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Léa Toulemon

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Institut d'études politiques de Paris
ECOLE DOCTORALE DE SCIENCES PO
Programme doctoral en économie
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Doctorat en Sciences économiques

Job quality, Health Insurance and the Price of Medical Products: Essays in Applied Economics

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Defended on December, 16 2016

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Chapter 1

Introduction

This thesis focuses on two major aspects of individual well-being, which are job quality and the availability of medical care.

Work is a time-consuming activity, and 58% of Europeans consider work as “very important” in their life (Halman et al., 2011). Being unemployed is associated with low levels of well-being (Clark et al., 2008). As for employed individuals, well-being depends on multiple aspects of the job, defined as job quality (Clark, 2015). In the early 1990s, new work practices such as job rotation and just-in-time management have led to work intensification, both in the United States and in Europe - Askenazy et al. (2002), Askenazy (2004). Reductions in working-times have also translated into more flexible working hours and increasing time pressure (Gollac and Volkoff, 2010).

In a context of growing job instability and work intensification, current earnings and employment status may not be sufficient to reflect the impact of work on well-being, and researchers have begun to use broader definitions of job quality. Osterman (2013) identifies two ways researchers have thought about job quality, namely through the use of economic models focusing on compensating differentials, and via a more institutional perspective

that underlines the importance of sociological and political considerations. According to the theory of compensating differentials, workers trade off all job characteristics, and jobs with low quality will offer higher wages. The firms will improve job quality if the cost of doing so is lower than the benefit for employees. Gollac and Volkoff (2010) challenges this view by underlying the need for objective measures of working conditions and collective bargaining for workers to be compensated for low job quality. Two equilibria can occur. In the best case scenario, workers stay in the same firm for a long time, working conditions are negotiated, and the firms increase job quality. A second equilibrium would be a vicious circle where jobs are of low quality and job turnover is high because of low job quality. In this case, workers do not take time to negotiate improvements in working conditions and prefer to change job. Consequently, job quality improvements do not always occur naturally, and we need to understand the context in which such improvements can occur. In a nutshell, job quality is an emerging area of research for economists because of the possible presence of suboptimal equilibria and the growing evidence on its importance for individual well-being.

Another important aspect of individuals well-being rests on the availability of adequate care in the event of illness. In a seminal paper, Arrow (1963) defines health economics as the study of the medical-care industry, and details its specificities compared to other markets. First, as patients cannot know in advance when they will be sick, demand for healthcare is irregular and unpredictable. A second specificity of the healthcare market pertains to the uncertainty about the quality of the care one is receiving. There is asymmetric information between the physician-supplier and the patient-buyer. Then, healthcare consumption creates positive and negative externalities, for instance vaccines form a well-known example of positive externalities. In addition, healthcare markets sometimes exhibit increasing returns. In the production of medicines for instance, the cost of medical research does not depend on the quantity produced, and research costs account for a significant part

of average costs. As a consequence, the marginal costs of producing medicines are often decreasing. Last but not least, medical care is not an usual consumption product. Medical care consumption directly relates to the survival probability and can be seen as an investment for being able to work, as Arrow states in his influential paper of 1963:

“The demand for medical services is associated, with a considerable probability, with an assault on personal integrity. There is some risk of death and a more considerable risk of impairment of full functioning. In particular, there is a major potential for loss or reduction of earning ability”

These particular features of healthcare markets explain the extent of government intervention in the health system, and make healthcare markets interesting for economists to study.

Objective of the thesis

This thesis uses microeconomic analysis to investigate questions related to well-being at work and to the design of the health system.

We first focus on the impact of job displacement on subsequent job quality. In the first chapter, we investigate the long-term effects of job displacement on several dimensions of job quality and satisfaction, focusing only on individuals who lose their job because of plant closure. Previous studies find a long lasting impact of displacement on income in the United-States, and more recently in Europe (Jacobson et al. (1993), Eliason and Storrie (2006)). Simultaneously, there is growing evidence in the literature that displacement increases mortality - (Sullivan and von Wachter, 2009), Eliason and Storrie (2009), Browning

and Heinesen (2012). In this context, we identify the impact of job displacement on job quality, to give a broad picture of the impact of displacement on the career path.

In chapters two and three, we focus on two important features related to the sustainability of the health care system: health insurance and drug pricing. Issues of adverse selection may prevent private insurance markets from emerging, and there are considerable economies of scales to insurance. These specificities provide ground for large scale insurances, often public and compulsory. In this setting, a critical issue is to determine the level of moral hazard, i.e. how insurance may shape the demand for medical care. If moral hazard effects are large, insurance could decrease welfare, even if all agents are risk adverse (Pauly, 1968). We take advantage of the co-existence of two compulsory public health insurance systems to measure the impact of public reimbursement rates on medical care expenditures, using an administrative database from the National Health Insurance.

The third chapter of the thesis focuses on yet another important aspect of medical care markets. While retail drug prices are regulated at the national level, the prices of inpatient hospital drugs are negotiated between pharmaceutical companies and hospitals. Given the weight of research on average costs, medicines production often exhibit increasing returns, which makes price setting challenging. A major concern for price setting is whether it should be done at the national level or in a decentralized manner, in which case hospitals' purchasing prices depend on their willingness to buy the medicine and on their bargaining power. One of the answer to these issues was to introduce an intermediate level, regional purchasing groups, through which hospitals can launch common calls for tender. We will show how hospitals group purchasing affect inpatient medicine prices.

Finding relevant databases

In all the chapters of this thesis, finding the relevant dataset has been a major concern. We have worked on three distinct databases. The database used in the first chapter is the German Socio-Economic Panel (G-SOEP), it provides extensive information about career paths and job characteristics of thousands of individuals, from the 80's to today. We use wages from 1984 to 2012 including about 46,600 individuals. Withdrawing individuals below eighteen or above sixty years old, we obtain a final sample of 37,500 individuals, including 2,396 displaced workers.

The databases we have used for chapters two and three, on the opposite, are administrative data. They include information which were not collected for research purposes. They both consist of information used by the National Health Insurance for reimbursement purposes, either to patients (chapter two) or to hospitals (chapter three). The database of chapter two is the “Hygie” database. Recently made available for researchers, it aims at providing information about sick-leaves. To do so, it merges information about health expenditures reimbursed by the National Health Insurance and about the career - from the National Old Age Pension Fund. It is a random sample of individuals between 22 and 70 years old in 2005, who have contributed to the national pension fund at least once during their life, and have used the health care system at least once between 2003 and 2005. Among them, 2,500 individuals move from or to Alsace Moselle and form the treatment group. The control group is made of the 33,000 individuals moving from and to other French regions.

The database we make use of for the third chapter is directly utilized by the National Health Insurance to reimburse innovative medicines to public hospitals. To my knowledge,

this is one of the few databases with actual buyer-supplier transfers in a business-to-business market. It contains annual quantities and annual average prices of innovative medicines dispensed by all French public hospitals from 2008 to 2014, with hospitals and medicines identifiers. These identifiers allow us to use to merge the database with other public sources and to add relevant information both at the hospital and medicine level. In particular, we were able to add information about regional purchasing group membership for 350 hospitals. Fifty-four hospitals have joined a group during the period and form the treatment group.

Administrative micro-data are deemed critical for research and in particular for public policy evaluation (Card et al., 2010) because of their high quality. They have two important advantages: they allow to have large panels and to avoid both non-response and attrition. However, using administrative databases involves issues of reliability and documentation, especially when they have been seldom used by researchers before. Because of confidentiality concerns, administrations are sometimes reluctant to make their data available. The growing availability of administrative micro-data is a promising avenue for future research, and this thesis is a step in this direction.

Estimation of causal effects with panel data

Each chapter of this thesis aims at measuring the impact of a situation on an outcome with similar methods. In this section, we will present the methodologies adopted and the underlying assumptions. Chapter one measures the impact of job displacement on job quality, Chapter two measures the impact of reimbursement rates on health expenditures, and Chapter three measures the impact of group purchasing on drug prices. The “situations” we consider are binary: workers are either displaced or stay at the same job. Individuals

are either affiliated to the Alsace Moselle local system or affiliated to the national system only. Some hospitals engage in regional group purchasing whereas some do not. In what follows, we will call the affected group the “treatment group”, and the unaffected the “control group”, using the terminology used in randomized experiments, because all chapters attempt to reproduce an experimental setting.

When measuring treatment effects, there exist different possibilities: (1) treatment is random, this is close to the experimental setting, and comparing outcomes in both groups will lead to an unbiased estimate of treatment impact. The situation where treatment is non random, but correlates to characteristics that are not linked to the outcomes also falls into this category. (2) treatment status depends on observable characteristics and/or permanent unobservable characteristics, which have a time-invariant impact on the outcomes. However, treatment status does not depend on time-varying characteristics linked to the outcome, as expected shocks. In this case, we can make use of the panel structure of the data to obtain unbiased results, using a difference-in-difference strategy or a matching estimator - depending on the assumption we want to make about trends in the control and treatment groups. (3) There are time-varying unobserved characteristics which impact both treatment probability and the outcome. This happens if individuals choose to get the treatment when they have just received - or expect to have - a shock on the outcome. With the example of health insurance, one could choose to get higher reimbursement rates when anticipating that their health status will deteriorate, which would bias the estimates of health insurance expenditures upwards. In this case, observing the outcome (health status) before treatment does not help to reduce the bias. A situation where there are time-invariant characteristics that impact treatment probability and have a time-varying impact on the outcome also falls into this category. In this last case, we need to rely on instrumental variable estimation to estimate the treatment effects.

All chapters of this thesis focus on situations that fall into situation (2). The challenges for estimating causal relationship in this case are numerous. First, we need to have reasons to think that factors that have an effect on the outcome are not linked to treatment status, or are time-invariant and have a permanent impact on the outcome. The exact way of addressing these issues is different in each chapter, but there exist common features. The first point in common is the study of a situation where the treated individuals have not chosen to enter treatment, and treatment is a priori unlikely to be linked to the evolution of the outcomes. In the first chapter, workers are fired as a result of a plant closure. We exclude individuals who lose their job because of voluntary quits or layoffs, situations where job loss is more likely to be correlated with their individual characteristics. In the second chapter, we utilize the coexistence of two compulsory public health insurance systems in France, the National system, and the Alace Moselle local system, which offers higher reimbursement rates. Individuals moving to Alsace Moselle form the treatment group. We do not use individuals who choose an insurance with higher reimbursement rates. In the last chapter, we focus on hospitals that join a purchasing group when it is created, and exclude hospitals who choose to join later. Therefore, treatment status is based on regional factors, not individual hospitals' decisions.

The second important point is to find the relevant control group, i.e. to find individuals - or hospitals - who have not been treated, but who could have been. In the first chapter, we choose to pick workers who have the same characteristics before treatment, including the outcome of interest, to account for time-invariant unobserved characteristics. The idea is that plant closure does not occur randomly, some workers are more exposed to this risk, but the moment when a worker loses his or her job is unrelated with the variations of their current or expected individual characteristics. For instance, blue-collar workers may be

more likely to lose their job because of plant closure than white-collar workers, but taking two blue-collar workers with one about to stop working because of health issues, the former is as likely to lose his or her job because of plant closure as the latter. In the second chapter, we identify the impact of moving to Alsace Moselle, a region where the compulsory public insurance is more generous. We take individuals moving from and to other French regions as a control group, because the treatment group is more likely to resemble other movers than to resemble the general population. Finally, in the third chapter, we estimate the impact of joining a regional purchasing group when it is created. The control group is made of hospitals that have joined a regional purchasing group the year of its creation, before 2008, the first year of the database, or that are in a region where there are no purchasing groups. We exclude hospitals which are not comparable: university hospitals and private hospitals.

Once the control group is chosen, a crucial thing is to give a detailed description of the control and treatment group, to check whether they have different characteristics, and whether the trends of outcomes vary before treatment are similar across groups.

In the setting used in this thesis' chapters, three econometric methods could be used: difference-in-difference, matching or change-in-change - developed in Athey and Imbens (2006). As we use panel data, the change-in-change is similar to matching on pre-treatment outcomes. In both cases, an individual in the treatment group is matched with an individual in the control group having the same outcome before treatment. This matched individual from the control group is used to have the counterfactual value of the outcome for the treated individual in the absence of treatment.

The main assumption needed for matching - used in chapter one - is unconfounded-

ness: potential outcomes - i.e. outcomes that each group would have had if they had been respectively treated (Y_1) and untreated (Y_0) - do not depend on treatment status. In other words, the treatment group if they were not treated would have the same outcome as the control group, and the control group if they were treated would have the same outcome as the treatment group. With panel data, we can implement exact matching on pre-treatment outcomes, thus there is no need for difference-in-difference. Control and treated individuals who are matched have the same value for the outcome before treatment.

Following Lechner et al. (2011), in a setting with no covariates, the assumptions for difference-in-difference we need in chapter two and three are the following: SUTVA, no effect on the pretreatment population and common trend. SUTVA is the Stable Unit Treatment Value Assumption (also called “no-interference”), individual outcomes should not be affected by other individuals’ assignment, this rules out externalities of treatment on the control group (Rubin, 1978). No effect on the pre-treatment population rules out the case where treatment is anticipated. Common trend, or bias stability, is the central assumption of difference-in-difference methods: the control group and the treatment group should follow parallel trends.

These methods solely allow to identify the average treatment effect on the treated. The importance of this focus depends on the question asked and the pool of treated individuals. In chapters one and three, the treated sample is representative of the whole treated population. Indeed, they are selected mainly based on their treatment status. On the opposite, in paper two, individuals need to move from or to Alsace Moselle to be in the treatment group, and the outcome of interest is the reimbursement rate of health insurance, not the move itself. Movers’ characteristics are different from the general population that may be affected by changes in the public reimbursement rates. A close look at the heterogeneity

of the effects and the difference between the treated and the general population allows to assess the external validity of the results.

Summary

We present a brief summary of each chapter and an overview of the results.

Chapter 1: Long-term Impact of Job Displacement on Job Quality and Satisfaction: Evidence from Germany

This chapter is a joint work with Lexane Weber-Baghdiguian. We investigate the long-term effects of job displacement on several dimensions of job quality and satisfaction. Our job quality indicators are monetary outcomes - earnings, hourly wages -, self-reported labour market security and the quality of the working environment - as measured by the probability to work full-time and to work long hours, distance to work, the gap between desired and actual hours worked and job satisfaction. Using the German Socio-Economic Panel data from 1984 to 2012, we build a database containing 2,396 individuals who have lost their job because of plant closure. Our control group is created by finding workers who have similar characteristics to the displaced workers three years before their displacement. The matching method that we use is a coarsened exact matching on pre-treatment covariates and pre-treatment outcomes. We use individuals having the same outcome and characteristics before treatment to estimate the counterfactual evolution of the outcome in the treatment group. We compare displaced workers' outcomes to their non displaced counterparts from one to ten years after treatment, to see how the effects vary across time and whether and when recovery occurs. Our main findings indicate a large and long-lasting impact of displacement on employment probability. When displaced workers

are reemployed, their monthly earnings and hourly wages are lower than wages in the control group until four years after displacement. As for job security, displaced workers report to be more likely to lose their job again. Indicators of quality of the working environment (working full time, working more than forty-eight hours a week, distance to work), are all affected by displacement in the long run except the probability of working full time. These results are robust even when we focus on individuals who do not experience a significant period out of employment.

Chapter 2: Does Moving to a System with a More Generous Public Health Insurance Increase Medical Care Consumption?

This chapter is a joint work with Laurent Davezies, the corresponding article has been published in December 2015 in *Annals of Economics and Statistics*. Using a natural experiment, we evaluate the impact of reimbursement rates on health expenditures. For historical reasons, the reimbursement rates of public health insurance are higher in the *Alsace Moselle* region of France than in other French regions. For both the regional and national systems, affiliation is compulsory. Individuals moving between *Alsace Moselle* and the rest of France undergo an exogenous change in reimbursement rates. We use a difference-in-difference method on a panel dataset of individuals. Our treatment group consists of individuals changing system. Our control group consists of individuals who move between other French regions. We study the impact of reimbursement rates on a broad range of health care expenditures: for the visits to the dentist and the doctor, consumption of medicines, and absenteeism due to sickness. We find heterogeneous impacts of reimbursement rates on those items. Overall, higher public reimbursement rates do not lead to an increase in spending for medical care.

Chapter 3: Regional Purchasing Groups and Hospital Medicine Prices: Evidence from Group Creations

This chapter estimates the impact of group purchasing on medicine prices in French hospitals. This is the first paper studying the impact of hospital purchasing groups using actual purchasing prices and group creations. Innovative medicines markets are characterized by low competition, and are either monopolies or oligopolies. Purchasing groups have been created to increase hospitals' bargaining power when negotiating medicines prices. We take advantage of the creation of regional purchasing groups between 2009 and 2014 to measure the impact of belonging to a group on medicine prices. We use a unique database providing average annual prices paid by public hospitals for all innovative and costly medicines. Using a fixed effects model controlling for medicine-specific bargaining abilities of hospitals and medicine-specific price trends, we find that group purchasing reduces prices of medicines in oligopoly markets, but has no impact on prices of medicines which have no competitors.

Outline

This thesis is organized as follows. Chapter one measure the effects of plant closure on multiple dimensions of job quality. Chapter two studies the impact of increasing public reimbursement rates on health expenditures. Chapter three shows how regional purchasing group creations impact medicine prices. The final chapter concludes.

Chapter 2

Long-term Impact of Job

Displacement on Job Quality and

Satisfaction: Evidence from Germany

This Chapter is a joint work with Lexane Weber

Abstract

In this research, we investigate the long-term effects of job displacement on several dimensions of job quality and satisfaction, focusing only on individuals who lose their job because of plant closure. Using the German Socio-Economic Panel data from 1984 to 2012, we build a database containing 2,396 individuals who have lost their job because of plant closure. Our control group is created by finding workers who have similar characteristics to the displaced workers before their displacement. The matching method that we use is a coarsened exact matching on pre-treatment covariates and pre-treatment outcomes. We then compare the evolution of job quality in both groups to measure the causal impact of displacement. Our main findings indicate a large and long-lasting impact of displacement on employment probability. The effects of displacement on earnings and hourly wages last until three or four years. As for job security, displaced workers report to be more likely to lose their job again. Indicators of quality of the working environment (working full time, long hours, distance to work), are all affected by displacement in the long run except the probability of working full time. These results are robust when we focus on individuals who do not experience a significant period out of employment.

2.1 Introduction

Job loss is a major shock on an individual's career and it often has devastating consequences on workers' life trajectories. Research has shown that it entails subsequent unemployment, long-term earnings losses, and declines in psychological and physical wellbeing. Moreover, as underlined by Brand (2015), "while reemployment mitigates some of the negative effects of job loss, it does not eliminate them". In this paper, we investigate the effects of displacement due to plant closure that carry over to new jobs, when individuals are reemployed. More specifically, we focus on the effects of job loss on the quality of the new jobs, as measured by earnings, labour market security and the quality of the working environment, from one to ten years after displacement.

Since the 90's, the costs of displacement have been studied in terms of earnings, hourly wages and hours worked in the United States (Jacobson et al. (1993); Fallick (1996); Couch and Placzek (2010)) and Europe (Eliason and Storrie (2009); Hijzen et al. (2010)). More recently, researchers have focused on the impact on health and well-being. They found that losing one's job increases not only the risk of mortality in the United States (Sullivan and von Wachter, 2009), Sweden (Eliason, 2014) and Denmark (Browning and Heinesen, 2012) but also unhealthy behaviours (Marcus, 2014) and mental health problems (Marcus, 2013). As for satisfaction, Clark et al. (2008) show that a shock in a career path, such as an unemployment spell, deteriorates subjective well-being - as measured by life satisfaction.

Another strand of the recent literature focuses on labour market outcomes and more precisely earnings, hourly wages, unemployment or self-employment status. These papers try to assess whether the impact of displacement is a long run one or whether recovery occurs shortly after displacement. They found an increase in the probability of being retired (Tatsiramos, 2010) or self-employed following job loss (Von Greiff, 2009). Among displaced workers, those who find a new job often experience a large and long-lasting drop

of earnings until six years after displacement in the United States (Ruhm (1991); Jacobson et al. (1993)). Since the 2000's, similar papers studied the impact of job loss on earnings in Europe but results are more heterogeneous.

Wage losses are much smaller in European countries than in the United-States as demonstrated for the first time by Kuhn (2002)¹. Eliason and Storrie (2006) find a large and long-run impact of job displacement on earnings in Sweden - until twelve years after displacement. Schwerdt et al. (2010) also show long-run effects in Austria, especially for white-collars. In Germany the impact seems to be smaller: displacement induces a loss of earnings until only two years after displacement (Couch, 2001). The duration of income losses depends on how long it takes to find a new job after displacement, on the definition of displaced workers, and on whether downsizing due to plant closure or layoffs are considered (Hijzen et al., 2010). Couch and Placzek (2010) revisit the impact in the United-States using matching estimators, and find lower impacts than previous studies (for instance Fallick (1996), Jacobson et al. (1993)). The impact also varies when using alternative definitions of displaced workers (Hijzen et al., 2010).

All these papers focus on earnings and/or hourly wages as the unique dimension of job quality.

However, job quality is a multi-dimensional concept and all dimensions contribute to the well-being of workers. Beyond wages and unemployment, other indicators must be taken into account to properly capture the overall impact of job displacement.

The European Foundation for the Improvement of Living and Working Conditions selects four indicators of job quality which are of greatest importance to workers: earnings, career prospects, working-time quality and the quality of the working environment (Eurofound (2015); Eurofound (2012)). The OECD (2014) develops a framework including three key

¹Bender et al. (2002), Borland et al. (2002) and Albæk et al. (2002) provide respectively evidence for France, Germany, United Kingdom, Australia, Belgium and Denmark.

dimensions of job quality which partly overlap with the previous ones: earnings quality, labour market security and quality of the working environment.

In this paper, we estimate the impact of displacement on monetary outcomes - earnings, hourly wages -, self-reported labour market security and the quality of the working environment as measured by the probability to work full-time and to work long hours, distance to work, the gap between desired and actual hours worked, job satisfaction and life satisfaction. The data we use come from the German Socio-Economic Panel (SOEP) which contains a large array of subjective and objective variables measuring job quality. Given the potential endogeneity of displacement, we consider only displacement due to plant closure, which is not directly linked with unobserved individual characteristics. We implement a matching method to identify non-displaced workers who have similar characteristics as displaced workers three years before displacement and then analyse the effects of job loss until ten years after displacement. Our results suggest that job loss induces a deterioration of several aspects of job quality. We find a large and long-lasting impact of displacement on the probability to be employed. For displaced workers who find a new job, we find a deterioration of job quality. Our results on monetary outcomes (earnings and hourly wages) are consistent with those found by Kuhn (2002) and Couch (2001): the impact of displacement lasts for three or four years. As regards job security, displaced workers report to be more likely to lose their job again. Indicators of quality of the working environment (long hours, distance to work), are all affected by displacement except the probability of working full time. In the long run the most affected dimensions are the probability to work more than forty eight hours a week and distance to work, which both increase.

To our knowledge this article is the first measuring the impact of job loss due to plant

closure on all dimensions ² of job quality. The only other articles studying links between job loss and job quality we are aware of are Lehmann et al. (2013) and Seim et al. (2012). They both estimate the impact of job loss - due to plant closure and lay off - on hourly wages, earnings and hours worked in Russia and Sweden. Not only do they find a large and persistent earning losses but also a decrease in hours worked.

Our contribution to the existing literature is twofold. First, we restrict the definition of displaced workers to plant closure - excluding individuals who are laid off - and we implement a matching method to identify a relevant control group in order to measure the causal impact of job loss. Additionally, we are able to follow individuals over a long time-period, and thus to estimate long-term effects. Second, our article contributes to the literature focusing on job quality. In this literature, indicators are more numerous than ours - see for example Osterman (2013), Green et al. (2013), Rubery and Grimshaw (2001), Crespo et al. (2013). Green et al. (2013) use four indices of non-wage job quality (work quality, working time, work intensity and physical environment) from the Fifth European Working Conditions Survey. With the same database, Cottini and Lucifora (2013) measure job quality with working conditions - such as shifts, job intensity, repetitive work and physical hazards. Contrary to these studies, we do not have a dataset specifically focusing on job quality. Therefore, as suggested by the OECD (2014) and Cazes et al. (2015), we approximate each dimension of the latter by earnings, hourly wages, self-reported labour market security, the probability to work full-time and to work long hours, distance to work, the gap between desired and actual hours worked, job satisfaction and life satisfaction.

The paper is organised as follows. Section 2 presents the empirical strategy. Section 3 provides a detailed description of the data. Section 4 begins with a presentation of the estimated effects of job displacement on the main dimensions measuring job quality. Then,

²Dimensions defined by OECD (2014), Cazes et al. (2015) and the European Foundation for the Improvement of Living and Working Conditions.

the results of various robustness checks and a presentation of heterogeneous impact follow. Finally, section 5 provides some concluding remarks.

2.2 Empirical strategy

We aim at estimating the long-term impact of job displacement on job quality indicators and individuals' satisfaction. Our strategy is based on Coarsened Exact Matching (CEM) with treated and control individuals being matched not only on their pre-treatment covariates but also on their pre-treatment outcomes. Thus doing, we control for the impact of time-invariant unobservables on outcomes - see Imbens and Wooldridge (2008), Lechner (2011).

Matching is a valid method as long as potential outcomes - i.e. outcomes that each group would have had if they had been respectively treated (Y_1) and untreated (Y_0) - do not depend on treatment status given covariates and pre-treatment outcomes. This assumption is called ignorability of treatment, or unconfoundedness, or conditional independence assumption (CIA). The richer the information available for matching, the more credible this assumption (Chabé-Ferret (2015)). The *ignorability of treatment* assumption is violated if individuals choose to be treated based on their return to treatment. In our framework, individuals in the treated group do not choose to lose their job, they lose it due to an exogenous shock: plant closure. In such a case, it is unlikely that the selection into treatment be based on expected gains. The existing literature usually focuses on workers who lose their job because of dismissal - whatever the reason - (Marcus (2014), Clark et al. (2008)), while few papers only use administrative data (Browning et al. (2006), Eliason and Storrie (2009)) and restrict the treatment to job loss due to plant closure. Dismissals whatever the reason are likely to be correlated with worker's characteristics since, even if these dismissals are due to downsizing, employers are likely to choose which employee to

dismiss. Here, we focus on workers losing their job because of plant closure, so there is no such selection. We do not control for post-treatment characteristics such as marital status, income or the number of children because job loss could affect post-treatment characteristics, so that the ignorability-of-treatment condition would be violated - i.e. potential outcomes given such variables would be correlated with treatment status.

To estimate the impact of job loss on job quality and satisfaction outcomes over a time span of ten years after job loss, we first define which individuals belong to the treatment and control groups, and then match subjects from the treatment group to subjects from the control group. Figure 2.A.1, in Appendix Section 2.A presents how groups are constructed. Treatment -i.e. job loss - can occur at different periods so that we have time varying treatment and control groups. Hereafter, we will call the treated group either “displaced workers” or “treated individuals”. We start with a sample of 2,121 individuals who lose their job at least once, in a panel of 27,267 individuals. We define treatment at a given year k as having lost one’s job due to plant closure at year k . Similarly, subjects from the control group are individuals with similar characteristics - see below - but who did not lose their job at year k .

The closing process takes time. German employers are required to inform works councils³ and to provide at least a 30-day notice before the final shutdown (Couch, 2001). For this reason, matching on workers’ characteristics just before treatment is likely to yield biased estimates, as it would violate the ignorability-of-treatment condition. For instance, a worker who is aware that he is about to be displaced could have a lower job satisfaction before displacement, meaning that pre-treatment characteristics (here job satisfaction) would be impacted by treatment status. When we match on five or six years before displacement we have suggestive evidence that the effects of plant closure are anticipated up

³Works councils were established by the Works Constitution Act of 1952, and additional powers were delegated to them by the Works Constitution Act of 1972. In general, works councils have powers regarding local personnel issues such as the scheduling of work, the hiring of new workers, and conditions for dismissal” (Couch, 2001)

to two years before displacement. Therefore, in order to match treated individuals with subjects from the control group, we use pre-treatment covariates and outcomes measured three years before the treatment year (k). In our setting, the ignorability-of-treatment is therefore:

$$\mathbb{E}(Y_{i,k+m}|X_{k-3}, Y_{k-3}, D = 0) = \mathbb{E}(Y_{i,k+m}|X_{k-3}, Y_{k-3}, D = 1)$$

for $i = 0, 1$, $m = 1, \dots, 10$, and $k = 1984, \dots, 2012$

where $(Y_{0,k+m}, Y_{1,k+m})$ are vectors of potential outcomes of treated ($Y_{1,k+m}$) or untreated ($Y_{0,k+m}$) individuals, m years after a displacement which occurred at year k for the treated group.

We then estimate differences in job quality and job satisfaction across both groups, by comparing the average outcomes in the treatment and control groups one to ten years after treatment.

To do so, we use coarsened exact matching (CEM) rather than exact matching or propensity score matching. While Exact Matching provides perfect balance - a treated unit is matched to control units with the same covariate values - it produces too few matches due to the richness of covariates in our data, and even no match when some variables are continuous. Given this problem, we implement a coarsened exact matching (CEM) method, which performs exact matching on coarsened variables. It is a Monotonic Imbalance Bounding matching method. The maximum imbalance between treated and control groups is chosen ex ante - see Iacus et al. (2009) and Iacus et al. (2011) -, in contrast to propensity score matching.

Then, the number of matched individuals has to be checked. All forms of imbalance - nonlinearities or interactions - are controlled for because we only match individuals if they have exactly the same characteristics for all coarsened variables. The critical point

of CEM is the coarsening level for each variable: categories have to be small enough to make sense, and large enough to have enough matched individuals. Given the discrepancy between the number of individuals that could belong to the control group (26,802) and the treatment group (at most 1,543 persons), we choose which variables to include and how to coarsen them checking which proportion of the treated individuals are matched. Treated and control individuals are eventually matched on gender, age (five classes), diploma (three classes), region (West Germany - East Germany), and industry (nine classes) - See Appendix 2.C. These variables are measured three years before job loss due to plant closure. For each regression, we also match individuals on the value of the outcome three years before displacement. This enables us to take into account the impact of any time-invariant unobservable variable. Among outcomes, continuous variables are coarsened: wage is coarsened in twenty categories, and distance to work in ten categories. Initially, we have 1,543 individuals in the treatment group (1,692 job-loss spells), and 26,802 individuals in the control group. Restricting our sample to individuals having non-missing values three years before treatment for each variable used for the matching, we end up with 959 individuals in the treatment group (corresponding to 1,041 job-loss spells) and 18,534 individuals in the control group. Overall, 474 to 1,026 job loss spells have been matched, depending on the outcome considered, i.e. they are in a stratum where there is at least one control and one treated individual. Then, we measure the difference in mean outcomes between the two groups from four years before treatment to ten years afterwards, with a separate regression for each time-distance to job loss m . We use the comparison of outcomes four years before treatment to assess the validity of our method. Matched control and treated individuals are similar in term of outcomes three years before displacement, because they are matched based on the outcome in year $k - 3$. Comparing outcomes in year $k - 4$ provides evidence that both groups are similar before being matched.

We use weights (μ) that equalize the number of treated and control individuals in each

stratum. The average impact of treatment on outcome Y is then given by:

$$\beta_m = \frac{1}{n_1 + n_2} \left(\sum_{i=1}^{n_1} \mu_i \mathbb{E}(Y_{i,k+m} | D = 1) - \sum_{j=1}^{n_2} \mu_j \mathbb{E}(Y_{i,k+m} | D = 0) \right)$$

for $m = -4, \dots, 10$,

where n_1 is the number of treated individuals, n_2 the number of control individuals, D is an indicator of treatment status, k is the year of job displacement, and m is the time-distance to job displacement.

2.3 Data and descriptive statistics

2.3.1 Data

The German Socio-Economic Panel

The data used in this paper are drawn from the German Socio-Economic Panel (SOEP). We use waves from 1984 to 2012 including about 46,600 individuals from German private households. The SOEP questionnaires cover several issues such as health, education and changes in the composition of the household.

Our analysis focuses on the long-term impact of job loss due to plant closure on labour market outcomes, job quality and self-reported satisfaction. Only individuals aged between eighteen and sixty years old are included in our study. Our final sample consists of 37,408 individuals.

Outcomes and control variables

Because we want to identify the long-term consequences of job loss on labour market

outcomes, job quality indicators and individuals' satisfaction, we use plant closure as an exogenous source of job loss.

The job loss variable is constructed using the following question asked yearly from 1990 to 1997 and from 2000 to 2011 to individuals who report a change in their labour market situation : "How was this job terminated?". Respondents may choose one of the following answers: dismissal, mutual agreement, place of work or office has closed, resignation, temporary job or apprenticeship, retirement, sabbatical or, "if you are self-employed, your business closed down". Individuals are considered to have lost their job due to plant closure when they select "place of work or office has closed". These displaced workers do not necessarily experience an unemployment spell.

Taking advantage of the abundance of information in the GSOEP survey, we use job quality indicators capturing each of the dimensions put forward by the OECD - see above -, i.e. earnings quality, labour market security and quality of the working environment. As recommended by the literature on job quality - see OECD (2014) and Green et al. (2013), for example - we consider as objective indicators of job quality the probability of being employed, distance to work, earnings, hourly wages, the probability of having a full-time job, the probability of working more than forty-eight hours a week, and as subjective indicators the fear of losing one's job (job insecurity) and the gap between hours worked and desired hours.

Even if these job quality indicators capture several dimensions of the well-being of workers, we also use self-reported satisfaction indicators to get a more comprehensive picture of how job displacement affects well-being. We consider two satisfaction variables: job satisfaction and life satisfaction, rated on the same 1 to 10 scale where 0 means completely dissatisfied with one's job (resp. life) and 10 means completely satisfied. This question is asked every year in the SOEP data.

Our specifications also include individual characteristics - age and education⁴ - and industry dummies. We use the NACE 1-digit standard industrial classification. We also include region and year dummies as control variables.

2.3.2 Descriptive statistics

Descriptive statistics are provided in Appendix Tables D1, D2, D3, D4 and D5.

In our sample, individuals may lose their job between 1990 and 2012. Over that period, 2,396 individuals have lost their job because of plant closure. In 1991, the year with the largest number of job losses, 259 individuals lost their job while only 3 individuals did so in 2012 - see Appendix Table D1. Among those who lose their job at least once (2,151 individuals), the vast majority (1,855 persons) have lost their job only once but 233 have lost their job twice, 21 three times and 2 four times - see Appendix Table D2. Workers who are displaced several times are considered as treated each time. We also restrict our sample to workers who are displaced only once as a robustness check.

Appendix Tables D3, D4 and D5 describe individual characteristics of displaced workers for all years before treatment whereas for the untreated they are computed over the whole period. On average, there are fewer women in the treatment group than in the rest of the population. In our sample, treated individuals are slightly older, live more often in East Germany - see Appendix Table D3 - and they are less likely to have a higher education diploma than the non-treated individuals. Treated individuals are also slightly more often employed in manufacturing industries, trade and construction sectors and less so in services and banks and insurance - see Appendix Table D4.

When considering job quality indicators and individuals' satisfaction, the treated group

⁴Age and education are coarsened into 3 classes - See Appendix 2.C

appears to report lower job and life satisfaction, lower job security and longer hours worked as well as poorer overall job quality before displacement, compared to the control group - see Table D5. Treated individuals are also more likely to have a full-time job but they work more often more than forty eight hours a week, they live further away from their job, earn less money and are less often in employment.

2.4 Results

2.4.1 Main results

We interpret the coefficients on years $k - 2$ and $k - 1$ (two years before to one year before treatment) as the impact of anticipating a displacement. We have matched our treated and control individuals based on their outcomes three years before displacement. We first investigate the impact of job loss due to plant closure on earnings, job security, quality-of-working environment ⁵ and satisfaction indicators. The main results presented in Figures 2.B.1 to 2.B.10 suggest that job loss induces a deterioration of several aspects of job quality and satisfaction. Each point of those graphs represents an estimate of the effect of displacement ($\beta_{Y,m}$) for a given outcome Y , and a given time-distance to job loss m , presented with 95% confidence intervals. The formula for $\beta_{Y,m}$ is provided in Section 2.2. In order to assess the validity of our method, we check whether the coefficients on the outcomes four years before displacement (Y_{k-4}) are different from zero. This can be seen as a placebo test: year $k - 4$ is long before displacement, so there should be no difference between the treatment and control groups at that time. For our ten outcomes, there is no significant effect of being displaced four years before displacement - see Figures 2.B.1 to 2.B.10. Overall, treated and control individuals are similar four years before job loss, and our matching method is therefore reliable.

⁵We use the nearest categories of job quality developed by OECD (2014)

Individuals who will lose their job at year k anticipate their displacement and are more concerned about their job security two to one years before job loss - see Figure 2.B.4. When considering the results in Figure 2.B.9 and Figure 2.B.10, it is interesting to notice that job and life satisfaction are also affected by anticipations and tend to decrease before displacement. In order to tackle this issue of anticipations, we do not match individuals on years $k - 2$ and $k - 1$ but rather on year $k - 3$. Furthermore, we cannot interpret the coefficient on the year of displacement, because some treated individuals are already displaced and some are not at the time of the survey.

During the whole period, the impact of displacement on the probability of being employed is negative and significant - see Figure 2.B.1. One year after displacement, 30% of displaced workers are not employed (they are either unemployed or inactive) and they are still 5% less likely to have job ten years after displacement. Displaced individuals who have found a new job after displacement earn less than others, but this effect is only significant three and four years after displacement. It represents at most a 10% decrease in monthly earnings - see Figure 2.B.2. Concerning hourly wages, the impact is significant until six years after displacement and represents a 10% decrease as well - see Figure 2.B.3. These results are close to those found by Eliason and Storrie (2006) although we find a much larger impact on employment probability (30% to 10% as compared to 7% to 3%), and less long-lasting effects on earnings (until 4 years instead of 12 years).

As regards job security, workers who have been displaced report to be more likely to lose their job again. This effect is significant until four years after displacement - see Figure 2.B.4. Our indicators of quality of the working environment are all affected by displacement, except the probability of working full-time, for which the impact is small and not significant. Displaced workers are more likely to work long hours (more than forty-eight) and to work far away from home, until ten years after job displacement. Overall, displaced

workers with a new job are 30% more likely to work long hours, and to commute 8 to 12 kilometres further away - see Figures 2.B.6 and Figure 2.B.8. They are more dissatisfied with their hours worked than the control group until nine years after displacement. These results are consistent with a negative impact of job loss on job satisfaction - until seven years after displacement, only for those who have a job - and life satisfaction - until eight years after displacement, for all displaced workers (Figure 2.B.9 and Figure 2.B.10).

To summarize, displacement worsens all aspects of job quality - i.e. the probability of being employed, earnings, hourly wages, job security and the quality of the working environment. Even when they find a new job, displaced workers have lower-quality jobs than non-displaced workers. In the long run, the most affected dimensions are hours worked and distance to work.

Robustness check

Our main results show that there are long-term impacts of displacement on several aspects of job quality. For some dimensions there is a recovery while for some other - long hours, dissatisfaction with hours worked and distance to work - there is no evidence of a decrease in the effect of displacement over time. Each point estimate is obtained considering individuals who are currently working that year. The further away from the year of displacement the more displaced workers find a new job and thus contribute to the estimates. Therefore, individuals who have been out of employment for a long period could explain the long-term effects.

In this paragraph, we estimate the effects of displacement on the subgroup of workers who were back to employment the year after displacement. Thus doing, we can see how the impact varies over time for a more stable pool of displaced workers and we have a clearer idea of the pattern of recovery in each dimension. On average, displaced workers in this subsample are younger, have lower tenure, are more often white collars and more

often men than displaced workers who did not find a job right after displacement - see Appendix 2.E.3, Table E1. They are as likely as non-displaced workers to be employed both in the short and long run following displacement.

Overall, our main results do not vary at all when we focus on individuals who do not experience a significant period out of employment, except for life satisfaction - see Figures E41 to Figures E50. Job displacement has no significant impact on life satisfaction for this new sample. This finding suggests that the drop in life satisfaction is driven by individuals who are out of employment more than one year after displacement. Concerning job quality, several dimensions are impacted by job loss even when individuals find a new job quickly after displacement. Therefore, displacement is not only about being out of employment, it is also a shock on the career path for displaced workers who find a job right after displacement.

2.4.2 Heterogeneity of effects

So far, we have presented average effects of job loss on the whole sample, but the impact of displacement is likely to vary, among others, across gender, age, tenure and occupation of the displaced workers. This is what we want to investigate in this section.

We estimate the same model separately for women and men and interpret the impact of job loss on each dimension of job quality and satisfaction indicator - see Appendix 2.E.1. For each category we consider, we use the same selection criteria for both treatment and control groups, i.e., male (resp. female) workers can only be matched with male (resp. female) workers.

When considering the sample of men - see Appendix 2.E.1 - we find that they are affected by a deterioration on some dimensions of job quality over a longer period of time - between six and ten years after displacement - than women even if the difference is not

always significant. Displaced men are less likely to have a job, have lower earnings and have a higher probability to work more than forty-eight hours a week than their female counterparts - see Appendix 2.E.1: Figures E1, E3, E4, E7. The impact of job loss due to plant closure on commuting time is particularly long-lasting for men - see Appendix 2.E.1: Figures E9. It increases until ten years and the difference between both genders remains significant from two to nine years after displacement. We also find a short-run negative effect of job loss on job security, job satisfaction and the satisfaction with hours worked by men - see Appendix 2.E.1: Figures E5, E10, E8.

The corresponding pattern of results is different for women except for the probability of being in employment. Fewer dimensions are impacted and the effects are less long-lasting. Displaced women do not experience a significant decrease in earnings, hourly wages or the probability to work full-time⁶ - see Appendix 2.E.1: Figures E3, E4, E6. The effects are short-term or mid-term for a couple of outcomes - job security, satisfaction with hours worked, probability to work long hours, job satisfaction and life satisfaction (see Appendix 2.E.1: Figures E5, E8, E7, E10, E2)⁷. Women seem to be more concerned about their job security until four years after displacement.

Finally, the impact of displacement on wages is more severe for men. Interestingly, this differentiated impact on wages and earnings is not due to the fact that women exit the labour force after displacement, because the effects on employment probability are similar.

Displacement is likely to impact differently older and younger workers, as they are impacted at different moments of their career. Figures E11 to E19, in Appendix 2.E.2, show how our results vary across displaced workers' age. We define as "young" those workers younger than 45 years old at the time of displacement. Other workers are consider

⁶The differences between the coefficients for men and women are almost never significant for hourly wage and earnings, and significant for half of the years for full-time

⁷The differences between the coefficients for men and women are significant for some years a few year only for most of the the outcomes, and most of the years for distance to work

to be “old”.

Most of our job quality indicators are impacted in a similar way for older and younger workers, but the magnitude of the coefficients is generally higher for older workers. First, older workers are more impacted than younger ones in terms of employment probability - see Appendix 2.E.2: Figure E11. One year after displacement, the difference between displaced and non-displaced workers is twice as high for older workers than for younger ones, and the difference of impact for younger and older workers is significant until six years after displacement. Concerning earnings, hourly wages and job security, the effects are very close - see Appendix 2.E.2: Figures E11 to E19. Older workers are more affected in some dimensions of the quality of the working environment, such as distance to work and job satisfaction, whereas the impact on life satisfaction is similar across ages. Senior workers also seem to be more impacted in term of hours worked - the probability to work long hours and the gap between actual and desired hours worked -, but the difference of impact with younger workers is often not significant.

Overall, the difference in the impact of job loss across age mainly concerns the probability of employment. Older workers are less likely to find a new job if displaced. Once they have found a new job, they suffer from a greater deterioration of job quality. However, the overall impact that we find is not entirely driven by older displaced workers, as the results also hold to a lesser extent, for younger workers.

In our sample, older displaced workers have on average fifteen years of tenure and seven years for younger workers. This could explain part of the difference between older and younger workers. Accumulating firm-specific human capital implies higher losses in case of displacement. If instead of age we distinguish workers according to their years of tenure before displacement, we find that more tenured workers (more than ten years of tenure) are more impacted in terms of employment probability - see Appendix 2.E.2: Figures E21 to E29. Once they have found a new job, the impact of job loss on job quality is very sim-

ilar whatever the length of tenure: the difference across tenure is almost never significant - except for the distance to work in the long run. Similar results are found when we use a threshold of three years of tenure instead of ten years.

In this paragraph, we consider potential heterogeneity across type of occupation. We divide our sample into white and blue collars in order to compare each type of displaced workers with its control counterpart. Displacement deteriorates job quality more for blue-collar workers than for white-collar workers, in almost all the dimensions we consider. The overview of the results indicates that a major shock on the career path such as job loss has a negative impact on all outcomes for blue-collar workers, except the probability to work full-time - see Appendix 2.E.3 Figures E31 to E39. Some dimensions of job quality are impacted only in a short-run - job security and earnings - whereas the effects on commuting distance and satisfaction with hours worked are still significant from four to ten years after displacement. Long-term effects - until nine years after displacement - can be observed for hourly wages and the probability of working long hours. The difference with white collars is significant in the long-run (after six years), when there is full recovery for white collars, while blue collars are still impacted. Overall, we find that blue collars are more impacted than white collars for employment probability, job satisfaction and life satisfaction over the whole period - see Appendix 2.E.3 Figures E31, Figures E39, Figure E40 - with the difference in impact across both groups being significant. The effects of displacement on job quality and satisfaction are generally concentrated among blue collars. White collars are impacted in the same way, but the effects are smaller, and often non significant.

2.5 Conclusion

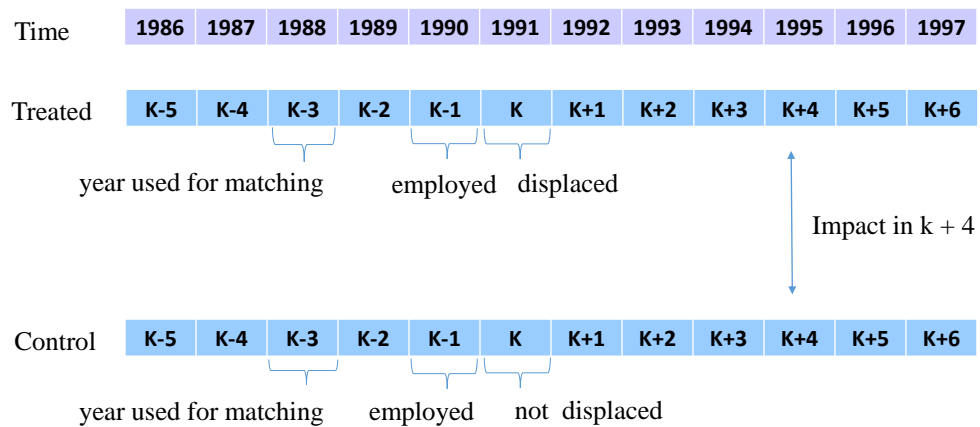
In this article, we provide evidence that job loss due to plant closure has a long lasting causal impact on a wide range of indicators of job quality. As for monetary factors, our results are close to the literature on Europe (Eliason and Storrie (2009); Hijzen et al. (2010)): we find long-lasting effects of displacement on the probability to be employed, on hourly wages ⁸ and only a middle-run impact on earnings contrary to the United-States (Jacobson et al. (1993); Fallick (1996); Couch and Placzek (2010)) where there is a lower impact on employment and a higher impact on earnings. Beyond earnings or hourly wages, job loss due to plant closure also induces a deterioration of several dimensions of job quality and satisfaction and not only earnings or hourly wages. The most affected indicators of quality of the working environment are the probability to work more than forty eight hours a week and distance to work which both increase after displacement. Even workers who find a new job within one year after displacement experience a worsening of job quality. So, we taking job quality into account the cost of displacement turns out to be much higher than whe considering only monetary aspects.

⁸The impact of displacement on hourly wages represents a 10% decrease on the whole period until six years after displacement

Appendix

2.A Empirical strategy

Figure 2.A.1: Treatment and control groups



2.B Main results

Figure 2.B.1: Impact of job displacement on employment

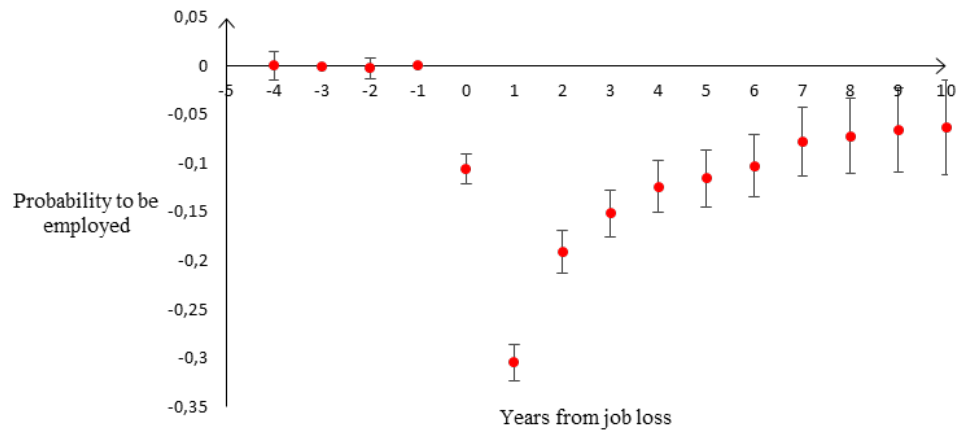


Figure 2.B.2: Impact of job displacement on earnings

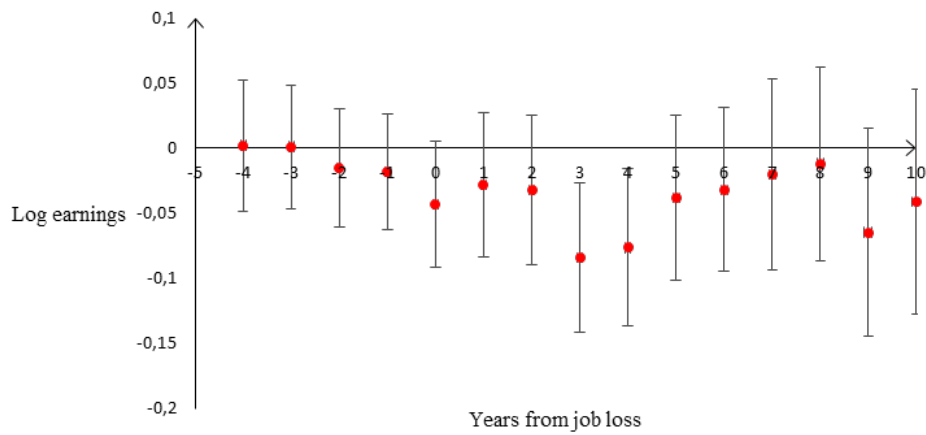


Figure 2.B.3: Impact of job displacement on hourly wage

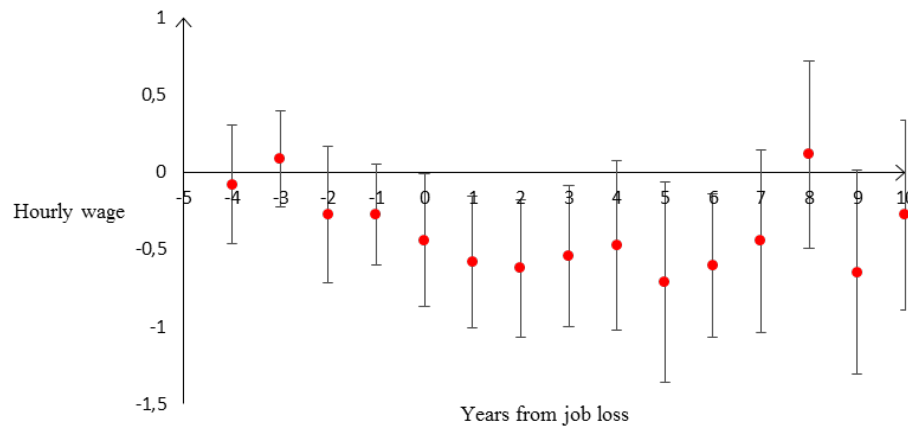


Figure 2.B.4: Impact of job displacement on job security

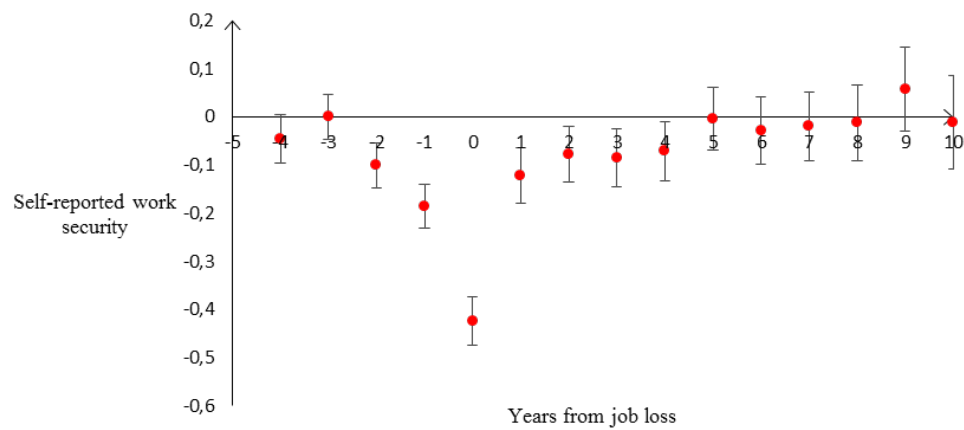


Figure 2.B.5: Impact of job displacement on probability to work full-time

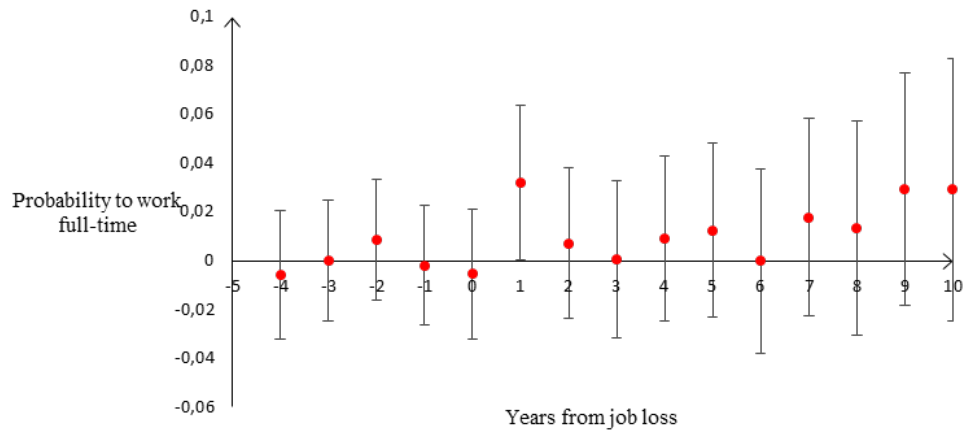


Figure 2.B.6: Impact of job displacement on probability to work more than 48 hours a week

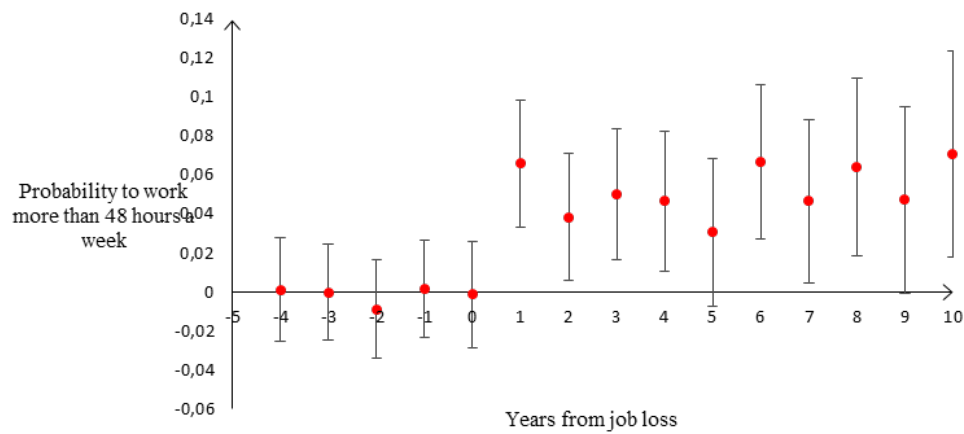


Figure 2.B.7: Impact of job displacement on the gap between actual and desired hours worked

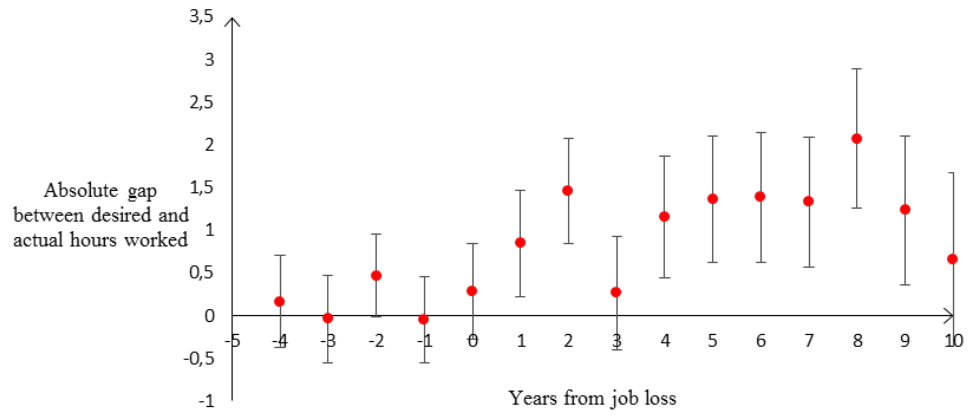


Figure 2.B.8: Impact of job displacement on distance to work

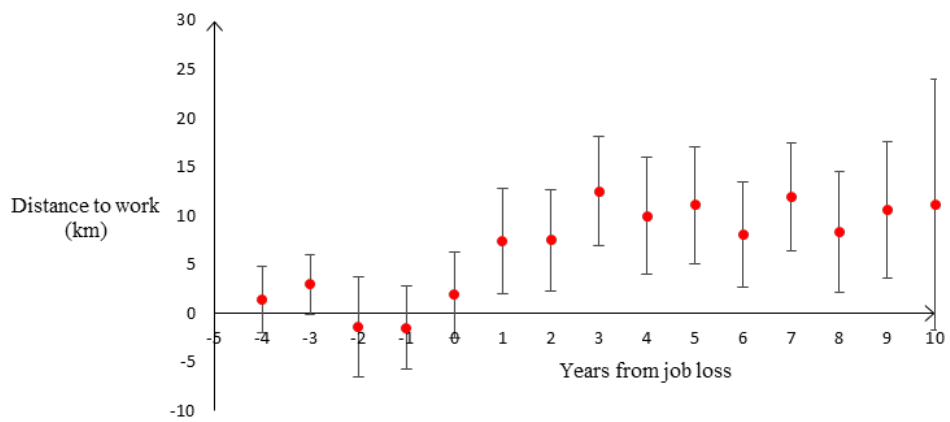


Figure 2.B.9: Impact of job displacement on job satisfaction

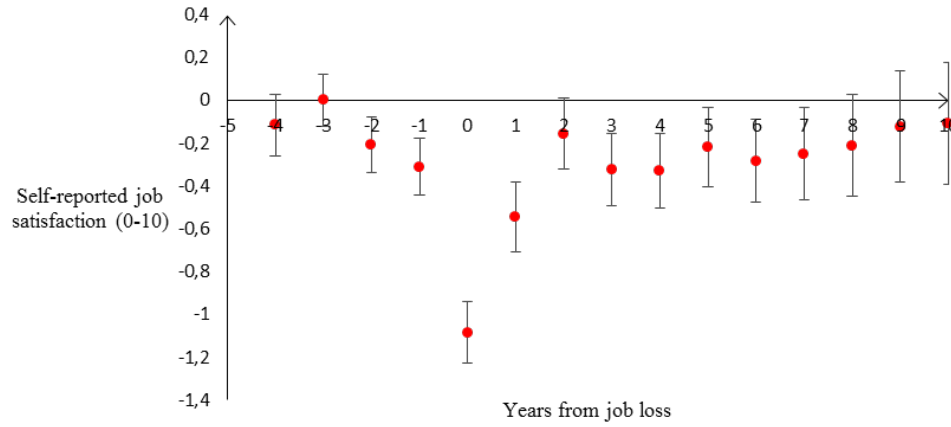
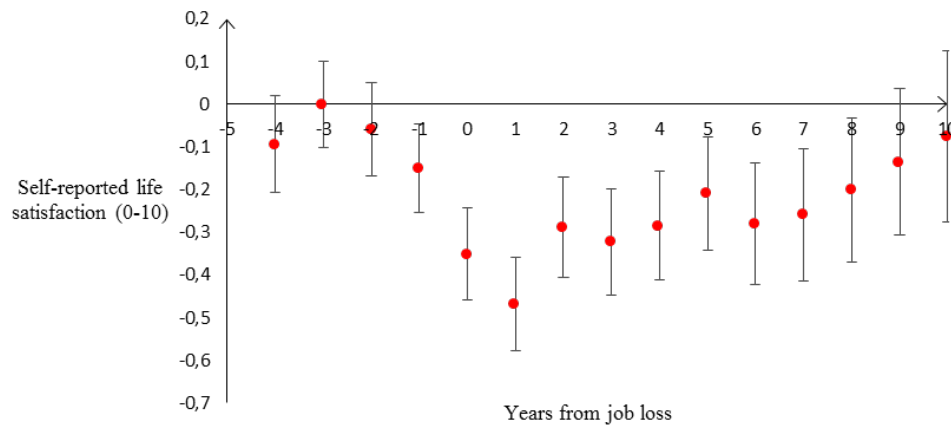


Figure 2.B.10: Impact of job displacement on life satisfaction



2.C Coarsened variables

Age is coarsened into five equal categories:

- (1) 18-32 years old
- (2) 33-39 years old
- (3) 40-45 years old
- (4) 46-51 years old
- (5) 51-65 years old.

Education (diploma) is coarsened into three classes:

- (1) less than high school
- (2) high school
- (3) more than high school.

Industry is coarsened into nine categories:

- (1) Agriculture
- (2) Energy
- (3) Mining
- (4) Manufacturing
- (5) Construction
- (6) Trade
- (7) Transport
- (8) Bank, Insurance
- (9) Services.

2.D Descriptive statistics

Table D1: Number of individuals losing their job because of plant closure per year

Year	Displacements
1990	167
1991	259
1992	165
1993	130
1994	127
1995	123
1996	121
1997	92
1998	12
2000	112
2001	150
2002	152
2003	147
2004	114
2005	73
2006	92
2007	79
2008	69
2009	87
2010	67
2011	55
2012	3
All years	2,396

Table D2: Number of job loss due to plant closure per individual

Number of job displacements	Freq
At least 1	2,151
1	1,895
2	233
3	21
4	2

Table D3: Descriptive statistics: individual characteristics

	(1)		(2)		(3)
	Treated Mean	SD	Non-Treated Mean	SD	Difference P-value
Women	0.46	0.50	0.49	0.50	0.02
<i>Location</i>					
East Germany	0.42	0.49	0.22	0.41	0.00
West Germany	0.58	0.49	0.78	0.41	0.00
<i>Age</i>					
16 to 30	0.18	0.30	0.27	0.39	0.00
31 to 38	0.21	0.23	0.19	0.28	0.04
39 to 45	0.23	0.23	0.19	0.29	0.00
46 to 54	0.20	0.22	0.17	0.28	0.00
55 to 69	0.19	0.30	0.17	0.32	0.01
<i>Education</i>					
7 to 11 years	0.39	0.45	0.39	0.46	0.98
11 to 12 years	0.39	0.45	0.34	0.45	0.00
13 to 18 years	0.22	0.40	0.27	0.43	0.00

Note: SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for treated individuals before displacement and non-treated individuals.

Table D4: Descriptive statistics: Industries

	(1)		(2)		(3)
	Treated Mean	SD	Non-Treated Mean	SD	Difference P-value
Agriculture	0.02	0.11	0.02	0.11	0.00
Energy	0.01	0.06	0.01	0.09	0.05
Mining	0.01	0.07	0.01	0.07	0.08
Manufacturing	0.24	0.33	0.22	0.37	0.02
Construction	0.18	0.31	0.15	0.31	0.00
Trade	0.21	0.33	0.16	0.32	0.00
Transport	0.05	0.17	0.05	0.20	0.46
Bank,Insurance	0.02	0.12	0.04	0.18	0.00
Services	0.26	0.35	0.35	0.44	0.00

Note: SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for treated individuals before displacement and non-treated individuals.

Table D5: Descriptive statistics: outcomes variables

	(1)		(2)		(3)
	Treated		Non-Treated		Difference
	Mean	SD	Mean	SD	P-value
Employed	0.74	0.25	0.81	0.26	0.00
Labor income	6.76	0.60	6.84	0.70	0.00
Full-time	0.77	0.35	0.74	0.39	0.00
Hourly wages	6.57	3.29	7.64	4.69	0.00
Long hours	0.21	0.28	0.16	0.29	0.00
Distance to work	22.13	47.65	19.50	41.76	0.01
Job security	2.03	0.51	2.29	0.55	0.00
Hours gap	7.16	5.24	6.41	5.60	0.00
Job satisfaction	6.57	1.46	7.03	1.56	0.00
Life satisfaction	6.59	1.27	7.09	1.29	0.00

Note: SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for treated individuals before displacement and non-treated individuals.

2.E Heterogeneous results

2.E.1 Job loss and gender differences

Figure E1: Impact of job displacement on employment

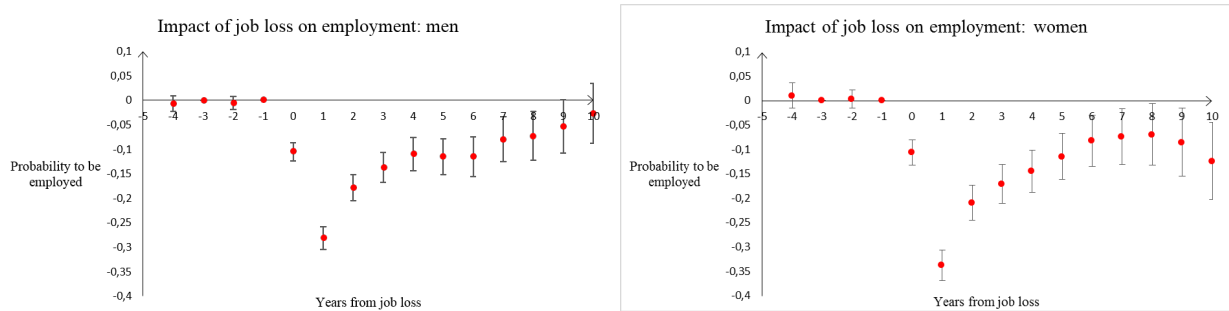


Figure E2: Impact of job displacement on life satisfaction

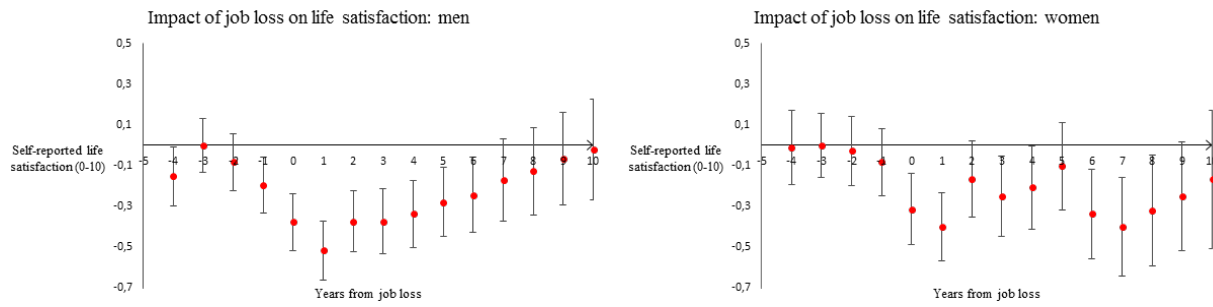


Figure E3: Impact of job displacement on earnings

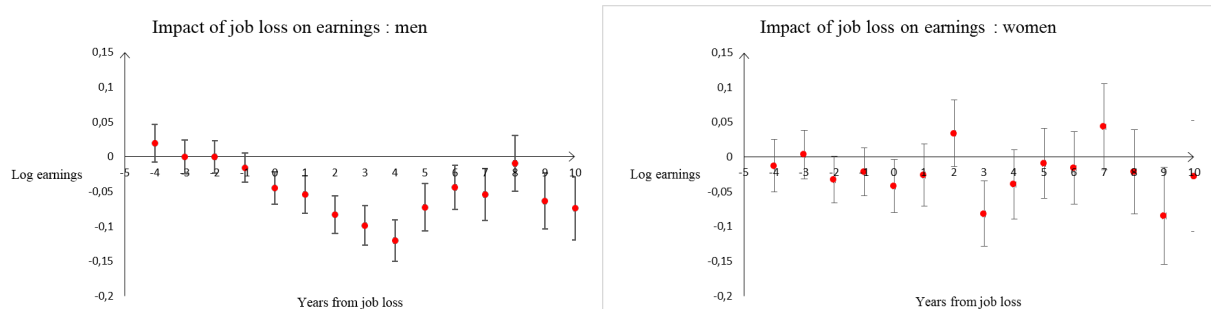


Figure E4: Impact of job displacement on hourly wage

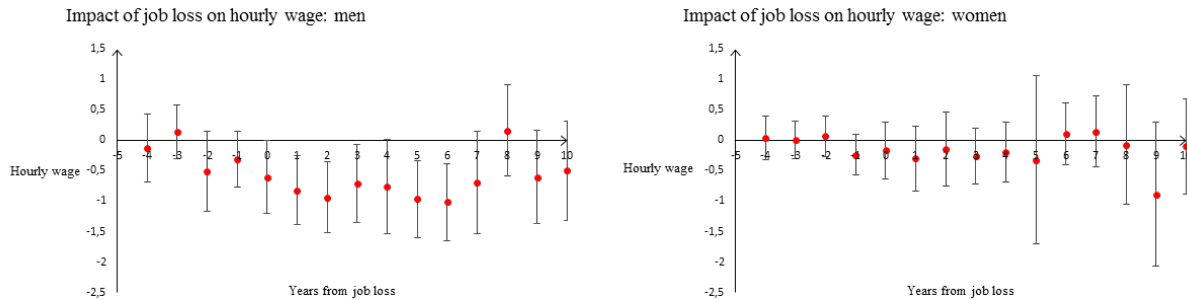


Figure E5: Impact of job displacement on job security

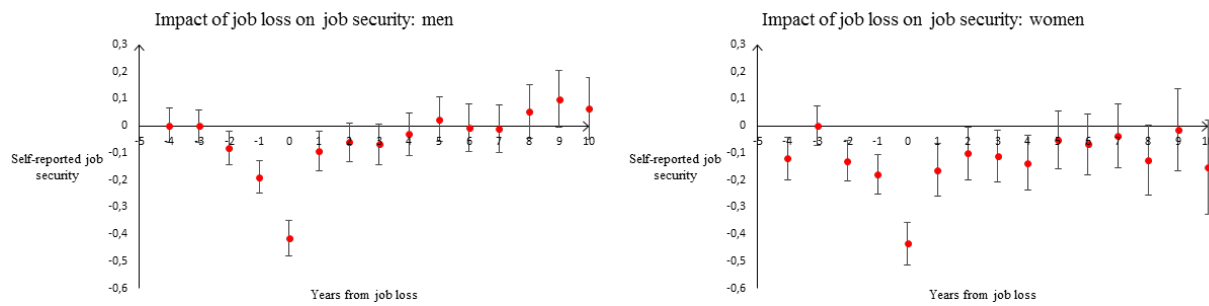


Figure E6: Impact of job displacement on probability to work full-time

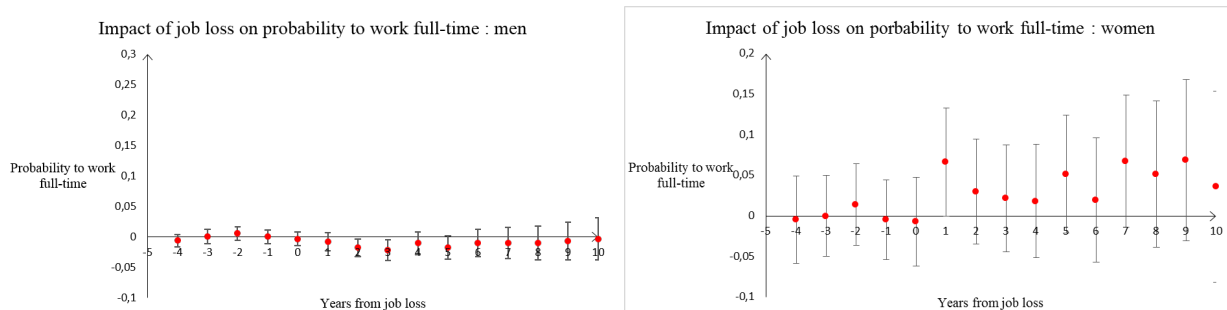


Figure E7: Impact of job displacement on probability to work more than 48 hours a week

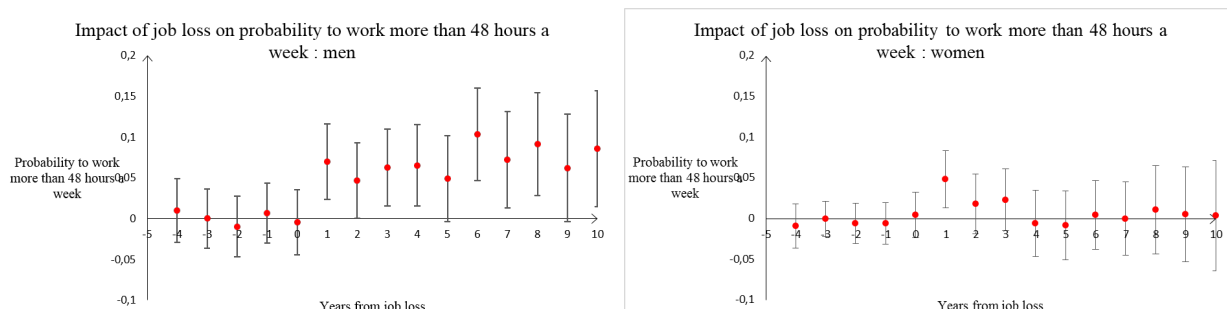


Figure E8: Impact of job displacement on the gap between actual and desired hours worked

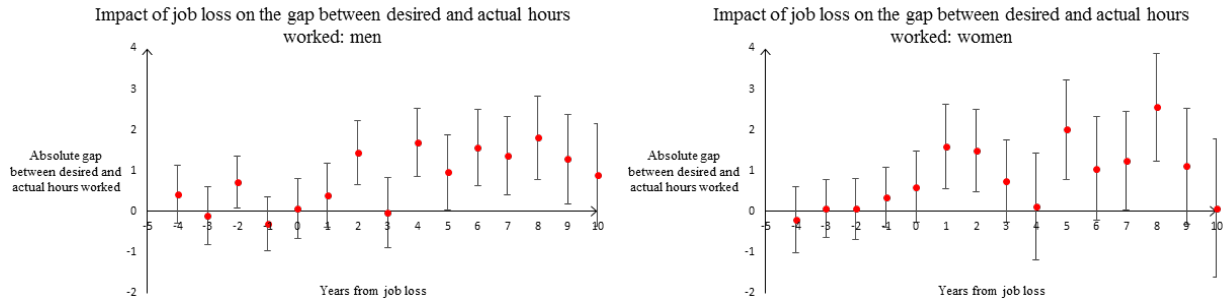


Figure E9: Impact of job displacement on distance to work

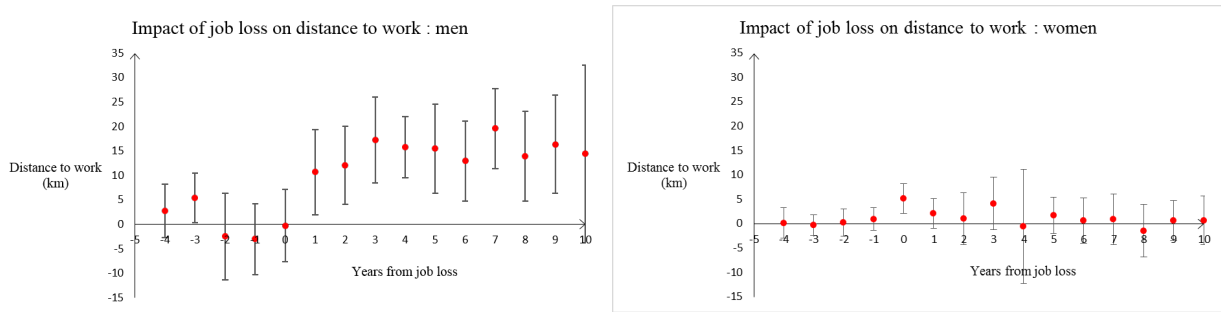
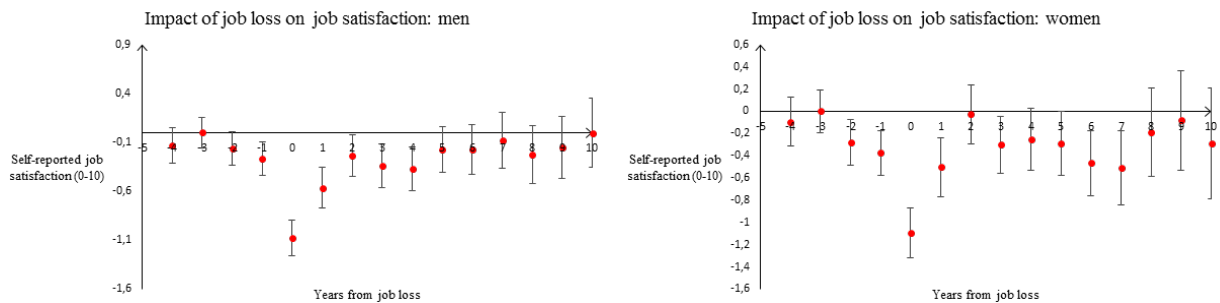


Figure E10: Impact of job displacement on job satisfaction



2.E.2 Age and tenure

Figure E11: Impact of job displacement on employment

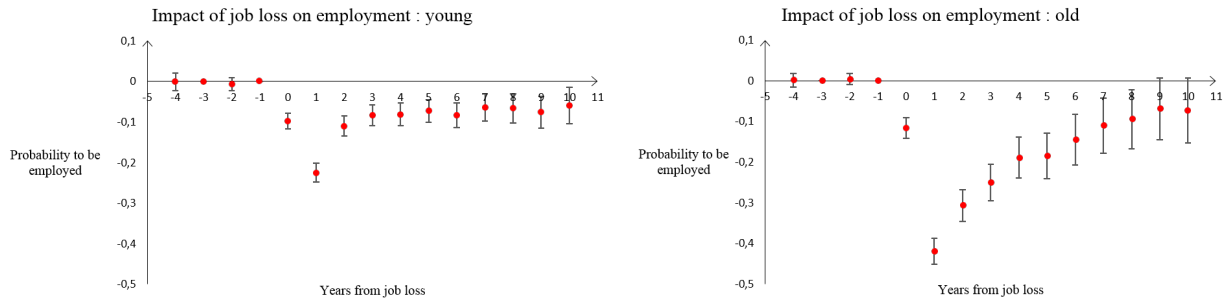


Figure E12: Impact of job displacement on earnings

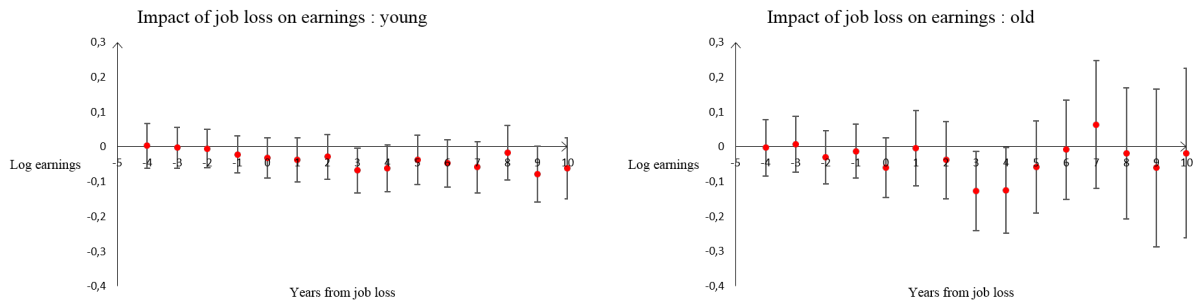


Figure E13: Impact of job displacement on hourly wage

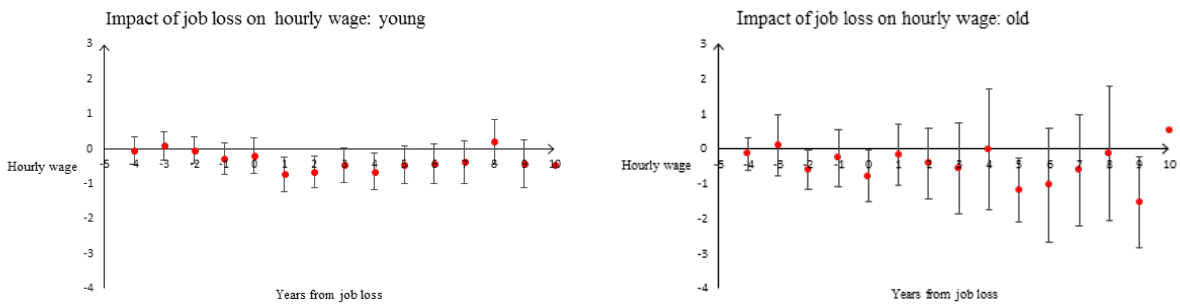


Figure E14: Impact of job displacement on job security

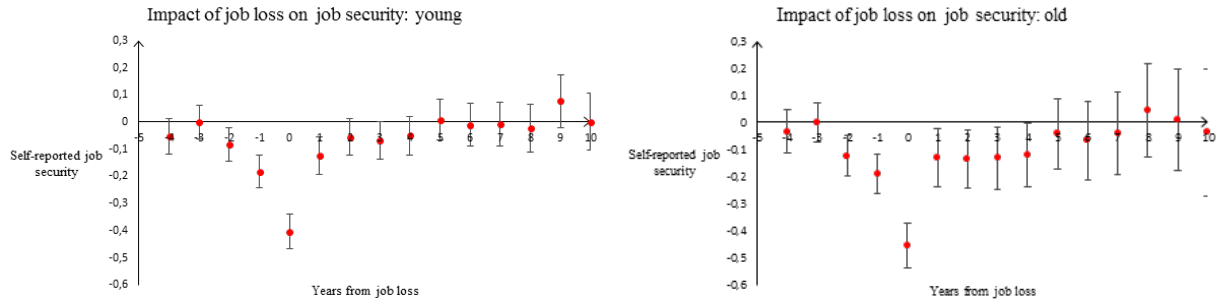


Figure E15: Impact of job displacement on probability to work full-time

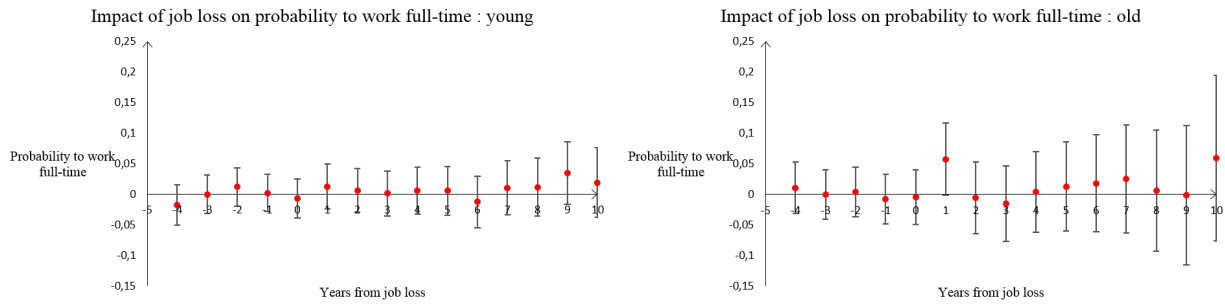


Figure E16: Impact of job displacement on probability to work more than 48 hours a week

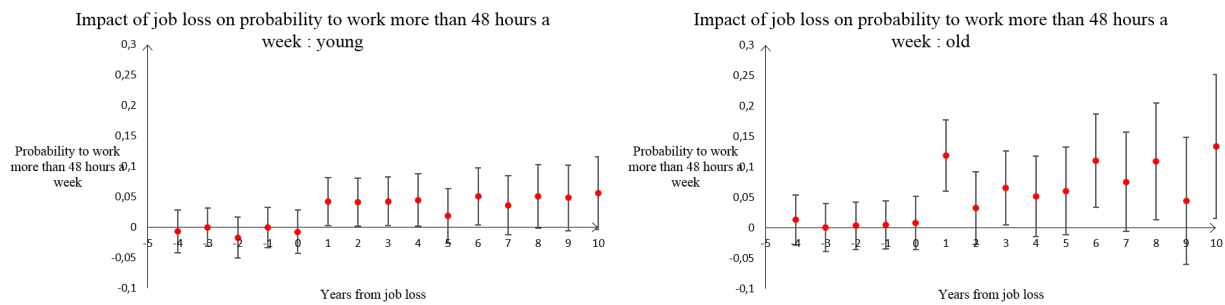


Figure E17: Impact of job displacement on the gap between actual and desired hours worked

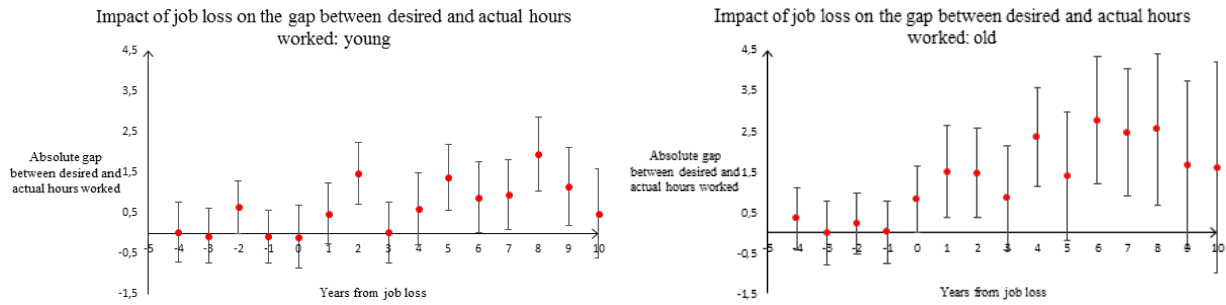


Figure E18: Impact of job displacement on distance to work

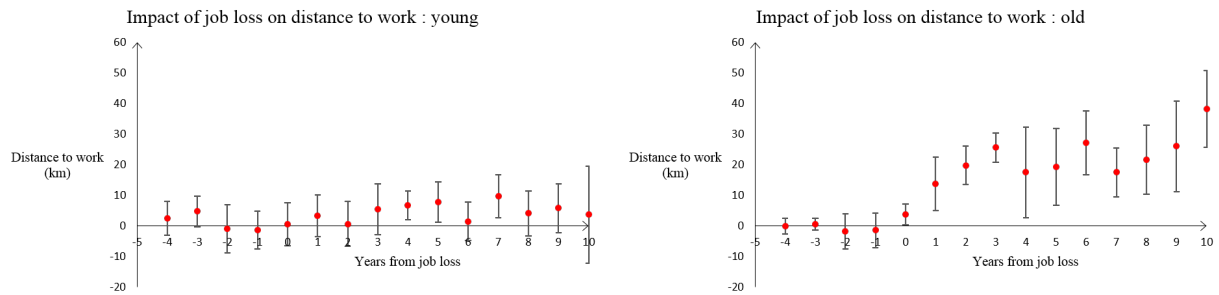


Figure E19: Impact of job displacement on job satisfaction

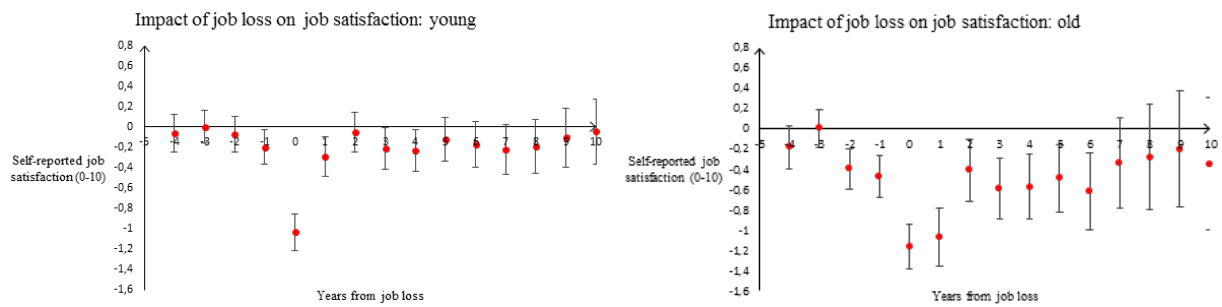


Figure E20: Impact of job displacement on life satisfaction

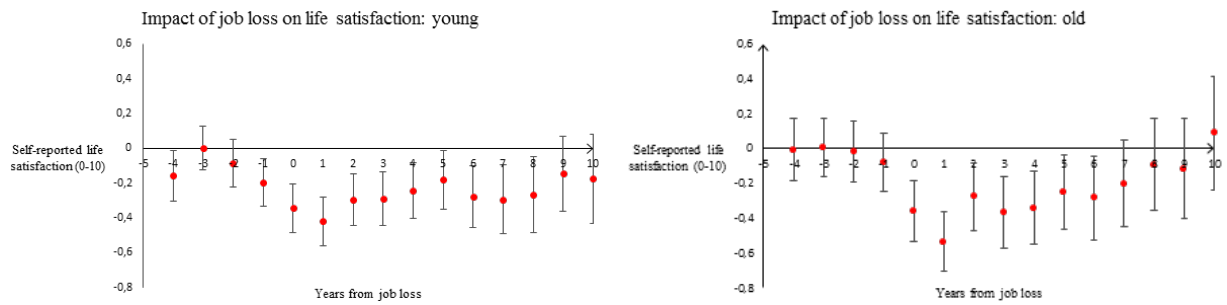


Figure E21: Impact of job displacement on employment

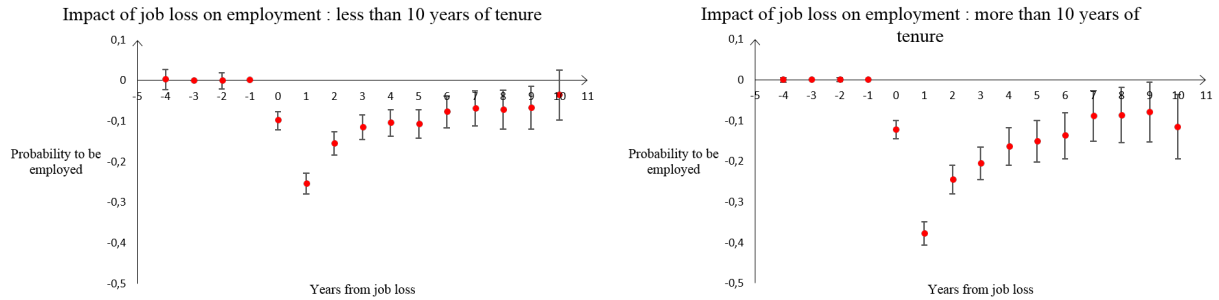


Figure E22: Impact of job displacement on earnings

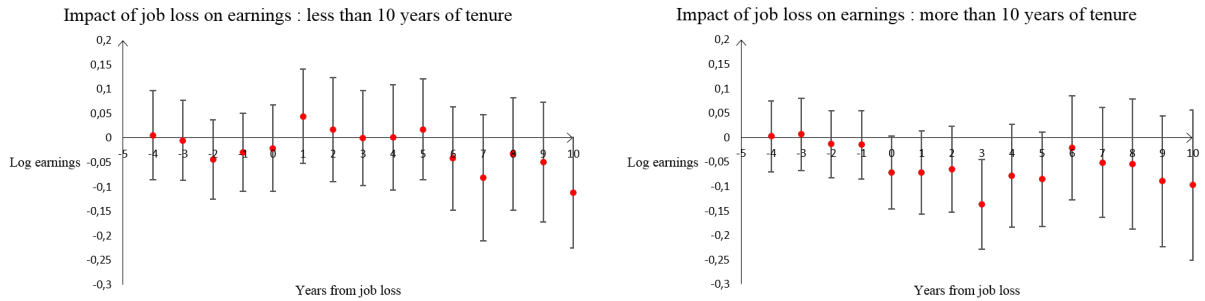


Figure E23: Impact of job displacement on hourly wage

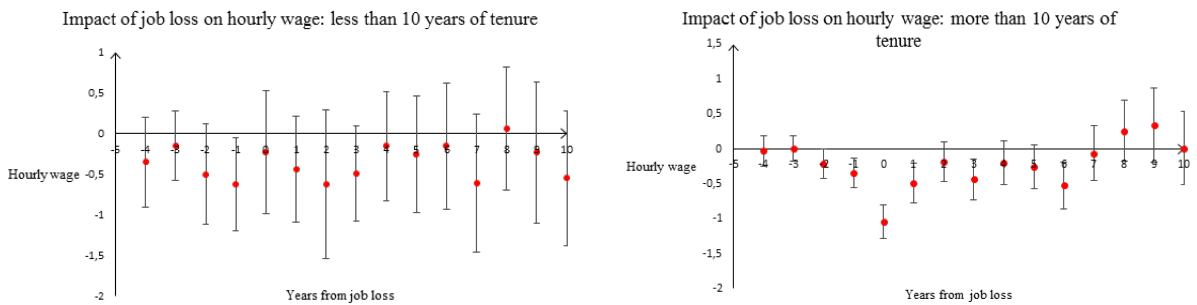


Figure E24: Impact of job displacement on job security

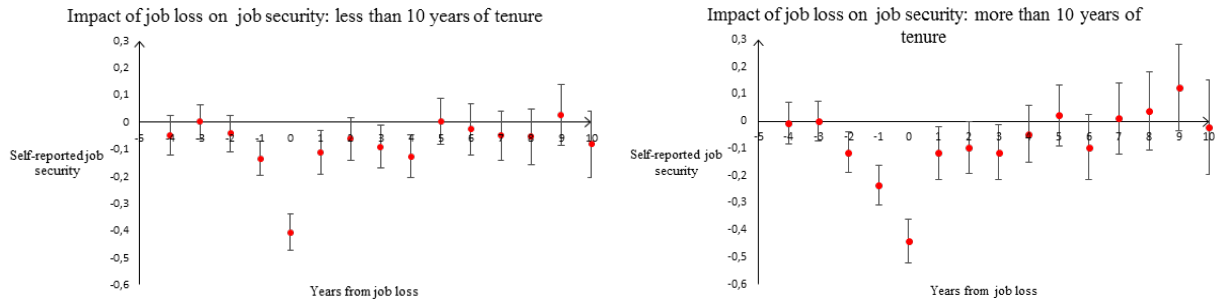


Figure E25: Impact of job displacement on probability to work full-time

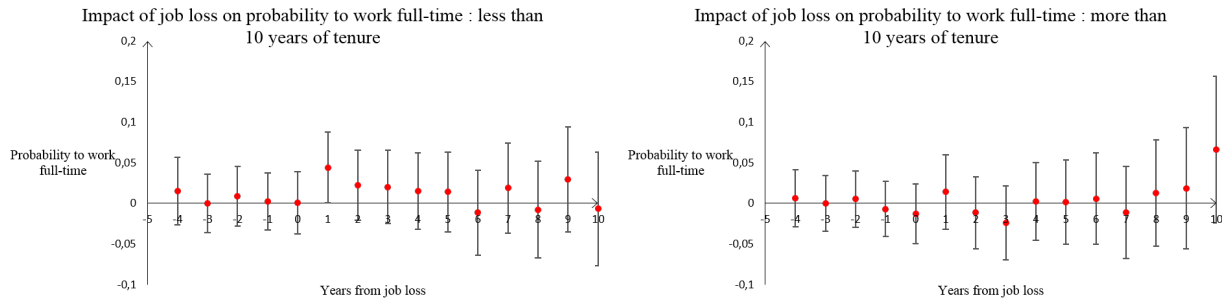


Figure E26: Impact of job displacement on probability to work more than 48 hours a week

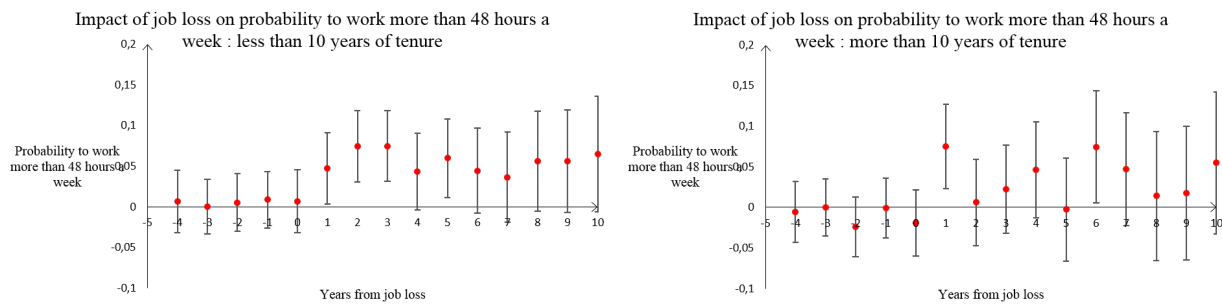


Figure E27: Impact of job displacement on the gap between actual and desired hours worked

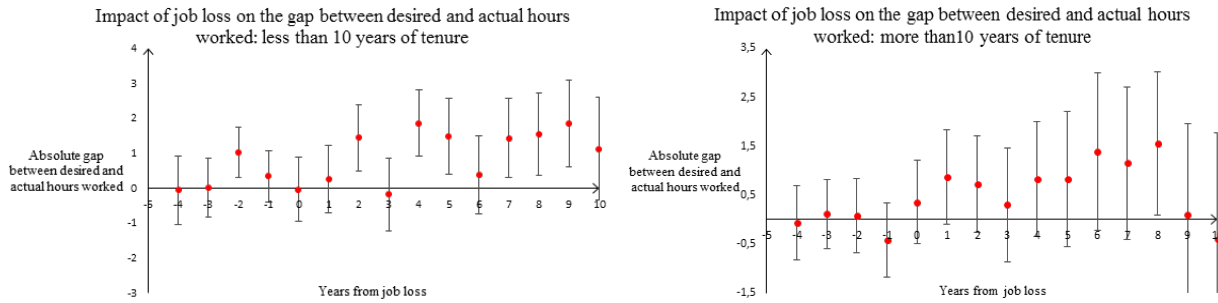


Figure E28: Impact of job displacement on distance to work

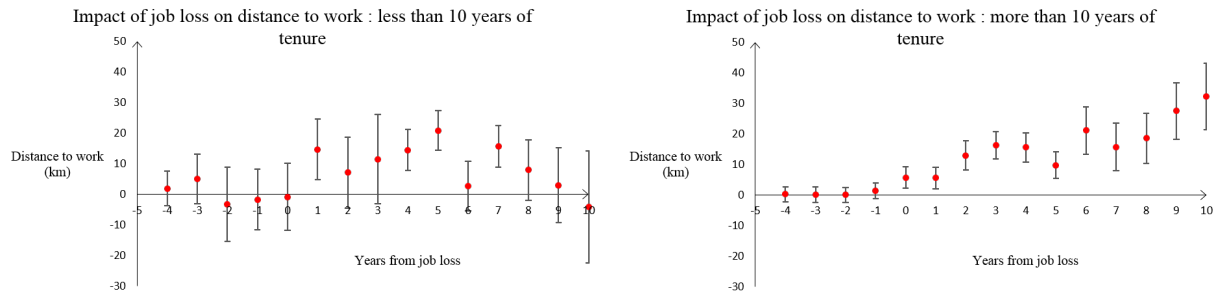


Figure E29: Impact of job displacement on job satisfaction

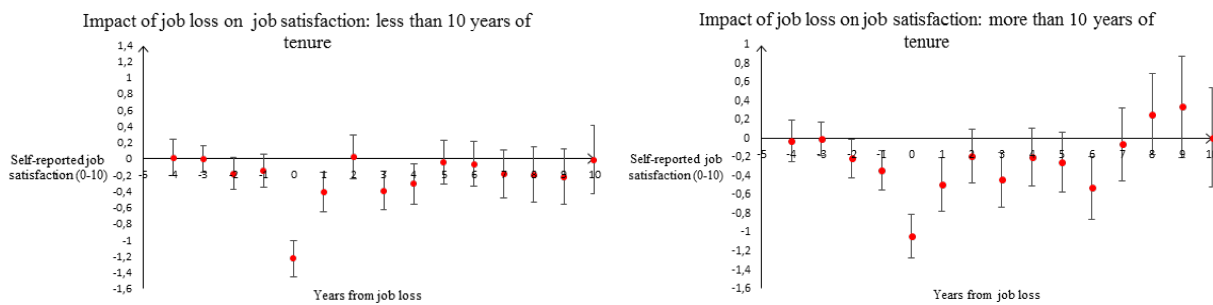
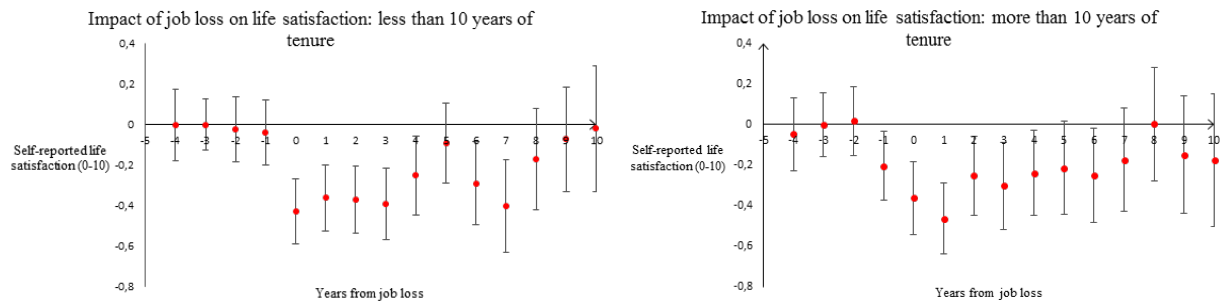


Figure E30: Impact of job displacement on life satisfaction



2.E.3 White and blue-collar

Figure E31: Impact of job displacement on employment

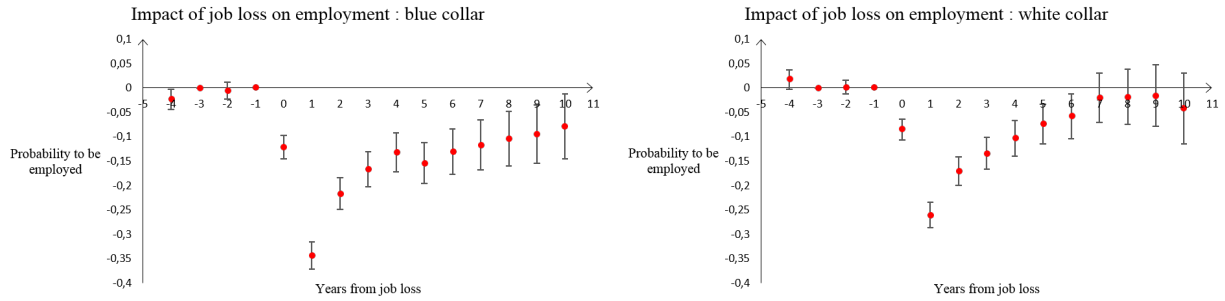


Figure E32: Impact of job displacement on earnings

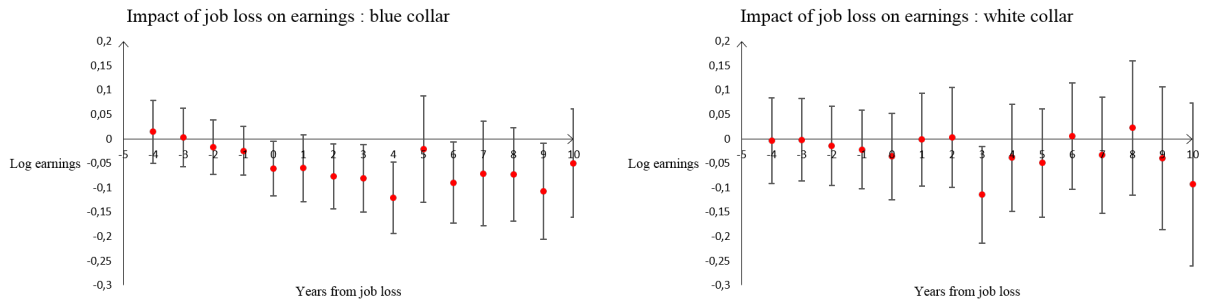


Figure E33: Impact of job displacement on hourly wage

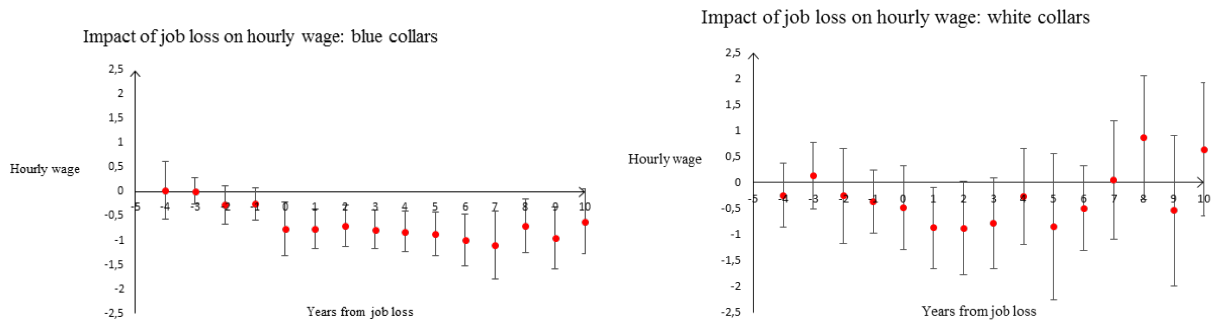


Figure E34: Impact of job displacement on job security

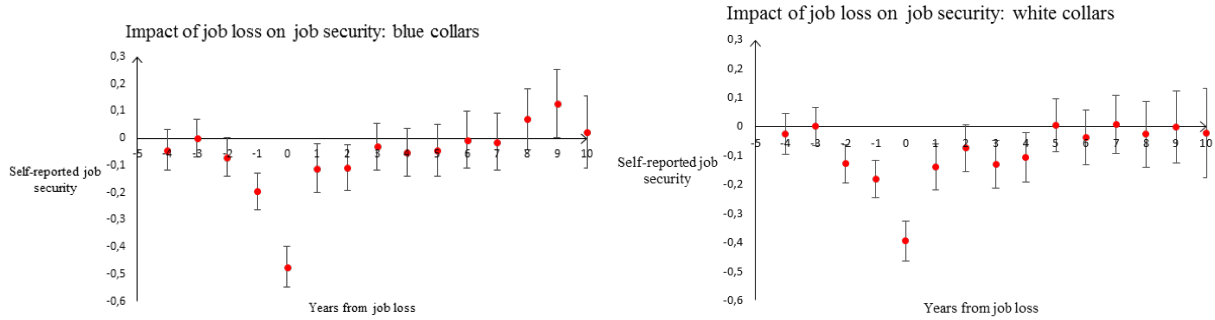


Figure E35: Impact of job displacement on probability to work full-time

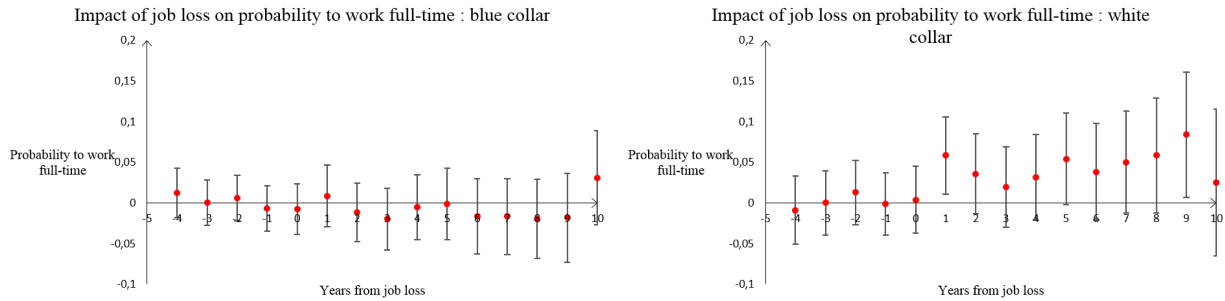


Figure E36: Impact of job displacement on probability to work more than 48 hours a week

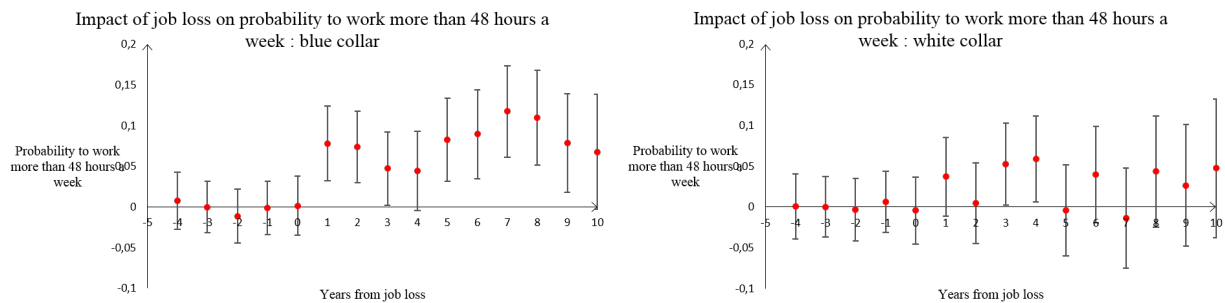


Figure E37: Impact of job displacement on the gap between actual and desired hours worked

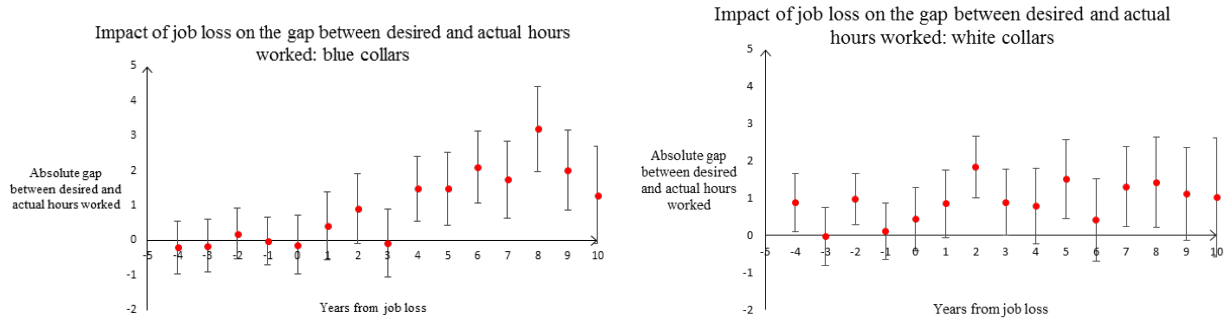


Figure E38: Impact of job displacement on distance to work

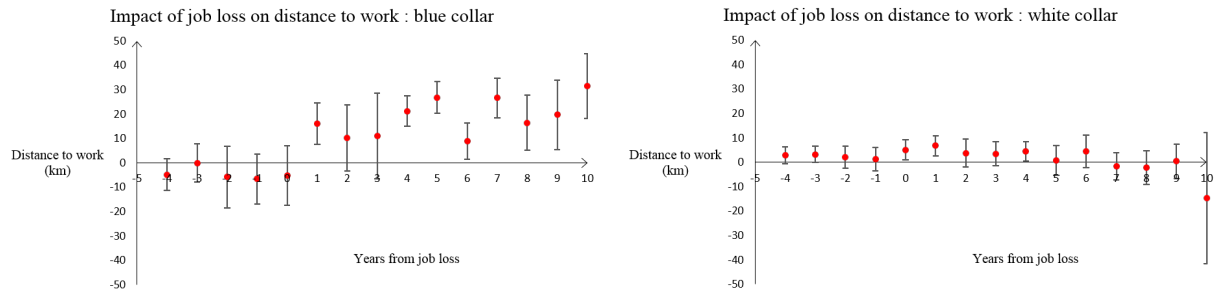


Figure E39: Impact of job displacement on job satisfaction

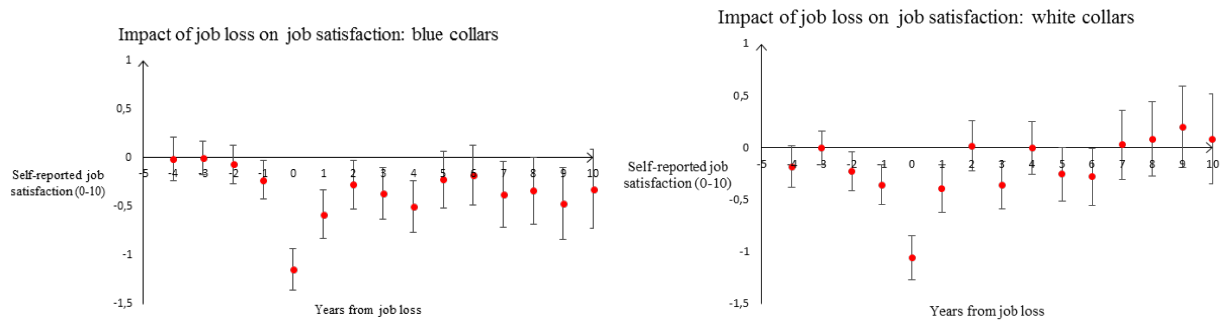
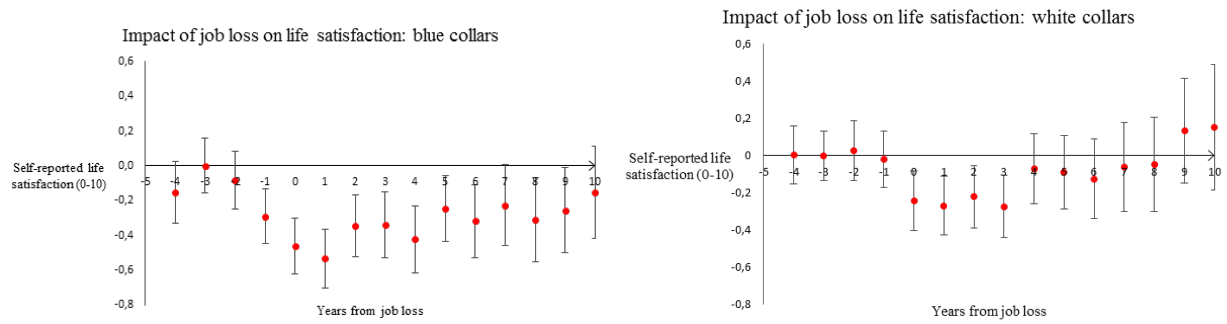


Figure E40: Impact of job displacement on life satisfaction



Robustness check

Table E1: Short and long-term unemployed: individual characteristics

Time not employed	Number	Female	Age	East	Tenure	Blue-Collars
Less than one year	621	0.38	40.96	0.30	9.32	0.45
More than one year	420	0.46	45.56	0.32	12.04	0.55

Figure E41: Impact of job displacement on employment



Figure E42: Impact of job displacement on earnings

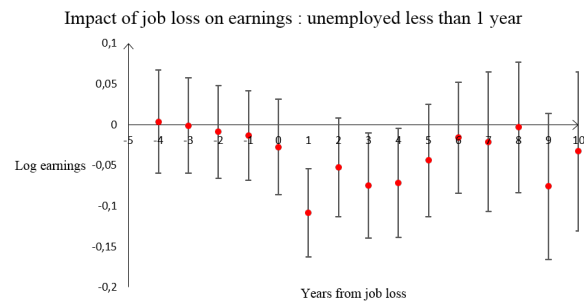


Figure E43: Impact of job displacement on hourly wage

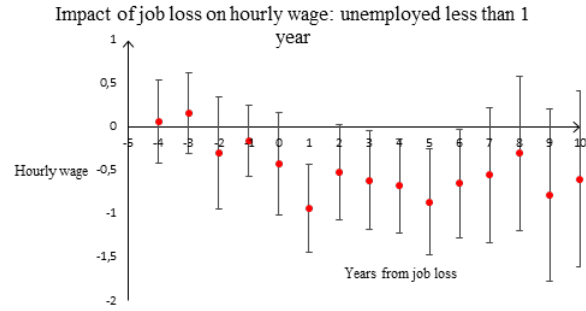


Figure E44: Impact of job displacement on job security

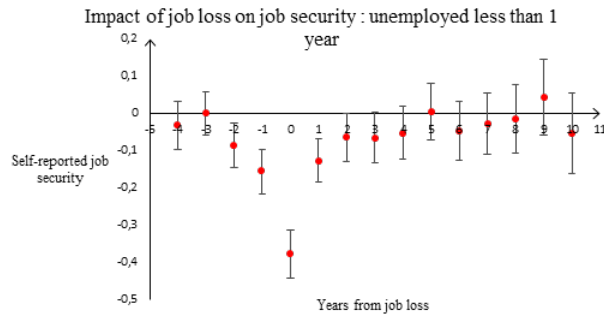


Figure E45: Impact of job displacement on probability to work full-time

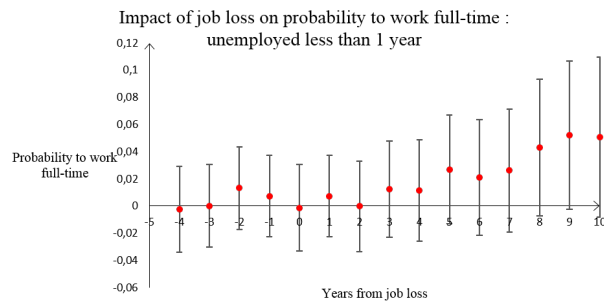


Figure E46: Impact of job displacement on probability to work more than 48 hours a week

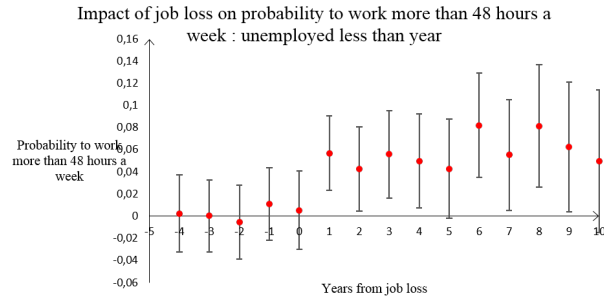


Figure E47: Impact of job displacement on the gap between actual and desired hours worked

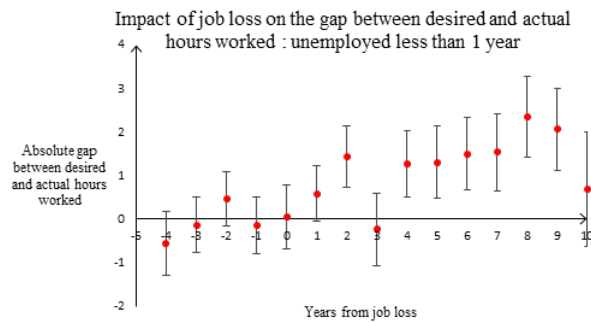


Figure E48: Impact of job displacement on distance to work

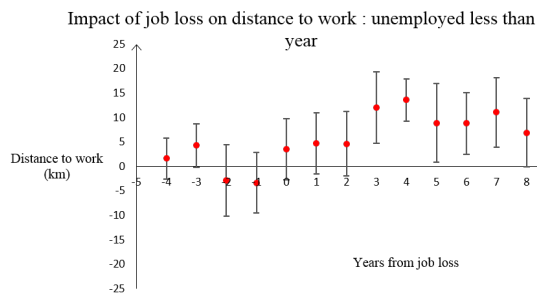


Figure E49: Impact of job displacement on job satisfaction



Figure E50: Impact of job displacement on life satisfaction



Chapter 3

Does Moving to a System with a More Generous Public Health Insurance Increase Medical Care Consumption?

This Chapter is a joint work with Laurent Davezies, the corresponding article has been published in December 2015 in Annals of Economics and Statistics

Abstract

Using a natural experiment, we evaluate the impact of reimbursement rates on health expenditures. For historical reasons, reimbursement rates of public health insurance are higher in the *Alsace-Moselle* region of France than in other French regions. For both the regional and national systems, affiliation is compulsory. Individuals moving between *Alsace-Moselle* and the rest of France undergo an exogenous change in reimbursement rates. We use a difference-in-difference method on a panel dataset of individuals. Our treatment group consists of individuals changing system, our control group consists of individuals who move between other French regions. We study the impact of reimbursement rates on a broad range of health care expenditures: for the visits to the dentist and the doctor, consumption of medicines, and absenteeism due to sickness. We find heterogeneous impacts of reimbursement rates on those items. Overall, higher public reimbursement rates do not lead to an increase in spending for medical care

3.1 Introduction

Faced with the warnings of a committee in charge of monitoring the balancing of the books of the National Health Insurance system, the French government regularly modifies some of the parameters of the system, such as the reimbursement rates, co-payment or the bundle of health goods and services supported by the system. Such modifications have a direct impact on the reimbursement paid through public insurance (for a given consumption of health care) but they can have more ambiguous effects on consumption, which also matters for the balancing of the public accounts.

We measure the causal impact of a change in the public reimbursement rate, which does not necessarily imply a decrease in co-payment. Many individuals also have a complementary insurance on top of public insurance. The choice to have complementary insurance is linked with the reimbursement rate of the public insurance system. Thus an increase in the public reimbursement rate does not necessarily lead to a decrease in co-payment. We do not claim to measure price elasticity of demand for medical care; rather, we measure how, overall, public insurance reimbursement rates impact on consumption.

There is a large literature on the impact of reimbursement rates on health expenditures. Comparing people's consumption with heterogeneous insurance to evaluate the effect of the generosity of insurance on health care consumption is probably misleading. Indeed, endogenous selection is a critical issue when trying to evaluate the impact of reimbursement rates on health care consumption. There is an incentive for people who expect to need more care to get higher reimbursement rates by choosing a generous private insurance, or by working for a firm which provides higher reimbursement rates. Since the pioneering theoretical work on asymmetric information in the 70's (Akerlof, 1970; Spence, 1973; Rothschild and Stiglitz, 1976) such selection has been widely studied theoretically. Testable implications of such selection have been more recently investigated (Chiappori and Salani,

2000; Chiappori et al., 2006; Jullien et al., 2007; Einav and Finkelstein, 2011) and the empirical literature on such selection in insurance is now quite large (concerning the specific topic of health insurance, we can mention Manning et al. (1987), Million et al. (2003), Buchmueller et al. (2004), Finkelstein and Poterba (2006), Jones et al. (2006), Schokkaert et al. (2010), Einav and Finkelstein (2011), Einav et al. (2013)). Neglecting this issue can lead to an overestimate (Manning et al. (1987), Million et al. (2003)) or an underestimate (Jones et al. (2006), Einav and Finkelstein (2011)) of the impact on health expenditures of increasing health insurance reimbursement rates.

In the U.S., researchers from the Rand Corporation during the 80's (Manning et al., 1987; Keeler and Rolph, 1988) and more recently (Finkelstein et al., 2012) overcame the selection bias by using random experiments. To the best knowledge of our such experiments have never been implemented on a large scale in France, and in other countries, for financial and practical reasons they are rare.

A second group of studies uses changes in the rules of compulsory health insurance to compare health care expenditures before and after a change of rule (for instance, Scitovsky and Snyder (1972), Phelps and Newhouse (1972), Beck (1974), Scitovsky and McCall (1977) for the U.S.). In France, no less than forty laws since 1976 have changed the reimbursement rules and funding of the National Insurance System, but to the best of our knowledge only a few academic publications evaluate the causal impact of such reforms, probably because in the absence of control groups an exogenous change in insurance cannot be distinguished from any other factor that changes over time. A third group of papers uses quasi-experiments to evaluate the effect of reimbursement rates on health care consumption, controlling for anti-selection, as Chemin and Wasmer (2008) did for absenteeism or Chiappori et al. (1998) for the provision of services by doctors in France. The present evaluation belongs to this last group because our identification strategy relies on a quasi-experiment that we will briefly outline now. However, it should be kept in mind that we

do not measure the same effect as most papers that examine insurance effects; instead we assess a rise in public reimbursement rates rather than a rise in total reimbursement rates, which include complementary insurances.

For historical reasons, some laws are different in the French region of Alsace-Moselle than in the rest of France. The reimbursement rates of public health insurance are higher, and there is no delay in providing sickness allowances. Individuals moving in and out of the Alsace-Moselle regional system make it possible for us to distinguish the effect of these local laws from the effect of individual heterogeneity. Because moving is potentially correlated with medical care consumption, we use movers that move from and to other regions than Alsace Moselle as a control group. We will not estimate a structural model in order to evaluate the impact of the change in a specific price on a specific outcome. Rather, to give an overall view of how consumption adapts when public reimbursement rates increase, we will estimate a reduced form in order to evaluate the global impact of more generous insurance on various aspects of health care consumption. Our outcomes are expenditures for dental visits, expenditures for visits to the doctor, consumption of medicines, and number and duration of sick-leave periods. We find a positive effect on dental visits, and a negative effect on consumption of medicines. We find no significant effect on visits the doctor and sick-leave.

Chemin and Wasmer (2009) use the unusual Alsace-Moselle system to evaluate the impact of working-time reduction. They use a Difference-in-Difference strategy on surveys in repeated cross-section to control for regional differences in individual heterogeneity. We use the same setting (the specificity of Alsace-Moselle), but because of availability of panel data we use a different method to control for individual heterogeneity.

Here the affiliations to the two systems (national and regional) are compulsory according to your place of work. Our quasi-experiment is valid as long as people do not choose to move to Alsace-Moselle in order to benefit from higher reimbursement rates. Such an assumption

could be questionable for close migrations. To check if our results are not led by endogenous selection, we conduct a robustness check without close migrations (individuals who move to or from neighboring regions). The results are similar.

When public reimbursement rates increase at the market-wide level, and demand increases, in the long run the supply of healthcare is likely to adapt to the rise in demand -more physicians are willing to enter the market, and they will adopt new practices (Finkelstein, 2007). This spillover strengthens the initial rise in demand implied by higher reimbursement rates. A change in public reimbursement rates is also likely to impact on the content of packages offered by complementary insurance companies. These impacts are hard to capture with a randomized experiment like the RAND Health Insurance Experiment which increases reimbursement rates of a randomized fraction of the whole population. Opposed to this approach, we compare two regions where reimbursement rates have been high (or low) for a long time, so that the level of supply corresponds to the long-run equilibrium level in each region. When they are non-treated (out of Alsace-Moselle), our treatment group faces a supply corresponding to a long-run equilibrium with no treatment. When they are treated (in Alsace-Moselle), our treatment group faces a supply corresponding to the long-run equilibrium with treatment. Therefore, our results capture the general equilibrium effects of the treatment.

Regressing various aspects of health expenditures on the public insurance system, including time-invariant individual effects, we find that when moving to a system with higher public repayment rates, individuals spend more at the dentist and the same amount for visits to the doctor, they have more numerous periods of sick-leave without, however, any change in the annual number of sick-leave days, and they decrease their consumption of drugs. In our case, overall consumption decreases when individuals move to Alsace-Moselle. Our results are in line with the fact that health care consumption in Alsace-Moselle is close to the national average (Cours des Comptes, 2011), which suggests a moderate effect of

higher reimbursement rates on health expenditures. Even if it is not possible to rule out the effect of some unobserved regional specificities, our results are robust to the robustness checks and placebo tests we perform to address this issue. In particular, including controls on the supply side using densities of physicians and pharmacies leads to even more provocative results and reinforces our main message: a higher public reimbursement rate does not necessarily lead to higher consumption of medical care.

The paper is organized as follows. In the second section, we present the specificities of public health insurance in Alsace Moselle. In the third section, we discuss our strategy of identification and our estimators. In the fourth section, we describe the data used in the estimation. In the two following sections, we present our results and some robustness checks. The last section concludes..

3.2 Institutional framework

The territory hereafter named *Alsace-Moselle* is in the North East of France, and includes one French *région*, Alsace, and a *département*, Moselle. They both share a border with Germany. Figure 3.A.2, in the Appendix, shows the geographical situation of the Alsace-Moselle region in France.

3.2.1 The origin of the particular status of AM regional system

Between 1870 and 1918, Alsace-Moselle was part of Germany, and German laws, including Bismarck's social laws, like national compulsory health insurance were implemented in 1883. When the region became French in 1918, the inhabitants of Alsace-Moselle wanted to preserve their public insurance, but France did not have such a national system. Thus, they maintained Bismarck's laws. A system of local laws (*Droit local*) was implemented, allowing Alsace-Moselle to maintain laws that came from the period when the territory

was German. This system was temporary at first, and then became permanent in 1991 (Cours des Comptes, 2011). In 1945, the French national health insurance system was implemented. It applied also to Alsace-Moselle, and implemented lower repayment rates. In 1946, it was decided that the regional system in Alsace-Moselle would act as a complement to the national system, allowing repayment rates to be higher in Alsace-Moselle.

3.2.2 Details about the local laws

Any individual affiliated to the regional system is also affiliated to the national system. The national system repays medical expenditure at a given rate. On top of that, the regional system repays part of the amount left over for people working in Alsace-Moselle. The regional regime thus works as a compulsory complementary insurance scheme.

Affiliation to the regional system is based on the place of work. From 1998 to 2012, people working in Alsace-Moselle, wherever the location of their firm's head office, and people employed by a firm whose head office was in Alsace Moselle were entitled to benefit from the regional regime. From March 2012, an individual has to work in Alsace-Moselle to benefit. For unemployed people, affiliation depends on their previous employer (Article L235-1 du code de la sécurité sociale). For retired people, the affiliation to the regional system is possible if they have paid contributions into that system long enough, i.e. either five years before retirement, or over a period of ten years among the fifteen before retirement, or for sixty quarters.

Individuals affiliated to the regional system benefit from higher public reimbursement rates. The public repayment rates for dentists and doctors (whether specialists or not) are 70% for the general system. An additional 20% is repaid by the regional system for individual working in Alsace-Moselle, thus these latter benefit from a 90% repayment rate. Repayment rates are also higher for almost all health care expenditures, including medicines, and hospital care. For medicines, the rates in both systems vary depending

on whether or not the medicine is considered essential. It varies between 15% and 100% for both systems, but for most medicines the repayment rate is higher in Alsace-Moselle at around 80% or 90%, whereas it is between 30% and 65% in the rest of France. These differences are summarized in Table 3.1. Details concerning the reimbursement rates of the general and the regional systems can be found in the Appendix.

People affiliated to the regional system also benefit from more generous sick-leave policies. Employers in Alsace-Moselle must pay the employee his or her allowance from the first day of sick-leave whereas in the rest of France there is a waiting period of three days¹. During those three days, an employer can give sickness benefits to the employee, but it is not legally required. From the fourth day, the public insurance system pays sick allowances in both cases.

The regional system also has different rules for pharmacy creations. In France, excluding Alsace Moselle, a new pharmacy can be opened only in municipalities of more than 2,500 inhabitants whereas in Alsace-Moselle, the threshold is 3,500 inhabitants and a new pharmacy can open if there are 4,500 additional inhabitants². As a result, there are fewer pharmacies in Alsace-Moselle where there are 25 pharmacies per 100,000 inhabitants, while the French average is 34.9 (in 2010). The departments of Alsace Moselle are the three where the density of pharmacies is the lowest in France, after Guyane, an overseas department where the same threshold of 3,500 applies. In all those departments, this density is close to the legal maximum, so it is likely that the local laws indeed have an impact on the density of pharmacies in Alsace-Moselle.

The regional regime is funded by a tax on wages, which fluctuates between 1.5 and 1.8%³, depending on need. If we want to evaluate inter-regional equity therefore, we need to bear in mind that people in Alsace-Moselle are the ones paying for their additional and

¹Source: <http://vosdroits.service-public.fr/F3053.xhtml>

²Source: <http://vosdroits.service-public.fr/professionnels-entreprises/F13777.xhtml>

³Between 2000 and 2013, source : *Barnes de l'IPP : prélèvements sociaux, Institut des politiques publiques, avril 2012*

Table 3.1: THE NATIONAL FRENCH SYSTEM AND THE ALSACE-MOSELLE REGIONAL SYSTEM

	French national system	Alsace-Moselle
Dentist's visits	70% repaid	90% repaid
Doctor's visits	70% repaid	90% repaid
Medecines	15%	15%
Depending on therapeutic value	30%	80%
	60-65%	90%
	100%	100%
Sick-leave	3-days waiting period for allowances, which can be covered by the employer	No waiting period, coverage by the employer is compulsory
Pharmacy opening	need 2500 inhabitants	need 3500 inhabitants

more generous system.

3.3 Identification Strategy and Estimator

The main objective of this paper is to find a credible counterfactual to health care consumption for some individuals. Our idea is to use the panel structure of the data to consider individuals who move in and out of the Alsace-Moselle local regime, and compare their consumptions when subject to the French national system, and when subject to a more generous system. Panel data is a powerful tool to control for the bias coming from unobserved variables. The people living in Alsace-Moselle differ not only in terms of where they live, their individual characteristics are likely to be both linked in an unobservable way to the region and to their demand for health care. For instance, health care preferences, which are unobservable, may vary across regions. Naive comparisons of average health care consumptions in Alsace-Moselle and the rest of France at a given date (cross-section models) lead to estimating the effects of the insurance system together with the effects of individual characteristics. Affiliation to the system is not exogenous because some unobserved variables can simultaneously correlate with both geographical location and health care consumption. The use of panel data models allows us to obtain consistent estimators in the presence of time-invariant omitted variables.

Of course, in such panel data models the treatment effect of a covariate X on the outcome is identified only for individuals such that X is not time-invariant (Wooldridge (2002) Chapter 10). Moreover, in case of heterogeneity of treatment effects such as in Imbens and Angrist's framework (Imbens and Angrist, 1994), estimated parameters are only the average treatment effects on such individuals. In our case, therefore, individuals who move in and out of the Alsace-Moselle regional regime will play a crucial role. We will present how such people differ from the others in terms of observable characteristics in the next section.

Even if we use people moving in and out of Alsace-Moselle, there is a risk of attributing

to the social security system a change in health care consumption that is due to professional or geographic mobility. Thus it could be misleading to compare individuals moving in and out of Alsace-Moselle with individuals staying in the same firm over time. To avoid such a problem our estimators are identified using comparisons between individuals moving in and out of Alsace-Moselle with individuals undergoing regional mobility without changing their health insurance reimbursement rates. To use these other “movers” as a control group, we add in our regression supplementary dummies to capture an effect of the move before and after it occurs.

We can now turn to the econometric specification of our models. We assume that the health care consumption of individual i at year t (y_{it}) is such that:

$$y_{it} = \beta AM_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it}, \quad (3.1)$$

Following this specification, health care consumption depends on observable and unobservable characteristics which are stable in the time series dimension throughout term α_i . Health care consumption can also depend on macro shocks ϕ_t , for instance due to epidemics. The x_{it} is a vector of observables that change in the time series dimension, i.e. status on the labor market and income. The effect of generation is controlled in α_i . Age is a linear function of generation and period, and therefore age effects are controlled by both terms α_i and ϕ_t .

Because our strategy of identification relies on people changing systems when they change the region where they work, we need to carefully control the correlation between health expenditures and the change of region of work. For people changing region of work between January 1 of year $t_d - 1$ and January 1 of year t_d , the variable w_{kit} is a dummy variable that takes value 1 if and only if $t = t_d + k$. Such controls are used to control for the variation in health care consumption before and after the change of region of work,

and compare only with other movers when estimating our parameter of interest β . Indeed, distribution of heterogeneity in individual preference such as risk aversion is probably very different between movers and non-movers.

AM_{it} is a dummy indicating whether individual i is affiliated to the Alsace-Moselle insurance system at time t and β is the parameter of interest. The magnitude of β is not explained by differences in population characteristics between Alsace-Moselle and the rest of France because we control for individuals effect α_i . Estimation of β is close to a Difference-in-Difference estimator using people who change their region of work as a control group, and people who change simultaneously both their region of work and their affiliation to the Alsace-Moselle regional system as a treatment group. People who never move enter the estimation by contributing to the estimation of the effect of the control variables (ϕ_t and x_{it}).

Finally ε_{it} is the effect of unobserved variables and the main identifying assumption of β is simply the usual exogeneity assumption:

$$\mathbb{E}(\varepsilon_{it} | \alpha_i, (x_{it})_{t=2005, \dots, 2008}, (AM_{it})_{t=2005, \dots, 2008}, t) = 0$$

Under a supplementary assumption of independence across individuals, the “within” estimator of β is consistent. Health care consumption across time is likely to be serially correlated. If people consume health care regularly, with time periods greater than one year, health care in a given year is negatively correlated with consumption the year before. For the inference, serial correlation of the dependent variable is taken into account using clusters at the individual level (cf. Wooldridge (2002)).

3.4 Data and Descriptive Statistics

3.4.1 Data

We use the Hygie dataset, an administrative database, merging information from the French national health insurance fund, *Caisse nationale de l'assurance maladie des travailleurs salariés* (CNAMTS), and from the national old-age pension system, *Caisse nationale de l'assurance vieillesse des travailleurs salariés* (CNAV). The Hygie database contains the annual health care consumption of about 554 000 individuals from 2005 to 2008 together with information about their careers. It is a random sample of individuals between 22 and 70 years old in 2005, who have contributed to the national pension fund at least once during their life, and have used the health care system at least once between 2003 and 2005. The dataset includes health expenditures both for individuals affiliated to the Alsace-Moselle system and for individuals affiliated to the national system. This dataset has previously been used to study sick-leave absences (Halima et al., 2011).

We restrict the sample to individuals for whom we have information about consumption, working situation and place of work during the four years. We lose about 5% of the sample, due to missing information about health care consumption⁴. We need the place of work to determine who is affiliated to the Alsace-Moselle insurance system. We remove individuals whose place of work is not always known (18% of the sample). Finally, we remove individuals whose occupation is not known for the four years (14% of the sample⁵). In the end the sample was reduced to 335,000 individuals.

⁴The observations deleted can be people who did not consume any health care for three consecutive years. Under such conditions they would exit the dataset. They can also be people who have died or who have moved to another country. As we do not know the reason why each person exits the dataset, we have no reliable measure of the outcomes we wish to estimate. By removing them, the effects will be estimated only for people who do not have three consecutive periods of non-consumption.

⁵People for whom there is no information can be individuals who were public servants for a period, or who were affiliated to specific retirement systems, and thus not included in the national dataset of the CNAV.

The dataset does not contain affiliation to the regional system. Between 1998 and 2012, employee of a firm whose head office was in Alsace-Moselle, whatever their place of work, and employee working in Alsace-Moselle could be affiliated to the regional system⁶. Our criteria for affiliation is the *Département* of the firm employing the individual: we consider that someone is affiliated to the regional system if and only if he or she is working for a firm in Alsace-Moselle, either in *Bas-Rhin*, *Haut-Rhin*, or *Moselle*, the three *Départements* of Alsace-Moselle. This is accurate for everyone except someone who does not work in Alsace-Moselle but benefits from the regional system because the head office of his or her firm is in Alsace-Moselle. For the unemployed and pensioners, we have information about the *Département* of the last employer, which is the criteria for affiliation.

3.4.2 Descriptive statistics

In this section, we present descriptive statistics concerning health care consumption and individual characteristics.

Health care consumption in Alsace-Moselle

We give comparative figures for health care consumption of people working in Alsace-Moselle, and people working in the rest of France. We use the restricted sample presented in the previous section.

Table 3.1 presents the annual average of the share of expenditures on health care that is repaid by the national insurance system, and annual averages of total expenditure on health care, at the individual level. If someone goes to see a general practitioner once during the year and pays 30 euros, the patient's "Total amount" for doctor's visits will be the amount considered by the public insurance system as the base for repayment -23

⁶We thank Karen Aissaoui, project officer of the regional regime, for helpful information about conditions of affiliation

Table 3.1: HEALTH CARE EXPENDITURES AND SICK LEAVE

	(1) Alsace-Moselle	(2) Rest of France	(3) P-value
Amount repaid			
Dentist's visits	67.4	49.5	0.000
Doctor's visits	76.1	62.4	0.000
Specialist's visits	108.7	108.2	0.055
Medecines	214.0	190.3	0.000
Total expenditures			
Dentist's visits	193.9	135.9	0.000
Doctor's visits	112.3	95.2	0.000
Specialist's visits	168.8	167.3	0.030
Medecines	293.0	264.6	0.000
Sick-leave periods	0.44	0.34	0.000
Sick-leave days	11.5	11.0	0.002
Nb. Ind	16 342	320,885	
Nb. Obs	60,228	1,278,788	

Amount repaid: average annual amount repaid by the national public insurance, for dentist's visits (line 1), general practitioners (line 2), specialists (line 3), and medecines (line 4). Total expenditures: average annual amount spent by the individual for health care consumption.

Column 3: Two-sided p-value of a t-test comparing the mean of individual means over time in Alsace-Moselle with the mean of individual means over time in the rest of France

Source : *Hygie*

euros- plus extra fees if any (*dépassements d'honoraires*). The patient's "Amount repaid" will be the fraction of the total amount that is repaid by the national public insurance, that is, $0.70 \times 23 = 15.10$ €, irrespective of whether he or she is affiliated to the Alsace-Moselle system. Indeed, the national insurance repays the same amount for everyone, and the Alsace-Moselle system repays an additional amount, like a compulsory complementary insurance.

The part reimbursed for each medical act (15.10 € in our example) excludes any extra fees varying across physicians. If this changes, it means that the individual consumes more or less medical care, not that the price has changed. This measure allows us to quantify costs for the national insurance. For these reasons, we will use the amount repaid for our main estimations.

The total amount spent is higher than the part repaid by the national insurance, especially for dental care. This gap is generally shared between the individual and his or her complementary insurance, the share depending on the reimbursement rate of the complementary insurance. Acting as a compulsory complementary insurance, the Alsace-Moselle regional regime repays part of the gap for those who are affiliated.

Table 3.1 shows that average annual consumptions of health care are higher in Alsace Moselle, for visits to the dentist and doctor, and medicines. All these differences are significant (column 3). On the other hand, there is no significant difference for visits to specialists.

Concerning sick day absences, our measures do not include the first three days of sick-leave, because the public insurance system does not reimburse anything for those days. Sick-leave periods therefore enter our data only from the fourth day of absence. We use two measures: the annual number of sick-leave periods and the annual number of sick days. People in Alsace-Moselle tend to take more sick-leave periods, and more days of sick-leave. The differences are significant.

There are significant difference in health care consumption between Alsace-Moselle and the rest of France. The point of our paper is to use a fixed effect model to see how those differences vary when controlling for time-invariant unobserved individual heterogeneity.

Descriptive statistics about the groups considered

In our model, we will use three groups of individuals. The first group, hereafter called “movers AM”, enter or leave Alsace-Moselle during the period, and contribute to the estimation of the effect of Alsace-Moselle on consumption, to the effect of the move, and to the controls. The second group, called “other movers”, are movers who move between other regions, never working in Alsace-Moselle. They contribute to the effect of the move, and to the controls. Individuals in the third group, “non-movers”, never change their region of work, and contribute to our model only by contributing to the estimation of the controls.

We will present descriptive statistics for these three groups, about their individual characteristics and their use of health care (Table 3.2). We present those means only for descriptive purposes.

Table 3.2 presents comparisons between movers AM, other movers, and non-movers.

Movers to or from Alsace-Moselle have different characteristics when compared to non-movers. Columns 1 and 3 of Table 3.2 shows outcomes for movers AM and non-movers. Column 5 gives the significance of the difference. There is a significant difference between people moving to or from Alsace-Moselle and those not moving for all individual characteristics, except for retired people. People who changed their affiliation to the regional system between 2005 and 2008 were on average five years younger and had less income. They benefited more often from the CMU-C (public complementary insurance for low income levels), the coverage rate by CMU-CU was 4.8% for movers AM as opposed to 2.7% for people not moving.

There are 8% less women in the group moving to or from Alsace-Moselle. People moving

Table 3.2: COMPARING INDIVIDUALS MOVING TO OR FROM ALSACE MOSELLE, MOVING FROM AND TO OTHER REGIONS, AND NOT MOVING

	(1) Movers AM	(2) Movers	(3) Non-movers	(4) P-value 1-2	(5) P-value 1-3
Women	40.2	42.5	47.7	0.020	0.000
Age	37.6	37.5	42.2	0.861	0.000
Income	21226.1	19241.1	23698.3	0.000	0.001
Cmuc	4.80	6.04	2.74	0.002	0.000
Unemployed	0.22	0.25	0.10	0.000	0.000
Retired	0.84	0.81	0.65	0.418	0.000
Diseases	0.078	0.075	0.11	0.680	0.000
Dentists	51.9	45.7	50.8	0.000	0.277
Doctor	138.8	139.8	174.9	0.789	0.000
Medecines	140.7	146.5	196.7	0.652	0.000
Sick-leaves	0.31	0.27	0.36	0.000	0.001
Sick days	8.14	8.25	11.3	0.819	0.000
Nb. Ind	2,473	32,703	299,578		
Nb. Obs	9,892	130,812	1,198,312		

Movers AM: have moved at least once from or to Alsace-Moselle. Movers: have moved at least once between other regions than Alsace-Moselle. Non-movers are individual who never move.

P-value 1-2 is the two-sided p-value of a t-test comparing movers AM with other movers (comparison of group means of individual means over time, $t=4$). P-value 1-3 compares movers AM with non-movers.

Women: percentage of women. Age: average age in the group. Income: mean annual income in euros. Cmuc: % benefiting from public complementary health insurance for low income levels. Unemployed: average share of the year spent unemployed. Retired: average share of the year spent retired. Diseases: number of chronic diseases. Dentist denotes the annual amount repaid by the national health insurance for dentist's visits, doctor denotes the amounts repaid for doctors (general practitioners and specialists), medecines denotes the amount repaid for prescription medecines, sick-leave denotes the number of periods of sick-leave, sick-days denotes the annual number of sick-leave days.

Source : *Hygie*

to and from Alsace-Moselle are less likely to have chronic diseases. People moving between Alsace-Moselle and the rest of France spend on average 22% of their time unemployed, which is higher than for the rest of the population (10%). Those features are linked to the fact that people moving are on average five years younger. However, differences in age explain only part of the difference in unemployment rates. We include a dummy for being unemployed in our model, because this is likely to be correlated with the move. Concerning health care consumption, movers AM consume less health care than the non-movers. The differences are significant for all items except dental care. The individuals contributing to our estimates of the impact of higher reimbursement rates are not representative of the general population. Therefore, our results apply only to a specific subpopulation.

Movers to or from Alsace-Moselle are similar to other movers. Columns 1 and 2 of Table 3.2 compare movers AM with the other movers. Column 4 tests the significance of the differences.

Differences in the percentage of women, average income and average fraction of time spent unemployed are significant. Although differences are significant, the magnitude is not high: 3% difference for unemployment, almost 2% in the percentage of women. The difference in average income is 9%. There is no significant difference in the prevalence of chronic diseases, or in the proportion of retired people. The population of people moving to or from Alsace-Moselle is more similar to other movers than to non-movers, so the common trend assumption is more credible for the former group of movers than the latter group of non-movers. Concerning health care consumption, movers AM go more often to the dentist, and take more sick-leave periods. For other items, the difference is not significant. At this stage, we do not know if health care consumption of movers AM is different because they spend some periods in Alsace Moselle, or because of different unobserved characteristics. We have seen descriptive statistics about the different groups that are of interest given our estimation method. We will now give a more precise description of the health care

consumption of movers AM and other movers to assess the validity of our estimation method.

The common trend assumption

Our paper is based on a Difference-in-Difference method, comparing the health care consumption of movers AM with the health care consumption of other movers. Non-movers contribute to the estimation of the controls only.

The Difference-in-Difference method we use is based on the common trend assumption: without the treatment, both groups would have followed the same evolution. As our dataset is a panel, we can check whether the health care consumption of movers AM and other movers follows the same trend before treatment. Our case is not a standard difference-in-difference, as treatment does not arise at the same time for everyone. Some individuals enter treatment in 2006, some in 2007, some in 2008. Some individuals exit treatment in 2006, 2007 and 2008, and some both enter and exit.

The common trend assumption, in our case, would be: (1) before they enter treatment, movers AM entering treatment follow the same trend of in health care consumption as the control group, other movers before they move, and (2), after they leave treatment, movers AM leaving treatment follow the same trend as the control group after they move.

The time scale of the trend is the distance to the move (two years before, one year before...) rather the calendar year (2005, 2006...).

Before moving, individuals entering Alsace-Moselle and other movers should have similar outcomes. On the right hand side of Equation 3.1 we add supplementary dummies w'_{kit} corresponding to each possible duration before the move interacted with the fact of being a mover who enters Alsace-Moselle. We create a dummy variable taking the value one if an individual enters Alsace-Moselle once during the period and is one year after the move, and zero otherwise. We create another variable for two years after and another variable for

three years after. Then we estimate this augmented model on our sample without movers leaving Alsace-Moselle.

$$y_{it} = \beta AM_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \sum_{k=-3}^{-1} \delta'_k w'_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it}, \quad (3.2)$$

For any individual, the sums $\sum_{k=-3}^2 w_{kit}$ and $\sum_{k=-3}^2 w'_{kit} + AM_{it}$ are time invariant and hence collinear with the fixed effects. Supplementary normalizations are therefore necessary. Without loss of generality, δ_{-3} and δ'_{-3} are normalized to zero.

In the model derived from Equation 3.2, a testable implication of the common trend assumption is the nullity of δ'_{-2} and δ'_{-1} . The p-value of the joint significance of $w_{-2,it}$ and $w_{-1,it}$ is 0.67 for dentist's visits, 0.31 for doctor's visits, 0.46 for prescription medicine consumption, 0.46 for the number of sick-leave periods, and 0.51 for the total number of sick-leave days.

An alternative testable implication of the common trend assumption involves individuals leaving Alsace-Moselle instead of individuals entering. In that case the supplementary dummies w''_{kit} interact times dummies after the move with the fact of being a mover leaving Alsace-Moselle and the model is estimated on the sample without movers entering Alsace-Moselle.

$$y_{it} = \beta AM_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \sum_{k=0}^2 \delta''_k w''_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it}, \quad (3.3)$$

After necessary normalization that could be without loss of generality $\delta_{-3} = 0$ and $\delta''_0 = 0$. The common trend assumption implies that $\delta'_1 = 0$ and $\delta''_2 = 0$. The p-value of the joint significance of $w''_{1,it}$ and $w''_{2,it}$ in the model derived from Equation 3.3 is 0.90 for dentist's visits, 0.12 for doctor's visits, 0.46 for prescription medicine consumption, 0.28 for the number of sick-leave periods, and 0.32 for the total number of sick-leave days.

To conclude, the common trend assumption cannot be rejected with our data when we

compare the treated group with the control group using observations that correspond to a period of non-treatment.

3.5 Main Results

We run a panel data model with fixed effects, as presented in section 3.3. We control for year fixed effects, employment status (employed or unemployed), income, and the impact of changing work location (using all movers). Details about the variables used and the default values can be found in the Appendix. Each regression presents the impact of the treatment -being affiliated to the regional regime- on one particular health care outcome, namely, expenditures on dentist's visits, on doctor's visits, for prescription medicines, number of sick-leave periods, and total number of sick-leave days.

Table 3.1 presents the results of the estimation for expenditures for dentist's visits, doctor's visits (including general and specialist physicians), the number of periods of sick-leave, and the total number of sick days. In the first column, the explained variable is the annual expenditures on dental care repaid by the national insurance system for the expenditures of individual i . Working in Alsace-Moselle has a significant impact on this amount. The national insurance repays on average 5.3 euros more for dental expenditures when the movers are in Alsace-Moselle compared to when they are in the rest of France, holding all else constant. The effect is a 10% increase, as the average annual dental care consumption of movers is around 50 euros (Table 3.2).

The treatment effect on annual expenditures for doctor's visits repaid by the national insurance system is not significant (Table 3.1). People moving in and out of the Alsace-Moselle regime do not change their level of expenditures for doctor's visits (including general practitioners and specialists) when they are subject to higher reimbursement rates.

When affiliated to the regional system, individuals decrease their consumption of pre-

Table 3.1: EFFECT OF THE ALSACE-MOSELLE REGIONAL SYSTEM ON MEDICAL CARE USE

	Dentist	Doctor	Medecines	Sick leaves	Sick days
Alsace-Moselle	5.259** (2.665)	-1.351 (3.746)	-19.81** (8.705)	0.0269* (0.0146)	0.503 (0.672)
unemployed	0.0215 (0.662)	-53.58*** (1.470)	-34.40*** (2.942)	-0.254*** (0.00369)	-26.03*** (0.307)
partly unemployed	-1.513*** (0.544)	-18.64*** (1.114)	-10.23*** (2.124)	-0.103*** (0.00324)	-7.672*** (0.185)
log income	1.407*** (0.233)	-32.06*** (0.629)	-25.48*** (1.409)	0.0139*** (0.00128)	-10.97*** (0.138)
income dummy	11.07*** (2.122)	-268.3*** (5.563)	-190.5*** (11.97)	-0.108*** (0.0109)	-91.61*** (1.181)
Constant	39.28*** (2.313)	504.8*** (6.281)	459.8*** (14.03)	0.257*** (0.0128)	121.2*** (1.374)
Nb. Ind	334,754	334,754	334,754	334,754	334,754
Nb. Obs	1,339,016	1,339,016	1,339,016	1,339,016	1,339,016

Notes: 2473 persons entered or left Alsace-Moselle, contributing to the estimation of the impact of the Alsace-Moselle regional system. Dependent variables: dentist denotes the annual amount repaid by the national health insurance system for dentist's visits, doctor denotes the amounts repaid for doctor's visits (general practitioners and specialists), medecines denotes the amount repaid for prescription medecines, sick-leave denotes the number of periods of sick-leave, sick-days denotes the annual number of sick-leave days. Additional controls included: years fixed effects, and controls for the effect of moving. Standard errors are clustered at the individual level.

scription medicines by about 20 euros per year. This represents about 14% of annual expenditures, if we compare it to the value in Table 3.2. People consume less medicine when they are in Alsace-Moselle, subject to higher public reimbursement rates.

When working in Alsace-Moselle, people take 2.7% more sick-leave periods of more than three days (Table 3.1). The effect is only significant at 10%. This is a 9% increase, as people changing their place of work take on average 0.30 sick-leave periods of more than three days per year (Table 3.2). However, the effect on the total number of sick-leave days is not significant. Individuals do not have more sick-leave days in total when subject to a more generous sick-leave policy.

To interpret our results, it needs to be borne in mind that we are not assessing the impact of an increase in the total reimbursement rate, but rather the impact of a change in the share reimbursed by the public insurance system.

According to economic theory, more generous reimbursement rates should lead to higher expenditures because some unmet medical needs become affordable. However, authors like Pauly (1968) also emphasize a moral hazard effect which is negative for public welfare. In our case, we find a decrease in the consumption of medicines. Our results are therefore not consistent with moral hazard on the consumption of medicines at least for the population of movers that we consider. Note also that in France, expenditures on medicines are particularly high (Sabban and Courtois (2007)), due to a culture of heavy consumption of medicines. Decreasing the consumption of medicines is an important concern for public policy (Cabut and Clavreul (2012)). On the other hand, we find an increase in dental care consumption. Dental care expenditures greatly depend on income, and inequalities in access to dental care are sizeable. Dental care is the first type of health care that is cited when people are asked about unmet needs for financial reasons, as Fantin et al. (2012) explain. Hence, this suggests that the increase in consumption of dental care observed in the data is not (mainly) driven by a moral hazard effect. Moreover, because reim-

bursement rates change for all items, the relative prices for patients are not the same in Alsace-Moselle as in the rest of France. Our results are consistent with the fact that the relative price of medicines compared to other health care related goods could be higher in Alsace-Moselle, explaining the decrease in consumption. Thus, consumption of medicines may be substituted for dental care and short sick-leave periods. People in Alsace-Moselle may consume less prescription medicines because they can consume more dental care and short sick-leave periods, which enables them to take less medicines to treat symptoms like antipyretics or painkillers, for instance.

The existence of unobserved aspects of the regional regime -such as differences in the supply of medical care- are alternative explanations of our results, but when we run the same regressions controlling for some measures of the supply of medical care our negative results on overall consumption are reinforced or unaffected. This robustness check is presented in the next Section.

3.6 Discussion and Robustness Check

3.6.1 Is mobility exogenous?

We use a difference-in-difference method, the treatment being affiliation to the regional system. Treated individuals change their affiliation because they change their place of work. Individuals from the control group also move, but do not change their affiliation to the regional system. This method relies on the exogeneity of mobility to Alsace-Moselle. As we use a fixed-effect panel data model, mobility does not need to be uncorrelated to time-invariant individual heterogeneity, but it does need to be uncorrelated to unobserved time-varying characteristics affecting health care expenditures. We checked whether we would obtain the same results while using a weaker assumption.

If individuals change their place of work to benefit from a more generous system when

they anticipate they will have high expenses, our estimates would be positively biased. This could be the case for dental health care consumption or the number of sick-leave periods, for which we find a positive impact for higher reimbursement rates. However, for medicines, we find a negative impact, so if it is biased, the real impact would be a larger negative impact.

To deal with this possible endogeneity, we ran the same model, but excluding people who move from or to neighboring areas (namely, *Lorraine*, or *Franche-Comté*, see Figure 3.A.2). If individuals go to a region which is far away, this would imply high costs in terms of housing or transport, and moving would be more costly than obtaining expensive private insurance with a large reimbursement rate. Excluding individuals who move from or to border regions, we keep only moves that are likely to be undertaken for other motives than reimbursement rates. For the control group to be comparable, we also select individuals in the control group who move to a region far away.

Table 3.1: EFFECTS WITHOUT MIGRATIONS TO NEIGHBORING REGIONS

	Dentist	Doctor	Medecines	Sick leave	Sick days
Alsace-Moselle	5.52	-0.086	-24.8*	0.039*	1.35
	(3.29)	(4.39)	(12.5)	(0.018)	(0.87)
Nb. Ind	319,445	319,445	319,445	319,445	319,445
Nb. Obs	1,277,780	1,277,780	1,277,781	1,277,780	1,277,780

Notes: Significance levels: * : 10% ** : 5% *** : 1%. N= 319925 individuals, including 2196 individuals changing system, t=4 years. Individuals moving to a neighboring region are excluded. Dependent variables: dentist denotes the annual amount repaid by the national health insurance for dentist's visits, doctor denotes the amounts repaid for doctors (general practitioners and specialists), medecines denotes the amount repaid for prescription medicines, sick-leave denotes the number of periods of sick-leave, sick-days denotes the annual number of sick-leave days. Additional controls are included: income, employment status, years fixed effects, and controls for the effect of moving. Standard errors are clustered at the individual level.

Table 3.1 presents the results for expenditures on dentist's visits, doctor's visits, consumption of medicines, number of sick-leave periods and total number of days of absence due to illness, and without close migrations.

The effects are similar to those which include close migrations, so that the effects observed do not appear to be driven by endogenous mobility. The standard errors are larger because the sample of movers contributing to the effect of Alsace-Moselle is smaller when movers to neighboring regions are excluded.

3.6.2 Robustness checks

In this section, we change the specification of the model to assess its robustness. We change the controls included, add information about health supply, and apply the same model, but only to the population of movers, excluding individuals who never move. The results obtained using these different specifications are similar to the original results. Table 3.2 presents the results for each specification and health spending outcome, and the baseline model for comparison.

In the first test, Row 2, we exclude time-variant individual characteristics (controls). In the second (Row 3) we add information about chronic diseases, which is the only information we have about health status: i.e. whether or not the individual is registered as having a chronic disease for which the public reimbursement rates are 100%. Row 4, we add information about the density of medical professionals. We can see that the sign and magnitude of results are similar in each case. The result on dental care is no longer significant when we include the density of physician. The result on the consumption of medicines remains significant at 5%, and the magnitudes do not change much. The results from the model where we exclude non-movers (Row 5) are also similar to baseline results. The overall negative impact of the Alsace-Moselle system on health expenditures is robust across alternative specifications.

In Row 4 of Table 3.2, we add the densities of medical professionals as controls. We did not add medical density in our main model because we think that reimbursement rates can impact in the long run on the density of physicians as supply responds to changes in

Table 3.2: ROBUSTNESS TO CHANGES IN CONTROLS INCLUDED AND IN THE SAMPLE USED

	(1) Dentist	(2) Doctors	(3) Medecines	(4) Sick-leave	(5) Sick days
Baseline model	5.26** (2.67)	-1.35 (3.75)	-19.8** (8.71)	0.027* (0.015)	0.50 (0.67)
No controls	4.98* (2.67)	-3.49 (3.71)	-22.3** (8.68)	0.028* (0.015)	0.034 (0.65)
With chronic diseases at t	5.26** (2.67)	-1.48 (3.74)	-20.1** (8.63)	0.027* (0.015)	0.56 (0.67)
With health care availability	4.59 (2.94)	-10.8** (4.37)	-18.6** (9.11)	-0.00069 (0.016)	-2.17*** (0.76)
Only movers	5.27** (2.66)	-2.74 (3.70)	-21.0** (8.70)	0.025* (0.015)	0.054 (0.64)

Significance levels: * : 10% ** : 5% *** : 1%. Standard errors are shown in parentheses. Nb Ind=334754, t=4 years. Only movers: Nb Ind=35176, t=4 years. With health care availability, we exclude 24 individuals for whom we do not have information about health care availability in the *département* of work. Nb Ind=334,754. Densities of physicians, dentists and pharmacies are used as proxies for health care availability. Sick-leave: number of periods of sick-leave. Sick-days: total number of sick-leave days

demand. On the other hand, our effects could be due to changes in the supply of care among regions that are unrelated to reimbursement rates. We check if the effects do not vanish when controlling for variability in the density of dentists, doctors and pharmacies.

We run the same model adding the density of doctors and pharmacies in each department in 2009⁷. We do not add doctors who are specialists and dentists because more than 85% of the variation in their density across regions is explained by the density of pharmacies and general practitioners. A model with the four densities used as controls does not change the coefficients a lot but it does substantially increase the estimated standard errors, reflecting high degrees of correlation among the four densities. Row 4 of Table 3.2 presents the results with density of physicians. The coefficient on dental care use is no

⁷Information about density is at the department level (*Direction de la recherche, des tudes, de l'valuation et des statistiques*)
Source : <http://www.drees.sante.gouv.fr/IMG/apps/statiss/default.html>

longer significant. The effect on the consumption of medicines is still significant at 5%. The annual number of sick days and expenditures on doctor's visits are now significant at 5%, and negative. When the availability of health care is held constant, working in Alsace-Moselle decreases the use of medicines, doctor's visits and sick days. The effect is -11 euros for doctor's visits, and two days less absence due to illness per year. These provocative results showing a lower consumption of medicines when the public repayment rate is higher cannot be explained simply by the supply side. On the contrary, taking into account the density of physicians and pharmacies leads to even more provocative results.

3.6.3 Placebo tests

We found an effect of changing the place of work on dental care consumption, and drug consumption in Alsace-Moselle. We now test whether individuals change their consumption when they move to other regions.

We run the same model on each region, except *Corse*, and overseas departments. This leaves all 21 regions of mainland France, except Alsace-Moselle. On average, if there is no effect for these regions, we should find approximately one region significant at 5%, and one at 10%, for each outcome. The *Lorraine* region includes Moselle, so we replace it by a region including all subregions of *Lorraine* with the exception of Moselle.

Given that a no impact would result in two significant regions (at 10%) for each item, and we obtain two regions for dental care and three for the consumption of medicines, our placebo tests are consistent with the hypothesis that regions other than Alsace-Moselle do not impact on consumption.

To obtain better insight into whether the effects we observed are due to regional particularities or to the regional regime, we perform the same model, but using people who change their place of living but not their place of work. The correlation between changing the place of work and the place of residence is quite low, at around 0.35. Individuals are

affiliated to the regional system if they work in Alsace-Moselle, regardless of where they live. We estimate the impact of moving to Alsace-Moselle only for individuals who do not change their affiliation to the regional system, that is, who do not change their place of work. These individuals are not subject to changes in the local system affiliation, but they are subject to possible changes in other regional aspects. If they are affected, this would mean that other aspects of the Alsace-Moselle region are driving our results. Detailed results are given in Appendix 3.B, Table 3.B.1.

The result for the consumption of dental care is similar (4.8), but no more significant. The effect on the consumption of medicines (-1.5) is much lower, and no longer significant. There is an impact of working in Alsace-Moselle on the consumption of medicines, but living in Alsace-Moselle has no impact. It seems that it is being affiliated to the regional system that matters, and not local particularities like differences in the supply of care, which would impact on people living in Alsace-Moselle as much as people working in Alsace-Moselle.

3.6.4 Decomposing the effects

We evaluate how the effects on medical care use vary depending on the direction of the move (from Alsace-Moselle, to Alsace-Moselle, and going back and forth), depending on the subregion (*département*) of Alsace-Moselle concerned by the move (*Haut-Rhin*, *Bas-Rhin* or *Moselle*), and depending on individual characteristics. The results can be found in Appendix 3.B.

The effects across regions and directions of the move have similar signs, but their magnitudes vary a lot. This is due to large standard errors, which makes the results difficult to interpret.

To obtain greater insight into the interpretation of our effects, and to see whether some particular groups of people drive the average impact we measure, we evaluate how the

effects vary with individual characteristics. We interact time-invariant characteristics with working in Alsace-Moselle. We run models to see how our effect varies with four main characteristics: health status (chronic diseases), age, gender and income. To summarize, the effect on medicines is higher for males, for people with chronic diseases, for people over forty and for high incomes. However, this analysis is conducted separately for each variable, and because these variables are positively correlated (at least for age, male and incomes) it is not clear which of them drives the heterogeneity of the effect of the treatment.

3.6.5 Effects on other measures of health care consumption

To evaluate the effects of changes in total expenditure on medical care, we use total expenditures for health care as dependent variables, instead of only the part repaid by the national insurance system. Table 3.B.4 in Appendix 3.B presents the results.

Concerning expenditures for dental care, the coefficient is no longer significant at 5%, and it is higher than the value for the share repaid. The effect on doctor's visit expenditures is not significant. The effect on expenditures on medicines is -21 , significant at 5%.

3.7 Conclusion

We assess whether moving to a region where the public health insurance is more generous impacts medical care expenditures, using an administrative panel dataset. Regressing health expenditures on the public insurance system, controlling for individual fixed effects, we find that individuals moving to a region with higher public reimbursement rates do not consume more care. There is even a decrease in overall consumption, driven by a decrease in prescribed drug consumption. Given the method we use, our results are valid for a specific population: individual who change their place of work.

Reimbursement rates of the public health insurance are not decisive factors affecting

medical care use in France, at least for a specific population of movers. Some elements, as expenditures at the hospital, are missing from our analysis. Measuring individual spending at the hospital is challenging, and we do not have a reliable measure in our dataset.

Our provocative results may be due to the duality between the public compulsory insurance and the complementary insurances. Indeed, the decision to take a complementary insurance depends on the generosity of the compulsory insurance and on the risk type of individuals. In theory, individuals with low risk never take a complementary insurance, individuals with high risk always take one and individual with intermediate risk take a complementary insurance in the less generous scheme. Ex-post, if the decision to consume health care depends on the total reimbursement rates (compulsory and complementary insurance), the correlation between reimbursement rates of the compulsory insurance and health care consumption may be negative or positive depending on the evaluation of their own risk by individuals and their ability to select the optimal contract of complementary insurance. Due to the lack of data concerning complementary insurances, we are not able to integrate these aspects in our present investigations. Studying the interaction between the public compulsory insurance reimbursement rates and complementary insurances would be a promising avenue for further understanding of health care use in France

Appendix

3.A Details about the regional system

PRESTATIONS	REGIME GENERAL	REGIME LOCAL	TOTAL
A. ENSEMBLE DES ASSURES			
1. Honoraires et indemnités de déplacement			
▪ Médecins et chirurgiens-dentistes – généralistes ou spécialistes – sages-femmes -----	70 %	20 %	90 %
▪ Auxiliaires médicaux -----	60 %	30 %	90 %
▪ Indemnités kilométriques -----	100 %		100 %
2. Actes techniques <120 €	70 %	20 %	90 %
▪ => 120 €	100 % - 18,00 €	18,00 €	100 %
3. Frais divers :			
▪ Médicaments :			
• Spécialités irremplaçables ou coûteuses	100 %		100 %
• Autres spécialités :			
- Vignette bleue -----	30 %	50 %	80 %
- Vignette blanche -----	65 %	25 %	90 %
- Vignette orange -----	15 %		15 %
▪ Autres frais pharmaceutiques (accessoires, pansements) -----	60 %	30 %	90 %
▪ Prothèses –orthopédie –optiques -----	60 %	30 %	90 %
▪ Examens de laboratoire : analyse et prélèvement effectuée par du personnel non praticien -----	60 %	30 %	90 %
4. Frais de transport -----	65 %	35 %	100 %
5. Hospitalisation :			
▪ Etablissements publics ou assimilés :			
• Honoraires des praticiens et auxiliaires médicaux, examens de laboratoire, frais de séjour -----	80 %	20 %	100 %
▪ Etablissements de soins privés			
• Honoraires des praticiens et auxiliaires médicaux, examens de laboratoire, frais de salle d'opération, forfait pharmacie, frais de séjour -----	80 %	20 %	100 %
• Médicaments remboursés en plus du prix de journée selon le type de médicaments (cf. A.3) ----	30 %,65 %	70 %,35 %	100 %
	100 %		100 %
6. Frais de cure thermale :			
▪ Hospitalisation médicalement justifiée -----	80 %	20 %	100 %
▪ Cure thermale libre :			
• Honoraires (forfait de surveillance, pratiques complémentaires) -----	70 %	20 %	90 %
• Frais d'hébergement, frais de transport -----	65 %		65 %
• Frais d'hydrothérapie -----	65 %	25 %	90 %
B. BENEFICIAIRES DU FONDS SPECIAL INVALIDITE ET FONDS DE SOLIDRAITE VIEILLESSE			
▪ Médicaments :			
• Spécialités irremplaçables ou coûteuses -----	100 %		100 %
• Autres spécialités :			
- Vignette bleue -----	30 %	50 %	80 %
- Vignette blanche -----	65 %	25 %	90 %
- Vignette orange -----	15 %		15 %
▪ Autres prestations : (honoraires – examens de laboratoire, prothèses, optiques, orthopédie, cure thermale) sauf frais de transport -----	80 %	10 %	90 %
▪ Frais de transport -----	100 %		100 %
▪ Hospitalisation -----	80 %	20 %	100 %

Figure 3.A.1: Share of expenditures repaid by the national regime, share repaid by the regional regime, and total public repayment rate for someone affiliated to the regional regime

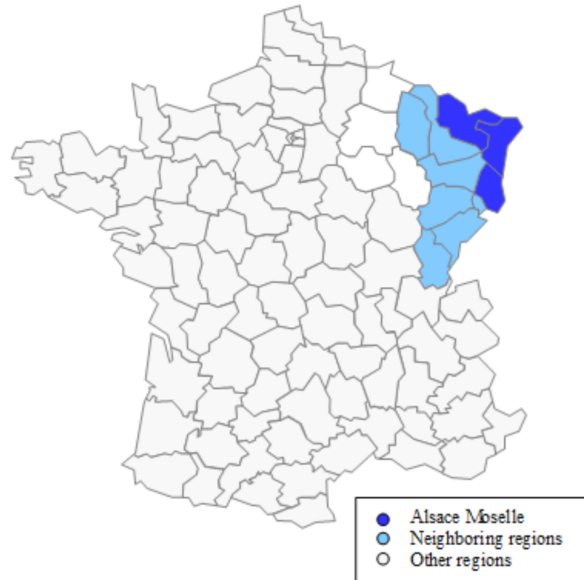


Figure 3.A.2: The Alsace Moselle region.

3.B Additional Regressions

3.B.1 Placebo tests using the place of living

Table 3.B.1: EFFECT OF LIVING IN ALSACE-MOSELLE ON MEDICAL CARE USE

	Dentist	Doctor	Medecines	Sick leaves	Sick days
Alsace Moselle	4.843 (5.570)	4.110 (10.11)	1.501 (9.319)	-0.0587* (0.0317)	-0.475 (1.260)
Nb. Ind	332,281	332,281	332,281	332,281	332,281
Nb. Obs	1,329,124	1,329,124	1,329,124	1,329,124	1,329,124

Notes: Significance levels: * : 10% ** : 5% *** : 1%. We exclude individuals who change their affiliation to the regional system from these regressions. 1005 individuals change their place of living from or to Alsace-Moselle, without entering or leaving the regional system (either always or never working in Alsace-Moselle). Dependent variables: dentist denotes the annual amount repaid by the national health insurance scheme for dentist's visits, doctor denotes the amounts repaid for doctors (general practitioners and specialists), medecines denotes the amount repaid for prescription medecines, sick-leave denotes the number of periods of sick-leave, sick-days denotes the annual number of sick-leave days. Additional controls are included: income, employment status, years fixed effects, and controls for the effect moving. Standard errors are clustered at the individual level.

3.B.2 Heterogeneity of the effects: the direction of the migration

To see how the effect varies if we focus on people entering and leaving the Alsace-Moselle regional regime, we split people moving into three groups: those moving from Alsace-Moselle (N=1059), those moving to Alsace-Moselle (N=874), and those who both enter and leave the system during the period (N=540). Belonging to each group is time-invariant. The model is the following:

$$y_{it} = \beta_1 \text{Entering}_i * \text{AM}_{it} + \beta_2 \text{Leaving}_i * \text{AM}_{it} + \beta_3 \text{Back and forth}_i * \text{AM}_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},$$

Table 3.B.2 presents the results.

Table 3.B.2: EFFECT ACROSS DIRECTIONS OF THE MOVE

	Dentist	Doctors	Medecines	Sick-leave	Sick days
Entering AM	8.90* (4.61)	-0.43 (6.91)	-39.8** (19.8)	0.0092 (0.024)	-0.083 (1.14)
Leaving AM	3.79 (4.39)	-4.76 (5.83)	-11.8 (12.3)	0.028 (0.025)	0.95 (1.16)
Both	2.23 (4.65)	3.82 (6.72)	-3.05 (5.05)	0.053** (0.026)	0.58 (1.07)
Test	0.57	0.63	0.17	0.48	0.82
Nb. Ind	334,754	334,754	334,754	334,754	334,754
Nb. Obs	1,339,016	1,339,016	1,339,016	1,339,016	1,339,016

Significance levels: * : 10% ** : 5% *** : 1%. Standard errors are shown in parentheses

Leaving AM: effect of the local system for someone who left it, 1,059 individuals left Alsace-Moselle. Entering AM: effect for someone who entered the system, 874 individuals entered Alsace-Moselle. Both: effect for someone who left and entered the system during the period, 540 individuals entered and left Alsace-Moselle. The average period spent in Alsace-Moselle is similar across groups: around 2 years. Test:p-value of testing equality of the three coefficients. Sick-leave: number of periods of sick-leave. Sick-days: total number of sick-leave days

For all outcomes, the difference between β_1 , β_2 and β_3 is not significant, due to the large standard errors.

3.B.3 Heterogeneity of the effects: subregions (*Département*)

To assess if the effects are homogeneous across departments, we divide people moving into three categories, corresponding to the three departments of Alsace-Moselle: Haut-Rhin,

Bas-Rhin and Moselle. The model is the following:

$$y_{it} = \beta_1 Haut-Rhin_{it} + \beta_2 Bas-Rhin_{it} + \beta_3 Moselle_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},$$

Table 3.B.3 presents the coefficients on each subregion of Alsace-Moselle, and a test of quality of those coefficients, for each outcome.

Table 3.B.3: EFFECT ACROSS ALSACE-MOSELLE SUBREGIONS

	Dentist	Doctors	Medecines	Sick-leave	Sick days
hautrhin	9.36** (4.65)	-7.63 (5.83)	-20.8** (10.2)	0.027 (0.027)	-0.40 (1.08)
basrhin	1.22 (4.10)	-6.51 (5.15)	-33.4** (16.7)	0.030 (0.020)	0.092 (0.96)
moselle	6.69* (3.59)	9.87* (5.60)	-4.63 (6.74)	0.033 (0.022)	1.94* (1.03)
Test	0.26	0.039	0.12	0.99	0.23
Nb. Ind	334,754	334,754	334,754	334,754	334,754
Nb. Obs	1,339,016	1,339,016	1,339,016	1,339,016	1,339,016

Significance levels: * : 10% ** : 5% *** : 1%. 895 individuals entered or left *Haut-Rhin*. 1,375 individuals entered or left *Bas-Rhin*. 1,166 individuals entered or left *Moselle*. Standard errors are shown in parentheses. Test:p-value of testing equality of the three coefficients

Sick-leave: number of periods of sick-leave. Sick-days: total number of sick-leave days

For every outcome, the effects are not significantly different.

3.B.4 Heterogeneity of the effects: individual characteristics

We assess whether some groups of people drive the effects we measure. It must be borne in mind that the results are hard to interpret due to large standard errors. Detailed results are available upon request.

We run models to see how our effect varies with four main characteristics: health status (chronic diseases), age, gender and income.

Health status We split the sample between individuals registered as having at least one chronic disease during the four years, and individuals never suffering from chronic disease. The model we estimate is the following:

$$y_{it} = \beta_1 \mathbf{1}_{(\text{Chronic disease})_i} * \text{AM}_{it} + \beta_2 \mathbf{1}_{(\text{No chronic disease})_i} * \text{AM}_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},$$

For the consumption of medicines, the impact is much higher for individuals having at least one chronic disease during the period. It is -220 , significant at 5%, whereas it is -2 , not significant, for individuals having no chronic disease during the four years. Concerning dental care, effects are more alike: for individuals with a chronic disease, it is 7.6 - which is not significant - whereas for individuals with no chronic disease, it is 5.1, which is significant at 10%.

Age We split the sample of movers who entered or left Alsace-Moselle into five groups of equal size, based on their average age across the four years. Our group of movers from or to Alsace Moselle is made up of individuals who are, on average, between 25 and 70. The model we estimate is the following:

$$y_{it} = \beta_1 \mathbf{1}_{(25 \text{ to } 28)_i} * \text{AM}_{it} + \beta_2 \mathbf{1}_{(29 \text{ to } 33)_i} * \text{AM}_{it} + \beta_3 \mathbf{1}_{(34 \text{ to } 39)_i} * \text{AM}_{it} + \beta_4 \mathbf{1}_{(40 \text{ to } 46)_i} * \text{AM}_{it} \\ + \beta_5 \mathbf{1}_{(47 \text{ to } 70)_i} * \text{AM}_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},$$

The impact of the Alsace-Moselle regional system is concentrated among older individuals (over 40), for whom it is the highest (β_4 and β_5 around -50).

For dental care, results are concentrated among individuals over 47, for whom it is significant at 1%. The coefficient is the highest (19) for individuals over 47. The other coefficients are not significant.

Gender We run the following model.

$$y_{it} = \beta_1 \mathbf{1}_{\text{Women}_i} * \text{AM}_{it} + \beta_2 \mathbf{1}_{\text{Men}_i} * \text{AM}_{it} + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},$$

We find that the effect on the consumption of medicines is higher for men (-27 , versus -8 for women). It is significant for men only. The effect on dental care consumption is similar for men and women (4.7 and 6.1), and not significant.

Income We run the following model

$$\begin{aligned}
 y_{it} = & \beta_1 \mathbf{1}_{(0 \text{ to } 6,970)_i} * AM_{it} + \beta_2 \mathbf{1}_{(6,971 \text{ to } 12,850)_i} * AM_{it} + \beta_3 \mathbf{1}_{(12,851 \text{ to } 19,005)_i} * AM_{it} \\
 & + \beta_4 \mathbf{1}_{(19,006 \text{ to } 28,159)_i} * AM_{it} + \beta_5 \mathbf{1}_{(\text{more than } 28,160)_i} * AM_{it} \\
 & + \sum_{k=-3}^2 \delta_k w_{kit} + \gamma x_{it} + \phi_t + \alpha_i + \varepsilon_{it},
 \end{aligned}$$

The impact on the consumption of medicines is concentrated among the richest individuals, who earn above 28,160 euro per year, for whom the coefficient, -72 , is significant at the 5% level. For other groups, the coefficient is not significant.

Concerning dentist's visits, the coefficient is between 3.8 and 7.8 for all groups; it is never significant.

3.B.5 Effects on other measures of health care consumption

Table 3.B.4: EFFECT OF THE ALSACE MOSELLE LOCAL SYSTEM ON MEDICAL CARE USE

	Dentist	Doctor	Medecines
Alsace Moselle	17.43*	-0.601	-21.02**
	(10.55)	(5.485)	(9.118)
Nb. Ind	334,754	334,754	334,754
Nb. Obs	1,339,016	1,339,016	1,339,016

Notes: Significance levels: * : 10% ** : 5% *** : 1%. 2473 individuals entered or left Alsace-Moselle. Dependent variables: dentist denotes the annual amount spent for dentist's visits, doctor denotes the amount spent for doctors (general practitioners and specialists), medicines denotes the amount spent on prescription medicines, sick-leave denotes the number of periods of sick-leave. Additional controls included: income, employment status, years fixed effects, and controls for the effect moving. Standard errors are clustered at the individual level.

Chapter 4

Regional Purchasing Groups and Hospital Medicine Prices: Evidence from Group Creations

Abstract

This paper estimates the impact of group purchasing on medicine prices in French hospitals, taking advantage of the entry of hospitals into regional purchasing groups between 2009 and 2014. I use a unique database providing the average annual prices paid for all innovative and costly medicines in public hospitals. Using a fixed effects model that controls for hospitals' medicine-specific bargaining abilities and medicine-specific price trends, I find that group purchasing reduces prices of medicines in oligopoly markets, but has no impact on the prices of medicines with no competitors.

4.1 Introduction

Medicine prices are major concerns for public policy, and regularly give rise to controversies¹. In particular, recent reports have raised awareness about the quick increase of hospital inpatient medicine prices - NORC (2016) in the United-States, HSCIC (2015) in England, Duhamel and Morelle (2012) in France.

In European countries, the prices of retail medicines are negotiated with manufacturers and set at the national level, often based on international comparisons (Kanavos et al., 2011). On the opposite, prices in the inpatient sector are based on tendering or negotiations, often by individual hospitals or hospitals purchasing groups. Individual negotiations between a single hospital - or hospital groups - and a manufacturer take place for instance in Austria, Germany, Finland, France, the United-Kingdom, Hungary and in the United-States - Vogler et al. (2012), Grennan (2014). Pharmaceutical companies can therefore engage in third-degree price discrimination by offering different unit prices to different hospitals.

This paper estimates the impact of purchasing groups on prices hospitals pay for innovative medicines using a natural experiment: purchasing group creations. Controlling for medicine-specific hospitals' bargaining ability, I find that regional purchasing groups enable hospitals to get lower prices for medicines in oligopoly markets, and do not change the prices of medicines in monopoly markets.

Innovative medicines are either the only treatment for a given disease, or one of the few treatments available. In France, the decision to prescribe is regulated by national guide-

¹For instance Jirillo et al. (2008) on the price of an expensive oncological medicine, Avastin. Alpern et al. (2014) on the increase of old generic medicines prices in the United-States, or Trooskin et al. (2015) on prices of Hepatitis C medicines, which are rationed by Medicaid because of their high prices.

lines - the agreements on good use of care (Villepin, 2005) - and medicines are directly reimbursed to hospitals by the National Health Insurance. The fact that hospitals can buy a medicine and get a reimbursement makes hospitals solvent for innovative medicines - as long as the purchase price is not above the reimbursement rates. In this setting, the pharmaceutical companies face inelastic demands and little constraints on pricing, especially when there are no competitors.

The wish to increase hospitals' bargaining power has been one of the reason for hospitals to engage in group purchasing (Vincent and Montalan, 2013). Since the 2000s, French hospitals are incentivized to engage in group purchasing, with several programs - Operah in 2006, PHARE in 2011, (Vincent and Montalan, 2013). A national purchasing group with all university hospitals was created in 2005. In the late 2000s, one by one, most hospitals joined a purchasing group. These policies toward massification of purchases have two main objectives: to professionalize procurement and share buying costs, and to reduce and standardize purchase prices. The move toward group purchasing has also taken place in other countries, for instance in the United-Kingdom, Germany, and Italy (Sorenson and Kanavos, 2011), Austria, Finland, Ireland, the Netherlands, Portugal, Sweden (Vogler et al., 2010).

However, the theoretical effects of purchasing groups on negotiated prices remain unclear, and the scarce evidence shows mixed results - Burns and Lee (2008) survey hospital directors who report a decrease in prices for some products, but Grennan (2013) suggests that prices could increase.

The market structure for innovative medicines is characterized by low competition, as firms selling innovative medicines to hospitals are either monopolies or oligopolies. Theo-

retical predictions are not clear-cut, and depend on whether a firm has competitors. If a pharmaceutical company is a monopolist and it can price-discriminate, the optimal pricing strategy is to give each buyer a price equal to its willingness to pay (Varian, 1989). In this case, moving to more uniform prices would have a heterogeneous impact on the prices. The new price would lie between the highest and lowest price hospitals had paid under discrimination (Stole, 2007).

In an oligopoly setting, the impact of more uniform prices is more ambiguous and depends on demand symmetry. Demand is symmetric when companies agree about the ranking of high-price markets and low-price markets - the strong markets and the weak markets. If demand is symmetric, the situation is similar to price discrimination in the monopoly case, and a shift to more uniform prices decreases the average price in the strong market and increases the average price in the weak market- Corts (1998), Stole (2007), Armstrong (2006). However, these theoretical results do not take into account the possible changes in the bargaining powers, which are likely to matter for innovative medicines whose prices are negotiated (Vogler et al., 2012). The importance of bargaining power is underlined in Grennan (2013). To the best of my knowledge, Grennan (2013) is the only paper studying the impact of group purchasing for hospitals using actual data on transaction between firms and hospitals. The author studies the impact of more uniform prices on the market for medical devices - the coronary stent industry - in the United States by simulating hospital mergers. The author points out that hospitals' demands for stents are asymmetric and that consequently, more uniform pricing would soften competition and increase prices. Therefore, there must be an important gain in bargaining power in order to compensate this effect and for prices to be lower with group purchasing.

This paper allows to assess the net impact of these two effects: the changes in competi-

tion levels and the changes in hospitals' bargaining powers. Contrary to Grennan (2013), I use a situation where we have information about hospital prices for medicines before and after the creation of a purchasing group to account for heterogeneity across hospitals. I use the whole market of innovative medicines, and then measure the effects for two markets with different levels of competition. Demands are more likely to be symmetric across hospitals for innovative medicines than for stents because medicines are not physician preference items - contrary to implantable medical devices such as stents, which are mainly chosen by surgeons (Lerner et al., 2008).

If preferences are symmetric, more uniform prices would lead to price decreases for hospitals with high willingness to pay, and prices will rise in hospitals with low willingness to pay. In addition, the changes in bargaining power suggested by Grennan (2013) are likely to play an important role, as both the hospitals and the pharmaceutical company can have market power. Group purchasing may increase bargaining power for all hospitals which are in the newly created group, which would decrease average prices.

I use a unique dataset containing information on prices each hospital obtained for a group of innovative medicine - accounting for about half of the expenditures for medicines in hospitals. This is one of the few databases with actual buyer-supplier transfers in a business-to-business market. Hospitals entering into purchasing groups enable me to identify the impact of belonging to a purchasing group on price negotiation. There are different levels of purchasing groups: local, regional or national. The available data on hospital prices spans five years, from 2009 to 2014. Most hospitals belong to local and regional groups during this period. In order to be able to exploit hospital entries, I focus on the regional groups. This level of group purchasing is by far the most common among public hospitals today, except for the thirty-two university hospitals.

The paper is structured as follows. In Section 4.2, I describe the institutional setting and my database. Section 4.3 and Section 4.4 present the identification strategy and the descriptive statistics. Section 4.5 shows the results. The final Section concludes.

4.2 Medicine Expenditures in Hospitals: Context and Data

This section gives information about the funding of innovative and expensive medicines used in this paper.

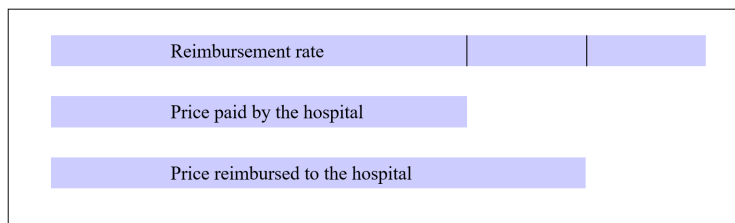
4.2.1 The outside-HSG list

Patients do not pay for medicines they get when hospitalized. Hospitals buy medicines through a tendering process, which can be specific to a particular medicine, or to a therapeutic class. Medicines are used for a patient in a given homogeneous stay group (hereafter HSG). Since 2005, and the implementation of activity-based payment, most medicines are funded through the fees the National Health Insurance pays to the hospital for each HSG. However, some medicines have emerged as exceptions to the general rule and are not reimbursed through HSGs. They are reimbursed separately on the basis of the purchasing volumes and prices transmitted to the National Health Insurance. Given the incentive hospitals have to report this information for reimbursement purposes, the National Health Insurance has accurate and exhaustive information on prices. More details about hospital funding and the implementation of activity-based payment can be found in Appendix, Section 4.A.

This paper focuses on medicines from the *liste en sus* - also called *Liste hors GHS* (outside HSG list). This list comprises medicines for which funding through the HSG is not a good fit, because they are expensive and not systematically used for a given HSG, which could lead to important cost heterogeneity within a HSG. The outside-HSG list aims to ensure equal access to costly treatments across regions and hospitals. It also stimulates innovation by helping to fund new and expensive medicines. The medicines from this list accounted for 55% of medicine expenditures in hospitals in 2011, and their share in hospital medicine expenditures is constantly increasing (Comité économique des produits de santé, 2011). To be included in the list, a medicine has to be classified as useful by the National Health Authority (HAS), it must be significantly more expensive than the rest of medicines in the same HSG, and not systematically used in a given HSG.

When a medicine enters the list, the pharmaceutical firm producing it and the Economic Committee for Health Products (CEPS) negotiate to set a fixed reimbursement rate (*Tarif de responsabilité*). These reimbursement rates were implemented in 2005, and contributed to a convergence of medicine prices across hospitals (Degrassat-Théas et al., 2012). The National Health Insurance reimburses the hospital for the medicine based on the following formula, and illustrated by Figure 4.2.1.

Figure 4.2.1: Reimbursement of medicines of the outside-HSG list



$$\text{Reimbursement} = \frac{\text{price} + \text{rate}}{2}$$

If the hospital buys at a price lower than the reimbursement rate, the mark-up is shared between the National Health Insurance and the hospital, and the National Health Insurance reimburses the average between the fixed rate and the price the hospital pays. If the hospital buys above the reimbursement rate, the National Health Insurance repays the reimbursement rate. In practice, this situation is rare - in my database, less than 5% of medicines prices are more than 5% above the reimbursement rate. This system aims to split the gain from price negotiation between the hospitals and the National Health Insurance, while maintaining an incentive for the hospitals to obtain low prices.

4.2.2 Data

I use a database with information about prices and quantities of medicines from the outside-HSG list bought by public hospitals between 2008 and 2014. In order to be reimbursed, hospitals report prices and quantities of medicines dispensed on the outside-HSG list to the Technical Agency of Information on Hospitalization (*Agence technique sur l'hospitalisation*, ATIH). Price reporting is compulsory since July 2008, therefore I exclude all observations of year 2008 from my sample. There are 714 hospitals in the database, including 83 local hospitals, 31 university hospital, and 19 centres for cancer research.

The ATIH database contains one line for the use of a medicine for a given patient at a given hospital. It includes both medical product identifiers and patient identifiers. The medicine identifier is the Common Unit of Dispensation code (CUD), which identifies a

given chemical or biological substance, produced by a given pharmaceutical company in a given pharmaceutical form.

I merged this data with several public datasets, thanks to the hospital and medicine identifiers. Data on hospitals include quality indicators from Hospidiag. With the CUD code, I could merge the database with information about when the medicine was on the Outside-DRG list, thanks to the database provided by the “Omedit Centre” (Regional Monitoring Centre for Medicines, Centre Region). I also added the Anatomical Therapeutic Chemical (ATC) code, and the pharmaceutical company that produces it. This information was collected on the National Health Insurance website and on the “Club Inter Pharmaceutique” website (Pharmaceutical Industry’s Association).

Information about which hospital is part of a regional or national purchasing group does not appear in the ATIH database. While the list of some purchasing groups’ members of some groups can be found on their website, information about each hospital’s accession date is not public. I have collected the information on group purchasing by contacting each regional purchasing group. The list of regional purchasing group comes from the website *alliance-groupement.fr*, a network of regional purchasing groups. I was able to collect information on group membership and year of entry for 150 hospitals in eight regional purchasing groups. Four groups were created between 2009 and 2014. In addition, there are three regions and 76 hospitals where there are no regional purchasing group in 2016: hospitals in these regions are never members of a regional purchasing group.

4.3 Empirical Strategy and Estimator

4.3.1 Main model

My database is a three dimensional panel, with one observation per hospital, per medicine and per year. I take advantage of this structure to build a model that controls for the potentially heterogeneous effects of hospitals and time on medicine prices.

My model includes a medicine-hospital fixed effect. This allows for a heterogeneous impact of hospital on the price of each medicine. The estimation is different from having both medicine and hospital fixed effect if and only if the impact of hospital on medicine prices depends on the medicine. This is likely to be the case, as the bundles of medicines bought vary across hospitals, and hospitals could exert more pressure in the price negotiation of the medicines they use the most.

The other set of fixed effects taken into account in the model are price - medicine fixed effects. Medicine prices vary across time. Annual prices of medicines can vary for many reasons - a new competitor, changes in population health. Since hospitals are more likely to be in a regional purchasing group over time, macro changes of medicine prices could introduce a bias in my estimates. I use medicine - year fixed effects to account for these confounding factors. These fixed effects are equivalent to including a medicine-specific time trend, with a fully flexible impact of time on each medicine considered.

These two sets of fixed effects encompass hospital fixed effects (through the set of hospital-medicine fixed effects), year fixed effects (through the medicine-year fixed effects), and medicine fixed effects (through both). In the end, what I measure is how the price of a medicine in a specific hospital varies when this hospital belongs to a regional purchasing

group (in percentage points), and compare this variation to the variation of price for the same medicine in other hospitals this year. In a robustness check, I show how the results vary when we use different fixed effects.

I estimate the following model:

$$\ln p_{ijt} = \alpha_{ij} + \beta_{jt} + \gamma \text{group}_{it} + \varepsilon_{ijt} \quad (4.1)$$

Where $\ln p_{ijt}$ denotes log price of medicine j in hospital i at year t , α_{ij} the hospital - medicine fixed effect, β_{jt} the medicine - year fixed effect, and ε_{ijt} the effect of unobserved variables. Standard errors are clustered at the hospital level.

Because of the inclusion of hospital - medicine fixed effects, which encompass hospital fixed effects, identification relies on hospitals joining a regional purchasing group during the period 2009-2014. Most hospitals join a group when it is created, therefore the year at which they join the group is unlikely to be correlated with variation in hospital characteristics. The main assumption for estimating γ is the exogeneity assumption:

$$\mathbb{E}(\varepsilon_{ijt} | \alpha_{ij}, \beta_{jt}, \text{group}_{it}, t) = 0$$

for $t = 2009, \dots, 2014$

Fifty-four hospitals in my sample join a regional purchasing group during the period and appear in the database both before and after joining the group. Hospitals never changing group membership contribute to the estimation only through the estimation of medicine-year fixed effects on prices.

4.3.2 Alternative specifications

The main hypothesis made in this paper is the exogeneity of group creation, given hospitals time-invariant characteristics. Two alternative hypothesis could be made, (1) group membership does not depend on hospitals characteristics, (2) group creation depends on time-varying hospitals' characteristics.

If group creation is exogenous, the accurate model would be a regression with medicine-specific time trends but not hospital-medicine specific fixed effects (Pooled Ordinary Least Square).

$$\ln p_{ijt} = \beta_{jt} + \gamma \text{group}_{it} + \varepsilon_{ijt} \quad (4.2)$$

Where $\ln p_{ijt}$ denotes log price of medicine j in hospital i at year t , β_{jt} the medicine - year fixed effect, and ε_{ijt} the effect of unobserved variables.

If group creation is endogenous, and depends on time-varying factors that also impact medicines prices, an instrumental-variable strategy should be used. A good instrument would be a time-varying variable that impacts group creation, but has no effect on medicines prices. Hospitals could decide to enter groups when their financial situation is deteriorating for other reasons than changes in medicines prices, and they need the help of the purchasing group to manage to buy cheaper medicines. To investigate this possibility, I run the following equation:

$$\text{Group member}_{ijt} = \alpha_{ij} + \beta_{jt} + \gamma F_{i,t-1} + \varepsilon_{it} \quad (4.3)$$

Where Group member_{it} is a dummy with value one when hospital i is in the group at year t , α_{ij} are hospital-medicine fixed effects, and β_{jt} are medicine-year fixed effects. $F_{i,t-1}$ are

financial indicators at $t - 1$.

The information about hospitals' finances come from Hospidiag, a public database on hospitals' performance. Nine financial indicators are available from 2009 to 2014: (1) the mark-up the hospital makes, which enables it to finance its investment, (2) the ability to invest, (3) financial independence, (4) investment intensity, (5) the wear and tear rates of equipment and (6) buildings, (7) the need of funding arising from routine operation, (8) resources stability, (9) the risk that receivables owed to the hospital will not be recovered, and (10) the average payment term. I take lagged indicators to avoid reverse causality. The F-test of joint significance of the financial indicators is 0.67. Therefore, hospitals' finances are unrelated to group membership, and cannot be used as excluded instruments. In addition, anecdotal evidence from regional purchasing group managers suggests that group membership is not linked to any hospitals characteristics, which makes the use of instrumental variables challenging.

4.4 Descriptive Statistics

4.4.1 Sample selection

The medicines I use in this study are all innovative and expensive, and competition is relatively low. Degrossat-Théas et al. (2012) study the evolution of oncologic drugs prices from the *Liste hors-GHS* between 2004 and 2007. They show that prices have become more homogeneous. With the implementation of the fixed reimbursement rates, negotiation has moved from the hospital level to the national level, and few hospitals manage to get a price lower than the national rate when a firm has no competitors. This is why I expect to find no impact on medicines that do not have competitors. Some medicines have several therapeutic indications, and can be the only treatment for a specific disease, but in competition with other medicines for another pathology. I use a sample of medicines for

which I can identify whether the market is an oligopoly or monopoly.

I rely on the Anatomical Therapeutic Chemical (ATC) Classification System, developed by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOCC). The ATC classification sorts active ingredients of medicines, according to their therapeutic, pharmacological and chemical properties. The ATC code has different levels of detail. The more general is the anatomical main group, which has fourteen categories. The bottom-level ATC code is the substance level, identified with seven letters and digits. For instance, infliximab is a chemical substance identified by the code “L04AB02”, where “L” is the anatomical group, antineoplastic and immunomodulating agents, “L04” is the therapeutic subgroup, Immunosuppressants, and “L04AB” stands for Tumor necrosis factor alpha (TNF- α) inhibitors, the chemical subgroup (WHO Collaborating Centre for Drug Statistics Methodology, 2012).

In my database, three groups of this first level account for 96% of the observations: group “B”, Blood and blood forming organs (35%), group “J” antiinfectives for systemic use (15%) and group “L”, antineoplastic and immunomodulating agents (46%). Regarding the bottom-level, I have 120 distinct categories. As for the CUD code, for Infliximab, there is a single CUD: “REMICADE 100 mg powder for solution for de 20 ml bottle-infusion ”, Remicade being the brand name.

I want to identify CUD codes for which there is only one pharmaceutical company on the market, in a monopoly situation, and CUD codes that have substitutes produced by other firms. Hereafter, I will call these groups “monopoly medicines” and “oligopoly medicines”. An important question while doing so is the definition of the market, which could be the most detailed ATC level, i.e. substance level, or the chemical subgroup level

(ATC fourth level). Some medicines will be substitutable with other medicines with the same chemical subgroup, while some are not substitutable at the fourth level. I compute the Herfindahl-index at these two levels. The Herfindahl Index is the sum of each company's market share squared.

$$HI = \sum_{i=1}^n a_i^2$$

where a_i is the market share of firm i and n the number of firms. If a firm is alone in a market, the Herfindahl Index will have the value one. If n firms have equal market shares, the Index will be $\frac{1}{n}$. Graph 4.B.1, in Appendix, shows the distributions of both indices. I identify as monopolies medicines which have no competitor at both levels, or where a single firm covers more than 95% of the market at the substance level and at the ATC 4 level - Herfindahl Index greater than 0.9. On the opposite, I identify as oligopolies the medicines that have competitors at the substance level - Herfindahl Index lower than 0.7. With this classification, I remain agnostic about which medicines are substitutes at the ATC 4 level, because I exclude medicines for which there are competitors at the ATC 4 level but no competitors at the substance level. I exclude from the groups medicines whose status (monopoly or oligopoly) changes over time, and medicines that appear in less than fifty hospitals. I do so because my estimates rely on comparisons of prices across time and hospitals. In the end, the "monopoly medicine" group corresponds to 15% of the sample, and the "oligopoly" group accounts for 20%. In the monopoly group, 74% of observations are antineoplastic agents, 14% are immunostimulants, and 10% are antimycotics. Half of the "oligopoly" group are biosimilar versions of a biological medicine - erythropoietin, or EPO. Erythropoietin is a hormone controlling blood cell production; it is used to treat anemia. Polyvalent immunoglobulins of human plasma origin, account for another 40% of the "oligopoly" group. The rest of the competition group is made of coagulation factors.

Among the eight substances associated with the largest expenditure in hospitals (“ANSM”, 2012), three belong to the monopoly group, and two are in the oligopoly group.

As for hospitals, I only consider hospitals which are not university hospitals or centres for cancer research - which all belong to national purchasing groups - or local hospitals, which buy few expensive drugs. This leaves a total of 570 hospitals. I drop hospitals for which regional group membership is missing, 250 hospitals remain. I withdraw then hospitals that belong to the national purchasing group of university hospitals even though they are not university hospitals. Most hospitals join the groups the year of their creation, or the year of the first common purchase - usually one year after the group creation. I exclude the five hospitals which joined a group after it was created. My final sample is comprised of 251 hospitals, among which 52 changed group membership during the period. Given the medicines and hospitals selection, the final sample is comprised of 45,000 observations, with 6,800 observations for monopoly medicines and 9,100 for oligopoly medicines.

The measure of the effect of belonging to a regional purchasing group relies on hospitals that enter a group between 2009 and 2014. They are in the treatment group. The other hospitals of the final sample form the control group, they are used to control for trends in medicine prices.

4.4.2 Hospitals’ characteristics

The estimation relies on different groups of hospitals. First, some hospitals are included in the sample, and some are excluded, mainly because of missing information on group membership. We can see in Appendix, Section 4.B a comparison of hospitals in and out of the sample - Table 4.B.1. Among hospitals in the final sample, some are in the treatment group, some are in the control group. Table 4.B.2 compares characteristics of hospitals in

the treatment and control groups. Finally, among hospitals in the control group, whose purchasing group membership does not change over the period, some are always in groups and some are never. Table 4.B.3 presents the characteristic of hospitals always in a regional group, i.e. the ones in the first regions where groups were created, versus the hospitals never in a group.

I use nine indicators. The first one is a measure of total purchases at median prices, which measures the size of the hospital-buyer. Then, I present the shares of quantities of pills in each of the three major anatomical groups. Finally, I present indicators from *Hospidiag*, which provides measures of hospitals' activity and performance. I choose five other indicators, the first one being oncology - the percentage of activity in oncology -, because most medicines of the "liste en sus" are oncological drugs. The four other indicators are quality indicators: technicality, severity, emergency, beds occupancy. Technicality is the share of patients who are mainly (but not exclusively) treated in university hospitals. This is a measure of the complexity of the hospitals' case-mix. For each homogeneous patient group, there are four levels of severity. The indicator "Severity" is the share of patients classified in the highest levels, three and four. Emergency measure the share of activity coming from the emergency department, which has an impact on the hospitals' planning, and is a proxy for low attractiveness. Finally, I use beds occupancy rates in medicine, to proxy for the efficient use of resources.

Table 4.B.1 shows how hospitals in the sample compare to hospitals not included in the sample, excluding university hospitals and centres for cancer research. The hospitals "Out of the sample" are mainly hospitals with missing information about group membership. The comparison of hospitals in and out of the sample gives insights about the external validity of the results. We see that there are no significant differences in the nine indicators

used, except for the fraction of pills bought which are blood and blood forming organs, more used in the sample.

Within hospitals included in the sample, Table 4.B.2 presents characteristics of hospitals entering a group and hospitals not entering - either always in a group or never. This is a comparison of my treatment and control group. There are no significant differences, except the fraction of pills of antineoplastic and immunomodulating agents, which is higher in the treatment group. This difference is not likely to bias the results, because I use a model that measure price variation within a given medicine. Overall, hospitals in the treatment and control groups are similar, which makes the common trend assumption credible.

To see whether group membership is related to hospitals characteristics, I compare hospitals in regions with the first regional purchasing groups, which were created before 2009, to hospitals never in a group which regional groups are not yet created. Table 4.B.3 presents the results. The two groups of hospitals are very similar, none of the differences are significant at the 5% level.

4.4.3 Price variations

This section shows how prices have changed in the different groups considered. First, to have a general idea about price variations during the period, we can see the variation of prices in hospitals never in a group and always in a group, and the variations in average reimbursement rates. Hospitals never in a group or always in a group will be used to have average annual prices, to estimate the counterfactual evolution of prices without change in group membership. Figures 4.B.2, 4.B.3 and 4.B.4, in Appendix, Section 4.B.1 show that during the period, average prices in hospitals always and never in groups have increased. The patterns in the two groups are close, and similar to the evolution of average

reimbursement rates. Comparing Figures 4.B.3 and 4.B.4 enables to see that on average, hospitals manage to get lower prices than the reimbursement rates for medicines identified as oligopolies, but not for medicines in monopolies. For the latter, the prices are very close to the reimbursement rates. This shows that reimbursement rates are used as a benchmark for price bargaining both by hospitals and pharmaceutical firms. For monopoly medicines, bargaining over prices is mainly centralised and happens when the reimbursement rates are negotiated. On the opposite, for oligopoly medicines, some hospitals manage to get prices lower than reimbursement rates, which enable them to get a margin.

I also show how prices have moved in hospitals used as a treatment group - hospitals entering a regional purchasing group between 2009 and 2014 - and compare them to the control group - hospitals always in a group and never in a group. We can notice the average prices changes in the groups contributing to the estimation in Figures 4.B.5, 4.B.7 and 4.B.6 - Appendix, Section 4.B.1. The group engaging in group purchasing in 2010 have similar average prices than the control group in 2009. Afterwards, average prices seem to increase. As for the group entering in 2011, trends before entry are similar to trends in the control group. We notice a decrease in average prices for all medicines and oligopoly medicines after 2011. The group entering in 2014 have similar patterns than the control group overall. These figures give us an overview of the price variation within each group. However, they do not enable to draw conclusions about the impact of group purchasing on bargaining power, because the composition of hospitals and medicines changes from one year to the other in each group. To assess the impact of group purchasing, we have to focus on variation within hospitals and within medicines.

4.4.4 Hypothesis testing

The estimation is based on a Difference-in-Difference method, comparing hospitals entering a group with hospitals either always or never in a purchasing group. The main assumption is the common trend assumption: without treatment, the evolution of log prices in the treatment and the control group would have been the same. This cannot be tested directly, as we do not observe potential outcomes, i.e. we do not know what prices the treatment group would have had without treatment. However, we can compare trends in the treatment and control group before treatment. This is not a standard Difference-in-Difference setting, as hospitals enter purchasing groups at different times: either in 2010, 2011 or 2014. Hospitals joining the groups in 2011 and 2014 are in the database at least two years before they join. This allows to measure the differences in trends before treatment, by running the following regression

$$\ln p_{ijt} = \sum_{k=-5}^4 \delta_{1k} w_{kijt} \text{monopoly} + \sum_{k=-3}^4 \delta_{2k} w_{kijt} \text{oligopoly} + \alpha_{ij} + \beta_{jt} + \varepsilon_{ijt}$$

Where $\ln p_{ijt}$ denotes log price of medicine j in hospital i at year t , α_{ij} the hospital - medicine fixed effects and β_{jt} the medicine - year fixed effects. k is the year of entry, for instance $k = -2$ indicates that a hospital will enter a regional purchasing group in two years. Standard errors are clustered at the hospital level.

Nine hospitals enter in 2011, and thirteen enter in 2014. As my database spans five years - from 2009 to 2014 - the possible distances to entry are five years before, for the hospitals entering in 2014 to four years after, for the twenty-seven entering in 2010. For hospitals entering purchasing groups, the sum $\sum_{k=-5}^4 \delta_{1k} w_{kijt}$ is collinear with the hospital - medicine fixed effect. I normalize $w_{(k=-1)ijt}$ to zero without loss of generality. A testable

implication of the common trend assumption is the equality between $k - 5$ to $k - 2$, and $k - 1$. As $k - 1$ is normalized to zero, the difference is given by the value and significance of $k - 5$, $k - 4$, $k - 3$, $k - 2$.

We can see on Table 4.1 that these coefficients - $k - 5$ to $k - 2$ - are not significant for all medicines, and both for monopolies and oligopolies. The joint significance of $k - 5$ to $k - 2$ is 0.10 for all medicines, 0.34 for monopoly medicines, and 0.86 for oligopoly medicines. Prices in hospitals entering purchasing groups are not following specific trends before entry.

Table 4.1: Trends in log prices before and after entry

Variable	All medicines	Monopolies	Oligopolies
5 years before entry (k-5)	0.0084 (0.016)	0.0020 (0.0064)	-0.015 (0.072)
4 years before (k-4)	-0.0086 (0.015)	-0.015 (0.011)	-0.011 (0.044)
3 years before (k-3)	0.010 (0.013)	-0.0056 (0.0072)	-0.0058 (0.037)
2 years before (k-2)	0.011 (0.014)	0.0023 (0.013)	0.014 (0.034)
Year of entry (k)	-0.0059 (0.011)	0.0010 (0.0052)	-0.048** (0.020)
1 years after (k+1)	-0.022** (0.0098)	-0.0071 (0.0083)	-0.082*** (0.024)
2 years after (k+2)	-0.038*** (0.0092)	-0.0035 (0.0065)	-0.13*** (0.029)
3 years after (k+3)	-0.031*** (0.0086)	0.00055 (0.0078)	-0.13*** (0.033)
4 years after (k+4)	-0.024*** (0.0087)	-0.0073 (0.0087)	-0.099*** (0.026)

Note - This Table shows how all medicine prices vary depending on the number of years before or after group creation, using hospital - medicine fixed effects and medicine - year fixed effects. The reference year is the year before entering a purchasing group. The outcome is log prices. Standard errors clustered at the hospital level in parenthesis. In this table, we can see that within a hospital-medicine pair, log prices before treatment do not significantly differ from log prices one year before treatment, for all medicines, for oligopoly medicines and for monopoly medicines. On the opposite, log prices are significantly different from log prices one year before treatment for all medicines and oligopoly medicines one to four years after treatment. The numbers of observations are 39,052 for all medicines, 6,253 for monopoly medicines and 8,014 for oligopoly medicines.

Significance levels: * : 10% ** : 5% *** : 1%

4.5 Results

4.5.1 Main results

Table 4.1 presents the average effects of belonging to a group for all medicines, monopoly medicines and oligopoly medicines, using the two models described in Section 4.3, namely Pooled Ordinary Least Squares - row one, POLS - and Fixed Effects - row two, FE. As for the Pooled OLS model presented in equation 4.2, we can see that on average, hospitals in a purchasing group do not get significantly different prices for all medicines. When we look at different medicines, we see heterogeneous correlations depending on market structure. Hospitals in a regional purchasing group get higher prices for medicines that have no competitor (“monopoly medicines”). The effect is a 0.4% increase, it is significant at 5%. On the opposite, hospitals who are in a regional purchasing group get lower prices for medicines which have some competitors. The effect is a 4% decrease, it is significant at 5%. These results only capture the correlation between group membership and medicine prices, but do not identify a causal impact of group purchasing. Indeed, when controlling for hospitals time-invariant unobserved characteristics, I obtain different results.

Estimating equation 4.1, a fixed effect model allowing hospitals to have medicine specific bargaining abilities, I find different results - row two of Table 4.1. Belonging to a regional purchasing group has a significant impact on medicine prices, as they decrease by 2% on average. There is no longer a significant impact of group purchasing for monopoly medicines, the 95% confidence interval going from -1% to 1% . As for oligopoly medicines, average price are 7.5% lower when hospitals are in a regional purchasing group compared to what they were before they joined. Overall, once I take into account hospitals’ time-invariant characteristics, group purchasing is associated with a larger decrease in prices.

Table 4.1: Impact of group purchasing on medicine prices

Variable	All medicine	Monopolies	Oligopolies
POLS	-0.0046 (0.0065)	0.0039** (0.0020)	-0.041** (0.019)
FE	-0.017** (0.0069)	-0.000060 (0.0053)	-0.075*** (0.017)

Row one presents the results from a pooled OLS regression, controlling for medicine - specific time trends. Row two presents the main results, using a regression with hospital - medicine fixed effects, and medicine - year fixed effects. The outcome is log prices. Standard errors clustered at the hospital level in parenthesis. The numbers of observations in the POLS model are 44,689 for all medicines, 6,778 for monopoly medicines and 9,130 for oligopoly medicines. The numbers of observations in the FE model are 39,052 for all medicines, 6,253 for monopoly medicines and 8,014 for oligopoly medicines.

Significance levels : * : 10% ** : 5% *** : 1%

4.5.2 Robustness checks

I conduct several robustness checks. Table 4.2 and 4.3 present the results for these estimations.

First - Table 4.2, column (1) - I use alternative fixed effects : hospital fixed effects and medicine fixed effects, assuming that the impact of hospital on medicine prices is homogeneous. Then, I withdraw extreme values for prices - column (2). In another set of results, I estimate the same model but focusing on hospital - medicine pairs that are in the database for the whole period, to hold the pool of hospitals and medicines constant - column (3). I also withdraw the entry year, in case the regional group takes time to obtain lower prices - column (4). Finally, I use an alternative definition of monopoly and oligopoly medicine, using the threshold 0.8 for the Herfindahl Index, instead of above 0.9 for monopolies and below 0.7 for oligopolies.

Overall, the impact of purchasing group on prices is never significant for monopolies, and negative and significant for oligopolies. The size of the impact on monopolies medicines

ranges between -0.7% and 0.1% . The coefficients for oligopoly medicines are between 6% and 13% . When I change the set of fixed effects, considering that the bargaining power of hospitals is constant across medicines, the overall impact of entering a group is no longer significant. The fact that I find different results suggests that hospitals' bargaining abilities are not constant across medicines. Therefore, the right specification is to allow for heterogeneity in hospitals' bargaining abilities across medicines.

I also change the pool of purchasing groups included in the study - Table 4.3. My results rely on the specific cases of four purchasing groups created between 2009 and 2014. To check whether the impacts are driven by one of these groups, I estimate the same model, but excluding one of the four purchasing groups - Table 4.3. For all medicines, the coefficients are always negative, between -1% and -2% , and always significant, except when I withdraw the larger group (column 1) - then it is still negative but not significant. As for monopoly medicines, the coefficients are close to zero in each case. The results on oligopoly medicines are always significant at 5% . The results vary from -5% to -10%

Overall, the patterns remain the same. Entering a regional purchasing group decreases prices by one or two percentage points when considering the whole pool of medicines. It does not impact prices of monopoly medicines, and it decreases prices for oligopoly medicines, decreasing prices by about $5-10\%$.

Table 4.2: Robustness checks: impact of group purchasing on medicines prices

Variable	(1) FE	(2) EV	(3) Balanced	(4) No entry year	(5) Medicines
Group member	-0.0034 (0.0099)	-0.024*** (0.0045)	-0.018*** (0.0053)	-0.036*** (0.0067)	-0.017** (0.0069)
Group x monopoly	-0.017 (0.013)	0.00024 (0.0055)	-0.0020 (0.0044)	-0.0069 (0.0075)	0.0012 (0.0051)
Group x oligopoly	-0.072*** (0.016)	-0.082*** (0.017)	-0.060** (0.027)	-0.13*** (0.027)	-0.076*** (0.017)

Column (1) uses hospital, medicine fixed effects, instead of hospital - medicine fixed effects. Column (2) withdraw 5% extreme values (EV) for quantities (compared to median quantities and hospital size) and 5% extreme values for prices (compared to median prices), column (3) keeps only hospital - medicine pairs that are in the database for the whole period. Column (4) does not use the year of the first group purchases. Column (6) uses an alternative definition of oligopoly and monopoly medicines. The outcome is log prices. Standard errors clustered at the hospital level in parenthesis

Significance levels : * : 10% ** : 5% *** : 1%

Table 4.3: Robustness checks: changing the pool of purchasing groups

Variable	(1)	(2)	(3)	(4)
Group member	-0.0089 (0.0069)	-0.014* (0.0073)	-0.023*** (0.0065)	-0.019** (0.0090)
Group x monopoly	-0.0028 (0.0075)	0.0024 (0.0056)	0.00093 (0.0059)	-0.0031 (0.0050)
Group x oligopoly	-0.045*** (0.016)	-0.078*** (0.019)	-0.079*** (0.019)	-0.098*** (0.021)

Columns (1) to (4) show how the results vary when we withdraw one of the four a purchasing groups contributing to our estimation. The outcome is log prices. Standard errors clustered at the hospital level in parenthesis

Significance levels : * : 10% ** : 5% *** : 1%

4.6 Conclusion

This paper is a first attempt to estimate the impact of regional purchasing group creation on innovative medicines prices in French hospitals. I use a unique database that includes all public French hospitals and about half of expenditures for medicines in hospitals. On average, belonging to a purchasing group allows hospitals to get prices which are 2% lower for innovative medicines. When focusing on medicines whose market structure can be identified, I find no impact for medicines that have no competitors, but a 8% decrease in the prices for medicines in oligopoly markets. Purchasing groups increase hospitals bargaining ability, especially when there are at least some companies producing the same substance.

This study has several limitations. First, my sample of hospitals joining a regional purchasing group is limited (fifty-four hospitals). Second, I measure gains in prices for a given medicine. If purchasing groups enable to switch to cheaper medical products, this benefit is not captured. Such an analysis would require to have knowledge about the level of substitutability between medicines, which is challenging because it depends on the pathology the medicine is used for. Finally, due to data limitation, only innovative and costly medicine which are directly reimbursed by the National Health Insurance are included - approximately one half of hospital expenditures for medicines.

Appendix

4.A Hospital funding

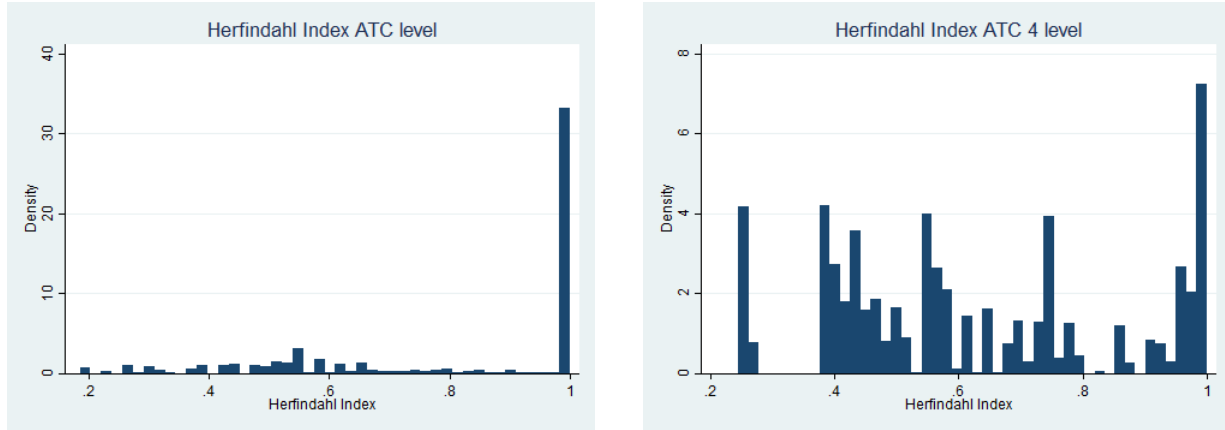
Law 2003-1199 of December 2003 reformed the core principles of hospital financing in France. Before 2004, public hospitals' budgets were annual budgetary envelopes, mainly determined by the previous year' budgets. From 2005 to 2008, activity-based payment was implemented gradually in all hospitals (Degrassat-Théas, 2013). In the current system, hospitals' budgets depend on the number and case-mix of patients (Or, 2009).

The objectives of this payment system are to increase transparency and to induce efficiency (Scheller-Kreinsen et al., 2009). Before activity-based payment, hospital budgets were subject to important agency problems, because budgets were not related to efficiency, or even to activity. The third-party payer had little information on hospitals efficiency. Implementing activity-based payment means creating a classification of diagnostics and their average costs as a way of improving transparency. The second objective is efficiency. The alternative funding rules are overall budget or fee-for-services. Overall budget based on historical costs penalizes efficiency because it gives lower budgets to hospitals with lower expenditure. Fee-for-service may lead to unnecessary care. On the opposite, activity-based payment should theoretically reward efficiency, as long as fees are accurately set.

The case-mix is defined through the creation of homogeneous patient groups (*Groupes homogènes de malades*), based on the American Diagnosis Related Groups (DRG). A homogeneous stay group (*Groupe homogène de séjour*, hereafter HSG) corresponds to each DRG. Then, a reimbursement rate is assigned to each HSG, with different levels of severity.

4.B Descriptive statistics

Figure 4.B.1: Identifying monopolies and oligopolies



Note - These graphs show the distribution of Herfindahl indices for all medicines in the database, at the ATC level - substance level -, and at the ATC 4 level - chemical subgroup.

Table 4.B.1: Hospitals in and out of the the sample

	(1)		(2)		(3)
	Mean	SD	Mean	SD	P-value
Spending	1.3e+06	2.7e+06	1.4e+06	2.0e+06	0.60
ATC1 B	0.54	0.40	0.38	0.33	0.00
ATC1 J	0.19	0.20	0.19	0.18	0.89
ATC1 L	0.57	0.26	0.60	0.23	0.26
Cancerology	11.34	11.35	10.59	8.01	0.43
Technicality	1.19	1.09	1.14	0.98	0.59
Severity	14.41	10.54	13.30	8.65	0.24
Emergency	41.04	25.39	44.53	22.71	0.13
Beds occupancy rate	82.10	10.98	82.56	18.12	0.75

Note - This Table shows how hospitals in the sample compare to hospitals out of the sample. SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for hospitals in the sample and other hospitals. Spending: total spending at average prices. ATC1: proportions of observations in the three major anatomical groups per hospital Cancerology: percentage of activity in cancerology. Technicality: indicator of number of patients coming from an university hospital. Severity: percentage of stays with high level of severity. Emergency: share of activity coming from the emergency department, proxy for low attractiveness

Table 4.B.2: Treated and control hospitals

	(1) Treated		(2) Non-Treated		(3) Difference
	Mean	SD	Mean	SD	P-value
Spending	1.3e+06	2.0e+06	1.5e+06	2.1e+06	0.58
ATC1 B	0.32	0.30	0.40	0.33	0.11
ATC1 J	0.16	0.17	0.20	0.18	0.23
ATC1 L	0.68	0.22	0.58	0.23	0.01
Cancerology	10.73	3.30	10.50	8.89	0.86
Technicality	1.00	0.40	1.17	1.09	0.28
Severity	14.06	5.81	13.06	9.27	0.48
Emergency	46.56	21.90	43.91	22.84	0.48
Beds occupancy rate	86.57	9.76	81.40	19.69	0.08

Note - This Table shows how hospitals in the treatment group compare to hospitals in the control group. SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for hospitals entering a group and other hospitals. Spending: total spending at average prices. ATC1: proportions of observations in the three major anatomical groups per hospital Cancerology: percentage of activity in cancerology. Technicality: indicator of number of patients coming from an university hospital. Severity: percentage of stays with high level of severity. Emergency: share of activity coming from the emergency department, proxy for low attractiveness.

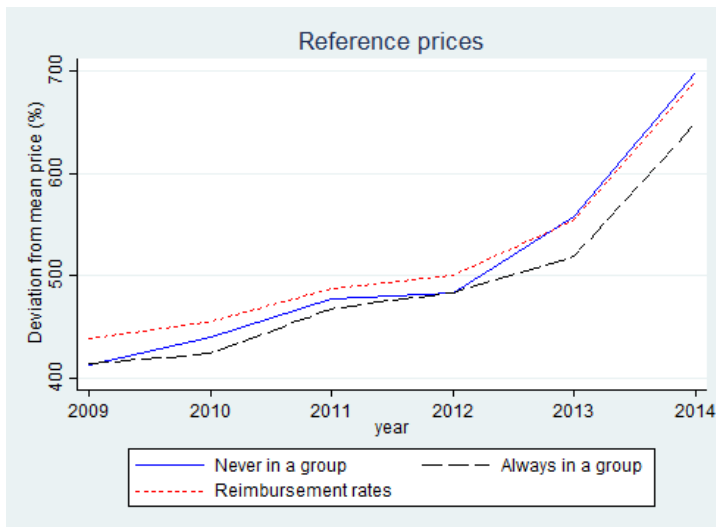
Table 4.B.3: Hospitals never in a group and always in a group

	(1) Never in a group		(2) Always in a group		(3) Difference
	Mean	SD	Mean	SD	P-value
Spending	1.5e+06	2.0e+06	1.3e+06	2.2e+06	0.55
ATC1 B	0.40	0.32	0.42	0.35	0.59
ATC1 J	0.21	0.17	0.19	0.19	0.65
ATC1 L	0.57	0.22	0.58	0.24	0.80
Cancerology	9.36	3.04	12.14	13.26	0.05
Technicality	1.12	0.63	1.25	1.53	0.46
Severity	12.15	7.15	14.38	11.60	0.13
Emergency	44.73	20.20	42.72	26.30	0.58
Beds occupancy rate	79.64	9.57	83.93	28.51	0.17

Note - This Table shows how hospitals always in a group compare to hospitals never in a group. SD stands for standard deviation and column 3 reports two-sided p-value of a t-test comparing averages of variables for hospitals in the sample and other hospitals. Spending: total spending at average prices. ATC1: proportions of observations in the three major anatomical groups per hospital Cancerology: percentage of activity in cancerology. Technicality: indicator of number of patients coming from an university hospital. Severity: percentage of stays with high level of severity. Emergency: share of activity coming from the emergency department, proxy for low attractiveness.

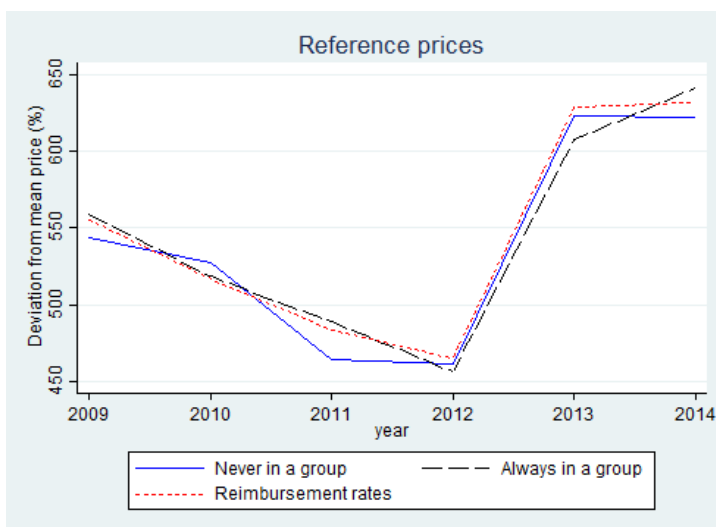
4.B.1 Trends in prices in different hospitals

Figure 4.B.2: Prices variations in reference groups, all medicines



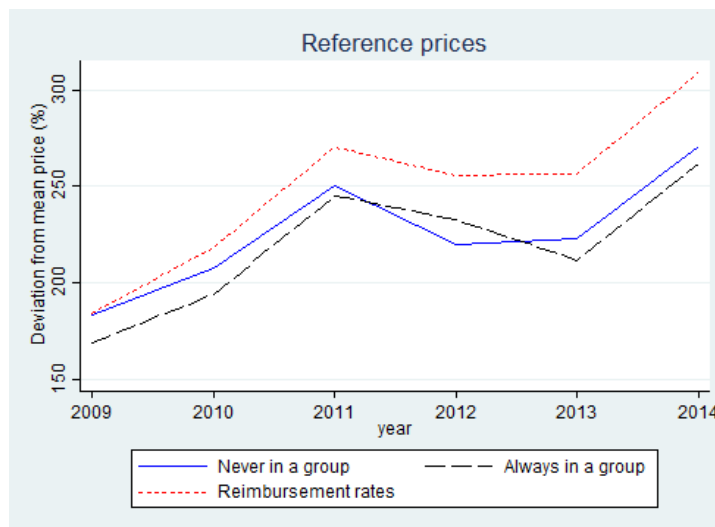
Note - This graph shows the variation of average prices of all medicines in three potential reference situations: hospitals always in a groups, hospitals never in a group, and national reimbursement rates.

Figure 4.B.3: Prices variations in reference groups, monopolies



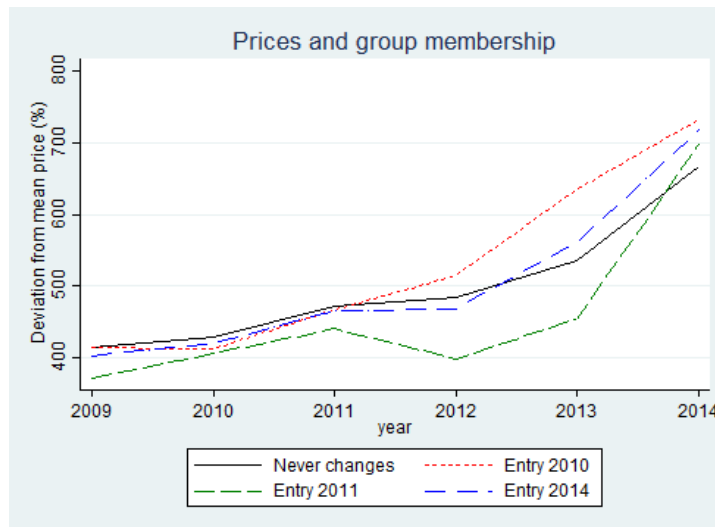
Note - This graph shows the variation of average prices of monopoly medicines in three potential reference situations: hospitals always in a groups, hospitals never in a group, and national reimbursement rates. For medicines that have no competitors, hospitals pay prices that are close to the reimbursements rates.

Figure 4.B.4: Prices variations in reference groups, oligopolies



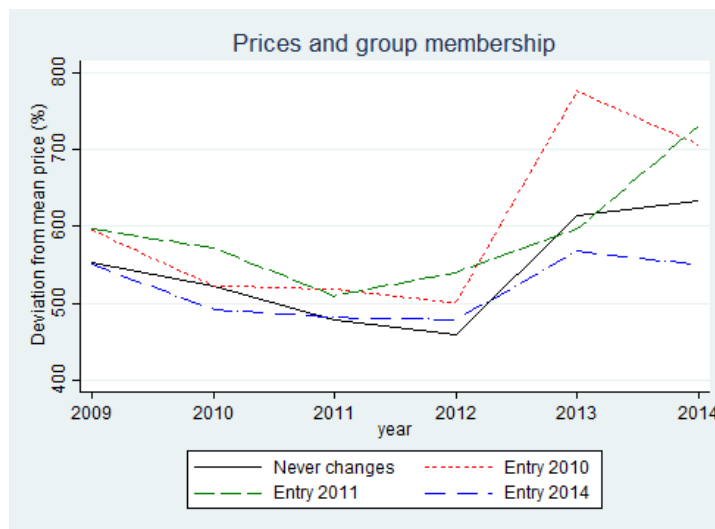
Note - This graph shows the variation of average prices of oligopoly medicines in three potential reference situations: hospitals always in a groups, hospitals never in a group, and national reimbursement rates. For medicines that have competitors, hospitals manage to get purchase prices that are below the reimbursements rates.

Figure 4.B.5: Prices in the treatment groups and the control group, all medicines



Note - This graph shows the variation of prices of all medicines in hospitals entering a group in 2010, 2011, 2014 - treatment group -, and in hospitals not entering - the control group, either hospitals always in a groups or hospitals never in a group. Before group creation, prices in hospitals in the treatment group follow the same trends as in the control group.

Figure 4.B.6: Prices in the treatment groups and the control group, monopolies



Note - This graph shows the variation of prices of monopoly medicines in hospitals entering a group in 2010, 2011, 2014 - treatment group -, and in hospitals not entering - the control group, either hospitals always in a groups or hospitals never in a group. Before group creation, prices in hospitals in the treatment group follow the same trends as in the control group.

Figure 4.B.7: Prices in the treatment groups and the control group, oligopolies



Note - This graph shows the variation of prices of oligopoly medicines in hospitals entering a group in 2010, 2011, 2014 - treatment group -, and in hospitals not entering - the control group, either hospitals always in a groups or hospitals never in a group. Before group creation, prices in hospitals in the treatment group follow the same trends as in the control group.

Chapter 5

Conclusion

This thesis aims to shed new light on three questions related to individual health and well-being. This section gives a summary of the main results and and suggestions for future research.

First, Chapter one follows displaced individuals from one to ten years after displacement. Among displaced individuals who have found a new job, we find a significant and lasting deterioration of job quality, even when focusing on displaced workers who have spent less than one year out of employment. This long-lasting deterioration occurs for different categories of individuals, both for men and women, young and old workers, individuals with short tenure and long tenure, white and blue-collar. Yet, men, older and more tenured individuals as well as blue-collarers are more affected. Given the growing evidence on the links between job displacement and mortality, a promising avenue for future research would consist in investigating whether part of the impact on mortality can be attributed to job quality deterioration.

Chapters two and three allow to see how administrative databases can be used to ad-

dress policy questions. Chapter two shows that increasing public reimbursement rates has counterintuitive effects on health expenditures. We measure changes in medical care use when individuals move to a region where the public health system offers higher reimbursement rates. We see a decrease in prescribed drug expenditures, an increase in spendings for dentists visits, and no impact on doctors visits or annual number of sick-leave days. In a setting where several policy measures aim to increase co-payment to avoid over-consumption of drugs, decreasing public reimbursement rates does not seem to be a promising way to decrease medicine use. Our results may be due to the duality of the French healthcare system, where the same product may be reimbursed in part by the compulsory public health insurance, and in part by complementary insurances. Measuring to what extent public and private insurances are substitutes could help toward explaining our provocative results.

While lower public reimbursement rates do not allow to decrease medicine expenditures, intervention on price negotiation can have a positive effect on public accounts. In Chapter three, we find that hospitals that join a purchasing group manage to get lower prices for medicines, as long as the market is sufficiently competitive. Our results only apply to innovative and expensive medicine, for which we have information about purchasing prices. To broaden the evaluation of purchasing groups, we would need to study the impact on all medicines, and see how they relate to funding mechanisms. For now, no database allows to carry out this project. In addition, investigating the ability of a purchasing group to make hospitals switch to cheaper medicines would be of major importance.

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