be explained by family constraints for women. They argue that married women with exogenous entries into unemployment are flexible in labour market participation as their labour supply is likely conditional on the labour supply of their partners. They show that the negative effect of exogenous entries into unemployment is mainly driven by married women.

Table 2.20: Estimated differences in LS: expected and unexpected unemployment – Female

(q_{t-1}/q_t) \ (x_{t-1}/x_t)	(0/1)
(0/0) vs (1/0)	H_0: $\beta_5 + \beta_{12} = 0$ p-value = 0.0031 $(\beta_5 + \beta_{12})$ = -2.1107
(0/1) vs (1/1)	H_0: $\beta_5 + \beta_{10} + \beta_{12} = 0$ p-value = 0.7083 $(\beta_5 + \beta_{10} + \beta_{12})$ = -0.1947

Note: $q_t = 1$ if expectations are positive, $x_t = 1$ if unemployed in t

2.6 Conclusion

Our empirical results show that mental well-being of individuals who expected to become unemployed is less affected by becoming unemployed than when the unemployment was not expected previously. We find that current and past expectations about the future employment status have an important impact, not only directly on mental well-being, but also, on the perception of the employment status. Our results are derived from the estimation of an econometric model which follows the structure of theoretical models with reference-dependent preferences and endogenous reference points that are determined by lagged expectations. We assumed that unemployment rates are used as an information to build expectations about the future employment status, and lagged expectations represent the reference point. We developed the hypothesis that depending on expectations (i.e. the reference point) becoming unemployed affects the individuals differently.

The contribution of our study is twofold. First, we add to the literature on unemployment and mental well-being where the mechanism of how unemployment rates and expectations affect the perception of unemployment remained unclear so far. While in this strand of literature only current expectations about the future are taken into account, we show that past expectations play an important role in the perception of unemployment. We find that previously expecting unemployment attenuates the negative effect of becoming unemployed. On the one hand, it seems important to give individuals sufficient notice of their unemployment so that they are able to anticipate their unemployment and adapt to the situation.

On the other hand, it could be important for re-employment programs to focus particularly on individuals who became unemployed unexpectedly as their higher drop in mental well-being may involve a higher risk of developing serious mental illnesses. This, in turn could reduce their chances of re-employment. Our results show that positive expectations about re-employment even in the case of unexpected unemployment are able to keep up mental well-being at the same level than when unemployment was expected.

Second, our finding that unexpected unemployment has a stronger negative impact on mental well-being than expected unemployment, supports theoretical models with reference-dependent preferences and endogenous reference point formation with empirical evidence. Therefore, we also contribute to the literature on the importance of reference points (DellaVigna (2009) for an overview). Our results suggest that lagged expectations about the future employment status indeed serve as a reference point, and that the size of the effect of unemployment on mental well-being reflects a deviation from an individual reference point rather than the final state of unemployment.

2.7 Appendix

Table 2.21: Estimates for life satisfaction with industrial sector fixed effects

Variable		All	
		Coefficient	HAC SE
x_{ist}	β_1	-0.2971	0.5951
x_{ist-1}	β_2	-0.1499	0.1481
$\overline{q}_{ist}$	β_3	0.2140***	0.0197
$\underline{q}_{ist}$	β_4	-0.4840**	0.2609
$\overline{q}_{ist-1}$	β_5	0.0633***	0.0193
$\underline{q}_{ist-1}$	β_6	0.0769	0.1572
$x_{ist} \times x_{ist-1}$	β_7	-0.6100	0.5942
$\overline{q}_{ist} \times \overline{q}_{ist-1}$	β_8	-0.0319	0.0267
$\overline{q}_{ist} \times \underline{q}_{ist-1}$	β_9	-0.5626*	0.3931
$\underline{q}_{ist} \times \overline{q}_{ist-1}$	β_{10}	1.5273*	0.8878
$\underline{q}_{ist} \times \underline{q}_{ist-1}$	β_{11}	1.2361	0.8947
$\overline{q}_{ist-1} \times x_{ist}$	β_{12}	-1.5971**	0.8272
$\underline{q}_{ist-1} \times x_{ist}$	β_{13}	-0.9352	0.6345
$\overline{q}_{ist} \times x_{ist-1}$	β_{14}	0.6279*	0.3741
$\underline{q}_{ist} \times x_{ist-1}$	β_{15}	0.0855	0.8202
Age		0.0039**	0.0508
Years of Education		-0.0149	0.0173
Married		0.1373***	0.0367
Children in household		0.0210	0.0141
Net Income		0.0001***	0.0000
Foreign		0.1257	0.1271
Private insurance		0.0549	0.0470
Blue collar		-0.0233	0.0317
Self assessed health		0.4555***	0.0108
Constant		5.3769***	1.9620
α_i		yes	
δ_s		yes	
λ_t		yes	
$\delta_s \times \lambda_t$		yes	
N		62135	

Note: *p<0.1, **p<0.05, ***p<0.01

Table 2.22: Summary statistics of life satisfaction and expectations by employment status and age years

		$x_{it}=0$			$x_{it}=1$		
Age	Variable	Mean	SD	N	Mean	SD	N
30	y	7.255	1.523	1662	5.824	2.430	17
	$\overline{q}$ / $\underline{q}$	0.425	0.495		0.882	0.332	
31	y	7.276	1.488	1729	5.222	2.108	9
	$\overline{q}$ / $\underline{q}$	0.426	0.495		0.778	0.441	
32	y	7.229	1.536	1871	6.524	2.205	21
	$\overline{q}$ / $\underline{q}$	0.433	0.496		0.905	0.301	
33	y	7.198	1.527	2022	6.111	1.779	18
	$\overline{q}$ / $\underline{q}$	0.409	0.492		0.944	0.236	
34	y	7.209	1.500	2140	5.050	1.820	20
	$\overline{q}$ / $\underline{q}$	0.422	0.494		0.800	0.410	
35	y	7.192	1.515	2275	5.583	1.311	11
	$\overline{q}$ / $\underline{q}$	0.411	0.492		0.667	0.492	
36	y	7.157	1.526	2404	5.294	2.085	17
	$\overline{q}$ / $\underline{q}$	0.395	0.489		0.941	0.243	
37	y	7.124	1.558	2464	5.867	1.767	15
	$\overline{q}$ / $\underline{q}$	0.395	0.489		0.933	0.258	
38	y	7.060	1.574	2583	5.889	1.530	17
	$\overline{q}$ / $\underline{q}$	0.391	0.488		0.611	0.502	
39	y	7.124	1.560	2672	5.125	1.893	16
	$\overline{q}$ / $\underline{q}$	0.397	0.489		0.813	0.403	
40	y	7.095	1.563	2728	5.611	3.183	18
	$\overline{q}$ / $\underline{q}$	0.391	0.488		0.833	0.383	
41	y	7.027	1.565	2785	5.500	1.713	16
	$\overline{q}$ / $\underline{q}$	0.386	0.487		0.875	0.342	
42	y	7.048	1.563	2836	5.692	2.175	13
	$\overline{q}$ / $\underline{q}$	0.384	0.486		0.769	0.439	
43	y	6.960	1.620	2787	5.000	1.440	25
	$\overline{q}$ / $\underline{q}$	0.375	0.484		0.750	0.441	
44	y	6.970	1.629	2792	5.462	1.964	24
	$\overline{q}$ / $\underline{q}$	0.371	0.483		0.731	0.452	
45	y	6.961	1.609	2727	4.762	1.921	21
	$\overline{q}$ / $\underline{q}$	0.371	0.483		0.762	0.436	
46	y	6.971	1.617	2630	5.294	1.993	16
	$\overline{q}$ / $\underline{q}$	0.390	0.488		0.765	0.437	
47	y	6.965	1.662	2595	5.360	2.378	23
	$\overline{q}$ / $\underline{q}$	0.378	0.485		0.600	0.500	
48	y	6.953	1.681	2568	5.261	2.220	23
	$\overline{q}$ / $\underline{q}$	0.385	0.487		0.783	0.422	
49	y	7.010	1.624	2487	5.053	2.041	19
	$\overline{q}$ / $\underline{q}$	0.403	0.491		0.895	0.315	
50	y	6.995	1.637	2416	5.217	1.882	21
	$\overline{q}$ / $\underline{q}$	0.404	0.491		0.609	0.499	
51	y	6.998	1.655	2324	4.895	1.792	18
	$\overline{q}$ / $\underline{q}$	0.402	0.490		0.684	0.478	
52	y	6.994	1.623	2227	5.526	1.806	19
	$\overline{q}$ / $\underline{q}$	0.431	0.495		0.684	0.478	
53	y	7.033	1.625	2127	5.500	1.900	10
	$\overline{q}$ / $\underline{q}$	0.455	0.498		0.500	0.527	
54	y	7.016	1.620	1989	6.400	1.776	10
	$\overline{q}$ / $\underline{q}$	0.454	0.498		0.700	0.483	
55	y	6.972	1.646	1838	6.143	1.864	6
	$\overline{q}$ / $\underline{q}$	0.477	0.500		0.429	0.535	

y: life satisfaction (0 = low, 10 = high)
$\overline{q}$: share of employed with positive expectations
$\underline{q}$: share of unemployed with positive expectations

Table 2.23: Counts of unemployment periods - by age

Counts $\sum_{t=1}^{T} x_{it}$	30 - 40 Number	30 - 40 Percent	41 - 55 Number	41 - 55 Percent
0	24 081	97.37	36 372	97.24
1	517	2.09	802	2.14
2	107	0.43	175	0.47
3	20	0.08	48	0.13
4	6	0.02	7	0.02
Total	24 731	100.00	37 404	100.00

Table 2.24: Counts of correctly predicted unemployment - by age

	30 - 40		41 - 55	
	$x_{it} = 0$	$x_{it} = 1$	$x_{it} = 0$	$x_{it} = 1$
$\overline{q}_{it-1} = 1$	10 086	22	14 886	10
$\overline{q}_{it-1} = 0$	13 796	93	21 490	180
Total	23 882	115	36 376	190
$\underline{q}_{it-1} = 1$	585	50	585	60
$\underline{q}_{it-1} = 0$	83	16	167	26
Total	668	66	752	86

$\overline{q}$: expectations of the employed

$\underline{q}$: expectations of the unemployed

Chapter 3

Public and Private Health Insurance in Germany: The Ignored Risk Selection Problem*

*This chapter is based on the joint work "Public and Private Health Insurance in Germany: The Ignored Risk Selection Problem" (2014, Health Economics, 23(6), 670 - 687; DOI: 10.1002/hec.2942.) with Robert Nuscheler. Both authors contributed equally to this work.

3.1 Introduction

We investigate risk selection between public and private health insurance in Germany. There are several differences between these two branches of health care financing, and perhaps the most important one is the calculation of premiums. While public premiums are subject to community rating, private premiums are risk rated. Moreover, public premiums are proportional to income (up to some ceiling), and private premiums do not depend on income. The private system is, thus, particularly attractive for high-income - low-risk individuals.

Using 2000 to 2007 data from the German Socio-Economic Panel Study (SOEP), we find advantageous selection in favor of private insurers as suggested by the differences in premium calculation. Interestingly, private insurers are unable to select the healthy upon enrollment. They profit from individuals who experienced a negative health shock: these individuals lean more towards switching from private to public health insurance. The associated health care spending then accrues in the public rather than in the private system. Fair competition between systems calls for a risk-adjusted compensation; that is, the dispensing private insurer should compensate the receiving public insurer.

Selection problems received considerable attention in both academic circles and policy debates (for overviews see, e.g., Van de Ven and Ellis 2000, Van de Ven et al. 2003, and for the German case Buchner and Wasem 2003). In most cases, the focus was on selection issues within the public system. Selection between public and private health insurance has been largely ignored. This is surprising, as health systems with sizeable public and private sectors exist in several countries, USA, Germany, and the Netherlands being the prime examples. Debate on public-private health care financing, however, extends to other countries, including Australia, Austria, Canada, Greece, Ireland, Italy, Portugal, Spain, and UK (Healy et al. 2006, Mossialos and Thomson 2004). But still, the impact of health care financing on access to health care and on health outcomes is not well understood (see Tuohy et al. 2004 for an international survey). The analysis in this chapter aims at improving the understanding of the effects of parallel health care financing by focusing on risk selection between public and private health insurance.

One of the very few studies that investigate risk selection between public and private health insurance is the analysis offered by Sapelli and Vial (2003). They analyze the Chilean health insurance market and find advantageous selection in favor of private insurers. As premium calculations in Chile and Germany are similar, selection incentives are similar as well. However, there are also differences between countries. In Germany, the purchase of private health insurance implies that an individual leaves the public system; thus, they have no access to the public system's benefits. In Chile, they, de facto, have. This is why Sapelli and Vial (2003) may, to some extend, be read as a paper on supplementary private health insurance. There are two studies on Germany that analyze risk selection between systems, Albrecht et al. (2007) and Greß (2007). While the former article only provides a descriptive analysis, the latter finds adverse selection against the public plan but does not analyze how selection actually works or what can be done about it. The current study fills this gap.

This chapter contributes to the literature on risk selection. Cutler and Reber (1998) and Nicholson et al. (2003) found evidence for selection in favor of Health Maintenance Organizations (HMO). Both studies analyze actual cost data and show that those who switch to a less generous plan; that is, to an HMO, have lower health care expenses than those who remain in the pool. Nuscheler and Knaus (2005) find no selection within the German public health insurance system. Other recent studies on Germany investigate the effectiveness of competition among public health insurers focusing on the price elasticity of switching (e.g., Schut et al. 2003 and Tamm et al. 2007) or on the means of competition (Becker and Übelmesser 2007). None of these studies, however, addresses the selection problem between public and private insurers in Germany.

This remainder of this chapter is organized as follows. Section 3.2 provides the institutional background. The dataset is introduced in in Section 3.3, followed by a discussion of the empirical strategy in Section 3.4. Section 3.5 presents our results and Section 3.6 offers concluding remarks.

3.2 Institutional background

The analysis sample used to identify risk selection ranges from 2000 to 2007. Accordingly, this section concentrates on the institutions and regulations of that period. As some aspects of the 2009 reform are relevant to our study, we refer to them whenever we see fit.

While most residents of Germany are obliged to buy public health insurance, there is still a sizeable share of the population that are allowed to opt for a private alternative. In the following, we will describe both the public and the private health insurance system in some detail, including a separate discussion of health care financing for civil servants, the so-called 'Beihilfe-scheme'. Table 3.1 provides an overview of the most important characteristics of public and private health insurance. The institutional differences govern the incentives to switch between systems. Switching regulations and switching incentives are analyzed and discussed in separate subsections.[1]

Table 3.1: Characteristics of public and private health insurance

Characteristic	Public Insurance	Private Insurance (regular)
Risk rated premiums	no	yes
Income related premiums	yes	no
Community rating	yes	no
Open enrollment	yes	no
Risk adjustment	yes	no
Regulation of benefits	medical necessities	at least public benefits
Provision of benefits	in-kind	cash
Family insurance	yes	no
PAYGO system	yes	no
Portability of old age provisions	NA	yes ($\leq$ 100%)

[1]Note that the public-private-terminology refers to the financing of health care and not to its provision. Providers are mostly private and usually hold contracts with both public and private insurers.

3.2.1 Public health insurance

There are currently about 150 non-profit public health insurers (sickness funds) that cover roughly 90% of the population. In most cases, enrollment in the public system is compulsory. Since the 1996 reform, publicly insured individuals can freely choose their sickness fund. Due to the individuals' remarkably high propensity to switch from one sickness fund to another, there is considerable competition among public insurers.

Premiums in the public system are subject to community rating; that is, a sickness fund is legally obliged to demand the same contribution rate from all its enrollees. As a result, the contribution rate is independent of individual characteristics, including risk status. The contribution rate is then used to calculate the insurance premium. Table 3.2 provides the annual gross income ceilings up to which the insurance premiums are proportional to income. For individuals with incomes higher than the ceiling, the insurance premium is flat and given by the product of contribution rate and income ceiling. Under these constraints (and a zero-profit requirement), sickness funds were free to set their contribution rate.

Table 3.2: Contribution ceiling and threshold for compulsory insurance*

Year	Income Ceiling (public premium calculation)	Income Threshold (compulsory public insurance)
2000	39576	39576
2001	40034	40034
2002	40500	40500
2003	41400	45900
2004	41850	46350
2005	42300	46800
2006	42750	47250
2007	42750	47700

Note: * Annual Gross Income in Euros.

With community-rated premiums, public insurers have an incentive to engage in risk selection. As a response to this market failure, several measures were introduced to prevent risk selection or to mitigate the incentives for risk selection. Open enrollment prevents insurers from denying coverage to individuals with an unfavorable risk profile. The benefit package is highly regulated so that sickness funds cannot design their benefit packages in a way to attract favorable individuals or to alienate unfavorable ones (about 95% of the benefit package is set by law, see Buchner and Wasem 2003). Finally, risk adjustment reduces the incentives to engage in risk selection. As Nuscheler and Knaus (2005) found no evidence for selection by funds, the aforementioned measures appear to be effective.[2]

Finally, two additional characteristics of public health insurance are relevant to this analysis. First, public benefits are provided in-kind. Second, one of the (potential) benefits of public health insurance is 'family insurance'; that is, all dependent family members without income (e.g., married partners and children below the age of 26 years) are covered for free.

3.2.2 Private health insurance

About 10% of the German population is covered by private health insurance. In total, there are 45 for-profit insurance companies providing such insurance. As already mentioned in Section 3.1, the most important difference to the public system is premium calculation: private premiums are risk rated, and typically, underwriting is conducted using a health questionnaire. Risk assessment is carried out only once; that is, private health insurers must not adjust the premium when health deteriorates. Unlike in the public system, the resulting premiums are not directly related to individual income. There is family insurance in the private system, but in contrast to the public system this insurance is not free; that is, an insured individual has to pay additional premiums for married partners and children.

Benefits of private health insurance must be at least as comprehensive as public benefits. In fact, most contracts come with more benefits in terms of services and service quality. It is noteworthy that all private benefits are provided in cash: patients pay providers and then seek reimbursement from their insurer.

In contrast to the public system, private health insurance is not a pure pay-as-you-go

[2]Nevertheless, politicians and several reports came to the conclusion that risk adjustment needed to be improved (see, e.g. Jacobs et al. 2002 and Lauterbach and Wille 2001). In 2009, a new risk adjustment mechanism was implemented that, in addition to some socio-economic characteristics, uses as many as 106 hierarchic morbidity indicators to calculate the transfers among funds.

system. To hold premiums constant over the life cycle, private insurers are legally obliged to build up old age provisions while the insured is young (and relatively healthy) to subsidize premiums when he or she is old (and relatively sick).[3] Due to technological progress and the lack of cost-containment measures in the private system, old age provisions are insufficient, and as a result, premiums increase over time.[4]

Competition among private health insurers was and still is severely undermined by regulation. Prior to 2009, old age provisions were not transferrable from one private insurer to another. Thus, an insured person who switched insurers lost all old age provisions and with it the financial resources needed to subsidize the insurance premium when he or she is old. The requirement to build up a new capital stock with the new contract increased the contract premium. As the premium effect increases with contract duration, consumers were – after a couple of years – essentially locked in. Competition was, thus, most effective for new customers but less so for existing customers.

3.2.3 Health care financing for civil servants

Civil servants may be appointed at the federal level or at the level of a regional state. The respective health authority covers a fraction of the insured benefits via the so-called 'Beihilfe-scheme'.[5] Covered benefits compare to those of the public system, but, in contrast to the public system, benefits are provided in cash and are financed from general taxation rather than social security contributions. Depending on marital status and the number of children, the respective health authority covers 50% to 70% of the costs of insured benefits (80% for children).

As an example, consider an unmarried civil servant without children. In this case, the health authority covers 50% of the costs of insured benefits. For the remaining 50%, the

[3] This obligation implies that premiums depend on the individual's age at the time private health insurance is purchased.

[4] Recall that the more important premium risk is insured: private insurers must not adjust premiums when individual risk changes, e.g., when the individual ages.

[5] There is no unified 'Beihilfe-scheme' applicable to all civil servants in Germany but rather 17 different laws (1 federal and 16 regional). Differences, however, are negligible and we here concentrate on the common aspects of these schemes.

individual may buy health insurance, either private or public. The problem with the latter option is that no public contract with less than 100% cover is available. As a result, an individual that opts for public insurance has to pay the full public premium (instead of 50% of it). The first option is usually more attractive, as the remaining 50% coverage can be purchased at the respective 50% premium. Consequently, most civil servants purchase private health insurance.[6]

3.2.4 Switching between insurance systems – regulations

Most residents of Germany must purchase public health insurance. Some professions (most notably, civil servants and self-employed persons) and high-income individuals, however, can choose either public or private health insurance. The income threshold above which individuals are allowed to buy private health insurance varies over time (see Table 3.2).[7] Those who purchase private insurance leave the public system; that is, they are not eligible for publicly financed benefits, and they do not directly contribute to the financing of the system.[8] Individuals who stay in the public system despite their eligibility to purchase the private alternative are *voluntarily insured* in the public system.

As individuals aged 55 years or older must not switch from private to public health insurance, we concentrate on the privately insured below 55 years of age. For our observation period, a switch was allowed if the insured was enrolled in a public plan for at least 24 consecutive months within the previous five years. These requirements created a problem for those who dropped out of the private system but did not meet these criteria. Self-employed individuals who lost their business and with it the ability to pay private premiums may serve as an example. As a result, some individuals were left without health insurance (about 0.4% in 2008).

There are several circumstances under which an individual must switch from private to

[6]In our analysis sample, about 90% of civil servants hold private health insurance.

[7]In our empirical model, this variation is captured by time fixed effects.

[8]There are indirect payments through general taxation (4.7% of public health care spending in the first half of 2009, see BMG 2009) and cross-subsidization via higher reimbursement rates in the private system.

public insurance. If, for instance, gross annual income regularly falls short of the compulsory public health insurance income threshold, then public enrollment is obligatory. To a large extent, this also applies to privately insured individuals who get laid-off. For the unemployed, there were and still are exemptions from this ‘obligation’. Those unemployed who have been privately insured for the previous five years (at least) are allowed to stay in the private system. If they do, however, they are on no account allowed to join the public plan in the future. The federal employment agency generally pays the health insurance premium for all unemployed individuals but only up to the amount that would have been due in the public system. If the private premium is larger, the insured person has to pay the difference. The inability to pay resulted in a loss of health insurance.[9]

3.2.5 Switching between systems – incentives

The institutional environment allows us to infer the directional effects of several variables on the propensity to switch between systems. Table 3.3 shows the predicted signs for a switch from public to private health insurance and vice versa.

Table 3.3: Switching incentives between systems

Variable	Public to Private	Private to Public
Health Status	+	−
Gross Income	+	−
Civil Servant	+	−
Self-Employed	?	?
Unemployed	−	+
Female	−	+
Age	−	−
Married	−	+
Number of Children	−	+

There are risk-rated premiums in the private market while public premiums are subject to community rating. All else equal, the public-private premium differential will be larger

[9]Since the 2009 reform, individuals cannot loose health insurance; they always have access to private health insurance (see Busse and Stock 2010 for details).

for healthy individuals so that their incentive to switch to the private system is larger. Now consider a privately insured individual that turns into a high risk; then, there is a supply side effect and a demand side effect that both work in the same direction. As the private insurer must not adjust the premium, the insurer has an incentive to dump the individual into the public system, and a negative effect of health on switching is expected. This supply side effect may be reinforced by the demand side. Due to the differences in the provision of benefits, an individual that experienced a negative health shock may have to seek reimbursement on a regular basis. The associated (opportunity) costs do not exist in the public system, as benefits are provided in-kind rather than in cash.

Only individuals with voluntary public health insurance are allowed to purchase private health insurance. As a sufficiently high gross income is one way to obtain this eligibility, we expect a positive effect of gross income on the probability to switch (see Table 3.2 for the income thresholds). If gross income of a privately insured individual regularly falls short of the income threshold for compulsory public insurance, enrollment in the public system is obligatory. Thus, gross income is expected to be negatively correlated with the probability to switch from private to public insurance.

The public private premium differential is particularly large for civil servants. If they purchase public insurance, then they have to pay the full public premium. In contrast, private insurance can be tailored to their needs; that is, they can buy coverage only for the fraction of health care expenses not covered by the 'Beihilfe-scheme'.

The self-employed are generally exempt from mandatory public health insurance, although the public benefit package is available at the maximum public premium. Depending on preferences, self-employed individuals may prefer a more comprehensive cover and purchase private health insurance. Although preferences are likely to be correlated with employment status, it is hard to sign their effect on switching probabilities for both directions. In contrast, there is a clear prediction for the unemployed. An unemployed publicly insured individual must not switch to the private system, as neither the income criterion nor the profession requirement is met. If a privately insured individual gets laid-off, regulation may dictate enrollment in the public system.

Women have higher expected health care expenses than men, and this difference is reflected in private health insurance premiums. As there is no such difference in the public system, the incentives to switch to the private system are smaller for females. The private system is, thus, generally less attractive for women. This also implies that women are more inclined to switch (back) to the public system.

For both directions, age is expected to have a negative effect on the propensity to switch between systems. Suppose that a publicly insured individual considers switching to the private system. The older this individual is, the less time is available for him or her to build up old-age provisions in the private system. Thus, the private premium is, ceteris paribus, set at a higher rate the older the individual is at the time the private health insurance is purchased. The negative age effect for the opposite direction is a direct implication of the regulations described above. It is, ceteris paribus, less likely that an older individual meets the criteria that allow him or her to switch from private to public health insurance.

The effects of marital status and the number of children on switching behavior are governed by family insurance. As this benefit is only available in the public system, the signs are fairly self-explanatory.

3.3 Data, Sampling, and Descriptive Statistics

3.3.1 The German Socio-Economic Panel

We use the 2000 to 2007 waves of the German Socio-Economic Panel (SOEP) to analyze risk selection between public and private health insurance in Germany. On an annual basis, this representative survey collects extensive information about the same 11,000 private households (more than 20,000 individuals). Since its inception in 1984, the SOEP has seen several extensions and refreshment samples, which are included in our analysis.[10]

In addition to standard socio-demographic and socio-economic variables (e.g., age, gen-

[10]For more information on the SOEP data set, see Wagner et al. (1993) and Wagner et al. (2007).

der, and employment status), the SOEP contains information on health status (e.g., self-assessed health) and health care utilization (e.g., the number of doctor visits and hospital stays). This information enables us to come up with a proxy for individual risk. Finally, the SOEP collects information about health insurance. For every single year, we know whether an individual was enrolled in the public health care system or whether private health insurance was purchased. Table 3.4 provides a description of all variables used in our analysis. The corresponding descriptive statistics are displayed in Table 3.5.

One of the advantages of the data set is its panel structure. It allows us to follow the same set of individuals for a reasonable amount of time and thereby to construct two dummy variables that indicate a switch from one system to the other. More precisely, the variable $PUB2PRIV_{it}$ is 1 if individual i was publicly insured in period t but privately insured in period $t+1$ and zero otherwise. Similarly, the variable $PRIV2PUB_{it}$ is 1 if individual i was privately insured in period t but publicly insured in period $t+1$.

To analyze switching decisions between systems, we split the sample into two sub-samples: one 'public sample' and one 'private sample'. The assignment is based on initial insurance status. The full data set contains 65,901 observations (55,626 public and 10,275 private), and there are a reasonable number of switches (705 switches from public to private and 539 from private to public).

3.3.2 Sample Selection

Sample selection is governed not only by Germany's health insurance institutions and its regulations but also by the research question we want to answer. First of all, we restrict ourselves to individuals aged between 26 and 53 years. The exclusion of individuals under 26 years minimizes the risk of measuring effects that are rooted in expiring family insurance. The upper limit guarantees that all switchers from public to private actually make the same decision. As it is impossible to switch from private to public once an individual is 55 years or older, switching for a person at least 54 year old would also be irreversible. We have further restricted the sample to observations with a maximum of one switch between systems, as

Table 3.4: Explanation of variables

Variable	Explanation
Dependent variables	
$SAH5_t$	Self-assessed health status, 1 = bad, 2 = not so good, 3 = satisfactory, 4 = good, 5 = very good
$SAH2_t$	1 = if $SAH5_t \geq 4$, 0 = if $SAH5_t \leq 3$ (cut-off 3/4)
$PUB2PRIV_t$	1 = switch from public into private system (public in t and private in $t+1$)
$PRIV2PUB_t$	1 = switch from private into public system (private in t and public in $t+1$)
Explanatory variables	
$DISAB_t$	1 = disability or incapacity to work
$VISITSDOC_t$	Number of visits to doctors during the last three month
$HOSPITAL_t$	1 = hospital stay during the last year
$SICKSIX_t$	1 = work disability for longer than 6 weeks during the last year
$LNINCOME_t$	Natural logarithm of net income of the last month
$INCOMEGR_t$	Gross income of the last month divided by 100
$CIVILSERV_t$	1 = if civil servant
$SELFEMP_t$	1 = selfemployed
$UNEMP_t$	1 = unemployed
$FEMALE_t$	1 = female
$AGE26\_30_t$*	1 = age between 26 and 30
$AGE31\_35_t$	1 = age between 31 and 35
$AGE36\_41_t$	1 = age between 36 and 41
$AGE42\_47_t$	1 = age between 42 and 47
$AGE48\_53_t$	1 = age between 48 and 53
$MARRIED_t$	1 = married
$CHILDNUM_t$	Number of children
$TRAINING_t$	1 = if receiving educational training
EDU_t	Number of years completed in the education system
$GERMAN_t$	1 = German nationality
$PUBLEMP_t$	1 = if public employee
$FULLTIME_t$	1 = full time employed
$JOBWEST_t$	1 = job in western part of Germany
$YEAR20XX_t$	1 = year XX=00*, . . . , 06
$PUBCOMP_t$	1 = compulsory public insurance
$PUBHOLDER_t$	1 = holder of public insurance contract (not covered by public family insurance)
$PRIVHOLDER_t$	1 = holder of private insurance contract (not covered by private family insurance)

Notes: * indicates that the variable is a reference category in our estimation.
If not otherwise indicated all variables are measured at present (time of interview).

Table 3.5: Mean and standard deviation for all variables

	Public				Private			
	Non-switcher		Switcher		Non-switcher		Switcher	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
SAH5	3.5931	0.8161	3.8268	0.7781	3.7110	0.7958	3.7826	0.8382
SAH2	0.5968	0.4905	0.7314	0.4437	0.6583	0.4743	0.6680	0.4719
DISAB	0.0469	0.2114	0.0113	0.1058	0.0353	0.1845	0.0237	0.1525
VISITSDOC	1.8950	3.1422	1.5959	2.8167	1.9296	3.2880	1.5692	2.5589
HOSPITAL	0.0763	0.2656	0.0519	0.2221	0.0700	0.2551	0.0514	0.2212
SICKSIX	0.0423	0.2013	0.0203	0.1412	0.0298	0.1699	0.0435	0.2043
LNINCOME	7.2792	0.7104	7.6394	0.6711	7.9149	0.6279	7.6445	0.7291
INCOMEGR	28.4760	20.2596	40.4062	24.5142	49.4750	41.3687	39.2497	27.7336
CIVILSERV	0.0060	0.0775	0.1467	0.3542	0.4165	0.4930	0.2846	0.4521
SELFEMP	0.0151	0.1221	0.0497	0.2175	0.0385	0.1924	0.0949	0.2936
UNEMP	0.0082	0.0904	0.0023	0.0475	0.0003	0.0175	0.0040	0.0629
FEMALE	0.4930	0.5000	0.3567	0.4796	0.3245	0.4682	0.3399	0.4746
AGE	40.1943	7.4027	38.1558	7.4646	42.2026	7.0429	40.8339	7.6529
AGE26_30	0.1209	0.3261	0.1828	0.3870	0.0611	0.2395	0.1304	0.3374
AGE31_35	0.1719	0.3773	0.2302	0.4215	0.1430	0.3501	0.1462	0.3541
AGE36_41	0.2577	0.4374	0.2686	0.4437	0.2369	0.4252	0.2292	0.4212
AGE42_47	0.2492	0.4326	0.1716	0.3774	0.2760	0.4471	0.2490	0.4333
AGE48_53	0.2002	0.4002	0.1467	0.3542	0.2831	0.4505	0.2451	0.4310
MARRIED	0.6819	0.4658	0.5711	0.4955	0.6824	0.4656	0.6285	0.4842
CHILDNUM	0.8589	1.0057	0.6704	0.9295	0.8159	0.9726	0.8340	1.0893
TRAINING	0.0371	0.1891	0.0670	0.2554	0.0399	0.1957	0.0672	0.2509
EDU	12.2205	2.4581	14.2088	2.9040	14.8794	2.9240	13.4387	2.8248
GERMAN	0.9155	0.2782	0.9413	0.2353	0.9701	0.1704	0.9407	0.2366
PUBLEMP	0.1146	0.3186	0.1535	0.3609	0.2400	0.4271	0.1779	0.3832
FULLTIME	0.7372	0.4402	0.8600	0.3473	0.8800	0.3250	0.8617	0.3459
JOBWEST	0.6485	0.4774	0.6569	0.4753	0.7241	0.4470	0.6482	0.4785
YEAR2000	0.1602	0.3668	0.1287	0.3352	0.1198	0.3248	0.1779	0.3832
YEAR2001	0.1497	0.3568	0.1783	0.3832	0.1146	0.3186	0.1225	0.3285
YEAR2002	0.1494	0.3564	0.1828	0.3870	0.1671	0.3731	0.1976	0.3990
YEAR2003	0.1430	0.3501	0.1467	0.3542	0.1609	0.3675	0.1225	0.3285
YEAR2004	0.1381	0.3450	0.1084	0.3112	0.1517	0.3588	0.1225	0.3285
YEAR2005	0.1279	0.3339	0.1220	0.3275	0.1425	0.3496	0.1107	0.3143
YEAR2006	0.1317	0.3382	0.1332	0.3402	0.1433	0.3504	0.1462	0.3541
PUBCOMP	0.7949	0.4037	0.4515	0.4982				
PUBHOLDER	0.9514	0.2150	0.9661	0.1811				
PRIVHOLDER					0.9641	0.1861	0.8893	0.3143
N	40185		443		6518		253	

there are strong indications of measurement error when more than one switch has occurred (see Table 3.6).[11]

Table 3.6: Sample selection

	Total		Public		Private	
Data Selection Process	All	Switches	All	Switches	All	Switches
Full Sample	65901	1244	55626	705	10275	539
Age 26-53	48358	921	41155	552	7203	369
Max. 1 Switch	47399	696	40628	443	6771	253

The resulting samples consist of two types of individuals. The first type is allowed to switch from one system to the other and the second type is not. A switcher analysis based on these samples, thus, measures the sum of two forms of risk selection, namely, active and passive risk selection. Active risk selection refers to the insurers' ability to screen out the healthy from the pool of those who are allowed to switch. If the pool of those who are allowed to switch is healthier on average than the pool of those who are not, risk selection is a direct consequence of the regulatory environment described in Section 3.2.4. We refer to this form of selection as passive risk selection. As we are interested in the full extent of selection, we do not distinguish between active and passive risk selection.[12]

We arrive at an analysis sample that comprises 40,628 (443) and 6,771 (253) observations (switches) for the public system and private system, respectively. Thus, within our observation period, there is a 1.1 (3.7)% probability that a publicly (privately) insured individual purchases the private (public) alternative. Without revealing a clear time pattern, Table 3.7 shows a considerable variation in switching probabilities over time.

[11] This is unproblematic for our analysis, as only persistent changes are relevant for risk selection.

[12] In fact, for the private sample, such a distinction is impossible, as the information needed to categorize individuals is not available in the SOEP. In contrast, this information is available for the public sample. In an earlier version of our analysis, we concentrated on active risk selection and found no evidence for it (see Grunow and Nuscheler 2010, for details). We find no evidence for selection for the (larger) public sample considered here so that a distinction between the two forms of selection is unnecessary.

Table 3.7: Distribution of switchers

	Public		Private	
Year	All	Switcher %	All	Switcher %
2000	6496	0.88	826	5.45
2001	6096	1.30	778	3.98
2002	6083	1.33	1139	4.39
2003	5811	1.12	1080	2.87
2004	5597	0.86	1020	3.04
2005	5192	1.04	957	2.93
2006	5353	1.10	971	3.81
Total	40628	1.09*	6671	3.74*

Note: * Mean share of switchers over time.

3.3.3 Self-Assessed Health Status

We approximate individual risk using the SOEP variable ‘self-assessed health at present’. As usual, this variable has five outcomes ranging from very good health to bad health, and we denote it by SAH5. The distributions of self-assessed health conditional on switcher status reveal a clear pattern (see Table 3.8): switchers tend to be healthier. Note, however, that the correlation between switching and self-assessed health is less pronounced for private to public switches.

Table 3.8: Distribution of self-assessed health (SAH5)

	Public		Private	
SAH5 Category	non-Switcher	Switcher	non-Switcher	Switcher
1 (Bad)	0.95	0.90	0.63	0.79
2 (Not so good)	8.43	4.51	6.49	5.53
3 (Satisfactory)	30.94	21.44	27.05	26.88
4 (Good)	49.73	57.34	52.82	48.22
5 (Very good)	9.96	15.80	13.01	18.58
Total	100	100	100	100

Note: Shares of respondents in each category averaged over years.

For the purposes of this analysis (see Section 3.4 for details), we collapse the variable

SAH5 into the binary variable SAH2, where SAH2 assumes the value 1 if self-assessed health is 'good' (SAH5 = 4) or 'very good' (SAH5 = 5) and zero otherwise. Given the concentration of responses on categories 3 and 4, the inevitable loss of information when using the more aggregate measure SAH2 instead of SAH5 is minimized.[13]

SAH2 reflects the share of individuals with good or very good health. As Table 3.9 shows, there is a downward trend in self-assessed health for those who remain in their respective health system (health deteriorates as individuals grow older). There is no such pattern for switchers. Table 3.9 also shows correlations between switching decisions and individual health. Similar to SAH5, this correlation is stronger for public to private switches. More precisely, there is a positive correlation between health and the propensity to switch from public to private health insurance, while no clear health effect is observed for switches from private to public health insurance. Taken together, Table 3.9 suggests advantageous selection in favor of private insurers, a conjecture that will be confirmed in Section 3.5. Note, however, that the econometric analysis reveals an insignificant health effect for a switch from public to private health insurance and a significantly *negative* health effect for the opposite direction.

Table 3.9: Mean self-assessed health (SAH2)

	Public		Private	
Year	non-Switcher	Switcher	non-Switcher	Switcher
2000	0.6024	0.7544	0.6825	0.6957
2001	0.6101	0.6835	0.6734	0.7576
2002	0.5953	0.7160	0.6639	0.6600
2003	0.6013	0.7846	0.6749	0.6875
2004	0.6015	0.7917	0.6491	0.7097
2005	0.5837	0.6852	0.6301	0.6071
2006	0.5795	0.7288	0.6381	0.5000
Mean	0.5968	0.7314	0.6583	0.6680

Note: SAH2 = 1 if self-reported health is good or very good.

[13] Our results are largely robust to altering the cut-off (see Section 3.5 for details).

3.4 Empirical strategy

In order to identify risk selection between public and private health insurance, we investigate the impact of health on switching behavior. As already mentioned in the previous section, the analysis sample contains several variables that can be used as proxies for individual health (Table 3.4). Self-rated health is a natural candidate, but the more objective health and health care utilization measures are also qualified measures of individual health. Although all these variables could be used as regressors in a switcher analysis, the inherent problem is identification. To identify risk selection, all coefficients of all health measures need to have the same sign. Otherwise, the incomplete ordering of the n-dimensional space would yield inconclusive results.

We solve this problem by constructing a health index that summarizes all relevant health information in a single one-dimensional index. Like van Doorslaer and Jones (2003) and Nuscheler and Knaus (2005), we use a regression of self-assessed health on the more objective health measures and some additional explanatory variables to obtain such an index. In the switcher analysis, this index is then used as an explanatory variable. This gives rise to the following recursive two-stage regression model:

$$SAH2_{it} = Z'_{it}\alpha + X'_{it}\beta + \lambda_{1t} + \upsilon_{1it} \tag{3.1}$$

$$SWITCH_{it} = \gamma\widehat{SAH2}_{it} + X'_{it}\delta + \lambda_{2t} + \upsilon_{2it}. \tag{3.2}$$

At the first stage, the binary measure of self-assessed health, $SAH2_{it}$, is regressed on more objective health measures, Z_{it}, and some additional covariates, X_{it}. The respective prediction yields the probability of being in good or very good health – a continuous measure that we interpret as health index. The matrix Z contains the following health measures: (legally obtained) disability status at present,[14] the number of visits to a doctor during the 3 months prior to the interview, whether the individual was in hospital during the previous year, and

[14]To legally obtain disability status, an individual has to undergo an audit. The auditor grants disability status only if limitations add up to at least 50%.

whether the individual experienced a sickness absence from work of 6 weeks or more during the previous year. Finally, Z contains the natural logarithm of net individual income. This variable captures that more affluent individuals may invest more funds in the production of health (see, e.g., Grossman 1972).

Necessary conditions for our approach to be valid are that (i) the first stage does not suffer from simultaneity bias, (ii) the variables contained in Z have no independent effect on the probability to switch between systems, and (iii) self-rated health is an appropriate measure for risk as measured by future expenses on health care. Three out of four objective health measures and net income refer to a time period prior to the interview so that causality can hardly run from self-rated health to these measures. Disability status is more critical, as it is measured at present just like self-rated health. It is hard to imagine, however, how a low subjective health rating affects the chance of legally obtaining the disability status. Thus, it is more plausible that disability causes a low health rating so that we are confident that requirement (i) is met. The second requirement is an identifying assumption. It implies that the health index captures all information contained in Z that is relevant to individual switching behavior. Health care utilization measures could only have an independent effect on switching if there was differential access to care between systems. In fact, there is not much room for such a difference, as the set of available providers in the two systems is almost identical. Moreover, Table 3.5 reveals no systematic difference in health care utilization between systems. The strong correlation between self-rated health and disability status prevents an independent effect of the latter variable. As gross income governs the incentives to switch between systems, net income can barely have an additional effect. Finally, self-rated health needs to be a predictor of future health care expenses. The literature on risk adjustment generally finds that self-rated health significantly contributes to explaining the variance in health care expenses (see van de Ven and van Vliet 1992 for an overview).

At the second stage, the health index $\widehat{SAH2}_{it}$ is used to identify risk selection. The dependent variable $SWITCH_{it}$ is either $PUB2PRIV_{it}$ for the public sample or $PRIV2PUB_{it}$ for the private sample. Note that $SWITCH_{it}$ assumes the value 1 if individual i switched between periods t and $t + 1$. Thus, in a regression of $SWITCH_{it}$ on the estimated health index and a set of control variables, X_{it}, reverse causality is ruled out, as all covariates refer to a time prior to the switch. This regression yields the individual probabilities to switch between systems.

In addition to solving the above-mentioned dimensionality problem, our two-stage estimation procedure comes with a second advantage: the approach mitigates attenuation bias. Self-rated health may be measured with considerable error (see Crossley and Kennedy 2002) so that such a bias may actually arise. As the health measures contained in Z are likely to be measured with smaller error, our health index is less plagued by measurement error.

Finally, we have to address the question of how to deal with the panel structure of the data. Tables 3.7 and 3.9 revealed a notable variation over time of both health and switching decisions. Equations (1) and (2) show that we generally control for unobserved heterogeneity that affects all individuals likewise using time fixed effects, λ_{kt}, $k = 1, 2$.

Without further assumptions about the error terms, v_{kit}, Equations (1) and (2) simply describe a pooled regression where observations on one and the same individual in different years are considered independent. This approach clearly understates standard errors so that internal validity would be compromised (significance levels are not met). In addition, the resulting estimates may be biased due to omitted variables. This is the case if individual unobserved heterogeneity is correlated with the explanatory variables.

Consequently, we need to specify a panel data model. As usual, this requires assumptions about the error terms in Equations 3.1 and 3.2. Let $v_{kit} = \mu_{ki} + u_{kit}$; then, we arrive at a random effects model if the individual error component μ_{ki} is uncorrelated with the explanatory variables. In case of a correlation, we obtain the fixed effects model. To select between models we conduct a Hausman test that generally rejects the random effects model. Accordingly, the next section concentrates on the fixed effects model.

Before we turn to the results, we have to address two questions: how do we estimate the model given by Equations 3.1 and 3.2 and how do we interpret its results? At both stages, the dependent variable is binary so that discrete choice models would have been in order. As there is no estimator available for the resulting highly non-linear recursive panel data model, we opt for a linear model at both stages. The model can then be estimated using two-stage least squares and STATA's `xtivreg` command is used.[15]

As usual, identification in fixed effects models rests on within-subject variation of covariates; that is, on variation of individual characteristics over time. This implies that we cannot estimate the impact of time invariant variables (e.g., gender) and only imprecisely measure the effect of those variables with little variation over time (e.g., years in the education system and the number of children). In short, identification rests on changes in variables and not on their levels. We can, thus, only estimate the effect of changes in health on the probability to switch between systems and this effect is – due to our two-stage procedure – identified off changes in the explanatory variables contained in Equation 3.1 but not in Equation 3.2, i.e., the variables contained in Z. Our approach may thus not measure the full extent of risk selection.[16] This may well be the case if (i) the health differential between switchers and non-switchers of Table 3.9 is not the result of health shocks but of systematic differences in health levels and (ii) the motivation to switch originates in health levels and not in health changes. This potential problem calls for an estimator that also uses between subject variation for identification, and the natural candidate is the random effects estimator. However, as already mentioned above, this specification was rejected by the Hausman-Test so that we are left with the fixed effects model.

[15] This also implies that second stage standard errors take into account that the health index is estimated at stage 1.

[16] We thank an anonymous referee for pointing this out.

3.5 Results

3.5.1 Switcher analysis

Risk selection

In Section 3.2.5, we discussed individual switching incentives and summarized them in Table 3.3. One of the predictions was that health (SAH2) positively affects the probability to switch from public to private health insurance. Our second-stage regression results show that the estimated coefficient (0.0087) has the predicted sign but is statistically not different from zero (see Table 3.10). Private insurers are, thus, unable to select the healthy from the public pool.

At first sight, this result is somewhat surprising. With community rating in the public system and underwriting and pre-existing condition clauses in the private system, the healthy are expected to lean more toward private health insurance. One can think of several reasons for the lack of statistical significance. It may be, for instance, that the health questionnaire used by private insurers is not all that informative about individual risk. Applicants may overstate their health, and this could prevent insurers from selecting the healthy. However, even if the health questionnaire was informative, a sufficiently large public-private premium differential could render private health insurance attractive (almost) independent of an individual's risk type. Insufficient variation of the health index may undermine identification. As about 80% of responses fall in two adjacent categories of self-assessed health (categories 3 and 4, see Table 3.8), insufficient variation is likely an issue. Aggravating this problem is the source of identification in fixed effects models: advantageous selection is identified off *changes* in individual health and not off its levels (see the discussion at the end of Section 3.4).

The results for a switch from private to public health insurance are more clear cut. As predicted, we find a negative effect of health on the probability to switch (−0.0618, Table 3.10). A 10 percentage point increase in the health index leads to a 0.6 percentage point decrease in the probability to switch. Given the baseline switching probability of 3.7%, this is a sizeable effect.

As already argued in Section 3.2.5, this effect may be driven by both demand side forces and supply side incentives. If an individual experiences a health shock, then health care utilization is likely to increase in the future. This makes the public system more attractive as benefits are provided in-kind. A switch to the public system allows the individual to avoid the hassle of seeking reimbursement for consumed health care services. Private health insurers can use the 'reimbursement channel' strategically to dump unprofitable customers into the public system. There is anecdotal evidence that private insurers delay or even deny reimbursement of services that are covered by the insurance contract. To eventually obtain reimbursement, the individual has to sue the insurer. This not only involves monetary costs (lawyers, opportunity costs, and risk) but also non monetary costs (stress) so that many patients eschew to go to court (see, e.g., Focus Online 2009).[17] Effective patient copayments may therefore be considerable so that a switch to the public system can be an attractive option.

Although we are unable to separate demand side effects from supply side implications, we can draw a firm policy conclusion. As it is impossible for public insurers to deny coverage (provided that the insured is legally allowed to switch), the costs of the health shock that should be borne by the private insurer are 'socialized'. Via the risk adjustment mechanism, the receiving public insurer is compensated by all other public insurers. The private insurer should then, in turn, compensate the public system. Obviously, such a compensation must be risk adjusted in order to guarantee just competition between insurance systems. Ideally, such a risk-adjusted transfer would eliminate private insurers' incentives to dump individuals that were subject to a health shock and also correct the distortions that originate in the demand-driven switching activities.

A potential concern about our empirical strategy is the aggregation of self-assessed health (SAH5) to the binary measure (SAH2). As about 80% of responses fall into categories 3 and 4 of SAH5 (see Table 3.8), the loss of variation is moderate. For our results, it appears more important how the cut-off to define SAH2 is chosen. We opted for a cut-off between

[17] It may also be that some individuals (at least partially) lose insurance cover when they get caught cheating on the health questionnaire.

Table 3.10: Fixed effects model for switching behavior

	Public		Private	
	Coefficient	SE	Coefficient	SE
SAH2	0.0087	0.0079	-0.0618**	0.0308
INCOMEGR	-0.0003***	0.0001	-0.0001	0.0001
CIVILSERV	0.0094	0.0163	-0.0230	0.0235
SELFEMP	0.0043	0.0044	-0.0075	0.0123
UNEMP	-0.0011	0.0069	0.2298**	0.0907
AGE31_35	0.0068**	0.0027	-0.0297**	0.0141
AGE36_41	0.0123***	0.0039	-0.0324*	0.0191
AGE42_47	0.0098**	0.0050	-0.0435*	0.0234
AGE48_53	0.0042	0.0061	-0.0446	0.0274
MARRIED	-0.0018	0.0027	0.0016	0.0108
CHILDNUM	0.0021	0.0013	0.0004	0.0057
TRAINING	-0.0091***	0.0031	0.0125	0.0124
EDU	0.0070***	0.0023	0.0013	0.0081
GERMAN	-0.0209**	0.0088	-0.0286	0.0840
PUBLEMP	0.0024	0.0018	0.0062	0.0066
FULLTIME	0.0064**	0.0025	-0.0110	0.0137
JOBWEST	-0.0012	0.0017	-0.0057	0.0078
YEAR2001	0.0092***	0.0015	0.0133*	0.0080
YEAR2002	0.0042**	0.0020	0.0030	0.0086
YEAR2003	0.0073***	0.0021	0.0078	0.0094
YEAR2004	0.0068***	0.0022	0.0094	0.0098
YEAR2005	0.0091***	0.0025	0.0138	0.0112
YEAR2006	0.0125***	0.0027	0.0288**	0.0120
PUBCOMP	-0.0121***	0.0020		
PUBHOLDER	0.0142***	0.0047		
PRIVHOLDER			-0.0102	0.0122
CONSTANT	-0.0741**	0.0298	0.1441	0.1508
N	40628		6771	
χ^2_4Age Dummies	22.84		5.25	
Prob > χ^2	0.0001		0.2624	

Note: *p<0.1, **p<0.05, ***p<0.01.

categories 3 and 4. Given the distribution of SAH5, the resulting variable SAH2 is the one with the highest variance among the alternative binary health variables. Table 3.11 shows that our risk selection results are relatively robust to variations in cut-offs. For a cut-off between categories 2 and 3, the results are almost exactly the same for both the public (the coefficient changes from 0.0087 to 0.0094 and remains insignificant) and the private sample (the coefficient changes from −0.0618 to −0.0739 and statistical significance is slightly weaker). A cut-off between categories 4 and 5, however, implies a large loss of information – the negative health effect on the probability to switch from private to public health insurance disappears (the coefficient, −0.0732; remains relatively stable though). This is perhaps not too surprising. Remember, the health effect is identified off health shocks, but if bad health already includes the four lowest out of five health categories, then there is not much room for a negative health shock.

Control variables

Gross income (INCOMEGR) has a negative effect on the probability to switch from public to private health insurance. This is surprising as a sufficiently high gross income is one way to obtain the eligibility to switch. As we can only speculate about the reasons and refrain from interpreting the coefficient. Table 3.10 also shows a significant negative effect of income on the probability to switch from private to public health insurance. The directional effect is as predicted, although significance is lacking.

When it comes to switching behavior, civil servants (CIVILSERV) and self-employed individuals (SELFEMP) are not different from other individuals. At least for the civil servant status, the insignificance comes at a surprise but only if one disregards the source of identification in fixed effects models. Insufficient within-subject variation in both, civil servant status and self-employment may explain why these variables fail to reach statistical significance.

If an individual is registered as unemployed (UNEMP), his or her income is below the compulsory public health insurance threshold. The individual, thus, must remain in the

Table 3.11: Fixed effects model for switching behavior for alternative cut-offs of SAH2

	Public				Private			
	cut-off 2/3		cut-off 4/5		cut-off 2/3		cut-off 4/5	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
SAH2	0.0094	0.0090	-0.0035	0.0314	-0.0739*	0.0420	-0.0732	0.1439
INCOMEGR	-0.0003***	0.0001	-0.0003***	0.0001	-0.0001	0.0001	-0.0001	0.0001
CIVILSERV	0.0091	0.0163	0.0096	0.0163	-0.0203	0.0235	-0.0269	0.0240
SELFEMP	0.0044	0.0044	0.0044	0.0045	-0.0124	0.0120	-0.0117	0.0121
UNEMP	-0.0013	0.0069	-0.0014	0.0070	0.2357***	0.0901	0.2449***	0.0927
AGE31_35	0.0067**	0.0027	0.0067**	0.0029	-0.0293**	0.0141	-0.0280**	0.0142
AGE36_41	0.0122***	0.0039	0.0124***	0.0041	-0.0318*	0.0190	-0.0305	0.0191
AGE42_47	0.0097*	0.0050	0.0099*	0.0051	-0.0434*	0.0233	-0.0423*	0.0234
AGE48_53	0.0040	0.0061	0.0042	0.0063	-0.0446	0.0273	-0.0436	0.0276
MARRIED	-0.0019	0.0027	-0.0020	0.0027	0.0031	0.0107	0.0009	0.0112
CHILDNUM	0.0020	0.0013	0.0019	0.0013	-0.0026	0.0056	-0.0014	0.0056
TRAINING	-0.0091***	0.0031	-0.0092***	0.0031	0.0140	0.0123	0.0138	0.0127
EDU	0.0070***	0.0023	0.0071***	0.0023	0.0009	0.0081	0.0008	0.0089
GERMAN	-0.0205**	0.0088	-0.0205**	0.0089	-0.0132	0.0832	-0.0456	0.1052
PUBLEMP	0.0023	0.0018	0.0024	0.0018	0.0065	0.0066	0.0055	0.0067
FULLTIME	0.0063**	0.0025	0.0062**	0.0026	-0.0143	0.0136	-0.0129	0.0139
JOBWEST	-0.0012	0.0017	-0.0012	0.0017	-0.0071	0.0078	-0.0071	0.0079
YEAR2001	0.0092***	0.0015	0.0091***	0.0015	0.0137*	0.0080	0.0122	0.0087
YEAR2002	0.0041**	0.0020	0.0038*	0.0021	0.0062	0.0083	0.0046	0.0095
YEAR2003	0.0072***	0.0021	0.0069***	0.0022	0.0103	0.0092	0.0076	0.0118
YEAR2004	0.0067***	0.0022	0.0063***	0.0023	0.0119	0.0095	0.0106	0.0139
YEAR2005	0.0088***	0.0024	0.0083***	0.0026	0.0179*	0.0106	0.0155	0.0165
YEAR2006	0.0120***	0.0026	0.0114***	0.0030	0.0338***	0.0113	0.0307*	0.0176
PUBCOMP	-0.0121***	0.0020	-0.0120***	0.0020				
PUBHOLDER	0.0140***	0.0047	0.0138***	0.0047				
PRIVHOLDER					-0.0113	0.0122	-0.0114	0.0123
CONSTANT	-0.0768**	0.0304	-0.0689**	0.0296	0.1667	0.1555	0.1431	0.2065
N	40628		40628		6771		6771	

Note: *p<0.1, **p<0.05, ***p<0.01.

public system. The expected negative effect of unemployment status on the probability to switch to the private system is not reflected in our regression results. Again, the insignificance is due to insufficient variation in unemployment status. Although a similar argument applies to the opposite switching direction, the few individuals that get laid-off are sufficient to obtain a positive and significant effect. The reason is that regulation may dictate enrollment in the public plan (see Section 3.2.4).

Due to the requirement to build up old-age provisions in the private system, we expected a negative effect of age (AGE) on the probability to switch from public to private health insurance (see Section 3.2.5). Our results show that individuals aged between 48 and 53 years are as likely to switch as 26 to 30 year-old individuals and that the middle-aged are more likely to switch. This points to the general attractiveness of the private system for all age groups despite the above-mentioned requirement. As expected, age has a significantly negative effect on the probability to switch from private to public health insurance.[18]

The availability of public family insurance makes the public system more attractive to married couples (MARRIED) and to families with children (CHILDNUM). This relative attractiveness is not mirrored in our results. Again, this may be due to insufficient within subject variation – only a change in marital status or in the number of children is used for identification. Obviously, the effect of gender is buried in the individual fixed effects.[19]

The coefficients of the year dummies show that the tendency to switch from public to private health insurance (slightly) increased over time. There is no clear pattern for the opposite direction. There are three additional variables that significantly affect the probability to switch from public to private health insurance.

Finally, if an individual is compulsorily insured in the public system (PUBCOMP), a switch to the private system is ruled out, and this is reflected in a significantly negative coefficient. Public insurance holders (PUBHOLDER) are more likely to switch to the private

[18]In our fixed effects model, we de facto measure the effects of ageing rather than the effects of age; that is, ageing has differential effects on switching probabilities and these depend on age. As all individuals age one year per year, the effects of ageing are partially picked up by the time fixed effects complicating the interpretation of age effects.

[19]As we had no prior how the variables TRAINING, EDU, GERMAN, PUBLEMP, FULLTIME, and JOBWEST affect health insurance choice, we do not interpret their coefficients.

system than those covered by public family insurance. The likely reason is that insurance holders have more opportunities to switch. While the insurance holder may switch to the private system (if eligible), the family stays in the public system; a family insured member must not switch to the private systems if the insurance holder remains in the public system. In contrast, private insurance holders (PRIVHOLDER) are as likely to switch to the public system as those who are covered by private family insurance. This mirrors the more symmetric switching opportunities: the family may switch to the public system while the insurance holder remains in the private system. In short, independent switching decisions of family insured members are more likely in the private system than in the public one.

3.5.2 Health index

Table 3.12 shows the regression results of the first stage. Self-assessed health (SAH2) is regressed on health care utilization measures (visits to a doctor, VISITSDOC, and hospital stays, HOSPITAL), disability status (DISAB), sickness absenteeism from work of more than six weeks (SICKSIX), the natural logarithm of net income (LNINCOME), and some additional controls. The first four variables are more objective health measures than self-rated health. For all of them, we find the expected negative sign for both samples. For the private sample, however, two of these variables fail to reach significance. For both samples, we find a negative time trend; individuals rate their health lower and lower – individuals grow older and health deteriorates. That the effect of ageing is (partially) picked up by the time fixed effects is the likely reason for the insignificance of the age dummies. For the most part, the remaining control variables are insignificant.

We use the linear instrumental variables panel data estimator implemented in STATA to estimate our econometric model. As should be clear from Section 3.4, this does not imply that we have an endogeneity problem. Switching the health insurance system and self-assessment of health are not simultaneous decisions. Consequently, when checking for valid instruments, we do not have to argue why the instruments (the Z_{it} variables) are exogenous, we only have to make sure that our analysis does not suffer from weak instruments. The respective F-statistics are well above 10 (151 for the public sample and 32 for the private sample) so that weak instruments are not an issue.

3.6 Conclusion

One of the potential problems of competitive health insurance markets is risk selection. Public policy has several measures at its disposal that are suited to prevent specific forms of risk selection; e.g., open enrollment prevents direct selection on observable characteristics and regulation of benefit packages prevents indirect selection on unobservable characteristics. Other measures aim at mitigating the incentives to engage in risk selection, most prominently risk adjustment. Research has largely concentrated on selection issues within social health insurance. Risk selection between public and private branches of health care financing was largely ignored. To the best of our knowledge, Sapelli and Vial (2003) is the only study that deals risk selection between public and private insurers.

This chapter contributes to the scarce literature on this matter, taking the German health system as an example. Given the institutional structure, risk-rated premiums in the private system and community-rated premiums in the public system, advantageous selection in favor of private insurers is expected. Using 2000 - 2007 data from the German Socio-Economic Panel, we find clear evidence for such selection. Interestingly, this selection occurs because privately insured individuals that have experienced a health shock have a higher probability to switch from private to public health insurance as compared to those without such a shock.

As a health shock will typically lead to an increase in health care spending down the road, these costs accrue in the public system rather than in the private one. Due to the public system's risk adjustment mechanism, the receiving public insurer is compensated by all other public insurers. The former private insurer should then compensate the public system, and because of the 2009 reform, such compensation can easily be administered. The reform included a switch from internal to external risk adjustment so that the private insurer can simply transfer the required funds to the central fund called "Gesundheitsfonds". To make ends meet, such a transfer should be risk adjusted.

Table 3.12: Fixed effects model for health status

	Public		Private	
	Coefficient	SE	Coefficient	SE
DISAB	-0.0804***	0.0200	-0.0416	0.0554
VISITSDOC	-0.0190***	0.0008	-0.0211***	0.0019
HOSPITAL	-0.0499***	0.0088	-0.0536**	0.0231
SICKSIX	-0.0446***	0.0117	-0.0478	0.0341
LNINCOME	-0.0016	0.0115	0.0297	0.0243
INCOMEGR	0.0001	0.0003	-0.0001	0.0003
CIVILSERV	0.0200	0.0755	0.0089	0.0608
SELFEMP	0.0134	0.0206	0.0712**	0.0312
UNEMP	-0.0354	0.0330	-0.0382	0.2349
AGE31_35	0.0031	0.0126	-0.0257	0.0365
AGE36_41	0.0099	0.0181	-0.0466	0.0493
AGE42_47	0.0112	0.0230	-0.0346	0.0603
AGE48_53	0.0043	0.0282	-0.0229	0.0708
MARRIED	-0.0211*	0.0124	-0.0151	0.0278
CHILDNUM	-0.0087	0.0059	0.0297**	0.0145
TRAINING	-0.0147	0.0146	-0.0529*	0.0319
EDU	0.0055	0.0107	-0.0252	0.0209
GERMAN	0.0215	0.0409	-0.2528	0.2159
PUBLEMP	0.0058	0.0084	0.0063	0.0171
FULLTIME	-0.0182	0.0122	0.0368	0.0360
JOBWEST	0.0037	0.0078	0.0172	0.0201
YEAR2001	-0.0001	0.0071	-0.0067	0.0207
YEAR2002	-0.0366***	0.0103	-0.0463*	0.0238
YEAR2003	-0.0313***	0.0105	-0.0314	0.0258
YEAR2004	-0.0498***	0.0110	-0.0838***	0.0258
YEAR2005	-0.0765***	0.0117	-0.1124***	0.0283
YEAR2006	-0.0972***	0.0124	-0.1219***	0.0299
PUBCOMP	0.0200**	0.0095		
PUBHOLDER	-0.0340	0.0220		
PRIVHOLDER			0.0095	0.0315
CONSTANT	0.6463***	0.1523	1.1099***	0.4100
N	40628		6771	
F (m, n)	35.05 (27, 29764)		9.22 (27, 4712)	
Prob > F	0.000		0.000	
Instruments (Z-variables)				
F (m, n)	150.55 (5, 29764)		31.62 (5, 4712)	
Prob > F	0.0000		0.000	

Note: *p<0.1, **p<0.05, ***p<0.01.

Chapter 4

The Effectiveness of Breast Cancer Screening in Germany: Evidence from a Natural Experiment*

*This chapter is based on joint work with Salmai Qari. Both authors contributed equally to this work.

4.1 Introduction

Cancer is the third leading cause of death worldwide and the second leading cause of death in Europe after cardiovascular diseases and caused 21.4% of all deaths in 2011. Among women in Europe breast cancer (IC-D10 C50) is the most prevalent form of cancer and approximately 154.5 thousand women died from this disease in 2011 which accounts for 18% of all deaths from malignant neoplasms and 3.5% of all deaths among women in Europe (WHO 2013). In Germany breast cancer accounted for 28% of all cancer diseases among women in 2009 and is by far the most frequent form of cancer. Approximately 12% of all women come down with breast cancer during their life. With a standardized mortality rate of 24.6 per 100 000 in 2008 breast cancer causes the most cancer related deaths among women in Germany (RKI 2010).

Several individual behaviors like smoking, regular alcohol intake, and late or no child birth are associated with an increased risk for breast cancer. Nevertheless, results from cross-country studies suggest an important role of environmental and non-behavioral factors like early menarche and late menopause, breast cancer history in the family for the individual risk to develop the disease (Vaionio & Bianchini 2002). The limited prevention possibilities highlight the importance of improving the treatment of breast cancer. Therefore – at first glance – it is seemingly self-evident that the early detection of breast cancer is a useful tool to improve the treatment possibilities.

However, it is important to distinguish between opportunistic mammography screening and organized screening programs. Opportunistic mammography screening is one of several useful diagnosis tools for females with a known elevated risk of developing breast cancer. For example, a gynecologist may apply opportunistic screening as a tool for further investigation after a manual breast exam. Organized mammography screening, on the other hand, refers to a mass-screening of the healthy population without clinical symptoms in order to increase the chance of early cancer detection.

Since the screening and subsequent diagnosis methods are associated with several negative side-effects, the mass-screening of healthy women raises a trade-off between costs (monetary and non-monetary) and benefits of the program. One obvious shortcoming is the high number of false-positives. Since the mammography cannot perfectly discriminate between women who actually have breast cancer and healthy ones, there can be a large number of patients who are wrongfully classified as having cancer. These women undergo a number of subsequent diagnostic checks until they are correctly re-classified as healthy women and these additional checks potentially have negative side-effects. Or, these women are ultimately treated as if they had breast cancer. A second negative aspect of organized screening programs is the detection and subsequent treatment of mild forms of cancer that would never cause any medical problems. This kind of „over-diagnosis“ causes a chain of „over-treatment“ that once again carries the risk of negative side-effects (Kalager et. al. 2012, Becker 2008, and Becker 2002).

The potential benefit of organized mammography screening is the reduction of the breast cancer mortality rate compared to a situation without an organized screening program. In order to assess such a potential benefit, we exploit the fact that the organized screening program in Germany was sequentially introduced. The program was started in three „pilot-regions“ during 2001 and 2002 and it was extended to Germany as a whole in 2005. Hence, this institutional setup provides randomization at the regional level. Our data set covers the years 1998 to 2010 and allows us to analyze whether the mortality rates in the pilot regions exhibit a different pattern over time compared to appropriate control regions.

The main result of the analysis in this chapter is that we do not find evidence for a reduction of breast cancer mortality induced by the organized screening program. The mortality rates in the pilot and control regions show a very similar pattern over time. There are a number of possible channels why there is no association between the organized screening program and breast cancer mortality. For example, the overall progress in medical treatment possibilities may overhaul the effectiveness of the organized screening program and a small take-up rate may render the program ineffective. The currently available data do not allow to investigate these channels. However, the missing evidence for a reduction of breast cancer mortality induced by the organized screening program suggests to reassess the findings once more data are available.

The remainder of this chapter is structured as follows: Section 4.2 provides an overview and brief discussion of the recent literature on the effectiveness of organized mammography screening programs. Section 4.3 explains in detail how the organized screening program was set up in Germany. Section 4.4 introduces our data set. Our empirical strategy for the estimation of the effect of the screening program on breast cancer mortality is presented in Section 4.5. Section 4.6 provides results from various regression models. Section 4.7 concludes.

4.2 Literature

During the last decades, many countries introduced organized mammography screening programs. Consequently, there is a large and growing literature that attempts to evaluate the effectiveness of these programs. Focusing on the studies for European countries, the results range from no reduction in breast cancer related mortality at all up to a reduction of 25% that is attributed to the screening.

A highly influential study by Olsen et al. (2005) analyzed the effectiveness of the mammography screening program in Denmark. They estimate a reduction in relative breast cancer mortality by 25% and attribute this reduction to the screening program. While the empirical strategy in this study is convincing from the outset, the interpretation of the estimated results by the authors is problematic. The only sizable (and statistically significant) reduction in breast cancer mortality was found three years after the implementation of the program. The point estimate of this one-time reduction in relative mortality is equal to 25%. However, after this single drop in mortality, there are no further significant changes. This single reduction in mortality after three years cannot be attributed to the organized screening program for two main reasons. First, as time goes by an increasing number of women is covered by the screening program and therefore reductions in mortality due to the screening are expected to increase as well. Hence, the observed pattern of a single drop does not match the expected pattern of a continuous reduction of mortality over time.

Second, reductions in breast cancer mortality that emerge earlier than five years after the implementation of the program cannot be attributed to the screening. The mechanism of organized screening is the detection of breast cancer at an early and better treatable stage. Early detection allows a more appropriate treatment according to stage and thus increases the chance to avoid a lethal course of the disease (Vaionio and Bianchini 2002). This means that reductions in mortality due to the screening program cannot be found shortly after its implementation but the effects from a better treatment need a certain time to affect mortality rates. Therefore, the one-time reduction in mortality found by Olsen et al. (2005) cannot be attributed to the screening program. Furthermore, a reduction in mortality has only been found for Copenhagen but not for other counties in Denmark.

Jørgensen et al. (2010) raise additional concerns about this study regarding sample selection and inappropriate choice of control groups. Using an additional screening region and more years of follow-up they cannot confirm the findings by Olsen et al. (2005) and conclude, that the mammography screening program in Denmark has no effect on the breast cancer mortality.

Gøtzsche and Olsen (2000) conducted a meta-analysis in order to shed light on the contradictory results from different studies on the effectiveness of the Swedish mammography program. They find that the results of six out of eight studies were biased due to inadequate randomization in the trials. With adequate randomization they find no effect of the mammography screening on breast cancer mortality in Sweden.

Autier et al. (2011) compared three pairs of European countries. They find that although these countries have time differences of 10 - 15 years in the implementation of mammography screening programs, the reductions in breast cancer mortality developed similar over time. The authors conclude that screening did not play a direct part in the reductions of breast cancer mortality.

In Germany three pilot projects were started in 2001 and 2002 before the organized screening program was implemented nationwide between 2005 and 2009. Since then, the program was repetitively evaluated by the Kooperationsgemeinschaft Mammographie (2006 and 2009), the same institution that is in charge for the implementation of the program.

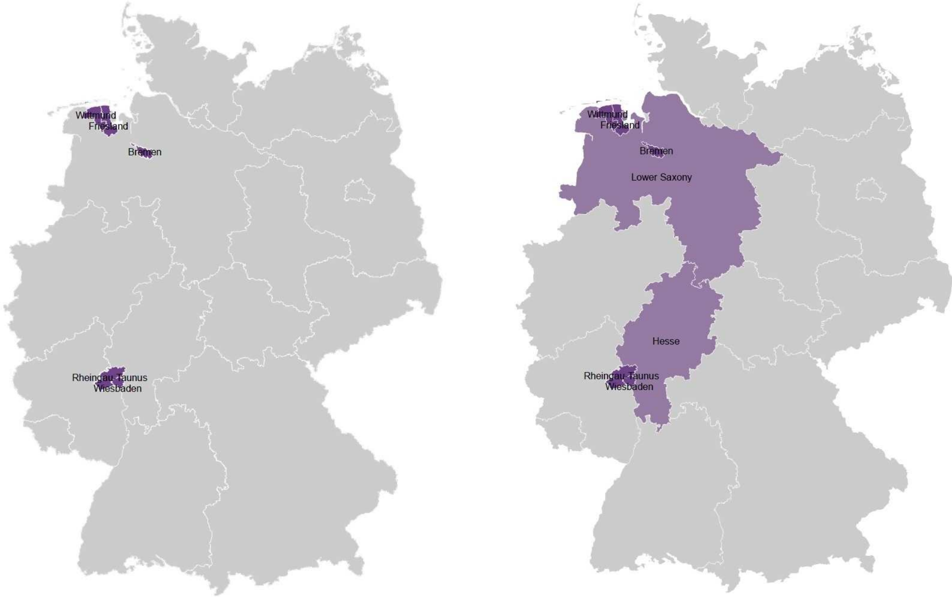
To the best of our knowledge, apart from the publications by the Kooperationsgemeinschaft Mammographie, the only study on the evaluation of the German screening program is provided by Biesheuvel et al. (2011); nevertheless, the authors are associated with the Kooperationsgemeinschaft Mammographie. The evaluation by Biesheuvel et al. (2011) is limited to the quality of program implementation in the Federal State of North Rhine-Westphalia since 2005. Although the authors emphasize that the primary aim of the German mammography screening program is the reduction of breast cancer mortality by early detection they only compare performance indicators (surrogate parameters) to reference values taken from the European guidelines for Quality Assurance in Breast Cancer Screening and Diagnosis (Perry et al. 2006). Biesheuvel et al. (2011) argue that besides the relatively low attendance rate of approximately 53% the implementation of the German organized breast cancer screening program was successful.

However, surrogate parameters can only be interpreted as short-term proxies for the long-term goal of each screening program - the reduction of the disease specific mortality. The performance of surrogate parameters delivers at most necessary but not sufficient evidence regarding the effectiveness of organized screening programs (Becker 2006). In light of this, our analysis is the first study that exploits the available variation in the mortality rates over time in order to evaluate the effectiveness of the German organized screening program.

4.3 Institutional Background

Until 2001 Germany had no organized mammography screening program. Females covered by public health insurers were entitled to opportunistic mammography services only in two cases: first, if they displayed clinical breast cancer symptoms and second if they had a known elevated risk for developing breast cancer (e.g. with breast cancer history in the family). In 2001 and 2002 a population based organized mammography screening program

Figure 4.1: Treatment and control regions in Germany



(a) treatment regions (b) control regions

has been implemented first as pilot projects in three regions in Germany. Between 2005 and 2009 the organized screening program was extended nationwide. The "Planungsstelle Mammographie-Screening" has been in charge for implementation and monitoring of the pilot project. The "Planungsstelle" was composed of two equally sized groups of delegates of the National Association of Statutory Health Insurance Funds (ASHIF) and the National Association of Statutory Health Insurance Physicians (ASHIP). Since 2003 the "Kooperationsgemeinschaft Mammographie" (KoopM) is responsible for the co-ordination, quality control and evaluation of the German mammography screening program and is again a composition of agents from the ASHIF and ASHIP.

The pilot projects started in 2001 and 2002 in three different regions in Germany: Weser-Ems in Lower Saxony, the city of Bremen, and the Wiesbaden/Rheingau-Taunus district in Hesse (Figure 4.1a). In the following we will describe the treatment regions in more detail.

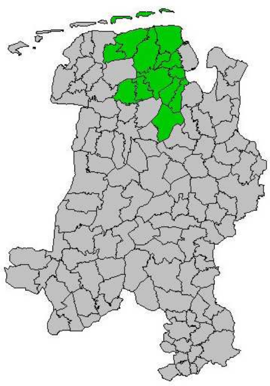
Weser-Ems was an administrative district in the Federal State of Lower Saxony (Figure 4.2a). The pilot project started in April 2002. Not all communities in the area of

Weser-Ems took part in the pilot project (see Urbschat et al. 2004 and Figure 4.2b). While in the district Wittmund all communities were treated, one community in the district Friesland was not under treatment (Varel). In the district Aurich only two communities and in the district Ammerland only one community were treated. In total, the treated area had about 200,000 inhabitants and 22,000 women in screening age. Given the small number of treated communities within Aurich and Ammerland, these two districts are excluded from our data set, and therefore avoiding a mixed population with and without treatment in our analysis. Hence, the remaining districts Wittmund and Friesland comprise the Weser-Ems treatment group in our analysis (Figure 4.2c); the Federal State of Lower Saxony (excluding the treatment area in Weser-Ems and the districts Aurich and Ammerland) is the corresponding control group (Figure 4.1b). The Federal state of Bremen is composed of two cities: the city of Bremen and the close city of Bremerhaven. Both cities are surrounded by the Federal state of Lower Saxony. Since Bremerhaven was not part of the pilot project, it is essential to separate the data for the city of Bremen and Bremerhaven for the analysis (Figure 4.3). Within the treated region (city of Bremen) about 70,000 women were in screening age. The organized mammography screening program started in July 2001. Once again, we use the Federal state of Lower Saxony (excluding the treatment area Weser-Ems and the districts Aurich and Ammerland) as the control group (Figure 4.1b). Within the Federal State of Hesse, the entire administrative district Rheingau-Taunus and the city of Wiesbaden were chosen as pilot regions (Figure 4.4). The entire pilot region had about 456,000 inhabitants and 58,000 women in screening age. The pilot project started in July 2001. As the corresponding control group we use the rest of the Federal State of Hesse (Figure 4.1b).

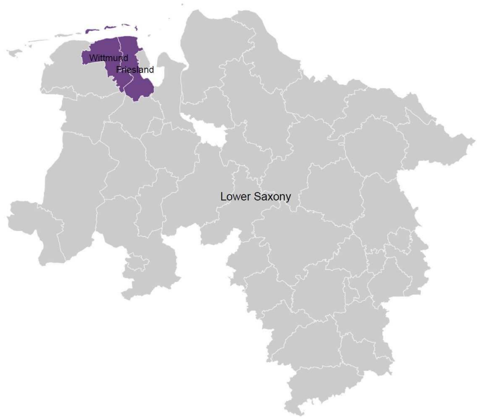
The Federal Council and the German Parliament requested the nationwide implementation of an organized mammography screening program already in 2002. Following this request, the Federal Joint Committee (G-BA) decided in 2003 the nationwide introduction. The organized screening program was set up between 2005 and 2009; in total 94 screening units started to operate (see Figure 4.5). Since then all women of age 50 to 69 are eligible for mammography every two years.

Figure 4.2: Treatment regions in Lower Saxony

(a) administrative district Weser-Ems in Lower Saxony



(b) pilot project communities in Weser-Ems, source: Urbschat et al. 2004



(c) screening area in Weser-Ems for analysis

Figure 4.3: Treatment regions in Bremen

Figure 4.4: Treatment regions in Hesse

The decision for a nationwide implementation of the screening program was made at the same time when the pilot project had just been started. This may raise questions about the information base for this decision and the intention of the pilot projects. Nevertheless, the early implementation in only three regions and the time-delayed nationwide implementation allows us to evaluate the effectiveness of the organized screening program in the framework of a natural experiment. Mammography screening in the pilot regions was available for all women between 50 and 69 years, i.e. all women of this age group with no clinical breast cancer symptoms and without diagnosed breast cancer were invited to make use of mammography. As any organized screening program is targeted at the reduction of breast cancer mortality, we can exploit the randomization induced by the pilot regions and compare the mortality rates (separately for each age group) across treated and non-treated regions. This difference-in-difference approach allows to analyze whether the organized screening program affected mortality rates.

As explained above, the focus of our study is the analysis of the *organized* screening program as a whole. However, in Section 4.7 we will also briefly summarize a few channels that can influence the effectiveness of the program, for example, self-selection depending on individual risk, advances in treatment methodologies, and the take-up rate. As discussed, distinguishing between these channels is orthogonal to our research question, as we are interested in the effectiveness of the *organized* screening program as a whole and not in a separate analysis of each part of the program.

A crucial part of our empirical strategy is the necessary length of the data set that allows to observe changes in the mortality rates that can be attributed to the screening program. As mentioned earlier, any change in mortality that occurs earlier than 5 years after implementation of the program cannot be attributed to the program, while any effect on mortality should be seen between five to ten years after implementation (Vaionio and Bianchini 2002). The pilot projects in Germany started in 2001 and 2002. In our analysis we use breast cancer mortality data from 1998 to 2010. Therefore, our data follow mortality up to 9 (8) years after the program was implemented. Starting in 2005, the organized program was implemented in Germany as a whole. Hence, our aggregate data covering the years up to 2010 comprise most of the available control groups. Figure 4.5 illustrates the effect of the gradual implementation of the German organized mammography screening program with 94 screening units in total on the number of possible control groups. In each year between 2005 and 2009 a certain number of new screening units started to operate. Whenever a new screening unit starts to operate, it becomes a fully effective screening region after five years. Hence, the pool of control groups rapidly starts to shrink in 2010 (five years after 2005) and from 2014 no control group is available anymore. In the following section we describe the data in more detail.

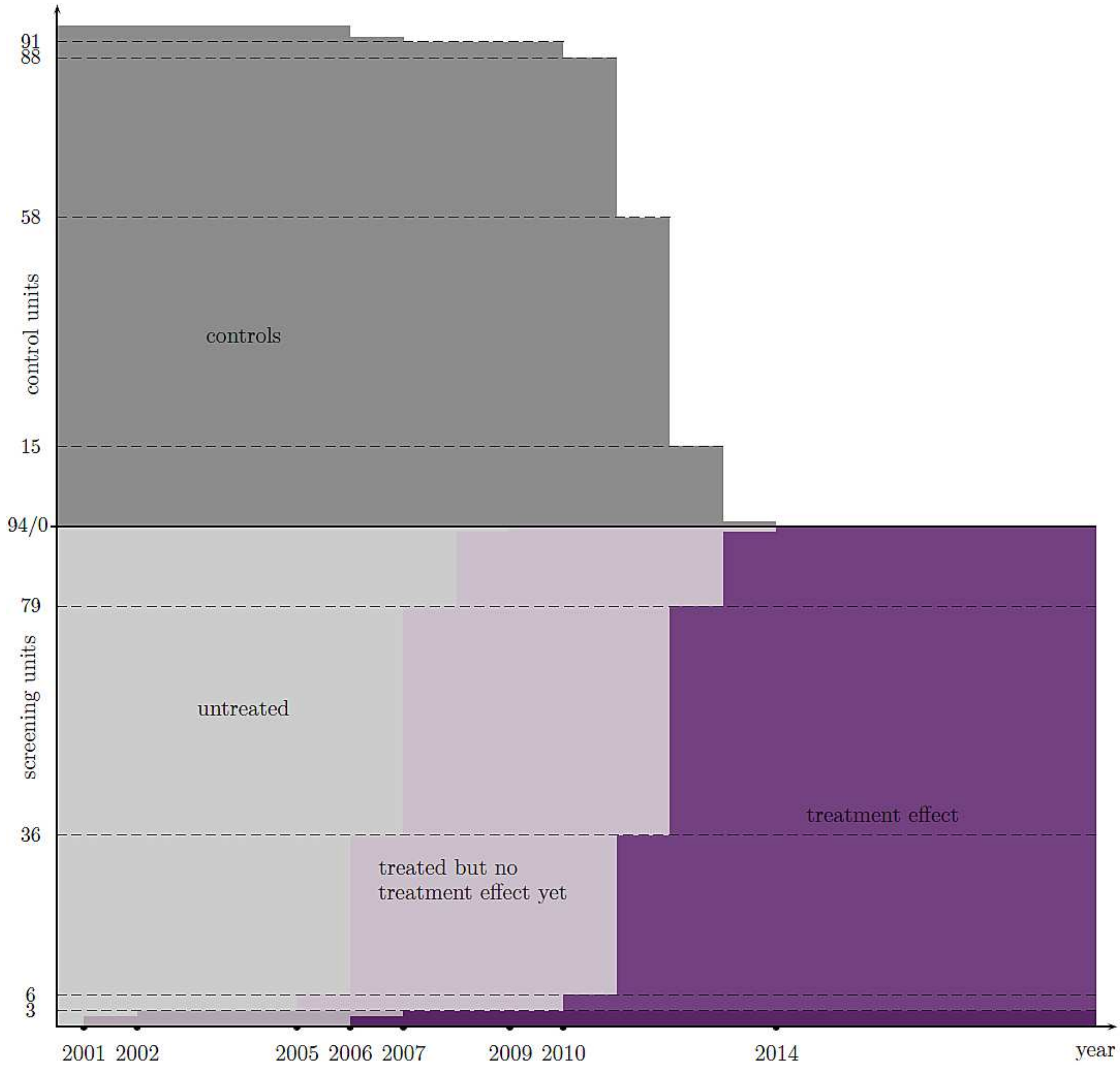
4.4 Data

For our analysis we use breast cancer specific mortality data from 1998 to 2010 from the statistics on the causes of death provided by the Statistical Offices of Lower Saxony, Bremen, and Hesse. We observe the number of fatalities due to breast cancer among women on the regional level of communities and by 10 year age classes. In order to standardize the number of fatalities by population size, we merge the mortality data with population statistics provided by the Federal Statistical Office at the same observational level and calculate mortality rates.

Lower Saxony

Table 4.1 shows mortality rates for treatment and control regions by age classes in Lower Saxony averaged over years. As described in Section 4.3, not all communities in the district of Weser-Ems have been under treatment during the pilot project. Therefore, we only analyze the communities of Wittmund and Friesland as the treated region in Lower Saxony. In order to analyze a reasonable number of observations, we merge both communities into one screening area. We observe mortality rates in the screening area and in 49 control regions over 13 years. Therefore, our analysis sample for Lower Saxony includes 13 observations for

Figure 4.5: Timeline of mammography program implementation in Germany



the screening area and 637 observations for the control area. The age gradient in the risk of dying from breast cancer is reflected in the clearly increasing levels of mortality rates in the age classes. In the screening area we observe a positive number of deaths from breast cancer for all age classes and years. In the control areas at least one administrative district reports zero deaths from breast cancer in each age class. In all age classes the average mortality rate is higher in the screening area than in the control area. Nevertheless, potential differences due to the screening cannot be seen from raw means over time.

Figure 4.6 provides more detailed information about mean mortality rates for the screening and control area by years and age classes in Lower Saxony. The vertical line in year 2002 indicates the begin of the pilot project. The dashed line indicates the first year a change in mortality can be attributed to the organized screening program. Women of age 50 to 69 were the target population in the treatment region.

Moving to the age class 60 - 69, the development of breast cancer mortality over time shows a similar pattern for the screening and control region.[1] Breast cancer mortality seems to be almost constant with a slight decrease in the screening region at the end of the observation period.

For the age class 50 - 59 the picture is not that clear. Between 2000 and 2004 mortality rates are lower in the screening area than in the control area. Since 2005 the mean mortality rate in the screening area is above the mean mortality rate in the control area and increases until 2009. For 2010 the mortality rate drops to the same level which was almost constant for the control area over time.

Finally, for the untreated age groups 40 - 49 and 70 - 79[2] the mortality rates seem to be nearly constant over time for both the screening and the control area.

[1]At first glance, the mortality rate in the screening region seems to be more volatile than in the control regions. However, the higher volatility is mainly driven by the small number of observations and fatalities in the screening area and therefore reflect a larger amount of sampling error compared to the control regions (see Table 4.1).

[2]Although the age class 70 - 79 is not directly subject to the treatment, this group is indirectly affected from the screening. In each year after 2002 an additional cohort that was prior subject to the screening program enters this age class. Therefore, one should expect an increasing reduction of mortality in this age group over time if the organized screening program would have an effect on breast cancer mortality.

Table 4.1: Mean breast cancer mortality rates in Lower Saxony*

	Screening Area			Control Area		
Age	Mean	SD	Min/Max	Mean	SD	Min/Max
40 - 49	24.83	14.59	7.58/ 56.97	22.64	15.76	0/ 91.54
50 - 59	60.89	30.61	29.45/150.98	54.45	27.31	0/181.96
60 - 69	94.06	30.04	49.39/143.99	85.18	33.52	0/246.04
70 - 79	120.81	44.34	76.98/229.48	115.24	42.84	0/322.97
N	13			637		

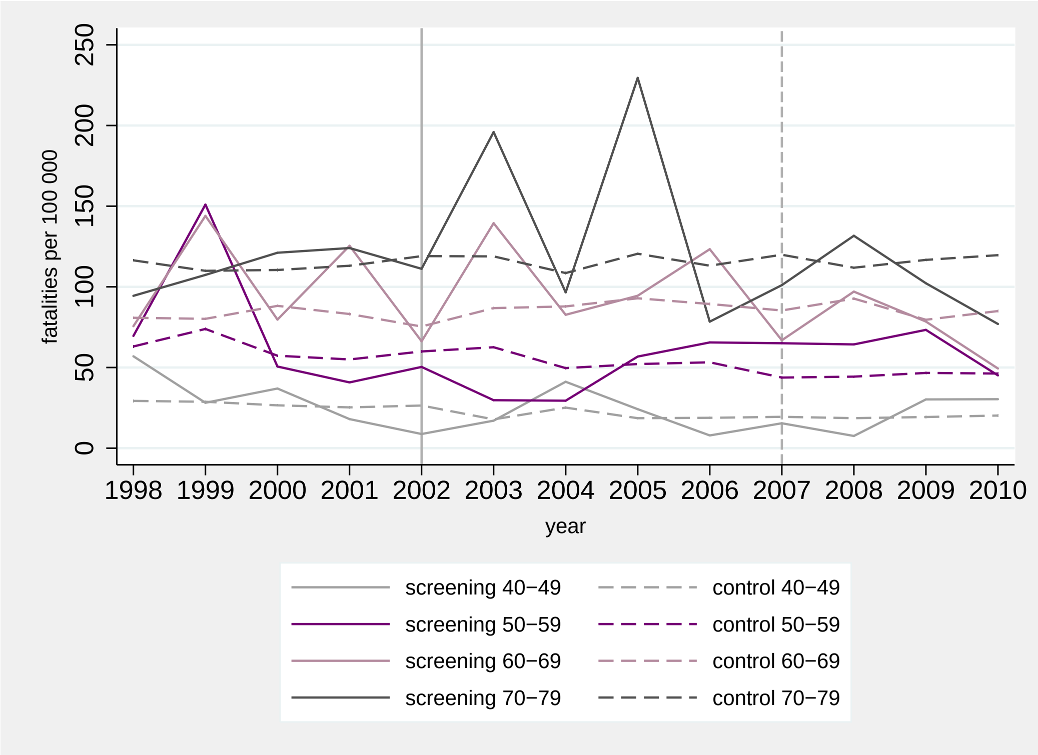
*fatalities per 100 000

Bremen

Table 4.2 shows mortality rates for the screening area Bremen and the control areas in Lower Saxony by age classes and averaged over years. Again, we observe mortality rates in the screening area Bremen and in 49 control regions over 13 years. Therefore, our analysis sample for Bremen includes 13 observations for the screening area and 637 observations for the control area. In the screening area we observe for all age classes and years a positive number of deaths from breast cancer whereas in the control areas in each age class at least one administrative district reports zero deaths from breast cancer. In contrast to descriptive results in Lower Saxony, the screening areas in Bremen exhibit higher mortality rates compared to their control areas for all but the highest age class.

Figure 4.7 provides more detailed information about mean mortality rates for the screening and control area by years and age classes in Bremen. The vertical line in year 2001 indicates the start of the pilot project in Bremen. Breast cancer mortality seems to be almost constant in all age classes over time. Only for the class 60 - 69 it seems that mortality rates in the screening and control area are diverging since 2007 due to a decrease in mortality rates in the screening region.

Figure 4.6: Breast cancer mortality rates in Lower Saxony



Hesse

Due to privacy protection, observations for Hesse were censored by the Statistical Office if the number of fatalities was 1 or 2 in an administrative district and a respective age class. As such low numbers mainly occur in lower age classes, our analysis data set is almost unaffected. Nevertheless, we imputed the censored cells in three ways: replacement with 1 for each censored observation (we refer to this imputation as "positive"), replacement with 2 for each censored observation ("negative" imputation), and replacement with the ratio of the difference in total fatalities[3] and observed fatalities and the number of censored cells ("mean" imputation). Our results are robust to all three imputations. In this section the descriptive statistics for the positive imputation are presented. The corresponding results for the negative and mean imputations are relegated to the Appendix (Tables 4.16 and 4.17 and Figures 4.15 and 4.16).

[3]The total number of fatalities over all age classes in each administrative district and for each year was delivered by the Statistical Office.

Table 4.2: Mean breast cancer mortality rates in Bremen*

	Screening Area			Control Area		
Age	Mean	SD	Min/Max	Mean	SD	Min/Max
40 - 49	18.93	6.97	11.88/ 32.09	22.64	15.76	0/ 91.54
50 - 59	48.69	14.14	21.62/ 68.38	54.45	27.31	0/181.96
60 - 69	78.89	17.74	48.90/116.61	85.18	33.52	0/246.04
70 - 79	122.03	23.62	82.31/153.23	115.24	42.84	0/322.97
N	13			637		

*fatalities per 100 000

The descriptive analysis of the positive imputed data for Hesse does not suggest a systematic difference between the screening and control area. Table 4.3 shows mortality rates for the screening and control area in Hesse by age classes and averaged over years. We observe mortality rates in the screening area and in 25 control regions for 13 years. Therefore, our analysis sample for Hesse includes 13 observations for the screening area and 325 observations for the control area. In both areas we observe a positive number of fatalities from breast cancer for all age classes and years. The differences in mortality levels between screening and control areas alter between age classes and show no systematic pattern.

Figure 4.8 provides more detailed information about mean mortality rates for the screening and control areas in Hesse by age classes over years. The vertical line in year 2001 indicates the begin of the pilot project in Hesse. There is a slight downward trend in mortality for the age class 50 - 59. Nevertheless, this trend seems to be similar in the screening and the control area. In general, mortality rates in Hesse seem to be constant over time both in the screening and the control areas.

Figure 4.7: Breast cancer mortality rates in Bremen

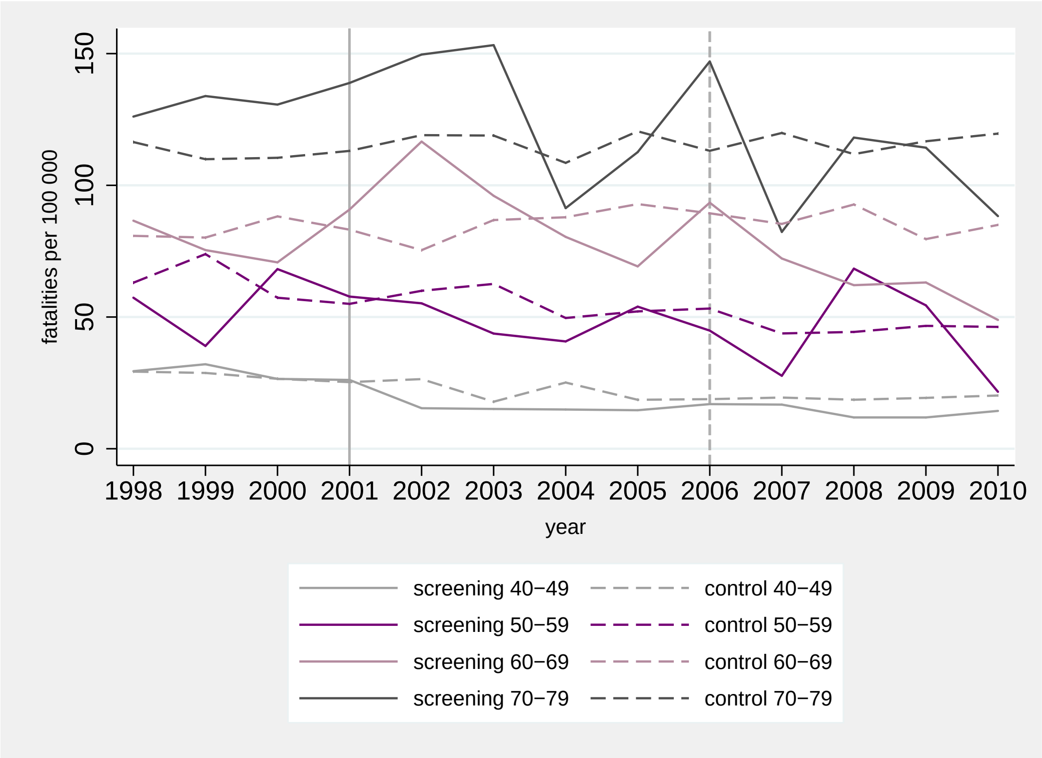
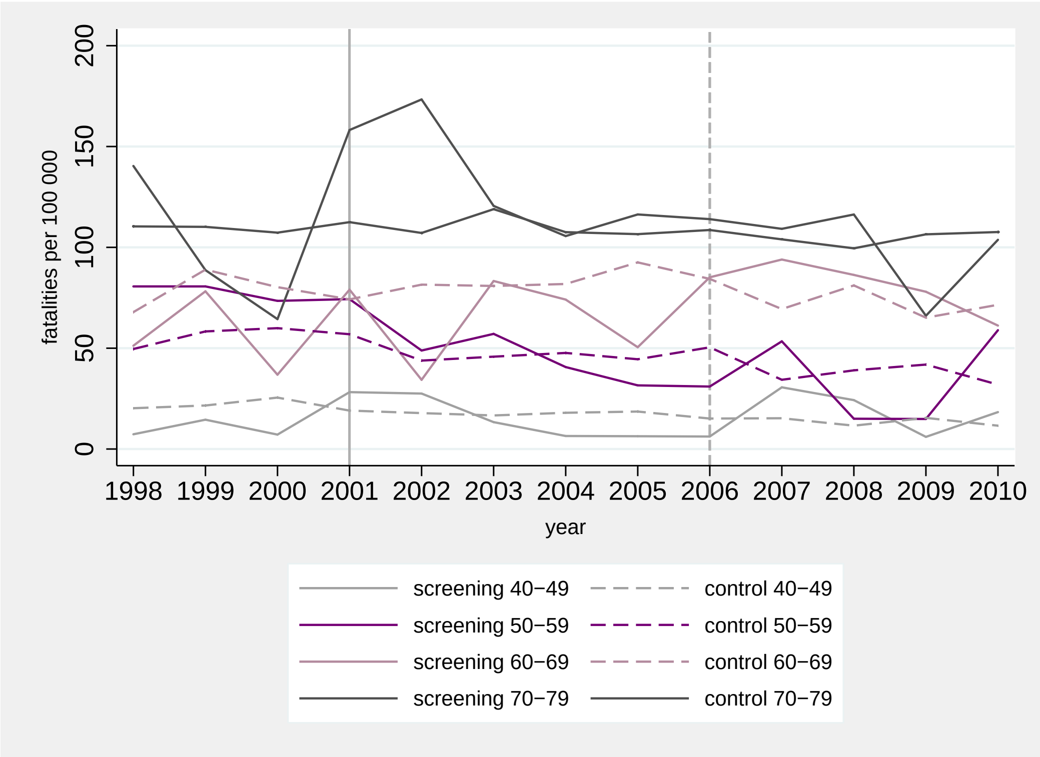


Table 4.3: Mean breast cancer mortality rates in Hesse*

	Screening Area			Control Area		
Age	Mean	SD	Min/Max	Mean	SD	Min/Max
40 - 49	15.10	9.57	6.04/ 30.60	17.42	11.45	0/ 66.66
50 - 59	50.81	23.09	14.91/ 80.63	46.48	21.56	0/134.77
60 - 69	68.63	19.65	34.33/ 93.97	78.44	27.96	0/183.56
70 - 79	113.60	31.38	64.42/173.36	108.21	35.22	12.49/207.88
N	13			325		

*fatalities per 100 000

Figure 4.8: Breast cancer mortality rates in Hesse



4.5 Empirical Strategy

We use different regression models to compare the evolution of mortality over time in the treated regions with the time patterns in the respective control regions. As mortality is increasing in age, we fit separate models for each age group as outlined in Section 4.4. Alternatively, we could interact the whole set of covariates with age group dummies. However, in terms of presenting and interpreting the coefficients, we prefer separate regressions.

Following the previous literature, our main model is a Poisson regression that models the *number* of fatalities in a specific region. As the number of females and hence the number of fatalities is increasing in the number of inhabitants of a specific region, the number of females of the respective age groups is entered as an exposure variable in the Poisson specifications. We inquire the robustness of these results by two alternative models that use the mortality *rate* as the dependent variable. The first model is a simple linear model fitted by OLS. While a linear OLS estimation is a useful robustness check, the bounded nature of the response variable may render the linear model as inappropriate. From the outset, all models that constrain the predictions to values between zero and one are equally suitable. We use the standard generalized linear models to handle this bounded response variable, and in particular, we use the probit model. Generalized linear models (GLM) are due to Nelder and Wedderburn (1972), who provide a common framework to estimate a large class of models including the probit, poisson and the linear model by maximum likelihood (see also McCullagh and Nelder 1989). The application of the probit model to fractional response data was also suggested by Papke and Wooldridge (1996).

In a first set of specifications, the set of explanatory variables consists of a constant α, year fixed effects $Time_t$ for each available year t, a dummy *Screening* indicating those regions where organized mammography screening was introduced during the pilot project and interaction terms between the year dummies and the screening dummy. Effectively, this model plots the average mortality numbers and rates over time in two completely separate plots: one for the treatment regions and a second for the control regions.

Formally, the poisson version of the model for a specific age group reads:

$$\log\left(E\left(y|\mathbf{x}\right)\right) = \log(\text{exposure}) + \alpha + \text{Screening} + \sum_{t \in T} \text{Time}_{\text{t}} + \text{Time}_{\text{t}} \times \text{Screening} \tag{4.1}$$

where y is the number of fatalaties, $T = [1999, ..., 2010]$ and the year 1998 is the omitted base line year.

The equation of the linear version of the model reads

$$E(y|\mathbf{x}) = \alpha + \text{Screening} + \sum_{t \in T} \text{Time}_{\text{t}} + \text{Time}_{\text{t}} \times \text{Screening} \tag{4.2}$$

where y is now the mortality rate.

Finally, the generalized linear (probit) version of the model is given by

$$\Phi^{-1}\left(E(y|\mathbf{x})\right) = \alpha + \text{Screening} + \sum_{t \in T} \text{Time}_{\text{t}} + \text{Time}_{\text{t}} \times \text{Screening} \tag{4.3}$$

where $\Phi(\cdot)$ denotes the cumulative distribution function of the Normal distribution. We refer to this first set as "time-flexible" models in the following.

In the second set of models, the time variable is recoded to a version with only three levels. The first time frame models the years before screening was introduced. The second time frame indexes the years when screening was introduced in the treatment regions but could not have an effect yet (5 years after implementation), while the third time frame indexes those years when the screening program could have an effect on mortality. Once again, we consider a Poisson, a linear and a Probit version. The equations have the same structure as the time-flexible models. The only difference is the time index $t \in \{1, 2, 3\}$ which now denotes the following time frames: t equals 2 for the years 2001(2) - 2006(7) and t is equal to 3 for the years 2006(7) - 2010. The omitted baseline category is $t = 1$ which indicates the years 1998 - 2001(2). The denoted differences of one year account for the fact that the organized screening was introduced in Bremen and Hesse in 2001 and one year later in Lower Saxony.

4.6 Results

In this section we present and discuss the main regression results. As described in Section 4.5, the most flexible models include interaction terms between each year dummy and the screening indicator. These models effectively plot the average mortality rate within the screening and respective control regions over time; a separate model for each age group is estimated. The year 1998 is the omitted baseline category in these regressions. The results of the three different estimators are virtually identical to the descriptive results in Section 4.4. We therefore discuss only the results obtained from the Poisson model and relegate the results of the linear and the Probit models to the Appendix. As seen in the descriptive analysis in Section 4.4, the number of observations for the screening areas is small. This raises issues regarding the precision of the most flexible model. Therefore, we focus on the more restrictive models that consider three time frames. The first time frame comprises those years before screening in the pilot regions was introduced. The second time frame consists of those years when screening was introduced in the screening regions but cannot have an effect. Finally, the third time frame covers the years when screening could have an effect on breast cancer mortality.

Lower Saxony

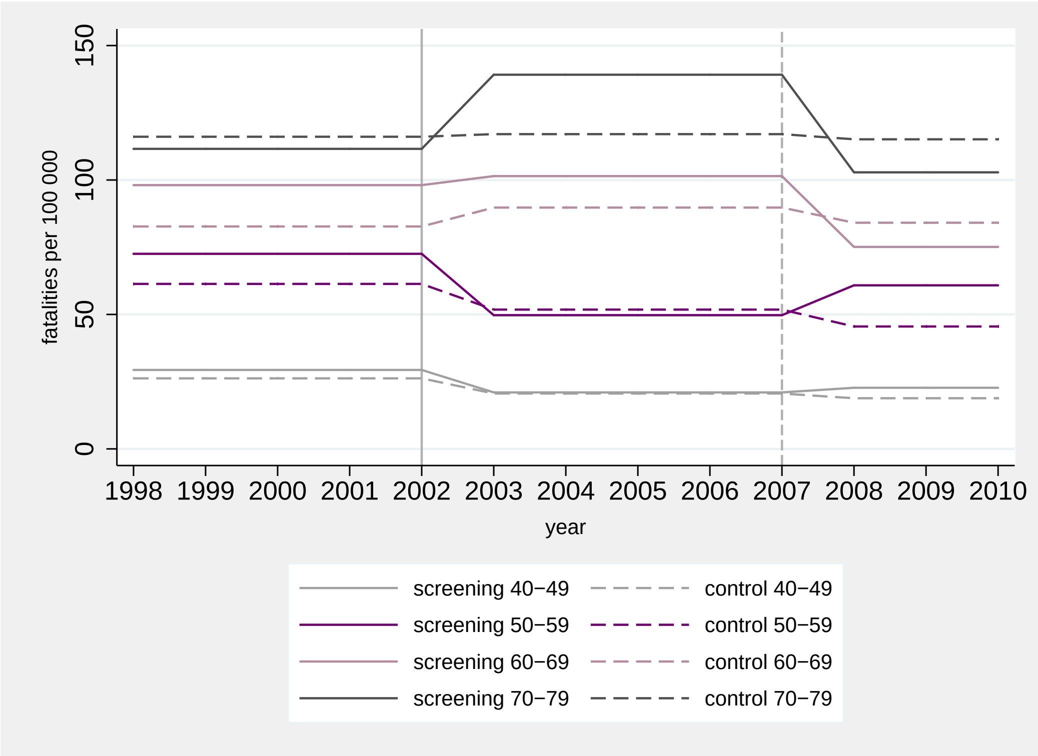
Table 4.4 shows the results obtained from the most flexible Poisson model. As discussed in Section 4.2, a possible treatment effect of the organized screening programme is only visible after five years. Hence, the time period covering the years 2008 - 2010 is the most interesting one. Starting the discussion for the women who are between 50 and 59 years old and therefore could benefit from screening, the coefficients in Table 4.4 indicate that the mortality rates in the control area are very similar for the years 2008 - 2010. A slightly different pattern is found for the treatment area. Here, the mortality rates in 2007 and 2008 are identical, while the point estimate for 2009 is about 40% larger than the 2008 estimate. After this peak in 2009, the mortality rate in 2010 drops to the 1998 level.

Table 4.4: Results from Poisson regression for Lower Saxony by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.7247***	0.0738	0.0786*	0.0456	-0.0839*	0.0468	-0.1894***	0.0378
1999	-0.0162	0.0959	0.0398	0.0681	-0.0655	0.0610	-0.0302	0.0549
2000	-0.1384	0.0999	-0.1488**	0.0704	0.1078*	0.0588	-0.0080	0.0531
2001	-0.0682	0.0950	-0.1305	0.0815	-0.0062	0.0619	0.0617	0.0571
2002	-0.0391	0.0916	-0.0459	0.0637	-0.0258	0.0608	0.0701	0.0532
2003	-0.2524**	0.1104	0.0021	0.0634	0.1116	0.0727	0.0008	0.0550
2004	-0.1561*	0.0922	-0.2597***	0.0697	0.0847	0.0633	-0.0138	0.0519
2005	-0.3890***	0.1027	-0.2789***	0.0790	0.0906	0.0635	0.0489	0.0481
2006	-0.2604***	0.0987	-0.2054***	0.0714	0.0843	0.0631	0.0436	0.0490
2007	-0.4073***	0.1079	-0.3475***	0.0692	0.0535	0.0585	0.0306	0.0502
2008	-0.3872***	0.1174	-0.3538***	0.0661	0.0914	0.0583	-0.0574	0.0574
2009	-0.4726***	0.1124	-0.3382***	0.0738	-0.0461	0.0660	0.0393	0.0528
2010	-0.3348***	0.0906	-0.3725***	0.0686	0.0274	0.0588	0.0449	0.0533
Screeningx1999	-0.6889***	0.0959	0.7349***	0.0681	0.7097***	0.0610	0.1587***	0.0549
Screeningx2000	-0.2939***	0.0999	-0.1695**	0.0704	-0.0551	0.0588	0.2570***	0.0531
Screeningx2001	-1.0822***	0.0950	-0.4042***	0.0815	0.5126***	0.0619	0.2112***	0.0571
Screeningx2002	-1.8313***	0.0916	-0.2772***	0.0637	-0.1064*	0.0608	0.0931*	0.0532
Screeningx2003	-0.9531***	0.1104	-0.8521***	0.0634	0.5007***	0.0727	0.7291***	0.0550
Screeningx2004	-0.1690*	0.0922	-0.6002***	0.0697	0.0048	0.0633	0.0363	0.0519
Screeningx2005	-0.4679***	0.1027	0.0759	0.0790	0.1323**	0.0635	0.8391***	0.0481
Screeningx2006	-1.7144***	0.0987	0.1457**	0.0714	0.4051***	0.0631	-0.2293***	0.0490
Screeningx2007	-0.8983***	0.1079	0.2803***	0.0692	-0.1757***	0.0585	0.0371	0.0502
Screeningx2008	-1.6293***	0.1174	0.2752***	0.0661	0.1583***	0.0583	0.3899***	0.0574
Screeningx2009	-0.1618	0.1124	0.3908***	0.0738	0.0835	0.0660	0.0401	0.0528
Screeningx2010	-0.2943***	0.0906	-0.0608	0.0686	-0.4532***	0.0588	-0.2492***	0.0533
Constant	-8.1950***	0.0738	-7.3491***	0.0456	-7.1035***	0.0468	-6.7757***	0.0378
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Figure 4.9: Poisson estimated breast cancer mortality in Lower Saxony – time restricted model



For the women who are too young to benefit from screening (40 - 49), a similar pattern emerges. Mortality rates in the control regions are very similar for the years 2008 - 2010, while there is a peak in 2009 for the screening areas. In 2010, the mortality rate in the screening areas is slightly below the 1998 level. The same pattern is found for women who are too old to be eligible for the organized screening program (70 - 79). Overall, the results do not indicate that mortality rates in the screening regions differ from the corresponding rates in the control regions.

Figure 4.9 presents the predictions obtained from the more parsimonious model. Table 4.5 provides the corresponding coefficients. For women who are too young to benefit from screening (40 - 49), the model indicates a reduction of mortality over time. In line with that, the time effects are precisely estimated, while all the coefficients of the interaction terms are noisy, i.e. the screening and the control areas exhibit the same pattern over time.

For women aged 50 - 59 years, a similar pattern emerges: mortality decreases over time,

Table 4.5: Results from time restricted Poisson regression for Lower Saxony by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.1127	0.2536	0.1677	0.2500	0.1705	0.1413	-0.0396	0.0462
Time2	-0.2421***	0.0436	-0.1691***	0.0366	0.0815***	0.0271	0.0083	0.0238
Time3	-0.3310***	0.0537	-0.2985***	0.0392	0.0167	0.0303	-0.0083	0.0293
ScreeningxTime2	-0.0926	0.3499	-0.2086	0.2910	-0.0479	0.1845	0.2125	0.1999
ScreeningxTime3	0.0749	0.3746	0.1223	0.2758	-0.2836	0.2080	-0.0732	0.1351
Constant	-8.2459***	0.0283	-7.3963***	0.0251	-7.0975***	0.0187	-6.7586***	0.0181
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

but the differences between the screening and control regions are small. For this age group, the estimated number of fatalities before screening was introduced (years 1998 - 2002) is higher in the screening regions than in the control regions. This positive difference is even higher in the years 2008 - 2010 which would indicate that mortality increases after the introduction of the organized screening program. Note that the interaction term that picks up the difference-in-difference estimate of this effect of the screening program (*Screening x Time3*), is only imprecisely estimated. The associated 95% confidence interval is $[-0.42, 0.66]$. The predicted number of fatalities in the control areas before screening was introduced is roughly equal to 61 per $100,000$[4]. Using this as a baseline, the confidence interval of the interaction term translates into an interval for the estimated effect in terms of fatalities of $[-21, 57]$[5]. This means that the estimated difference in mortality ranges between 21 fatalities less and 57 fatalities more per 100,000 in the screening area compared to the control area.

Moving to women aged 60 - 69 (who are eligible for screening), mortality in the second time frame is slightly higher than in the first time frame. Once again, for this age group, the estimated number of fatalities before screening was introduced (years 1998 - 2002) is higher in the screening regions than in the control regions. This pattern is reversed in the

[4] $\exp(-7.4) * 100,000 \approx 61$

[5] The predicted number of fatalities at the lower bound of the confidence interval is equal to $\exp(-7.4-0.42)*100,000 \approx 40$. This would imply an effect of -21 compared to the baseline of 61. Similarly, the predicted number of fatalities at the upper bound is equal to $\exp(-7.4+0.66)*100,000 \approx 118$ which implies a change of 57 compared to the baseline of 61.

third time frame, since mortality in the screening regions is notably smaller in the third time frame compared to the years before. While the direction of the effect is in line with a reduction of mortality due to the screening program, the point estimate of this effect is once again imprecisely estimated. The associated 95% confidence interval is $[-0.69, 0.12]$.

For women aged 60 - 69, most of the estimates are noisy, including the coefficient modeling the treatment effect of the screening program. Finally, moving the discussion to women aged 70 - 79, there is also no clear pattern. For this age group, mortality in the screening regions increases in the second time frame and decreases in the third. All corresponding coefficients are imprecisely estimated.

Overall, there is no evidence in support of the organized screening program in Lower Saxony. For women aged 40 - 49 and 50 - 59, there are discernable reductions in mortality over time. However, the time effects for both groups are similar across treatment and control regions. Furthermore, women aged 40 - 49 were not eligible for screening. Hence, the observed reductions in mortality cannot be attributed to the organized screening program. They could, for example, be driven by advances in treatment methodology.

Tables 4.10 and 4.11 in the Appendix provide the results for the time-flexible OLS and GLM probit model, respectively, and Figure 4.12 plots the predictions obtained from the time-restricted OLS and GLM models. These alternative models provide the same evidence as the Poisson model and therefore corroborate the conclusions so far.

Bremen

As discussed in Section 4.4, there is some evidence for time trends that affected both treatment and control regions in Bremen. For instance, within the screening region, the mortality rate of women who are too young to be eligible for the organized screening program (40 - 49) decreases over time. Furthermore, the plots suggest that this decline is slightly more pronounced in the screening regions, especially in the early 2000s. The magnitude of these two patterns is visible in the Poisson estimates (Table 4.6). For example, for this age group, the coefficients modeling the mortality rate in the control areas obtain values of -0.1

Table 4.6: Results from Poisson regression for Bremen by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.0298	0.0755	-0.1191***	0.0437	0.0581	0.0466	0.0983***	0.0367
1999	-0.0507	0.0958	0.0709	0.0680	-0.0328	0.0615	-0.0328	0.0531
2000	-0.1686*	0.0996	-0.1462**	0.0682	0.1049*	0.0586	-0.0114	0.0515
2001	-0.1126	0.0953	-0.1514*	0.0802	0.0151	0.0613	0.0548	0.0559
2002	-0.0996	0.0944	-0.0537	0.0615	-0.0317	0.0615	0.0664	0.0517
2003	-0.3041***	0.1122	-0.0180	0.0624	0.1295*	0.0702	0.0141	0.0528
2004	-0.1756*	0.0926	-0.2784***	0.0683	0.0860	0.0625	-0.0199	0.0505
2005	-0.4081***	0.1019	-0.2769***	0.0758	0.1010	0.0627	0.0727	0.0492
2006	-0.3194***	0.1018	-0.2076***	0.0690	0.0971	0.0618	0.0269	0.0497
2007	-0.4459***	0.1070	-0.3376***	0.0653	0.0521	0.0581	0.0228	0.0490
2008	-0.4214***	0.1162	-0.3503***	0.0645	0.0962*	0.0574	-0.0490	0.0560
2009	-0.4720***	0.1150	-0.3184***	0.0727	-0.0305	0.0650	0.0360	0.0510
2010	-0.3601***	0.0909	-0.3813***	0.0662	0.0160	0.0592	0.0299	0.0524
Screeningx1999	0.1363	0.0958	-0.4557***	0.0680	-0.1052*	0.0615	0.0928*	0.0531
Screeningx2000	0.0638	0.0996	0.3197***	0.0682	-0.3067***	0.0586	0.0468	0.0515
Screeningx2001	-0.0075	0.0953	0.1587**	0.0802	0.0325	0.0613	0.0416	0.0559
Screeningx2002	-0.5492***	0.0944	0.0159	0.0615	0.3294***	0.0615	0.1048**	0.0517
Screeningx2003	-0.3634***	0.1122	-0.2530***	0.0624	-0.0259	0.0702	0.1807***	0.0528
Screeningx2004	-0.5057***	0.0926	-0.0632	0.0683	-0.1597**	0.0625	-0.3025***	0.0505
Screeningx2005	-0.2916***	0.1019	0.2159***	0.0758	-0.3246***	0.0627	-0.1858***	0.0492
Screeningx2006	-0.2335**	0.1018	-0.0382	0.0690	-0.0211	0.0618	0.1267**	0.0497
Screeningx2007	-0.1179	0.1070	-0.3898***	0.0653	-0.2335***	0.0581	-0.4493***	0.0490
Screeningx2008	-0.4860***	0.1162	0.5264***	0.0645	-0.4287***	0.0574	-0.0163	0.0560
Screeningx2009	-0.4356***	0.1150	0.2664***	0.0727	-0.2855***	0.0650	-0.1343***	0.0510
Screeningx2010	-0.3579***	0.0909	-0.5943***	0.0662	-0.5874***	0.0592	-0.3862***	0.0524
Constant	-8.1600***	0.0755	-7.3448***	0.0437	-7.1099***	0.0466	-6.7742***	0.0367
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

and −0.3 for the years 2002 and 2003. The corresponding coefficients for the screening areas are −0.55 and −0.36.

Moving to women aged 50 - 59 who could benefit from screening, a different pattern emerges. While the mortality rate in the control area in the years 2004 - 2010 is smaller than in 1998, mortality in the treated area hovers around the baseline 1998 level.

The opposite pattern is visible for women aged 60 - 69. While the mortality rate is hardly changing in the control regions, there is a decrease over time in the screening regions. Hence, for this age group the flexible model would suggest weak evidence for a positive treatment effect for women aged 60 - 69 as mortality decreases in the treatment regions in the years 2008 - 2010 and it stays constant in the control region.

Once again, the more parsimonious model provides more precise estimates. Table 4.7 and Figure 4.10 summarize the results. For women who are too young to benefit from screening (40 - 49), the model confirms the aforementioned time trends. Mortality decreases over time and this decrease is more pronounced in the screening regions.

Table 4.7: Results from time restricted Poisson regression for Bremen by age groups

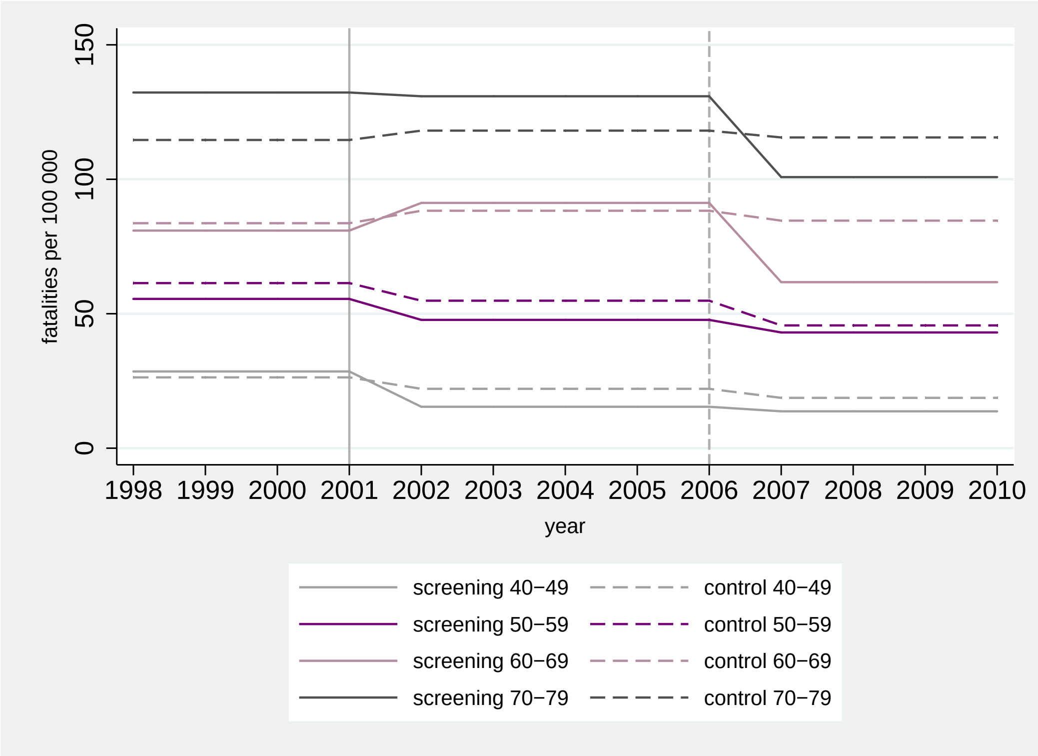
	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.0802	0.0535	-0.1006	0.0993	-0.0334	0.0551	0.1433***	0.0267
Time2	-0.1759***	0.0448	-0.1126***	0.0399	0.0539*	0.0293	0.0299	0.0258
Time3	-0.3417***	0.0510	-0.2960***	0.0391	0.0112	0.0292	0.0081	0.0278
ScreeningxTime2	-0.4405***	0.0662	-0.0388	0.1162	0.0657	0.0979	-0.0405	0.0890
ScreeningxTime3	-0.3905***	0.0992	0.0414	0.2453	-0.2817***	0.0892	-0.2797***	0.0837
Constant	-8.2423***	0.0326	-7.3957***	0.0295	-7.0861***	0.0213	-6.7713***	0.0202
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Moving to women aged 50 - 59, the pattern is less clear. Mortality decreases in the second time frame and the extent of this decrease is similar across treatment and control regions. In the third time frame, mortality in the treatment regions increases and reaches a level slightly above the level in the control regions.

For women aged 60 - 69, mortality during the first two time frames is very similar across

Figure 4.10: Poisson estimated breast cancer mortality in Bremen – time restricted model



treatment and control regions. In the years 2007 - 2010, the estimated number of fatalities decreases sharply in the treatment regions. The point estimate of the interaction term *Screening x Time3* is equal to -0.28. It is precisely estimated with a corresponding 95 % interval of $[-0.46, -0.11]$. The estimated number of fatalities in the control region for the first time frame is roughly equal to 56 per $100,000$. Using this as a baseline, the confidence interval translates into an interval for the estimated number of reduction due to the screening program of $[17, 32]$.

For women who are too old to (fully) benefit from screening (70 - 79), mortality in the screening areas decreases over time. Once again, the point estimate of the corresponding interaction term is precisely estimated.

The results for Bremen are mixed. While the estimates for women aged 60 - 69 would indicate a reduction of mortality due to the screening program, the results as a whole do not support this conclusion for two main reasons. First, for the women who are too young to benefit from screening, the interaction term for this placebo-effect of the screening program is sizable and precisely estimated too. Second, mortality of women aged 50 - 59 who are fully eligible for screening, is seemingly not affected. By contrast, there is a reduction of mortality in the age group 70 - 79 in the screening regions. However, this age group is only partially affected. In summary, the results as a whole are mixed and do not provide evidence in favor of the organized screening program. With respect to the robustness of the results, the alternative specifications generate once again the same findings as the Poisson model.[6]

Hesse

Table 4.8 summarizes the Poisson regression results for the positive imputed data for Hesse (see Section 4.4). As before, due to the small number of observations for each time effect, clear patterns are hardly discernable. The model indicates a reduction of mortality for the two younger age groups (40 - 49 and 50 - 59). This finding is in line with the previous results.

The parsimonious model (Figure 4.11 and Table 4.9) summarizes these trends more clearly. Similar to Bremen, the mortality rates of the two youngest age groups (40 - 49 and 50 - 59) decrease over time; one exception is the increase of mortality in the screening regions in the third time frame for the youngest group (40 - 49). No clear pattern is found for the two older groups (60 - 69 and 70 - 79).

Overall, there is once again hardly any evidence for an association between the mortality rate and treatment status. For women aged 50 - 59, mortality decreases in both screening and control regions. Although the *Screening x Time3*-estimate of -0.40 indicates a reduction of mortality in the treatment regions that is attributable to screening, the coefficient is only very imprecisely estimated. Therefore, the estimation does not indicate any difference from the overall time trend. For women aged 60 - 69, mortality is slightly increasing over time. Here, the coefficient *Screening x Time3* would even suggest that mortality increases after the

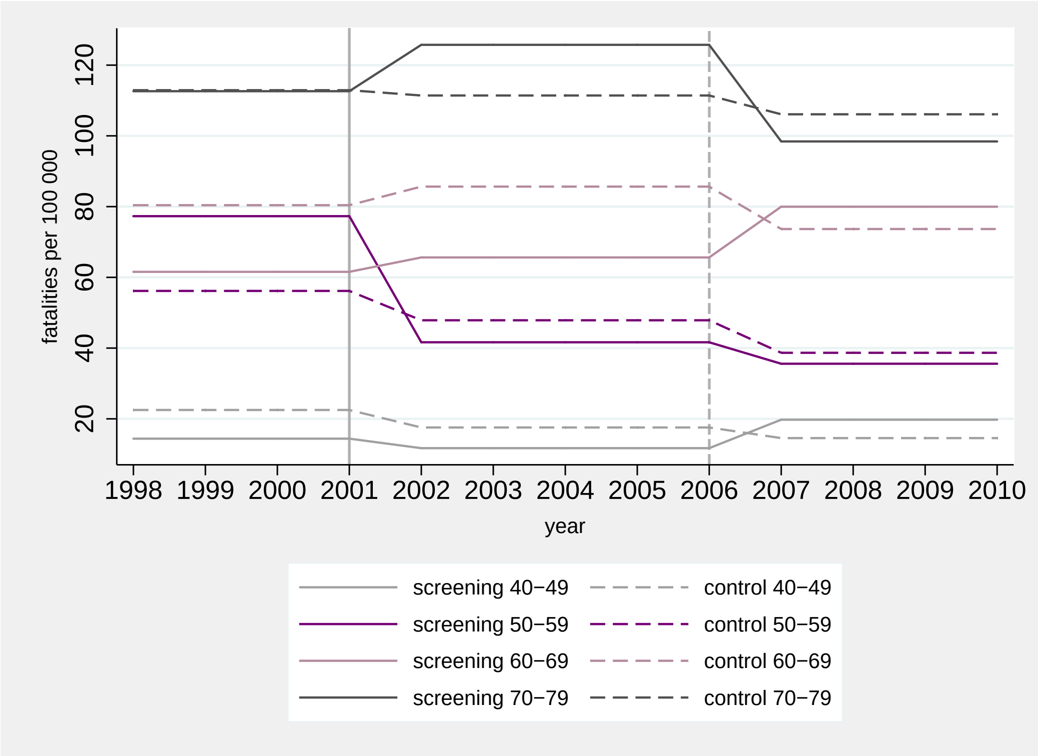
[6]See Tables 4.12 and 4.13 in the Appendix of this chapter for the time-flexible OLS and GLM models, respectively, and Figure 4.13 for the corresponding time-restricted models.

Table 4.8: Results from Poisson regression for Hesse by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-1.1049***	0.1085	0.3835***	0.0700	-0.3675***	0.0674	0.2145***	0.0460
1999	0.0418	0.1640	0.0165	0.1008	0.1938**	0.0892	0.0069	0.0751
2000	0.1250	0.1523	0.0588	0.0980	0.0992	0.0831	-0.0404	0.0767
2001	-0.1050	0.1650	0.0122	0.1004	0.0289	0.0864	0.0207	0.0731
2002	-0.1160	0.1705	-0.1970*	0.1091	0.1395	0.0981	-0.0334	0.0687
2003	-0.2527	0.1705	-0.1809*	0.1067	0.0581	0.0896	0.0507	0.0651
2004	-0.1693	0.1677	-0.1066	0.0973	0.1097	0.0792	-0.0408	0.0834
2005	-0.2382	0.1471	-0.1915*	0.1003	0.2355**	0.0922	-0.0509	0.0644
2006	-0.3831**	0.1648	-0.0312	0.0946	0.1856**	0.0941	-0.0104	0.0734
2007	-0.1977	0.1806	-0.3664***	0.1167	-0.0451	0.0915	-0.0442	0.0872
2008	-0.6170***	0.1813	-0.3020**	0.1174	0.1039	0.0869	-0.1340	0.0833
2009	-0.3009*	0.1640	-0.2541***	0.0931	-0.0602	0.0910	-0.0480	0.0673
2010	-0.6271***	0.1768	-0.4960***	0.1027	-0.0252	0.0898	-0.0399	0.0827
Screeningx1999	0.6453***	0.1640	-0.0165	0.1008	0.2291**	0.0892	-0.4657***	0.0751
Screeningx2000	-0.1468	0.1523	-0.1518	0.0980	-0.4283***	0.0831	-0.7384***	0.0767
Screeningx2001	1.4545***	0.1650	-0.0934	0.1004	0.4045***	0.0864	0.0988	0.0731
Screeningx2002	1.4396***	0.1705	-0.3044***	0.1091	-0.5399***	0.0981	0.2445***	0.0687
Screeningx2003	0.8532***	0.1705	-0.1640	0.1067	0.4283***	0.0896	-0.2029***	0.0651
Screeningx2004	0.0448	0.1677	-0.5784***	0.0973	0.2596***	0.0792	-0.2438***	0.0834
Screeningx2005	0.0957	0.1471	-0.7461***	0.1003	-0.2499***	0.0922	-0.1371**	0.0644
Screeningx2006	0.2176	0.1648	-0.9250***	0.0946	0.3226***	0.0941	-0.1976***	0.0734
Screeningx2007	1.6280***	0.1806	-0.0454	0.1167	0.6518***	0.0915	-0.2070**	0.0872
Screeningx2008	1.8148***	0.1813	-1.3775***	0.1174	0.4179***	0.0869	-0.0540	0.0833
Screeningx2009	0.1077	0.1640	-1.4340***	0.0931	0.4804***	0.0910	-0.7055***	0.0673
Screeningx2010	1.5441***	0.1768	0.1823*	0.1027	0.2033**	0.0898	-0.2623***	0.0827
Constant	-8.4172***	0.1085	-7.5065***	0.0700	-7.2091***	0.0674	-6.7833***	0.0460
N	338		338		338		338	

Note: *p<0.1, **p<0.05, ***p<0.01,

Figure 4.11: Poisson estimated breast cancer mortality in Hesse – time restricted model



introduction of the organized screening program. This effect is also visible in Figure 4.11, as mortality in the third time frame increases in the screening regions, but decreases in the control regions.

Finally, also for the oldest age group, the estimation results reveal no clear pattern over time.

In summary, the estimates suggest that there are some time trends, but there is no evidence for a reduction of mortality due to the organized screening program. For women aged 50 - 59, mortality decreases in both screening and control regions to the same extent and for women aged 60 - 69 the estimates would even suggest an increase of mortality due to screening. Once again, the alternative specifications support the results from the main Poisson model.[7] The corresponding results of the time-restricted Poisson estimations for the negative and mean imputations are presented in Tables 4.18 and 4.19 and Figures 4.17

[7]See Tables 4.14 and 4.15 in the Appendix of this chapter for the time-flexible OLS and GLM models, respectively, and Figure 4.14 for the corresponding time-restricted models.

Table 4.9: Results from time restricted Poisson regression for Hesse by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-0.4463	0.3070	0.3193***	0.0415	-0.2669*	0.1522	-0.0025	0.1698
Time2	-0.2484***	0.0813	-0.1600***	0.0484	0.0633	0.0407	-0.0134	0.0373
Time3	-0.4360***	0.0967	-0.3732***	0.0548	-0.0875**	0.0420	-0.0622	0.0437
ScreeningxTime2	0.0411	0.4362	-0.4582***	0.1207	0.0006	0.2050	0.1235	0.1915
ScreeningxTime3	0.7522*	0.3912	-0.4027	0.2978	0.3491**	0.1728	-0.0726	0.1995
Constant	-8.3989***	0.0585	-7.4845***	0.0352	-7.1259***	0.0290	-6.7863***	0.0282
N	338		338		338		338	

Note: *p<0.1, **p<0.05, ***p<0.01,

and 4.18 in the Appendix. Only for the negative imputation, the estimated coefficient for *Screening x Time3* in the age class 50 - 59 becomes significant at the 10%-level. Therefore, in the case of overestimating the number of fatalities a slightly significant higher reduction in mortality is found for the screening area compared to the control area in this age class and would suggest an effect of the screening program. But still, this result is contradicted by the findings for the age class 60 - 69 where the results suggest an increase in breast cancer mortality in the screening area compared to the control area for all imputations.

4.7 Conclusion

In summary, both descriptive and model-based results do not provide evidence for a reduction of the target population's breast cancer mortality rate. The findings are in line with recent observational studies for Denmark and Norway (Jørgensen et al. 2010 and 2013) that also cannot find any reduction in breast cancer mortality that can be attributed to an organized screening program.

There are a number of possible reasons why there is no discernable impact of the organized screening program on breast cancer mortality. First, it is possible that advances in treatment methodology are more important for a successful treatment of cancer than early detection (cf. Autier 2011). If treatment of cancer is successful whether or not it is detected early, the overall progress in medical treatment possibilities could overhaul the potential effect of an organized screening program on breast cancer mortality. In this case the organized screening program is limited in its effectiveness to additionally reduce mortality.

A second and closely related channel is that awareness of breast cancer has increased over time. For example, a higher level of cancer awareness could imply that women make more use of other prevention and early detection possibilities, e.g. manual breast exams. Both channels are compatible with the observed reductions in the mortality during the years 2001(2) and 2006(7) when it is too early to measure effects due to the screening program. These channels are also in line with the reduction in the number of fatalities in the age group of 40 - 49 which is entirely composed of women who are not eligible for mass-screening.

A further possible explanation is the relatively small take-up rate of approximately 53% in Germany (Biesheuvel et al. 2011). The take-up rate is a surrogate parameter that delivers necessary evidence for the effectiveness of the program. Naturally, insufficient acceptance of the program by the target group could severely hamper any potential effectiveness of the organized screening program. Further detailed analyses of the determinants of utilization and acceptance of the program could increase the understanding why the program shows no effect on breast cancer mortality so far.

Finally, as participation is voluntary, it is possible that women's individual risk to developing cancer is correlated with their individual decision to participate in the program and that healthy females have a higher propensity to make use of the mammography screening. This would imply that mainly healthy women without cancer are screened, and thereby driving down the probability that a serious cancer is detected due to the program which, in turn, would affect the effectiveness of the program. However, our main conclusions are unaffected by this. For instance, even if there is self-selection of healthy females into screening, there is no reason to believe that such self-selection differs systematically between pilot and control regions. The same holds for the other channels, e.g. the take-up rate.

Overall, our results suggest no effect on breast cancer mortality rates that can directly be attributed to the organized breast cancer screening program in Germany. Therefore, more detailed analyses of what determines and constrains the effectiveness of the program should be conducted in the future. A deeper understanding of these factors would allow the adjustment of the program such that it could develop its potential ability to reduce breast cancer mortality. Once detailed data at the individual level is available, it will be possible to analyze these different factors.

4.8 Appendix

Table 4.10: Results from OLS regression for Lower Saxony by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	27.6886***	3.2429	6.5381*	3.9376	-5.2213	5.1383	-21.9960***	5.9953
1999	-0.5106	4.1823	10.8576*	6.1091	-0.6515	7.0855	-6.5003	8.7556
2000	-2.7340	4.1889	-5.7215	5.4899	7.3835	6.9092	-5.9741	8.5024
2001	-3.9941	3.8571	-8.0319	6.0842	2.3402	7.0842	-3.3548	8.4248
2002	-2.8557	3.9419	-3.0769	5.5884	-5.4242	6.2276	2.6318	9.1715
2003	-11.4255***	3.6799	-0.4650	5.6539	5.9982	7.5868	2.4664	10.0024
2004	-4.1468	3.8778	-13.4067**	5.5141	7.0506	7.1298	-7.8881	8.4501
2005	-10.6975***	3.7885	-10.8992**	5.1595	12.0685*	7.1829	4.0877	8.5864
2006	-10.4531***	3.7509	-9.8168*	5.7679	8.5561	7.1012	-3.3115	8.0835
2007	-9.8490***	3.7213	-19.2697***	4.8793	4.5235	6.6797	3.4610	8.9408
2008	-10.6774***	3.9161	-18.6885***	5.0282	11.9011*	7.1931	-4.5694	8.0206
2009	-9.9753***	3.8455	-16.3861***	5.0350	-1.2301	7.2638	0.2947	8.1365
2010	-9.0684**	3.6152	-16.7824***	4.9201	4.1615	6.8628	3.2098	8.4887
Screeningx1999	-28.3135***	4.1823	70.5482***	6.1091	69.0379***	7.0855	19.4541**	8.7556
Screeningx2000	-17.2652***	4.1889	-13.2468**	5.4899	-3.2964	6.9092	32.6760***	8.5024
Screeningx2001	-34.9480***	3.8571	-20.7814***	6.0842	47.5040***	7.0842	32.9785***	8.4248
Screeningx2002	-45.3416***	3.9419	-16.1361***	5.5884	-3.9340	6.2276	14.1123	9.1715
Screeningx2003	-28.4829***	3.6799	-39.3723***	5.6539	57.8567***	7.5868	99.0461***	10.0024
Screeningx2004	-11.6654***	3.8778	-26.7225***	5.5141	0.0299	7.1298	10.0377	8.4501
Screeningx2005	-22.0934***	3.7885	-1.8797	5.1595	6.8116	7.1829	130.9652***	8.5864
Screeningx2006	-38.6145***	3.7509	5.7843	5.7679	39.1697***	7.1012	-12.6858	8.0835
Screeningx2007	-31.6852***	3.7213	14.7438***	4.8793	-13.2232**	6.6797	3.1586	8.9408
Screeningx2008	-38.7134***	3.9161	13.4334***	5.0282	9.5464	7.1931	41.8141***	8.0206
Screeningx2009	-16.7878***	3.8455	20.1378***	5.0350	4.1132	7.2638	7.5029	8.1365
Screeningx2010	-17.5342***	3.6152	-7.6831	4.9201	-30.3761***	6.8628	-20.6562**	8.4887
Constant	29.2860***	3.2429	63.0375***	3.9376	80.8234***	5.1383	116.4247***	5.9953
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Table 4.11: Results from GLM (binomial) regression for Lower Saxony by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.6658***	0.1086	0.0987	0.0613	-0.0668	0.0624	-0.2096***	0.0506
1999	-0.0176	0.1411	0.1590*	0.0872	-0.0081	0.0864	-0.0575	0.0762
2000	-0.0980	0.1463	-0.0952	0.0897	0.0875	0.0808	-0.0527	0.0737
2001	-0.1467	0.1355	-0.1364	0.1030	0.0286	0.0849	-0.0293	0.0721
2002	-0.1026	0.1368	-0.0501	0.0893	-0.0695	0.0774	0.0224	0.0764
2003	-0.4946***	0.1446	-0.0074	0.0883	0.0716	0.0887	0.0210	0.0832
2004	-0.1527	0.1367	-0.2393**	0.0979	0.0837	0.0833	-0.0702	0.0739
2005	-0.4547***	0.1499	-0.1899**	0.0877	0.1393*	0.0819	0.0345	0.0712
2006	-0.4416***	0.1464	-0.1694*	0.0990	0.1007	0.0824	-0.0289	0.0691
2007	-0.4100***	0.1424	-0.3650***	0.0890	0.0545	0.0794	0.0293	0.0742
2008	-0.4536***	0.1587	-0.3518***	0.0924	0.1375*	0.0820	-0.0401	0.0689
2009	-0.4166***	0.1510	-0.3012***	0.0901	-0.0153	0.0889	0.0025	0.0685
2010	-0.3706***	0.1334	-0.3097***	0.0876	0.0502	0.0816	0.0272	0.0706
Screeningx1999	-0.6877***	0.1411	0.6166***	0.0872	0.6530***	0.0864	0.1862**	0.0762
Screeningx2000	-0.3345**	0.1463	-0.2233**	0.0897	-0.0348	0.0808	0.3020***	0.0737
Screeningx2001	-1.0041***	0.1355	-0.3986***	0.1030	0.4784***	0.0849	0.3024***	0.0721
Screeningx2002	-1.7683***	0.1368	-0.2733***	0.0893	-0.0627	0.0774	0.1410*	0.0764
Screeningx2003	-0.7113***	0.1446	-0.8430***	0.0883	0.5413***	0.0887	0.7100***	0.0832
Screeningx2004	-0.1725	0.1367	-0.6210***	0.0979	0.0059	0.0833	0.0928	0.0739
Screeningx2005	-0.4026***	0.1499	-0.0131	0.0877	0.0838	0.0819	0.8548***	0.0712
Screeningx2006	-1.5337***	0.1464	0.1096	0.0990	0.3892***	0.0824	-0.1569**	0.0691
Screeningx2007	-0.8960***	0.1424	0.2977***	0.0890	-0.1768**	0.0794	0.0385	0.0742
Screeningx2008	-1.5635***	0.1587	0.2732***	0.0924	0.1125	0.0820	0.3729***	0.0689
Screeningx2009	-0.2181	0.1510	0.3537***	0.0901	0.0528	0.0889	0.0769	0.0685
Screeningx2010	-0.2587*	0.1334	-0.1238	0.0876	-0.4763***	0.0816	-0.2317***	0.0706
Constant	-8.1355***	0.1086	-7.3686***	0.0613	-7.1199***	0.0624	-6.7545***	0.0506
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Figure 4.12: OLS and GLM estimated breast cancer mortality in Lower Saxony

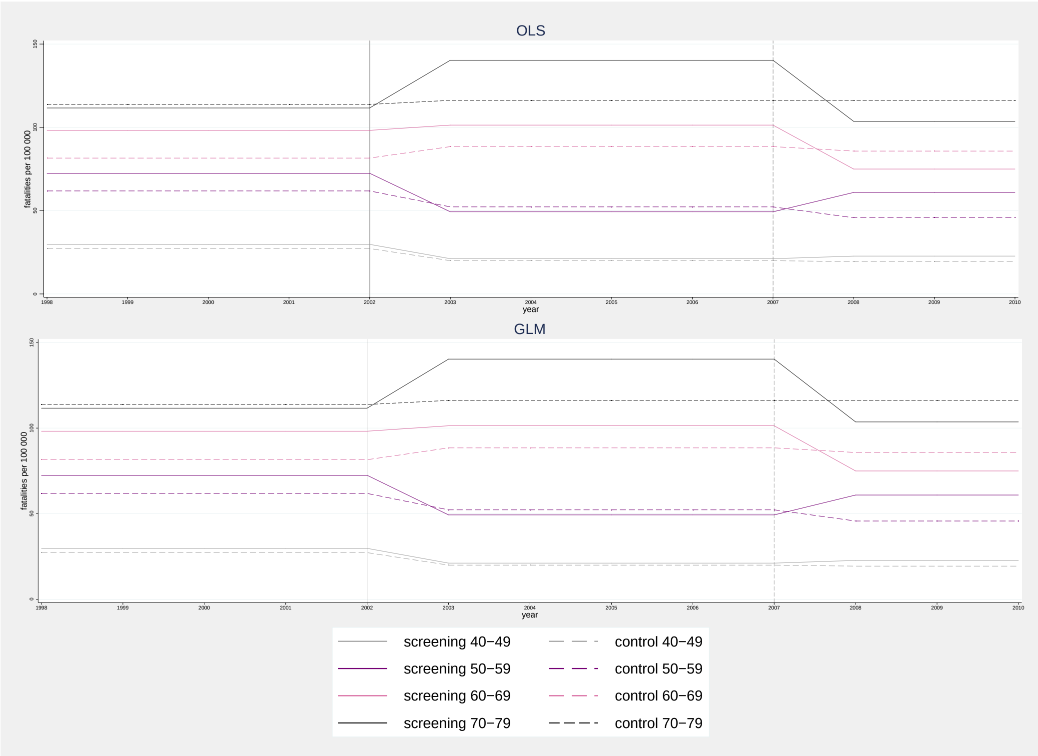


Figure 4.13: OLS and GLM estimated breast cancer mortality in Bremen

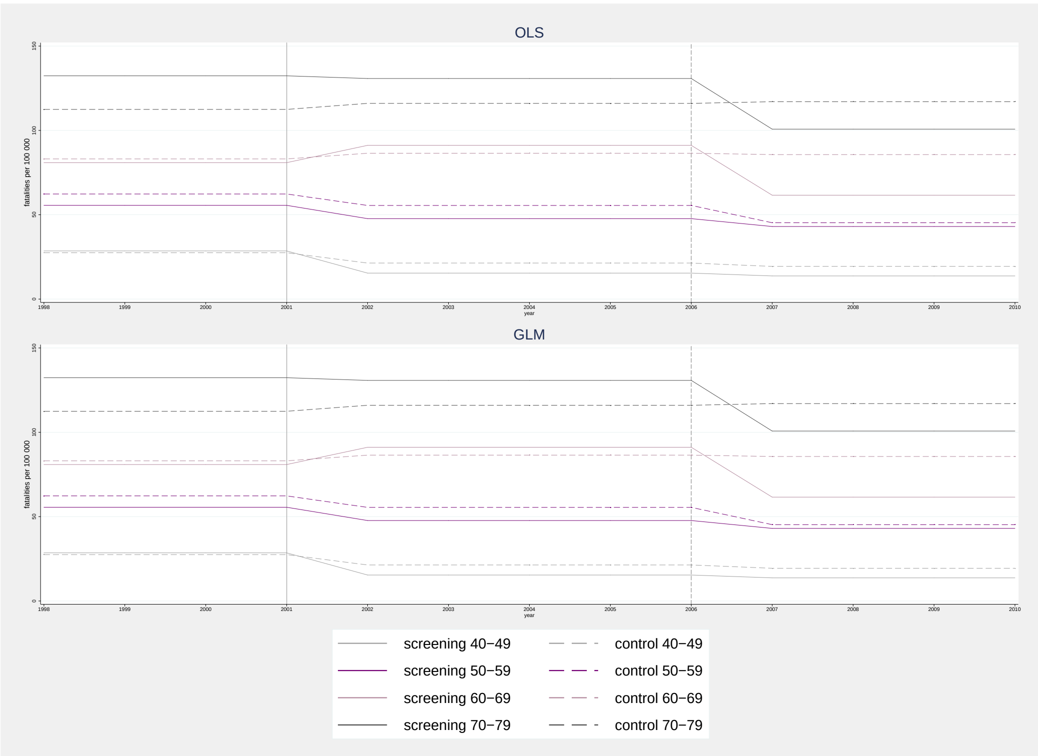


Table 4.12: Results from OLS regression for Bremen by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.1667	3.2429	-5.6965	3.9376	5.7593	5.1383	9.6744	5.9953
1999	-0.5106	4.1823	10.8576*	6.1091	-0.6515	7.0855	-6.5003	8.7556
2000	-2.7340	4.1889	-5.7215	5.4899	7.3835	6.9092	-5.9741	8.5024
2001	-3.9941	3.8571	-8.0319	6.0842	2.3402	7.0842	-3.3548	8.4248
2002	-2.8557	3.9419	-3.0769	5.5884	-5.4242	6.2276	2.6318	9.1715
2003	-11.4255***	3.6799	-0.4650	5.6539	5.9982	7.5868	2.4664	10.0024
2004	-4.1468	3.8778	-13.4067**	5.5141	7.0506	7.1298	-7.8881	8.4501
2005	-10.6975***	3.7885	-10.8992**	5.1595	12.0685*	7.1829	4.0877	8.5864
2006	-10.4531***	3.7509	-9.8168*	5.7679	8.5561	7.1012	-3.3115	8.0835
2007	-9.8490***	3.7213	-19.2697***	4.8793	4.5235	6.6797	3.4610	8.9408
2008	-10.6774***	3.9161	-18.6885***	5.0282	11.9011*	7.1931	-4.5694	8.0206
2009	-9.9753***	3.8455	-16.3861***	5.0350	-1.2301	7.2638	0.2947	8.1365
2010	-9.0684**	3.6152	-16.7824***	4.9201	4.1615	6.8628	3.2098	8.4887
Screeningx1999	3.1443	4.1823	-29.1753***	6.1091	-10.5120	7.0855	14.2929	8.7556
Screeningx2000	-0.1963	4.1889	16.5836***	5.4899	-23.2068***	6.9092	10.5201	8.5024
Screeningx2001	0.6612	3.8571	8.4526	6.0842	1.8787	7.0842	16.1192*	8.4248
Screeningx2002	-11.2021***	3.9419	0.9540	5.5884	35.4517***	6.2276	20.9248**	9.1715
Screeningx2003	-2.9181	3.6799	-13.1480**	5.6539	3.4550	7.5868	24.6619**	10.0024
Screeningx2004	-10.4043***	3.8778	-3.1881	5.5141	-13.2000*	7.1298	-26.8660***	8.4501
Screeningx2005	-4.1246	3.7885	7.5094	5.1595	-29.4183***	7.1829	-17.5742**	8.5864
Screeningx2006	-2.0575	3.7509	-2.6799	5.7679	-1.7258	7.1012	24.2334***	8.0835
Screeningx2007	-2.8429	3.7213	-10.3644**	4.8793	-18.8853***	6.6797	-47.2468***	8.9408
Screeningx2008	-6.8898*	3.9161	29.7282***	5.0282	-36.3885***	7.1931	-3.4004	8.0206
Screeningx2009	-7.5941**	3.8455	13.4817***	5.0350	-22.2289***	7.2638	-12.1004	8.1365
Screeningx2010	-6.0193*	3.6152	-18.9422***	4.9201	-41.8489***	6.8628	-41.0014***	8.4887
Constant	29.2860***	3.2429	63.0375***	3.9376	80.8234***	5.1383	116.4247***	5.9953
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Table 4.13: Results from GLM (binomial) regression for Bremen by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	0.0057	0.1086	-0.0948	0.0613	0.0689	0.0624	0.0799	0.0506
1999	-0.0176	0.1411	0.1590*	0.0872	-0.0081	0.0864	-0.0575	0.0762
2000	-0.0980	0.1463	-0.0952	0.0897	0.0875	0.0808	-0.0527	0.0737
2001	-0.1467	0.1355	-0.1364	0.1030	0.0286	0.0849	-0.0293	0.0721
2002	-0.1026	0.1368	-0.0501	0.0893	-0.0695	0.0774	0.0224	0.0764
2003	-0.4946***	0.1446	-0.0074	0.0883	0.0716	0.0887	0.0210	0.0832
2004	-0.1527	0.1367	-0.2393**	0.0979	0.0837	0.0833	-0.0702	0.0739
2005	-0.4547***	0.1499	-0.1899**	0.0877	0.1393*	0.0819	0.0345	0.0712
2006	-0.4416***	0.1464	-0.1694*	0.0990	0.1007	0.0824	-0.0289	0.0691
2007	-0.4100***	0.1424	-0.3650***	0.0890	0.0545	0.0794	0.0293	0.0742
2008	-0.4536***	0.1587	-0.3518***	0.0924	0.1375*	0.0820	-0.0401	0.0689
2009	-0.4166***	0.1510	-0.3012***	0.0901	-0.0153	0.0889	0.0025	0.0685
2010	-0.3706***	0.1334	-0.3097***	0.0876	0.0502	0.0816	0.0272	0.0706
Screeningx1999	0.1033	0.1411	-0.5441***	0.0872	-0.1301	0.0864	0.1176	0.0762
Screeningx2000	-0.0068	0.1463	0.2688***	0.0897	-0.2895***	0.0808	0.0882	0.0737
Screeningx2001	0.0265	0.1355	0.1437	0.1030	0.0191	0.0849	0.1258*	0.0721
Screeningx2002	-0.5463***	0.1368	0.0123	0.0893	0.3676***	0.0774	0.1491*	0.0764
Screeningx2003	-0.1730	0.1446	-0.2638***	0.0883	0.0321	0.0887	0.1742**	0.0832
Screeningx2004	-0.5287***	0.1367	-0.1026	0.0979	-0.1574*	0.0833	-0.2525***	0.0739
Screeningx2005	-0.2451	0.1499	0.1290	0.0877	-0.3631***	0.0819	-0.1478**	0.0712
Screeningx2006	-0.1115	0.1464	-0.0766	0.0990	-0.0247	0.0824	0.1826***	0.0691
Screeningx2007	-0.1538	0.1424	-0.3626***	0.0890	-0.2360***	0.0794	-0.4563***	0.0742
Screeningx2008	-0.4540***	0.1587	0.5280***	0.0924	-0.4702***	0.0820	-0.0253	0.0689
Screeningx2009	-0.4913***	0.1510	0.2492***	0.0901	-0.3009***	0.0889	-0.1009	0.0685
Screeningx2010	-0.3475***	0.1334	-0.6662***	0.0876	-0.6220***	0.0816	-0.3838***	0.0706
Constant	-8.1355***	0.1086	-7.3686***	0.0613	-7.1199***	0.0624	-6.7545***	0.0506
N	650		650		650		650	

Note: *p<0.1, **p<0.05, ***p<0.01,

Table 4.14: Results from OLS regression for Hesse by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-12.9234***	2.6720	31.0357***	4.6881	-16.6675***	4.9252	29.9641***	6.2836
1999	1.3364	3.9498	8.7113	6.6842	21.0584***	7.7837	-0.2277	9.8662
2000	5.2678	3.7238	10.3501	6.8252	12.3887*	7.1255	-3.1000	10.1493
2001	-1.1824	3.6662	7.3340	6.7977	6.3051	7.0236	2.1065	9.5414
2002	-2.4221	3.7446	-5.7418	6.5761	13.6268	9.0197	-3.2519	8.9223
2003	-3.5841	3.2989	-3.7929	6.0186	12.9671*	6.9305	8.5434	9.3132
2004	-2.2603	3.5477	-1.9356	5.9707	13.9390**	6.1353	-2.8522	10.9197
2005	-1.6578	3.3680	-5.0800	5.9271	24.6552***	7.7477	-3.8313	8.9869
2006	-5.1161	3.2473	0.8231	6.2771	16.4788**	8.0964	-1.7926	9.6576
2007	-4.9679	3.1405	-15.2357***	5.8778	1.5115	7.2451	-6.4374	10.1906
2008	-8.6602***	3.2372	-10.5790*	6.1640	13.2306*	7.7507	-10.8706	9.8888
2009	-4.7874	3.4649	-7.7156	5.8297	-2.7617	7.1660	-3.9345	8.6027
2010	-8.6649***	3.1368	-17.6494***	5.4782	3.6359	7.2921	-2.7929	9.6860
Screeningx1999	5.8964	3.9498	-8.7113	6.6842	5.9135	7.7837	-51.4123***	9.8662
Screeningx2000	-5.4257	3.7238	-17.5129**	6.8252	-26.7552***	7.1255	-72.8434***	10.1493
Screeningx2001	22.0879***	3.6662	-13.6228**	6.7977	21.4890***	7.0236	15.7198	9.5414
Screeningx2002	22.6055***	3.7446	-26.0503***	6.5761	-30.5304***	9.0197	36.2485***	8.9223
Screeningx2003	9.6105***	3.2989	-19.7290***	6.0186	19.1298***	6.9305	-28.3568***	9.3132
Screeningx2004	1.4032	3.5477	-38.0495***	5.9707	8.9422	6.1353	-31.9110***	10.9197
Screeningx2005	0.6859	3.3680	-43.9815***	5.9271	-25.3882***	7.7477	-20.2194**	8.9869
Screeningx2006	3.9995	3.2473	-50.4643***	6.2771	17.4488**	8.0964	-24.5710**	9.6576
Screeningx2007	28.2502***	3.1405	-11.9858**	5.8778	41.2279***	7.2451	-24.7389**	10.1906
Screeningx2008	25.5932***	3.2372	-55.0178***	6.1640	21.8659***	7.7507	-13.1831	9.8888
Screeningx2009	3.5012	3.4649	-58.0101***	5.8297	29.5218***	7.1660	-70.3533***	8.6027
Screeningx2010	19.6598***	3.1368	-4.0596	5.4782	6.3502	7.2921	-33.8110***	9.6860
Constant	20.2446***	2.6720	49.5964***	4.6881	67.8970***	4.9252	110.3957***	6.2836
N	338		338		338		338	

Note: *p<0.1, **p<0.05, ***p<0.01,

Table 4.15: Results from GLM (binomial) regression for Hesse by age groups

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-1.0172***	0.1270	0.4863***	0.0910	-0.2818***	0.0698	0.2404***	0.0548
1999	0.0639	0.1816	0.1619	0.1203	0.2704***	0.0956	-0.0021	0.0862
2000	0.2313	0.1603	0.1896	0.1209	0.1677*	0.0932	-0.0285	0.0901
2001	-0.0602	0.1794	0.1380	0.1233	0.0889	0.0954	0.0189	0.0824
2002	-0.1274	0.1903	-0.1231	0.1361	0.1830	0.1133	-0.0299	0.0790
2003	-0.1949	0.1692	-0.0796	0.1207	0.1749*	0.0908	0.0746	0.0781
2004	-0.1184	0.1781	-0.0398	0.1177	0.1869**	0.0820	-0.0262	0.0970
2005	-0.0855	0.1655	-0.1081	0.1201	0.3100***	0.0935	-0.0354	0.0799
2006	-0.2914*	0.1730	0.0165	0.1210	0.2175**	0.1013	-0.0164	0.0851
2007	-0.2816*	0.1641	-0.3672***	0.1347	0.0220	0.1015	-0.0601	0.0924
2008	-0.5583***	0.1979	-0.2400*	0.1343	0.1782*	0.0996	-0.1038	0.0920
2009	-0.2699	0.1871	-0.1692	0.1209	-0.0416	0.1039	-0.0363	0.0764
2010	-0.5587***	0.1865	-0.4400***	0.1248	0.0522	0.1006	-0.0257	0.0858
Screeningx1999	0.6232***	0.1816	-0.1619	0.1203	0.1529	0.0956	-0.4572***	0.0862
Screeningx2000	-0.2531	0.1603	-0.2827**	0.1209	-0.4970***	0.0932	-0.7511***	0.0901
Screeningx2001	1.4099***	0.1794	-0.2193*	0.1233	0.3448***	0.0954	0.1008	0.0824
Screeningx2002	1.4512***	0.1903	-0.3786***	0.1361	-0.5836***	0.1133	0.2414***	0.0790
Screeningx2003	0.7955***	0.1692	-0.2656**	0.1207	0.3119***	0.0908	-0.2270***	0.0781
Screeningx2004	-0.0061	0.1781	-0.6455***	0.1177	0.1826**	0.0820	-0.2587***	0.0970
Screeningx2005	-0.0569	0.1655	-0.8301***	0.1201	-0.3244***	0.0935	-0.1528*	0.0799
Screeningx2006	0.1259	0.1730	-0.9732***	0.1210	0.2911***	0.1013	-0.1919**	0.0851
Screeningx2007	1.7122***	0.1641	-0.0450	0.1347	0.5850***	0.1015	-0.1913**	0.0924
Screeningx2008	1.7563***	0.1979	-1.4401***	0.1343	0.3440***	0.0996	-0.0845	0.0920
Screeningx2009	0.0767	0.1871	-1.5196***	0.1209	0.4621***	0.1039	-0.7179***	0.0764
Screeningx2010	1.4758***	0.1865	0.1261	0.1248	0.1260	0.1006	-0.2769***	0.0858
Constant	-8.5048***	0.1270	-7.6085***	0.0910	-7.2943***	0.0698	-6.8077***	0.0548
N	338		338		338		338	

Note: *p<0.1, **p<0.05, ***p<0.01,

Table 4.16: Mean mortality rates in Hesse* - negative imputation

	Screening Area			Control Area		
Age	Mean	SD	Min/Max	Mean	SD	Min/Max
40 - 49	22.30	0.99	12.07/ 36.72	24.38	12.17	0/ 66.66
50 - 59	56.19	18.97	29.81/ 88.70	52.56	19.18	0/134.77
60 - 69	72.11	15.60	42.91/ 93.97	82.16	25.40	0/183.56
70 - 79	72.03	17.06	46.35/105.37	67.51	19.48	14.34/129.95
N	13			325		

*fatalities per 100 000

Figure 4.14: OLS and GLM estimated breast cancer mortality in Hesse

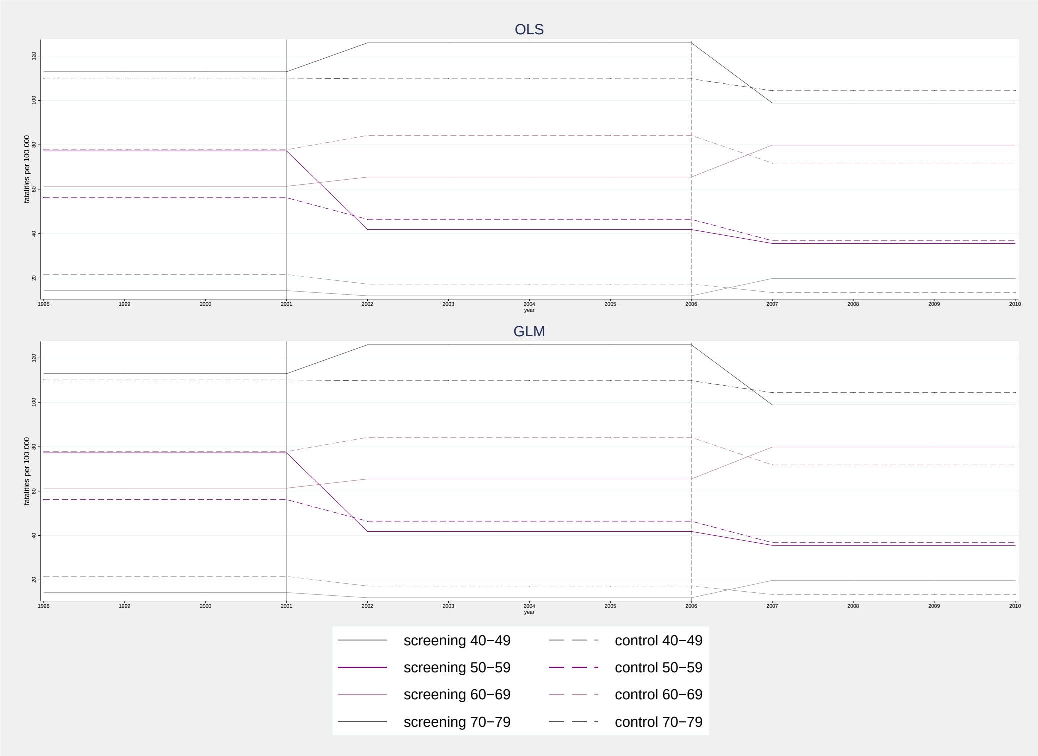


Figure 4.15: Breast cancer mortality rates in Hesse - negative imputation

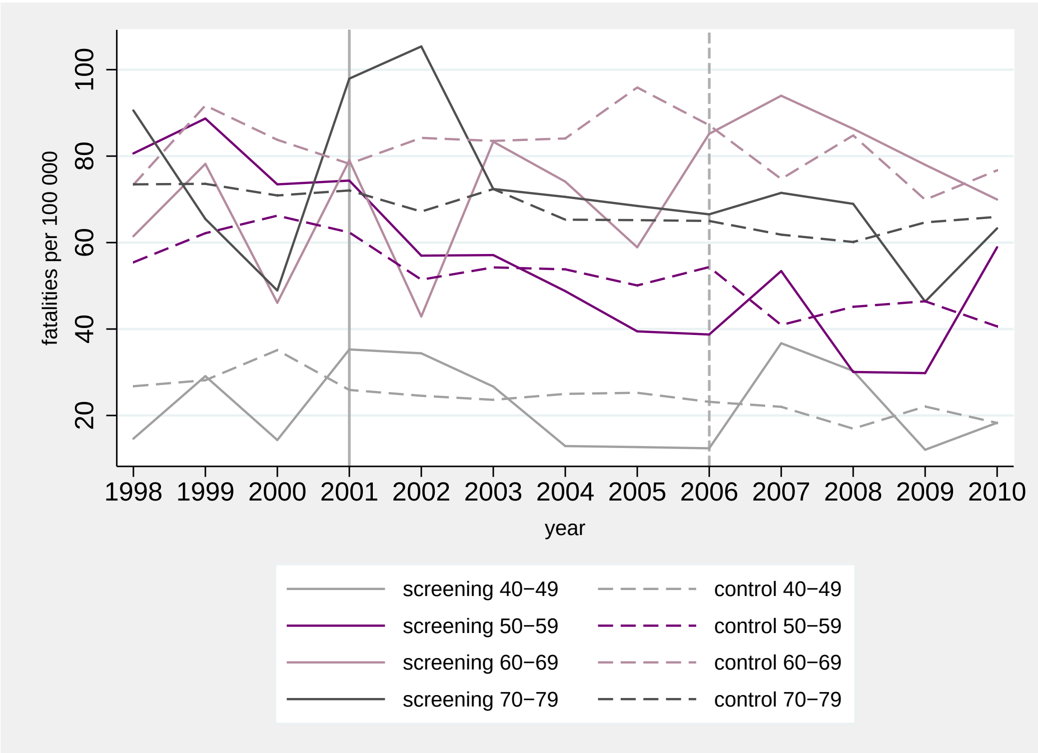


Table 4.17: Mean mortality rates in Hesse* - mean imputation

	Screening Area			Control Area		
Age	Mean	SD	Min/Max	Mean	SD	Min/Max
40 - 49	18.66	10.94	6.46/ 36.72	20.40	11.45	0/ 66.66
50 - 59	52.68	21.70	18.63/ 83.86	49.28	20.01	0/134.77
60 - 69	70.40	18.24	36.86/ 93.97	80.23	26.56	0/183.56
70 - 79	70.04	19.01	40.77/105.37	66.35	20.44	13.96/129.95
N	13			325		

*fatalities per 100 000

Figure 4.16: Breast cancer mortality rates in Hesse - mean imputation

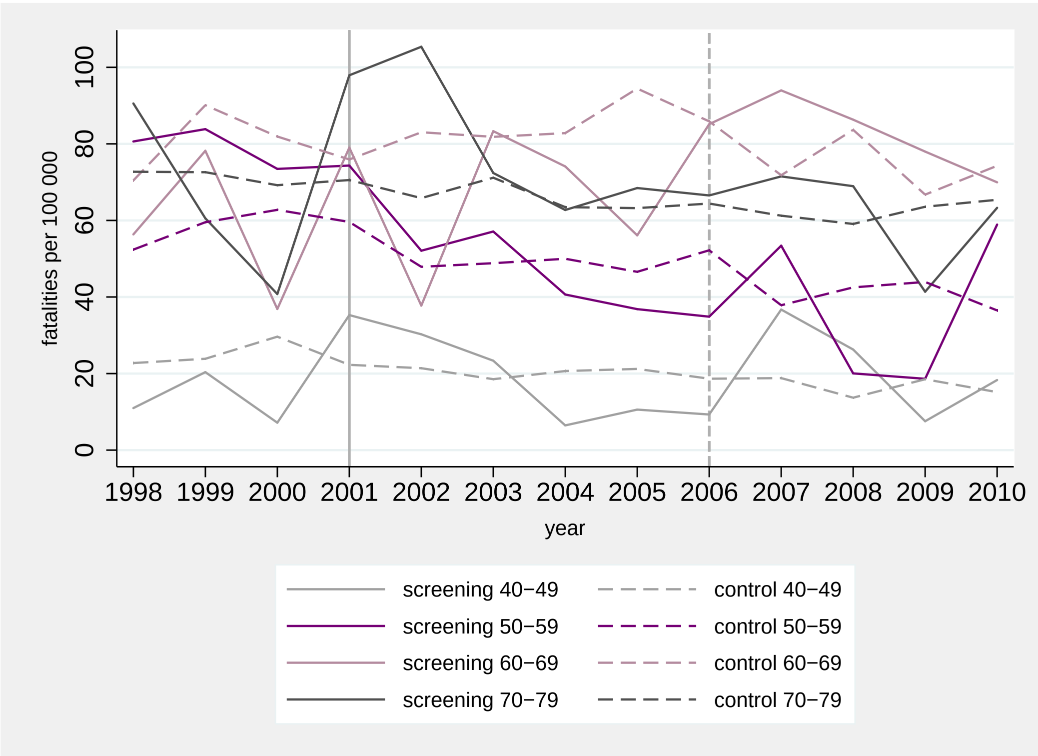


Table 4.18: Results from time restricted Poisson regression for Hesse - negative imputation

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-0.1950	0.2009	0.2890***	0.0499	-0.2206**	0.1082	0.0323	0.1322
Time2	-0.2054***	0.0563	-0.1683***	0.0408	0.0299	0.0362	-0.0971***	0.0324
Time3	-0.4268***	0.0737	-0.3110***	0.0476	-0.0665	0.0409	-0.1448***	0.0426
ScreeningxTime2	0.1671	0.2794	-0.3137***	0.0847	0.0678	0.1466	0.0942	0.1508
ScreeningxTime3	0.2806	0.3017	-0.3812*	0.2092	0.2306*	0.1230	-0.0964	0.1649
Constant	-8.1648***	0.0431	-7.4283***	0.0317	-7.0981***	0.0263	-7.2186***	0.0247
N	338	338	338		338			

Standard errors in second column
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4.17: Poisson estimated breast cancer mortality in Hesse: time restricted model – negative imputation

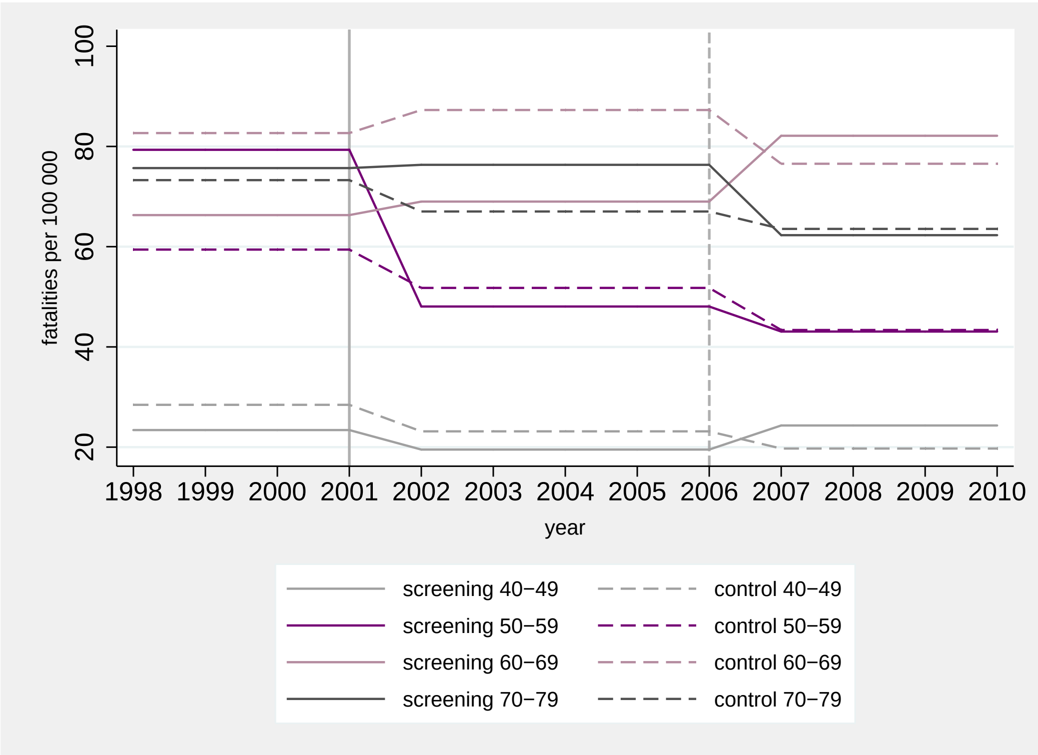
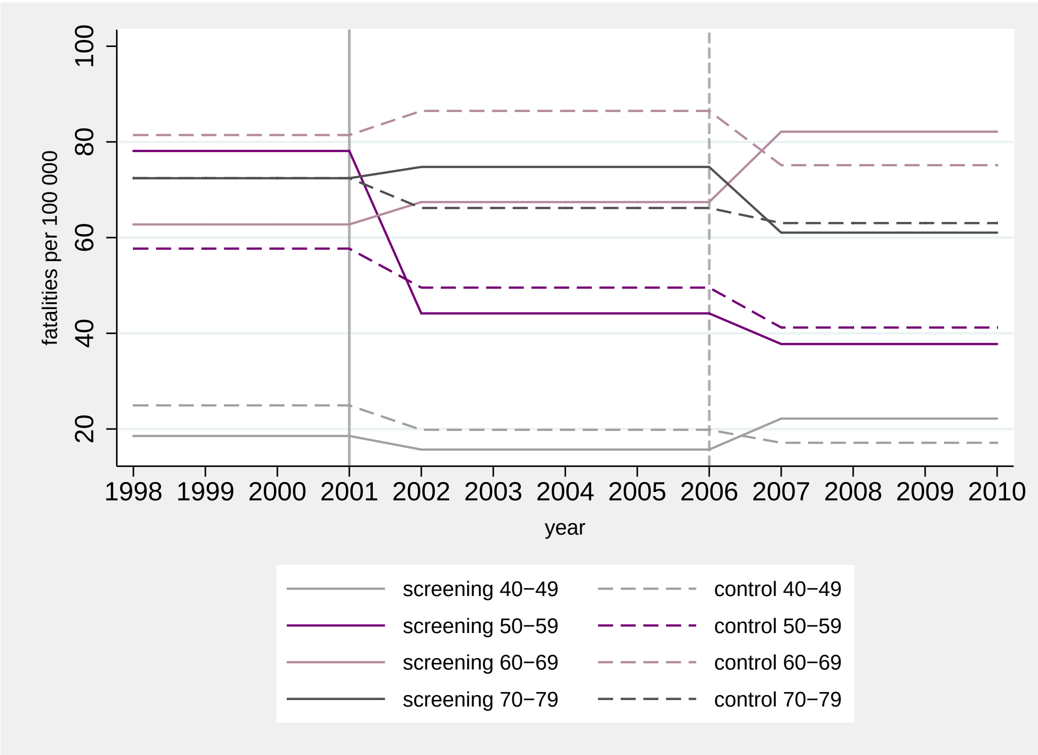


Table 4.19: Results from time restricted Poisson regression for Hesse - mean imputation

	40 - 49		50 - 59		60 - 69		70 - 79	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Screening	-0.2962	0.3006	0.3029***	0.0432	-0.2606*	0.1451	-0.0005	0.1617
Time2	-0.2201***	0.0670	-0.1840***	0.0435	0.0336	0.0378	-0.0972***	0.0343
Time3	-0.4512***	0.0854	-0.3325***	0.0518	-0.0681	0.0428	-0.1420***	0.0443
ScreeningxTime2	0.2642	0.3898	-0.3505***	0.0933	0.1011	0.1828	0.1214	0.1795
ScreeningxTime3	0.3850	0.4010	-0.5390	0.3373	0.2873*	0.1567	-0.0832	0.2031
Constant	-8.2963***	0.0509	-7.4577***	0.0330	-7.1131***	0.0276	-7.2300***	0.0263
N	338	338	338		338			

Standard errors in second column
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4.18: Poisson estimated breast cancer mortality in Hesse: time restricted model – mean imputation



Chapter 5

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