The Public Health Costs of Unemployment

Andreas Kuhn, University of Zurich*

Rafael Lalive, University of Lausanne, CEPR, CESifo, and IZA

Josef Zweimüller, University of Zurich, CEPR, CESifo, and IZA

July 2007

Abstract

This paper studies how unemployment affects public health costs. We use plant closure as an instrument for unemployment because bankruptcy is unlikely to be caused by deteriorating health but has a strong impact on workers' subsequent employment. The empirical analysis is based on an extremely rich data set with comprehensive information on various types of health care costs and day-by-day work history of individual workers. Our central findings are (i) expenditures on medical treatments are not strongly affected by joblessness, (ii) lack of employment reduces mental health for men but not for women, and (iii) sickness benefit payments strongly increase due to job loss. Our results also show that OLS estimates strongly overestimate the causal effect of unemployment on public health costs.

JEL classification: I12, I19, J28, J65

Keywords: Social cost of unemployment, health, non-employment, plant closure

*This paper has previously been circulated as "Does Unemployment Make You Sick?". We thank Joshua Angrist, Stefan Bender, Pierre-Carl Michaud, Michael Rosholm, Michael Svarer and seminar participants at Aarhus, Bonn, Brussels, Innsbruck, Mannheim, Munich, Nuremberg, and participants at the IZA conference on the "Well-being of the elderly", the IZA prize conference, and the IZA transatlantic meeting for comments on previous versions of this paper. Samuel Henz and Ruth Lalive suggested our medical classification of drugs. Financial support by the Austrian National Bank under the grant No. 10643 and the research fund of the University of Zurich (No. 53211602) is gratefully acknowledged. Address: Andreas Kuhn, IEW, Bluemlisalpstrasse 10, CH–8006 Zurich, Switzerland, e–mail: kuhn@iew.unizh.ch. Rafael Lalive, University of Lausanne, HEC/DEEP, CH–1015 Lausanne, e–mail: Rafael.Lalive@unil.ch. Josef Zweimüller, IEW, Bluemlisalpstrasse 10, CH–8006 Zurich, Switzerland, e–mail: zweim@iew.unizh.ch.

1 Introduction

This paper studies the causal effect of unemployment on public expenditures on health care in a typical European welfare state. Understanding this effect is important for at least four reasons. *First*, while ill health and lack of employment are the two major risks during an individual's working life, little is known about the effects of an individual's employment status on health. In a society where all employed individuals are covered by primary health insurance, health care costs are an informative measure of the costs of health shocks to society. *Second*, understanding the causal relationship between unemployment and health care is important for both labor market policy and health policy. Labor market policies that focus on job creation might be even more beneficial to society if they are providing employment to job seekers and improving their health at the same time. Health policy makers can be interested in this relationship to assess the effects of changing conditions on the labor market on the expenditures for health care. *Third*, the effects of joblessness on public health costs may be affected by institutional rules. The public health care systems of a typical European welfare state does not only cover the costs associated with medical treatment (such as doctor visits, hospitalizations, and medical drugs) but also provide insurance against income losses in case of sickness. While costs associated with medical treatment are more closely linked to a workers' health status, public health costs associated with replacement of income may be driven by institutional rules and by effects on individual incentives. *Fourth*, health care costs have risen strongly in the last decades in most industrialized countries (Hagist and Kotlikoff, 2005) and the dynamics of these costs may be related to job instability and loss of employment.

Our paper focuses on the case of Austria, which provides a good example for at least two reasons. On the one hand, health insurance in Austria is mandatory for all employed individuals (and their dependents). On the other hand, the unemployment insurance system is more restrictive than in other (European) countries and closer to the U.S. system: Regular unemployment benefits are paid for at most 30 weeks and the net replacement ratio (unemployment benefits relative to previous net earnings) is about 55 percent. As a result, job loss may have more severe financial consequences for job losers. To the extent that financial distress may lead to health problems, such an effects should be more severe in the Austrian context.

Many public health insurance systems do not only cover costs associated with take-up of health provisions (such as doctor visits, hospitalizations, and medical drugs) but also provide insurance against income losses. While direct take-up of health provisions is more informative on the health status of an individual, public health costs associated with sickness payments are also driven by institutional rules and incentives created by such rules. Therefore, our empirical analysis will distinguish between costs associated with take-up of health provisions and costs due to sickness benefit payments.[1]

Assessing the causal effect of unemployment on public health costs is difficult because deteriorating health status can be cause rather than a consequence of job loss. In other words, health selection of the unemployed will lead to a bias in the causal effect of unemployment on health costs in cross-section data.[2] To circumvent the problem of reverse causality, we use job loss due to plant closure as an instrument for individual unemployment. The experience of a plant closure strongly disrupts a worker's employment career but workers' health is unlikely to cause a plant closure.[3] While several other studies have adopted a similar procedure, our study has at least three innovative features that go beyond the existing economic literature on the effects of unemployment on health.

First, we study the effects of unemployment on costs associated with take-up of primary health-care rather than on the direct effect on a workers (self-reported or diagnosis-based) health. As the Austrian system provides comprehensive coverage of health care benefits for all employed workers, the public health care system faces potentially high additional costs associated with unemployment. As a public health care system is potentially very costly to society, it is of primary interest to policy makers to have reliable information on the health costs that are causally related to workers' employment status.

Second, our study aims to give a broad picture of the overall health costs to the public health insurance associated with the experience of individual unemployment. The Austrian system does not only cover costs associated with medical treatment (such as doctor visits, drug prescriptions, and hospitalizations) but also grants sickness transfer payments both for employed workers (incapable of working due to health problems) and for unemployed workers (incapable of searching for a new job). In our empirical analysis we will assess the causal effect of unemployment on overall costs. Moreover, we also analyze the costs structure, i.e. how these

[1] When we talk about "public health costs" associated with unemployment, we strictly refer to costs that are associated with payments by the public health insurance system (sickness benefits and take-up of health provision). From the point of view of public health insurance, additional costs arise due to reduced health insurance contributions when an individual looses his or her job.

[2] Stewart (2001) shows that the more unhealthy are more likely to enter unemployment and hence the unhealthy are over-represented in the unemployment stock. Martikainen and Valkonen (1996) show that in Finland the relationship between unemployment and mortality weakened as unemployment rises, suggesting that health selection varies over the business cycle. See also the discussion on the effects of health on labor market attachment in Currie and Madrian (1999).

[3] While our paper focuses on the impact of individual unemployment on public health costs for the same individual, a different literature looks at relationships at the more aggregate level. Ruhm's (2000) findings of lower mortality rates during recessions are consistent with such a hypothesis. Hence Ruhm (2000) documents an effect of aggregate (rather than individual) unemployment on individual health. This is in line with predictions of the economic theory of health production (Grossman, 1972) which holds that reduced opportunity costs of time increase incentives to undertake health investments through time-consuming activities which may improve health during times of high unemployment.

overall costs are divided into the interesting subcategories.

Third, in contrast to most previous studies, we use a very large and informative data set. Our data come from the Austrian health insurance register and cover all health-care related payments to private sector employees in one large Austrian region.[4] For the period 1998 – 2002, we can link the health cost data with social security register data (reporting a worker's employment and earnings history). Our analysis is based on 14,602 plant-closure workers and 39,701 non-plant closure workers. One obvious advantage of these data sets is their accuracy (not prone to measurement error both with respect to health- and employment-status information). Another advantage comes from the fact that all workers have the same health insurance coverage given by a standardized catalogue of health care benefits that are covered by the public health insurance system. Hence our measure of health costs is also highly informative on the workers health status.[5]

Our empirical analysis yields three major results. A *first* finding is that unemployment following a plant closure does not cause a significant increase in public health costs associated with take up of health provisions. Public health costs associated with hospitalizations and medical drugs prescription do not increase significantly, and doctor visits even fall. *Second,* while overall take-up is not significantly affected, we find that – for males, but not for females – mental health deteriorates. This result is in line with the hypothesis that, in the short run, unemployment causes mental health problems, whereas physical health is affected only in the long run. *Third,* we find that the public health costs that are associated with payments of sickness benefits strongly increase after a job loss. One additional day in unemployment increases the costs to public health insurance by 4.7 Euros (5.8 USD) per year for men and almost 2.5 Euros (3.2 USD) per year for women in the span of one year. However, this increase in costs does not reflect a deteriorating health status of displaced workers but is mainly due to sickness benefit rules: For employed workers, employers have to bear sickness benefits (for up to 12 weeks, depending on job tenure) whereas for unemployed workers, the public health insurance pays sickness benefits.[6] Since plant closure workers spend more time in unemployment than non-plant closure workers this increase in costs is largely mechanical. For males, we do not find that plant-closure workers do have more sickness *days* than non-plant closure workers. For females,

[4]Our study focuses on Upper Austria which is one of totally nine Austrian states. Upper Austria, located in the North and bordering Germany and the Czech Republic, comprises roughly one sixth of the Austrian population and work force.

[5]The public health insurance system aims at a basic coverage of all major health risks. Individuals with demand for services not covered by the public health insurance system (mainly better quality, such as one-bedroom hospitalization) can purchase such services from private health insurance companies. Private companies cover costs beyond the public system.

[6]When a worker gets sick during an unemployment spell, the time of regular unemployment benefits is interrupted and the worker becomes eligible for sickness benefits so each day on sickness benefits prolongs the maximum duration of regular unemployment benefits.

however, we find a significant increase in sickness days.

The paper is organized as follows. In the next section we provide a brief review of the previous literature. Section 3 presents the data and definitions of the crucial variables. In Section 4 we provide a descriptive analysis. Section 5 discusses the econometric methodology and identification strategy. Section 6 concludes.

2 Related literature

To the best of our knowledge, this is the first study that analyzes the causal effect of individual unemployment on overall cost to public health care. Our paper is related to two strands of the literature.

The first strand studies incentive effects in sickness insurance and the relationships between unemployment and sickness insurance use. Johansson and Palme (1996, 2005) study how changes in the income replacement level affect the incidence and duration of sick leave spells in Sweden.[7] Askildsen *et al.* (2005) argue that the negative relationship between unemployment and sickness insurance use may be due to worker moral hazard in a situation of full insurance against income loss. While our study is related to this literature, we do not assess the incentive effects of health insurance rules. Our paper contributes to this literature by studying the effects of exogenous job loss on take up of sickness benefits. Furthermore, access to direct data on health care costs allows assessing what medical conditions are prevalent among workers going on sickness insurance.

The second large strand of the literature literature that studies the relationship between individual unemployment and individual health status. An important strand of this literature is concerned with the impact of unemployment on mortality.[8] A large number of studies have examined the effect of unemployment on mortality. Early influential studies using individual data are Moser *et al.* (1987) and Morris *et al.* (1994) who find that the unemployed have significantly higher mortality rates.[9] These studies were based on longitudinal data which control for time-invariant individual effects. However, individuals are subject to health shocks over time so health selection may be of considerable importance. More recent studies have improved upon empirical designs. Gerdtham and Jonhannesson (2003) use a Swedish sample of initially equally healthy individuals and show that unemployment significantly increases mortality. Using administrative data from two US states Sullivan and von Wachter (2006) estimate a 15-20% excess risk of death

[7]See Henrekson and Persson (2004) for a related study.

[8]Cook (1985), Morris and Cook (1991) and Jin *et al.* (1995) survey the early literature. Platt (1984) documents the effects of unemployment on suicidal behavior. For recent surveys see Kasl and Jones (2000, 2006).

[9]An important strand of the literature has studied the impact of aggregate unemployment on mortality. The early work of Brenner (1979) points to a significantly positive relationship. However, the more recent literature has convincingly demonstrated that recessions and high local unemployment rates reduce rather than increase mortality (Ruhm, 2000, 2003, 2005, Gerdtham and Ruhm, 2006).

in the 20 years following a job loss. Eliason and Storrie (2006) show similar evidence for job losers in Sweden. Further interesting evidence comes from twins studies. Nylen *et al.* (2001) and Voss *et al.* (2004) examine mortality of Swedish twins in relation to unemployment. They find that those who were unemployment in 1973 are significantly more likely to commit suicide and or die from undetermined causes during the period 1974-1996.

Other related papers have studied the impact of unemployment on (physical and mental) health problems. Kessler *et al.* (1987, 1989) have documented the impact of unemployment (and re-employment) on self-reported health and Turner (1995) has looked at particular mechanisms (financial versus emotional distress) by which unemployment may affect health outcomes. However, it is difficult to interpret the results of these papers as a causal impact of unemployment on health. In contrast, a recent paper by Burgard *et al.* (2005) carefully addresses the issue of health causation versus health selection in a larger sample of involuntary job losers in the U.S.. While health effects are strongest for those who experience a health shock after a job loss or who loose their jobs for health reasons, adverse health effects are also existent for other workers experiencing a job loss.

The above studies are based on cross-sectional data and therefore are strongly subject to the problem of health selection bias. Other studies of take-up of health care provision use empirical strategies that are less prone to such a bias. Iversen *et al.* (1989) find rising hospital admissions in a sample of Danish workers after a large shipyard closure and Keefe *et al.* (2001) report excess risk of cancer registrations and public hospital admissions in a sample of workers displaced after bankruptcy of a meat-processing plant. The recent paper by Browning *et al.* (2006) applies propensity score matching of displaced and non-displaced workers in a large administrative data set from Denmark and finds no significant effect of job loss on rates of hospitalization for stress-related diseases (such as high blood pressure, heart diseases, gastric catarrh, ulcer).

A further related literature studies the impact of unemployment on take-up of particular public health care provisions, in particular doctor visits. Carr-Hill *et al.* (1996) and Field and Briggs (2001) find that the jobless workers in the UK do consult general practitioners more often than employed workers with similar characteristics. Similar evidence was found after a large furniture plant in Austria (Studnicka *et al.*, 1991). D'Arcy and Siddique (1985) provide evidence from the Canadian health care survey data that the unemployed use public health care more heavily than workers with a job. Such evidence may indicate that unemployment leads to health problems but is also consistent with the economic theory of health production (Grossman, 1972), which predicts increased incentives to invest in time-consuming health activities during periods of reduced opportunity cost of time (such as unemployment). However, other studies find that the unemployed make less use of the public health care system even when they are eligible to health care services. Ahs and Westerling (2006) and Virtanen (1993) study Scandinavian experiences and that find that unemployment is associated with lack of unmet care needs, particularly among unemployed who suffer from psychological symptoms. One possible explanation for such a result is based upon the behavioral model of health care use (Andersen, 1995) which stresses that take-up of health care benefits is not only influenced by need of care but also by individual predisposition and social context.

Further studies have shown that unemployment has a pronounced effect on subjective well-being. Clark and Oswald (1994) and Winkelmann and Winkelmann (1998) document the close relation between unemployment and unhappiness. Theodossiou (1998) finds that the unemployed suffer more from anxiety, depression and loss of confidence compared to otherwise similar employed individuals. Bjorklund (1985) finds evidence that unemployment has detrimental health effects in Sweden. Other studies focus on youth workers and find detrimental effects of unemployment on well-being, as Goldsmith *et al.* (1996) for the United States and Korpi (1997) for Sweden.

3 Data and definition of variables

3.1 Data sources

We draw on social security register data that can be linked to data of take-up of health insurance provisions form the statutory health insurance fund from a large region in Austria ("Upper Austria").[10] This data set covers individuals that are employed in the private sector. Social security register data provide information, on a daily basis, on the workers' earnings and employment history (collected for the purpose of calculating a worker's old age social security benefits, see Kuhn and Ruf (2006) for details concerning this data source). Data from the statutory health insurance record all payments by the health insurance fund related to a worker's take-up of health care benefits.

The combination of these two data sets provides enormously rich information and has two additional features that make it ideally suited for the present analysis. A *first* unique feature is that the data cover the *universe* of the private sector employees (more than 80 % of the active state population) in the region.[11] Moreover, each employed worker can be linked to a particular

[10] Health insurance administration is divided into regional units ("Gebietskrankenkassen", GBKK) and our data set comes from that GBKK of Upper Austria is one of nine Austrian states and located in the north of the country. This region covers about one sixth of the total Austrian population and work force.

[11] There are separate funds for private-sector employees, self-employed, farmers, public sector workers, and employees of several public utility firms. The data available to us comprises the universe of private sector workers only.

firm via a unique firm identifier. Because the data set covers the universe of workers we can perfectly reconstruct firms. A "firm" is simply defined as the set of individuals that is observed under a given employer social security number ("firm identifier") at a given date. The possibility of linking firm- and worker-information is particularly helpful for our estimation strategy which relies on a firm characteristic: the date of shut-down of a firm. Firm information is further helpful in making plant-closure workers better comparable to employees in ongoing firms and thus allowing to compare samples of workers with similar previous job situations.

A *second* unique feature is that these two data sets provide high-quality and comprehensive information on expenditures associated with a worker's health status. The reason is that health insurance is mandatory for all employees in Austria and that coverage is comprehensive and covers all costs associated with primary health care such as treatment by physicians, drug prescriptions, and hospitalized care. As a result, the data give a very detailed and broad picture of the health expenditures caused by a given individual.[12]

The payments recorded in the data can be broadly divided into the following categories (see Table A.1 for an exact definition of these categories and further subcategories used in the empirical analysis below):

(i) Sick leave transfers. These are payments to employed and unemployed workers during periods of sickness (when they are not capable of searching for a new job or not capable of working). When unemployed, sickness benefits are roughly equal to unemployment benefits. Days of sickness benefits do not reduce the number of (remaining) days during which an unemployed worker is eligible to regular unemployment benefits. When employed, a worker initially continues to receive her or his wage during the first up to 12 weeks (depending on previous tenure) in the sick leave spell. Thereafter the health insurance provides sickness benefits amounting to 80 % of the previous wage. In order to claim sickness benefits, a physician has to approve and repeatedly check a worker's impaired health situation.

Our data cover all days on sick leave but only the sickness benefits paid by health insurance. We therefore provide separate results for sickness benefits and days on sick leave. Sick leave payments may be higher for workers getting ill after a plant closure because the bankrupt firm can not continue to pay the wage for the initial 12 week period or because plant closure workers are more likely to enter sickness insurance from unemployment. Plant closure workers are thus more likely to be receiving sickness benefits paid by health insurance. The situation is different

[12]On top of mandatory public health insurance, individuals purchase supplementary insurance offered by private insurance companies. Provisions provided by these companies include higher quality treatment (e.g. single bedrooms during hospitalization) and specific treatments (e.g. non-standard treatments not generally accepted by orthodox/traditional medicine). As costs covered by these supplementary insurance contracts are *on top* of provisions covered by public health insurance, this does not cause any measurement problems for our empirical analysis.

for a worker getting ill in a firm that is not going bankrupt. Thus, sickness benefits can be mechanically higher for workers in plant closure firms than for workers in continuing firms. However, days on sick leave are recorded in the same manner for workers leaving bankrupt firms and other workers.

(ii) Doctor consultations. Doctors have contracts with the public health insurance and get paid a standardized rate for each consultation.

(iii) Hospitalization. The data record each hospitalization and details the particular reason for the hospitalization. In particular, it classifies the costs by the main diagnosis of the hospitalization according to the ICD-9 codes. We aggregate the diagnoses into the following causes for hospitalization: cancer, heart disease, mental health problems, respiratory diseases, cerebrovascular diseases, costs related to pregnancies, and other hospitalizations.

(iv) Drug prescriptions (including detailed types of prescribed drugs). The data record all payments to drug stores (or refund to individuals) for prescribed and self-medicated drugs. The data are extremely detailed concerning the type of drugs. We classify these drugs into a category that is "specific" to treat health problems associated with unemployment and a residual category of non-specific drugs. Among specific drugs we distinguish "psychosomatic" drugs (targeted at psychosomatic afflictions such as migraine therapeutics, anti-inflammatory drugs, etc.) and "psychotropic" drugs (treating psychological distress such as sedatives, benzodiazepins, antidepressants, etc.).

Table 1 gives an overview of health costs incurred in the year before the reference date (see the following subsection for the definition of the reference date). Overall yearly health costs of the workers covered in our sample amount to 455 Euros for men and 469 Euros for women.[13] A large fraction of these overall health costs is due to sick-leave transfer payments. For men, about 45 percent of overall health costs are due to such transfers whereas for women sickness transfer payments account for less than 20 percent. The main reason for this difference is that sick-leave benefits are closely linked to previous earnings. Hence the higher payments for men are mainly due to the fact that males get higher benefits per day on sick leave and to a lesser extent to more days on sick leave (women spend 10.4 days on sick leave, men remain 11.5 days on sick leave). Other health costs arise from medical treatments. The remaining categories health costs caused by men amount to 81 Euros for doctor consultations, 116 Euros for hospitalizations, and 56 Euros for drug prescriptions. For women, health costs are considerably higher in each

[13]Notice that these numbers are based on prime-age workers and are not representative for the whole population. To be included in the sample, a worker had to be employed at some date during our observations period (see below). The numbers would be much higher for retirees. Also note that the mean health costs are much lower than the standard deviation of health costs. This is due to the fact that most individuals are generating very low or zero health care costs but a small fraction of the population is incurring very large health care costs.

of these categories. Their yearly costs arising from doctor consultations are 156 Euros, from hospitalizations 136 Euros and from drug prescriptions 85 Euros. This descriptive analysis shows that public health costs differ strongly between women and men which suggests analyzing the effects of unemployment on health care use separately for women and men.

Table 1

The data on transfer payment from the health insurance fund cover the five-year period from January 1, 1998 to December 31, 2002. To get information on the the days not employed as well as on tenure with the current firm, we linked the data from the health insurance fund with the Austrian social security register data (ASSD) provided by the central social security agency ("Hauptverband der Sozialversicherungsträger").[14]

To link the information of individual health costs and individual unemployment experiences we construct a monthly panel of individuals' health and employment histories. Within the period January 1, 1998 and December 31, 2002, we measure the health costs of an individual by calculating overall health costs and disaggregate these costs into the above categories (and, in the case of drug prescriptions and hospitalization costs, also into subcategories). We measure unemployment by the days not employed per month. This measure is not sensitive to individual's decisions to eligibility for unemployment benefits and actual take up decisions. The disadvantage of the days not employed measure is that there is a mechanical relationship between days not employed and days on sick leave or days spent in the hospital because these days can not be recorded as days employed.[15] Note, however, that using days not employed as a proxy for unemployment gives rise to a lower bound on the causal effect of unemployment on health rather than an upward biased effect. This is because the IV estimator relates the effect of plant closure on health to the effect of plant closure on days not employed. Using days not employed as a measure of unemployment inflates the effect of plant closure on unemployment and therefore provides a lower bound on the causal effect of unemployment on health.

3.2 Definition of plant closure

In order to assess the causal impact of unemployment on health costs we use plant closure as an instrument for an individual's unemployment. The assumption is that workers in a plant-closing firm loose their job involuntarily, whereas for other job separations it is not clear whether such

[14] The Central Social Security Administration gets its data from the Funds and processes this information for the purpose of calculating of old-age social security benefits. So retrospective data from the Central Social Security Administration are collected in the same way as the recent data from the Fund.

[15] There are no important differences between using days not employed or days unemployed from the perspective of eligibility for sickness benefits. Both registered unemployed job seekers and job seekers who have exhausted unemployment benefits are eligible for sickness benefits if they are eligible for unemployment assistance.

separation results from a quit or a layoff. Let us first make precise how we define a "plant closure" and how we define a job-loss due to plant closure.

Definition of plant closure firms. To identify plant closure in our data it is particularly helpful that employer and employee information can be matched. In a first step, we use this information to identify, a "plant closure". A firm is considered as a plant closure firm if it fulfills the following criteria: (i) There has to be positive employment through at least 12 months up to some month t and zero employment from month $t+1$ through month $t+12$. (ii) If a firm disappears at date t, no more than 50 percent of the employees switch to the same employer at date $t+1$. (This latter criterion is adopted to rule out misclassification of a take-over as a bankruptcy). Whenever more than 50 percent of the employees are found under an identical new employer identifier these observations are excluded from the sample. To make the distinction between plant closure firms and non-plant closure firms as clean as possible all firms with large and long-lasting drops in employment (and thus all workers employed in theses firms) are excluded from the sample.

We consider all plant closures that take place between January, 1 1999 and December 31, 2001 (using the 10th of each month as the baseline date). This ensures that we have at least one year of health insurance information before and after the plant closure date (recall that the health insurance data runs from January 1, 1998 until December 31, 2002).

Definition of plant closure and non-plant closure workers. Just like plant closure firms, we define plant closure workers in a narrow sense. Our plant closure sample (PC) consists of all workers, who are employed in the month of plant closure or who were employed at least one month during the year before plant closure (but left before the effective shut-down of the plant). Hence our sample of plant-closure worker covers "stayers" who are employed in the closing firm in the month before it shuts down but it also includes "early leavers".

Due to the complex structure of the health insurance data, we work with a random sample of workers employed in non-plant closure firms. Non-plant closure workers are sampled randomly among all workers employed in non-plant closure firms and non-distressed firms. Specifically, on each reference date between January 10, 1999 and December 10, 2001, we take a 2.5 % random sample of from the universe of the control group of all small firms (3 or 4 employees) and a 0.25 % sample of all larger firms (more than 4 employees). All employees in firms with less than 3 employees are excluded from the data. (If such a firm disappears, it is likely that this is just a recoding of the firm identifier rather than a bankruptcy). This procedure provides a sample of control workers who were not employed in plant closure firms. Notice that the sampling procedure allows for workers to be included in the control sample repeatedly.

We measure monthly health care costs and monthly days in unemployment relative to the plant closure date for plant closure workers and relative to the reference date for non-plant closure workers. The *plant closure date* is the 10th day of the month before the plant closes for "stayers" and the 10th day of the month before leaving the firm for "early leavers".[16] The *reference date* for control workers is the 10th day of the month in which the control workers are sampled.[17] In the following, we use the term "plant closure date" to identify the plant closure date for plant closure workers and the reference date for control workers.

Figure 1 illustrates the construction of our dataset. On each date between 10th of January 1999 and 10th of December 2001, we first identify closing firms. In a second step, we identify the workers employed in the closing firms. In the third step, we we draw a stratified (by firm size) random sample of workers who are employed in firms that do not close. The fourth and final step consists of constructing information on work history and health care costs covering the year before and the year after the plant closure or reference date.

Figure 1 about here

4 Descriptive analysis

It is interesting to take a first look at the characteristics of the treated (plant-closure, PC) and control (non-plant closure, NPC) groups. Applying the sample selection procedure described above leaves us with 53,303 individuals of which 14,602 are PC-workers and 39,701 are NPC-workers.

4.1 Ex ante differences

We first provide in depth summary statistics on the year before the plant closure date. This provides a first test of the comparability of workers in PC-firms and workers in NPC-firms. Table 2 reports the health costs per individual in the year before plant closure, by gender and plant closure status. Clearly, plant closure workers incur somewhat higher health costs than non-plant closure workers. Women who work in firms that go bankrupt incur about 15 Euros

[16] For instance, suppose a firm is active on the 10th of January 2000 but no longer active in any of the subsequent 12 months. The plant closure date of workers who are employed in this firm on the 10th of January 2000 is the 10th of January 2000. An "early leaver" is a worker who has been employed in this firm on the 10th of February 1999 (10th of March 1999, ..., 10th of December 1999). The plant closure date for this worker is the 10th of February 1999 (10th of March 1999, ..., 10th of December 1999).

[17] For instance, suppose a control worker is included in the random sample drawn on 10th of January 2000. This individual's reference date is the 10th of January 2000. Moreover, this individual is going to be used to estimate the counterfactual for all workers employed in plants that close between the 10th of January 2000 and the 10th of February 2000.

in health costs more – 3 % of overall NPC-costs – than women in the NPC-sample. PC-men generate 68 Euros higher – 15 % of overall NPC-costs – health costs than NPC-men. Thus, female plant-closure and non-plant closure workers appear to be more comparable than their male counterparts. The differences in health costs before plant closure primarily arise because sickness benefits are higher for PC-workers than for NPC-workers (72 Euros for men, 21 Euros for women, second row)– primarily due to more days on sick leave (third row). In contrast, the remaining health cost categories are more balanced. Plant closure workers incur slightly lower costs due to doctor consultations and drug prescriptions and somewhat higher costs due to hospitalizations. This pattern is quantitatively and qualitatively similar for women and men. Thus, PC-and NPC-workers are quite similar with respect to the consultations, hospitalizations, and drug prescriptions but not with respect to sick leave.

Table 2

Table 3 provides information on the other background characteristics, by gender and plant closure status. The majority of male PC-workers are blue collar workers, relatively young (on average 35.6 years) and earning a relatively low income (on average 19,527 Euros during the year before the plant closure date). The sample of male NPC-workers has a substantially lower fraction of blue collar workers, is somewhat older (on average 36.9 years) and is earning more (on average 23,735 Euros during the year before the plant-closure). The differences between female PC- and female NPC-workers are qualitatively similar. NPC-workers were somewhat less frequently without work during the year *before* the plant closure date – on average men were 22.9 days non-employed, and women were 24.4 days non-employed. In contrast, male PC-workers spent 55.9 days in non-employment and female PC- workers were 34.0 days non-employed. NPC-workers have also been more continuously employed with their employer than PC-workers. Average tenure in the last five years is 3.2 years for men and 3.3 years for women in NPC-firms. In contrast, PC-workers joined their current employer more recently with tenure amounting to 1.8 years for men and 2.5 years for women. Table 3 also provides information on firm characteristics, such as size, industry, and location. The data clearly show that PC-firms are much smaller than NPC-firms, and that PC-firms tend to be more likely to be in the construction sector than NPC-firms. There are no important differences with respect to firm location for those firms employing the men in our sample. In contrast, there is a higher proportion of NPC-firms with unknown location employing women.[18] Finally, plant closures appear to be concentrated in December.

[18]Region and industry information can be missing for firms having several plants across Austria which are active in more than one industry.

4.2 Effects on non-employment and health

In a second step, we assess the effects of the plant closure event on the days spent in non-employment and on health care costs. Figure 2 depicts both the evolution of unemployment experiences for PC- and NPC-workers, by gender. The unemployment measure used in figure 2 is the number of days not in employment per individual during the last quarter. We see that male PC-workers spent about 15 days per quarter in unemployment throughout the year before the plant closure date.[19] In contrast, NPC-workers only spent about 5 days in unemployment per quarter during the last year before the reference date. After plant closure, unemployment soars for plant closure workers. In particular during the first six months the nonemployment rises to an average of over 30 days or more. In the third quarter after plant closure, nonemployment decreases to 25 days, and in the fourth quarter, nonemployment decreases to slightly more than 20 days – a level which is still markedly higher than in any quarter before plant closure. In contrast, unemployment for non-plant closure worker is very similar before and after the plant-closure date.

Figure 2

Women in PC-firms also spend more days in nonemployment (about 8days) than women in NPC-firms (about 4 days) in any quarter before plant closure. In comparison with men, this difference in smaller. Yet, the impact of plant closure is even stronger for women. In the quarter after plant closure, PC-women remain in non-employment for more than 35 days whereas there is no effect for NPC-women. The discrepancy in days non-employed decreases somewhat but even in the fourth quarter after plant closure, PC-women remain nonemployed for 30 days whereas NPC-women remain nonemployed for about 4 days.

Figure 3 shows the corresponding graphs for the evolution of health costs. We see that plant closure workers cause slightly higher health costs before the plant closure date than non-plant closure workers. After the plant closure date, health costs more than double during the first months after the plant closure and then fall again. Interestingly, we also find an increase in health costs for non-plant closure workers after the reference date. One reason for this increase is selection on workers who are employed (and hence in good health) at the date of plant closure (and most likely during the months before that date) but may become sick after that date. Therefore it is not surprising that we see an asymmetric evolution of the health care costs before and after the reference date. Moreover, the periods under consideration health costs were strongly increasing in general (in particular, in the years 2000-2002) which is reflected in

[19] The difference in the number of days in nonemployment decreases somewhat before the plant closure date due the fact that each worker in the sample has to be employed on the reference date.

the upward trend of health costs of non-plant closure workers after the reference (plant-closure) dates.

Figure 3

Overall health costs appear to be strongly affected by nonemployment resulting from plant closure. However, overall health costs can be misleading because sickness benefits are recorded differently between PC- and NPC-workers (see section 3). Figure 4 therefore reports the evolution of overall health costs excluding sickness benefits and sick leave days (which are not recorded in different ways between PC- and NPC workers). The results regarding overall costs (excluding sick pay) indicate that for both men and women PC- and NPC-individuals are very similar before the plant closure date. Plant closure increases health care costs only slightly among men in the first quarter after the plant closure date. In the second to fourth quarter after the plant closure date, health care costs are even slightly lower for PC-men compared to NPC-men. In contrast, PC-women are incurring much higher health care costs than NPC-women in the first and second quarter after the plant closure date. In the third and fourth quarter, PC-women use slightly less health care than NPC women. Thus, results for health costs excluding sick leave suggest that the effects of unemployment on health are modest.

Figure 4

The bottom panel of figure 4 shows results for days on sick leave. For men there is only a small difference in days on sick leave the fourth and third quarter before the plant closure date. However, in the two quarters immediately before plant closure, days on sick leave tend to increase for PC-workers whereas they tend to decrease (slightly) for NPC-workers. The fact that days on sick leave increase already before plant closure might be interpreted as a sign that the identifying assumption – health status of workers does not cause plant closure – is not valid. This interpretation is unlikely to hold because workers spend on average at most 3.5 out of 91 days per quarter on sick leave. This means that it is unlikely that the deteriorating health of a small group of workers accounts for the failure of the entire firm. A second interpretation of this fact is that anticipation of plant closure is already deteriorating health among some PC-workers. Thus, an analysis that is only looking at the effects of plant closure on health status after plant closure may provide a misleading picture of the overall health effects of non-employment.

The bottom panel of figure 4 also shows that in the first quarter after the plant closure days on sick leave are considerably higher for individuals formerly employed in PC- firms than for NPC-individuals. But the difference in sick leave days quickly diminishes in the second quarter and vanishes completely in the third and fourth quarter after the plant closure date. The findings for women are very much in line with the findings for men. Sick leave days increase already two quarters before the plant is actually going bankrupt. The difference in sick leave days is not persistent, sick leave days among PC-women reaching approximately the same level in the third and fourth quarter after plant closure as in the third and fourth quarter before plant closure. Thus, the evidence suggests that there is a temporary and relatively weak effect of plant closure on days on sick leave.

5 Estimating the causal impact of unemployment on health

This section discusses our strategy to identifying the causal effect of unemployment on health. Define Y_{it} as the payments incurred by the health insurance fund that are associated with take-up of health insurance provisions of a particular individual i in the year t where $t = 0$ is the year before plant closure for workers in firms that close and the year before the plant closure date for workers in continuing establishments and $t = 1$ is the year after plant closure for the treated and the year after the plant closure date for NPCs. Let D_{it} the number of days that the individual i spent in unemployment in period t. Suppose D_{it} and Y_{it} are related by the following linear relationship

$$Y_{it} = \beta_0 + \beta_1 D_{it} + X_i\gamma + u_{it} \tag{1}$$

where X_i is a vector of control variables measured on the plant closure date and u_{it} is an error term capturing the effect of omitted variables affecting health costs, and β_0, β_1, and γ are parameters to be estimated, the parameter of main interest being β_1.

The control variables X_i are age, blue collar status, tenure in the current firm, firm size, and earnings per day worked (wage), a vector of industry dummies, region dummies, and time dummies (a set of plant closure month and plant closure year controls). Moreover, estimates are performed separately by gender because labor market attachment differs strongly between women and men. Industry and time controls are important because plant closure risk is strongly seasonal, and it differs by industry. Time controls include a dummy for the year and the month of the plant closure date to account for the fact that plant closures are more likely in December than in other months of the year. Note that for workers leaving plant closure firms early – "early leavers" – the plant closure / reference date is the date at which the worker is leaving the firm rather than the date at which the plant closes. Region controls capture the location of the employer (inside upper Austria, outside upper Austria, or unknown).

The OLS estimate of β_1 is potentially biased because of endogeneity of the unemployment variable D_{it}. Days spent in unemployment D_{it} are likely correlated with u_{it} because a bad health status (that leads to high health costs) may also affect the duration that an individual spends in unemployment.[20]

To tackle the problem of endogeneity we use an instrumental variable approach. This approach utilizes variation in the treatment variable D_{it} which is generated by some exogenous factor. Hence we need to think of a situation where variation in unemployment is not driven by individuals' health status. This paper uses employment in a plant closure firm as the instrument for the number of days in unemployment during the year that follows a job loss. The idea is that employment in a plant closure firm is closely correlated with unemployment in the subsequent period D_{i1} but unrelated to a worker's (ex-ante) health status Y_{i0}.

We define Z_i as a binary variable that equals 1 if a person is employed in a plant closure firm at the plant closure date and equals 0 if a person is employed in a firm that continues to exist after the plant closure date. In order to assess if employment in a plant closure firm is a valid instrument for the causal effect of nonemployment on the subsequent health status, several assumptions have to be satisfied (Angrist and Imbens, 1995, Angrist *et al.*, 1996). The first assumption concerns the ignorability (i.e. the randomness) of the instrument. This first assumption essentially states that the instrument can be viewed as randomly assigned, so that both potential treatments and potential outcomes do not differ between the two sub-samples. This is a strong assumption in our case, as we cannot plausibly assume a-priori randomness of the instrument (see section 4). There are both differences in the probability of plant closures between different firms and also between different individuals (because firms with different probabilities of shut-down have not necessarily the same composition of their work force).

The central identifying assumption of our approach is that plant closure is ignorable conditional on observed (individual and firm) characteristics X_i. The idea is that the health status of a worker does not lead to the firm going bankrupt once firm size, firm industry, and worker age, tenure, and gender have been taken into account. This assumption implies an exclusion restriction, i.e. the instrument must not have any direct effect on health costs. The assumption is that job-loss due to plant closure does not directly affect the health status of a worker. Essentially, we assume that a worker who finds a new job immediately does not suffer from health problems and hence does not take up additional health insurance provisions. Any effect on health costs comes via days spent in unemployment. In principle, plant closure might affect health in other ways than via unemployment. For instance, areas with many plant closures might experience reduction in pollution levels thus benefitting health in these areas. Alternatively, tighter local budgets might imply deteriorating quality of health care negatively affecting health. We believe

[20] A similar endogeneity problem has been discussed in studies estimating the causal effect of education on health (Chevalier and Feinstein, 2006).

that it is unlikely that such spillovers give rise to direct effects of plant closure on health for two reasons. First, our descriptive analysis indicate that plant closures are small compared to the average employer within a region. Thus, plant closures are unlikely to generate regional spillover effects. Second, regional spillover effects would also be affecting the control group. This suggests that regional spillovers do not bias our estimates of the effects of unemployment on health.

We control for X_i parametrically using two stage least squares. Clearly, the assumption that Z_i is ignorable conditional on X_i can not be tested directly. However, it turns out that we can assess the plausibility of this assumption by exploiting the panel nature of our dataset.[21] Specifically, we estimate

$$Y_{i0} = \pi_0 + \pi_1 Z_i + X_i\theta + \epsilon_{i0} \tag{2}$$

This regression tests whether plant closure workers are similar with respect to health care costs in the year before the plant closure date.[22] Our results support the claim that ignorability is a plausible assumption for men but less so for women. We address the problem with non-balanced health costs in the period before plant closure by introducing pre-plant closure health status as an additional control variable.

The second assumption is that the instrument must affect the treatment intensity. Working in a plant closure firm at the plant closure date must increase unemployment duration during the year following the shut-down of the plant for at least some workers. As we show in section 4, working in a plant closure firm indeed has a huge impact on unemployment. Hence empirical evidence shows that this assumption is satisfied.

The third assumption postulates that the instrument has to affect all individuals in the same way (monotonicity). This assumption states that, for each individual, the (potential duration of unemployment during the year after the plant closure date is longer when the individual works in a firm that closes due to bankruptcy on the plant closure day than when the individual works in a regular firm on the plant closure day. Although this assumption is not verifiable (because it involves potential and thus fundamentally unobserved outcomes), it has a testable implication in the case of a non-binary treatment, in that the empirical cumulative distribution functions of the treatment variable does not cross for the two sub-samples (see Angrist and Imbens, 1995).

Figure 5 shows the empirical cumulative density of days not employed within the year pre-

[21] An alternative approach to exploiting the panel nature of our dataset is to estimate a worker fixed effects specification or a difference-in-difference specification. Results based on these approaches are qualitatively similar to those reported in the paper. The advantage of our approach is that it allows us to investigate the plausibility of the conditional ignorability assumption using data on the year before the plant closure date.

[22] Note that estimating model (2) in period $t = 1$ yields the reduced form – or "intention to treat" – effect of the instrument on health care costs.

ceding or following the plant closure date, respectively. As the upper figure shows, there is already a difference in the number of days not employed between the two groups within the year preceding the plant closure date (which is already evident in figure 2 above). But also note that this difference mainly manifests itself in the lower domain of the variable and that the difference gets smaller for longer durations of nonemployment. The lower panel of figure 3 shows the same graph for the year just following the plant closure / reference date. First note that the graph is essentially the same for the NPC-group (compared to the year before the plant closure / reference date), and thus there is practically no change in the distribution of the endogenous variable for these individuals. Compare this to the large shift in the empirical distribution for the treatment group. Although we see that our instrument has the largest impact on the probability for somewhat shorter durations of nonemployment, there is also a large increase in the probability of longer durations of nonemployment. This evidence does not contradict the assumption that the plant closure event affects the days in non-employment monotonically.

Figure 5

Angrist and Imbens (1995) show that – if all three of the above mentioned assumptions hold – the 2SLS estimator measures the average causal effect of non-employment on health care costs for individuals which are induced to remain non-employed longer because they have been employed in a plant that closed. The average causal effect depends on both the instrument used and on the distribution of the treatment variable. This means that any estimated causal effect does not necessarily coincide with health effects of nonemployment resulting from other sources of job-loss.

6 Econometric results

6.1 The causal effect of unemployment on health costs

Our aim is to study the causal effect of joblessness on public health costs that is based on a comparison of non-employment experiences of plant-closure and non-plant closure workers. This is a reliable identification strategy if the two groups do not differ with respect to their health status ex ante. To get a first hint whether this is indeed the case, we run a simple OLS regression on health costs in the year *before* the plant-closure date on the set of control variables and include the plant-closure status as an additional control variable.

Table 4 includes the treatment dummy (= 1 for plant closure workers, and 0 for non-plant closure workers) into this regression for males. We find a positive point estimate for overall health costs (column 1) suggesting that plant-closure workers incur higher health care costs than non-plant closure workers. However, the coefficient is not statistically different from zero. This suggests that our assumption of no differences in average health status between the two groups of workers cannot be rejected by the data. This conclusion remains unchanged if we look at different subcategories of health costs. In all cases the standard error is much larger than the point estimate indicating no significant differences between the two groups for health costs due to sickness benefits, doctor consultations, hospitalizations and drug prescriptions. The only exception is when we look at the number of days on sickness benefits (last column). Here we see that there is a significant and quantitatively important differences between PC- and NPC-workers. This finding is in line with results in Figure 4.

Table 4

The situation is similar for women as far as overall health costs are concerned (see table 5). We find a positive point estimate which larger than that of males but which is also not statistically different from zero. When we split up costs in subcategories for different types of health insurance provisions, we find that female PC-workers cause more costs due to doctor consultations (although quantitatively this effect is not very large – comparing the point estimate with the mean of the dependent variable at the bottom of Table 5 shows that female PC-workers accumulate roughly 5 percent higher costs that their NPC-counterparts). Just like for males, we see that also for females the number of days on sickness benefits are higher for PC-workers than for NPC-workers. Quantitatively, the difference is quite high, amounting to more than 20 percent higher sickness days than the average worker. There is no significant difference between PC- and NPC-workers in public health costs associated with the consumption of medical drugs. Moreover, we see that the PC-coefficient for hospitalization costs is close to significant and quantitatively important. In contrast, drugs and overall costs excluding sickness benefits do no show any significant impact on overall health costs.

Table 4 also displays the partial correlations between health care costs with the control variables. Column 1 shows that there is an inverse u-shaped relationship in health care costs with respect to age, tenure and firm size but a u-shaped relationship between health care costs and income per day. Columns 2 reports results for the costs of medical treatments. The analysis reveals that there is a u-shaped relationship between costs due to medical treatments and age, no relationship between medical costs and tenure, and a positive and linear relationship between medical costs and firm size. The result for age is very much in line with the fact that older individuals tend to incur higher costs due to medical treatments. Column 3 reports the results for sickness benefits. Interestingly, these results are in line with a standard income equation. This is not surprising since sickness benefits act as income replacement.

The results in Table 4 and Table 5 indicate that the plant closure is not directly related to health care costs before plant closure for men. In contrast, the picture is somewhat less favorable for women. While only doctor visits and days on sick leave are significantly higher for PC-women at the 5 percent level, hospitalizations are also significantly higher at the 10 percent level. This means that it will be important to assess the sensitivity of our results to additionally controlling for health care costs or to assessing the changes in health care costs.

Table 5

Table 6 shows the effect of plant closure on non-employment. We see that the instrument is very strong, i.e. the first stage F-statistic is 261.65. The PC-coefficient is highly significant (the coefficient being more than 75 times larger than the standard error) and the impact is quantitatively important. The average male plant closure worker spends 84 days more out of a job than the average non-plant closure worker. PC-women are even more strongly affected by plant closure remaining 123 days longer without work than corresponding NPC-women.

Table 6

In what follows we present IV-estimates on the causal effects of non-employment after a job loss on health costs. The dependent variable (i.e. the various health indicators) now refers to the years *after* the plant closure date. To show that it is important to adopt an IV-strategy to assess a causal impact of non-employment on health indicators, the following tables list both the IV-estimator of number of days in non-employment, and the corresponding OLS-estimator from a regression of health indicators after the plant closure date on the number of days in non-employment during the year after the plant closure date.[23]

Column 1 of table 7 panel A presents the IV-estimator for overall health costs for men. We see that an additional day in non-employment causes an increase in overall public health costs by 4,85 Euros. This is a large number. To see this recall that, before the plant closure date, the average health costs of PC- and NPC-worker amount to about 500 Euros per year and worker (see Table 2). The first-stage regression reveals that a plant-closure event raises days in non-employment by more than 83 days for males (and by more than 120 days of females). Taken together our estimate suggests that public health costs of the typical male PC-worker increase by more than 400 Euros (or by 80 percent!).

[23] Regressing health indicators on days not employed before the plant closure date yields qualitatively similar results with two exceptions (see Table A.2 in the appendix). Drug prescriptions for men are not significantly positively related to days not employed in the year before plant closure. Also, hospitalization costs for women are much less strongly positively related with days not employed. This is due to pregnancy costs which are much more important after plant closure than before plant closure.

Table 7

However, columns 2 and 3 of Table 7 show that the increase is almost entirely due to higher sickness benefits payments and not due to costs associated with increased take-up of health care provisions. The IV-coefficient in column 2 shows that the causal effect of non-employment on health-related take-up of insurance provisions (consultations, hospitalizations, drugs) is not only quantitatively very small but also statistically insignificant. In contrast, column 3 reveals that the increase in public health costs is entirely due to an increase in sickness payments which, for jobless workers, have now to be borne by the public health insurance system rather than by the employer. Columns 4 to 7 decompose this impact into various health provision subcategories. It turns out that that non-employment has no significant impact on hospitalization costs and on costs associated with the consumption of medical drugs. Interestingly, we find a significant (though quantitatively small) reduction in health costs due to doctor visits. This suggests that doctor consultations cannot be strongly driven by a lower opportunity costs of time for the non-employed.

In panel B of Table 7, we show the corresponding estimates for females. The picture is very similar. Overall health costs increase, but the bulk of this increase is due to increased sickness benefit payments. When we exclude these payment from the overall health costs, the coefficient becomes small and statistically insignificant. However, in contrast to males, non-employment causes additional health costs for females in various subcategories, in particular due to increased hospitalization costs and due to an increased number of sickness days. Similarly to males, however, doctor consultation decreases with more days in non-employment.

For means of comparison, Table 7 also shows the OLS-estimates. Both for males and for females we see a consistent picture. OLS-estimates are much higher than the IV-estimates. All coefficients shown in Table 7 indicate that more days in non-employment are associated with higher health costs, the only exception being doctor consultations for males. While the IV-coefficient rules out reverse causality by the assumption that plant-closure is unrelated to worker health, the OLS-coefficients may in addition be driven by reverse causality as less workers are more likely to become unemployed. We see that not appropriately accounting for this reverse causality may have a substantial impact on the estimated coefficient. This underlines the importance of our instrumental variable approach for identification.

6.2 Robustness Checks

One could argue that our IV-coefficients are biased as the health indicators of PC- and NPC-workers are not completely identical before the plant closure event. To shed light on this issue and to check the robustness of our previous estimates, Table 8 reports the IV-coefficients from regressions that enrich the set of control variables. In particular, to account for differences in employment performance and health indicators between PC- and NPC-workers *before* the date of plant closure we include as additional regressors the number of days in non-employment and the total health costs during the year prior to that date. It turns out that our main results presented in Table 7 remain unchanged when we perform this robustness test. In particular, both for males and for females the increase in public health costs is driven by sickness benefits payments rather than an increase in the health status of non-employed workers as measured by the health costs for hospitalizations, doctor consultations, and medical drugs. For males and for females we find that doctor consultation decrease rather than increase when the individual experiences more days in non-employment. For females but not for males, we see an increase in the number of sickness days. In sum, Table 8 reproduces almost exactly our main results presented in Table 7.

Table 8

Table 9 shows the main results using the change in health outcomes as the dependent variable. This specification allows for differences in health outcomes that are not captured with our control variables. Results for men indicate that the strong effects on overall health costs are primarily due to the effects of non-employment on sick leave pay. In contrast to the main results, estimates that are explaining the change in health outcomes suggest that doctor consultations are positively affected by non-employment. This change in results can probably be explained by the fact that PC-men are consulting doctors slightly less frequently than NPC-men before plant closure (see Table 4). Panel B reports results for women. There are two important differences between the results that account for pre existing differences in health outcomes between PC- and NPC-women and the main results that only focus on the time period after plant closure. The new results suggest that hospitalizations are only marginally significantly positively affected (z-Value 1.95) and that there are no effects on days on sick leave. The other results are qualitatively very similar to our main results.

Table 9

The robustness checks suggest that the main results are not sensitive to controlling for differences in health costs before plant closure. However, the main results are sensitive to measuring effects on changes in health costs rather than on levels of heath care costs. The key question is whether this sensitivity in results is due to permanent or temporary differences in health outcomes before plant closure. Permanent differences suggest that the main identification strategy fails because these permanent differences can not be caused by non-employment. In contrast, temporary differences in health outcomes can arise if health reacts in anticipation of plant closure. We discuss possible anticipation effects in more detail below (see Figure 6 and 7).

6.3 Detailed Results

Tables 10 and 11 look in more detail at hospitalization costs (Table 10) and on public health costs associated with the consumption of medical drugs (Table 11). Panel A of Table 10 shows that, for males, hospitalization costs associated with cancer, stroke, respiratory ailments and other hospitalization are not affected by days in non-employment. However, we see that hospitalizations due to mental illnesses are significantly affected by days in non-employment. This is in line with previous research that has emphasized that, in the short run, the experience of job loss and associated non-employment may be predominantly showing up in case of mental illnesses where physical illnesses manifest themselves only over a longer term. Panel B of Table 10 shows that, for females, days in non-employment cause a significant increase in overall hospitalization costs and that this increase is mainly due to hospitalization due to pregnancy and hospitalizations for other reasons. The results that pregnancies increase following a job loss is consistent with the hypothesis that there are fertility timing considerations of women. If a mother plans to have a child, she will choose the timing of a birth when the opportunity costs are low. Interestingly, also the catch-all category other hospitalizations increase for females which suggests that hospitalization costs are partly also associated with health problems. Table 10 again shows not only IV-coefficients but also OLS-coefficients. The picture established in Table 7 before is confirmed also in Table 10. The majority of OLS-estimates is positive and significant, suggesting that more days in non-employment are associated with higher hospitalization costs. However, with the few exceptions mentioned above this is most likely the result reverse of causality.

Table 10

Table 11 shows a similar analysis for the case of public health costs associated with the consumption of medical drugs. While overall drug costs are not significantly affected by days in non-employment, we find that, for males, the consumption of psychotropic drugs (antidepressants, etc.) treating depressive conditions significantly increases with more days in non-employment. This underlines the relevance of mental health problems as a possible effect of non-employment due to plant closure. However, we do not find a significant impact of the consumption of psychosomatic drugs , neither for males nor for females. Again, OLS-estimates suggest a positive relationship between days in non-employment and the consumption of drugs. These coefficients are significant in all subcategories but most likely driven by reverse causality.

Table 11

One could argue that the absence of any substantial difference in public health costs after a plant closure does not mean that health costs are unaffected as workers could suffer from health shock in anticipation of job loss and the fear of extended periods of joblessness. Figure 6 shows the IV-estimates in each of the four quarters before and after plant closure. This analysis allows discussing whether health care costs increase already before job loss due to plant closure. The first row reports the effects of the days not employed in the year after plant closure date on total health care costs in each of the 4 quarters before plant closure and after plant closure.[24] Results indicate that there is no significant effect before the plant closure date but a strong and significant effect in each quarter after the reference date. Results in the second row report the effect of days not employed on days on sick leave. For both men and women, there is no significant effect in quarters four and three before plant closure. However, results clearly indicate that workers enter sick leave already two quarters before the plant closure date. This anticipation effect is significant at the 5 percent level. Moreover the effect of nonemployment on plant closure remains significantly positive in the first (men) and in the first and second (women) quarter after plant closure date. This is consistent with an interpretation that the fear of loosing one's job can already lead to a deterioration of health.[25] The third row reports the effect of job loss due to plant closure on costs due to doctor visits. Results indicate that there is a significant reduction of these costs in the second and third quarter after plant closure for men. Women see doctors more frequently in the two quarters just before plant closure, but less frequently in the second quarter after plant closure. The result for women is consistent with our finding that job loss due to plant closure is associated with higher pregnancy hospitalization costs. It appears that women use periods without employment for family planning.

Figure 6

Figure 7 provides detailed results for hospitalizations due to mental health or other reasons, and for psychotropic drugs. Results for mental health costs for men indicate that there is no effect of non-employment on mental health for men both before and after plant closure date. The

[24] Technically, this is a decomposition of the effects reported in the main analysis. Moreover, this allows assessing whether there are effects already before plant closure.

[25] Note that the results for sick leave and overall costs (reflecting to a large extent sickness benefits) appear to be contradictory. Recall however, that the first up to 12 weeks of sickness benefits are paid by the employer and will therefore not be recorded as sickness benefits in our data. Thus, the finding of no effect on overall costs but a strong significant effect on days on sick leave before plant closure can be explained.

point estimates after plant closure are, however, consistently positive explaining why the overall effect is significantly positive as we find in table 11. Results for women indicate that mental health is not affected by job loss due to plant closure. Recall that hospitalization due to other reasons are significantly affected by job loss due to plant closure for women. The second row in Figure 7 indicates that there is an important upward trend in the effect of nonemployment on health care costs already before plant closure reaching marginal significance in the quarter just before plant closure. The effects of non-employment on other hospitalization costs are significantly positive in the first two quarters after the plant closure date and collapse to zero thereafter. The timing of these costs is consistent with these expenditures being indirectly related with health conditions arising due to pregnancies. The pattern of the effects on other hospitalization costs for men does not indicate any effect of nonemployment. The third row shows results for psychotropic drug prescriptions. Results for men indicate that the effects are significantly positive in the third and fourth quarter after plant closure. This suggests that mental health reacts in a relatively sluggish way to changes in the employment situation. There is no effect of nonemployment on the prescription of psychotropic drugs among women.

Figure 7

7 Conclusions

This paper studies the causal effect of unemployment on public expenditures on health care in a typical European welfare state. Our empirical analysis focuses on the case of Austria where public health insurance is mandatory for all employees. To assess the causal relationship between individual unemployment and public health care costs we have exploited a unique data set that combines detailed information on a worker's earnings and employment history (and their firms) with detailed information on payments by the public health insurance authority associated with take-up of health care benefits (both treatment-related health care provisions such as hospitalization, doctor visits, and drug prescriptions; and sickness benefits). To tackle the problem of reverse causality – bad health may cause unemployment – we use job loss due to plant closure as an instrumental variable. Job loss due to plant closure is a meaningful instrument because such job losses are very closely associated with higher subsequent unemployment. Moreover it is very unlikely that job losses due to plant closure are caused by a worker's health.

Our empirical analysis yields several interesting results. *First*, it turns out that unemployment following a plant closure does not cause a significant increase in public health costs associated with take-up of health provisions. Public health costs due to hospitalizations and medical drugs prescription do not increase significantly, and doctor visits even fall. *Second,* while overall take-up of health provisions is not significantly affected, we find that – for males, but not for females – an increase in public health costs due to mental health problems. This result is in line with the hypothesis that, in the short run, unemployment causes mental health problems, whereas physical health is affected only in the long run. *Third,* we find that the public health costs that are associated with payments of sickness benefits strongly increase after a job loss. However, this increase in costs does not reflect a deteriorating health status of displaced workers but is mainly due to sickness benefit rules (for employed workers, employers have to bear sickness benefits, whereas for unemployed workers, the public health insurance pays these benefits). We do not find that male plant-closure workers do have more sickness *days* than non-plant closure workers. For females, however, we find a significant increase in sickness days.

In sum, our results indicate that unemployment is not associated with strong changes in health care costs arising due to treatment of medical conditions but with strong changes in sickness transfer payments. There are two lessons from this result. First, public health expenditures are strongly tied to labor market performance in countries that use their health insurance to cover not only the costs of medical treatments but also to pay out sickness benefits. Second, short work career disruptions do not deteriorate health in the short run. Future research should therefore focus on assessing the long-term consequences of prolonged unemployment.

References

Ahs, A. M. and Westerling, R. (2006). Health care utilization among persons who are unemployed or outside the labour force. *Health Policy*, **78**, 178–193.

Andersen, R. M. (1995). Revisiting the Behavioral Model and Access to Medical Care: Does it Matter? *Journal of Health and Social Behavior*, **36**, 1–10.

Angrist, J. D. and Imbens, G. (1995). Two–Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity. *Journal of the American Statistical Association*, **90**, 431–442.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association*, **91**, 444–455.

Askildsen, J. E., Bratberg, E., and Nilsen, Ø. A. (2005). Unemployment, labor force composition and sickness absence: a panel data study. *Health Economics*, **14**(11), 1087–1101.

Bjorklund, A. (1985). Unemployment and Mental Health: Some Evidence from Panel Data. *The Journal of Human Resources*, **20**(4), 469–483.

Brenner, M. (1979). Mortality and the National Economy: A Review, and the Experience of Wales and England, 1936–76. *The Lancet*, **2**(8142), 568–573.

Browning, M., Dano, A. M., and Heinesen, E. (2006). Job displacement and stress-related health outcomes. *Health Economics*, **15**(10), 1061–1075.

Burgard, S. A., Brand, J. E., and House, J. S. (2005). Causation and Selection in the Relationship of Job Loss to Health in the United States. Mimeo, University of Michigan.

Carr-Hill, R. A., Rice, N., and Roland, M. (1996). Socioeconomic determinants of rates of consultation in general practice based on fourth national survey of general practices. *British Medical Journal*, **312**.

Chevalier, A. and Feinstein, L. (2006). Sheepskin or Prozac: The Causal Effect of Education on Mental Health. IZA Discussion Paper No. 2231.

Clark, A. E. and Oswald, A. J. (1994). Unhappiness and Unemployment. *The Economic Journal*, **104**, 648–659.

Cook, D. G. (1985). A critical view of the unemployment and health debate. *The Statistician*, **34**, 73–82.

Currie, J. and Madrian, B. C. (1999). Health, Health Insurance and the Labor Market. In O. Ashenfelter and D. Card, editors, *Handbook of Labor Economics*, volume 3C, pages 3309–3416. Elsevier Science.

D'Arcy, C. and Siddique, C. (1985). Unemployment and Health: An Analysis of "Canada Health Survey" Data. *International Journal of Health Services*, **15**, 609–635.

Eliason, M. and Storrie, D. (2006). Lasting or Latent Scars? Swedish Evidence on the Long–Term Effects of Job Displacement. *Journal of Labor Economics*, **24**, 831–856.

Field, K. S. and Briggs, D. J. (2001). Socio-economic and locational determinants of accessibility and utilization of primary health-care. *Health & Social Care in the Community*, **9**, 294–308.

Gerdtham, U. and Jonhannesson, M. (2003). A note on the effect of unemployment on mortality. *Journal of Health Economics*, **22**, 505–518.

Gerdtham, U. and Ruhm, C. J. (2006). Deaths Rise in Good Economic Times: Evidence from the OECD. Mimeo, Lund University and University of North Carolina.

Goldsmith, A., Veum, J., and Darity, W. (1996). The psychological impact of unemployment and joblessness. *The Journal of Socioeconomics*, **25**(3), 333–358.

Grossman, M. (1972). On the Concept of Health Capital and the Demand for Health. *Journal of Political Economy*, **80**(2), 223–55.

Hagist, C. and Kotlikoff, L. (2005). Who's Going Broke? Comparing Growth in Healthcare Costs in Ten OECD Countries. *NBER Working Paper No. 11833*.

Henrekson, M. and Persson, M. (2004). The effects on sick leave of changes in the sickness insurance system. *Journal of Labor Economics*, **22**(1), 87–114.

Iversen, L., Sabroe, S., and M.T.Damsgaard (1989). Hospital admissions before and after shipyard closure. *British Medical Journal*, **299**, 1073–1076.

Jin, R. L., Shah, C. P., and Svoboda, T. J. (1995). The Impact of Unemployment on Health: A Review of the Evidence. *Canadian Medical Association Journal*, **153**, 528–540.

Johansson, P. and Palme, M. (1996). Do economic incentives affect work absence? empirical evidence using swedish micro data. *Journal of Public Economics*, **59**(2), 195–218.

Johansson, P. and Palme, M. (2005). Moral hazard and sickness insurance. *Journal of Public Economics*, **89**(9-10), 1879–1890.

Kasl, S. V. and Jones, B. A. (2000). The Impact of Job Loss and Retirement on Health. In L. Berkman and I. Kawachi, editors, *Social Epidemiology*. Oxford University Press.

Kasl, S. V. and Jones, B. A. (2006). Unemployment and Health. Mimeo, Department of Epidemiology and Public Health, Yale University School of Medicine.

Keefe, V., Reid, P., Ormsby, C., Robson, B., Purdie, G., Baxter, J., and Incorporated, N. K. I. (2001). Serious health events following involuntary job loss in New Zealand meat processing workers. *International Journal of Epidemiology*, **31**, 1155–1161.

Kessler, R. C., House, J. S., and Turner, J. B. (1987). Unemployment and Health in a Community Sample. *Journal of Health and Social Behavior*, **28**(51-59).

Kessler, R. C., Turner, J. B., and House, J. S. (1989). Unemployment, Reemployment, and Emotional Functioning in a Community Sample. *American Sociological Review*, **54**, 648–657.

Korpi, T. (1997). Employment, unemployment, labor market policies and subjective well-being among Swedish youth. *Labour Economics*, **4**, 125–147.

Kuhn, A. and Ruf, O. (2006). Austrian Social Security Database. IEW working paper no. 309, University of Zurich.

Martikainen, P. and Valkonen, T. (1996). Excess mortality of unemployed men and women during a period of rapidly increasing unemployment. *The Lancet*, **348**, 909–912.

Morris, J. and Cook, D. (1991). A critical review of the effect of factory closures on health. *British Journal of Industrial Medicine*, **48**, 1–8.

Morris, J., Cook, D., and Shaper, A. (1994). Loss of employment and mortality. *British Medical Journal*, **308**, 1135–1139.

Moser, K., Goldblatt, P., Fox, A., and Jones, D. (1987). Unemployment and mortality: comparison of the 1971 and 1981 longitudinal study census samples. *British Medical Journal*, **294**, 86–90.

Nylen, L., Voss, M., and Floderus, B. (2001). Mortality among women and men relative to unemployment, part time work, overtime work, and extra work: a study based on data from the Swedish twins registry. *Occupational and Environmental Medicine*, pages 52–57.

Platt, S. (1984). Unemployment and Suicidal Behavior. *Social Science and Medicine*, **19**, 93–115.

Ruhm, C. J. (2000). Are recessions good for your health? *The Quarterly Journal of Economics*, **115**(2), 617–650.

Ruhm, C. J. (2003). Good Times Make You Sick. *Journal of Health Economics*, **22**, 637–658.

Ruhm, C. J. (2005). Commentary: Mortality increases during economic upturns. *International Journal of Epidemiology*, **34**, 1206–1211.

Stewart, J. M. (2001). The impact of health status on the duration of unemployment spells and the implications for studies of the impact of unemployment on health status. *Journal of Health Economics*, **20**(5), 781–796.

Studnicka, M., Studnicka-Benke, A., Wögerbauer, G., Rastetter, D., Wenda, R., Gathmann, P., and Ringel, E. (1991). Psychological health, self-reported physical health and health service use. *Social Psychiatry and Psychiatric Epidemiology*, pages 86–91.

Sullivan, D. G. and von Wachter, T. (2006). Mortality, Mass–Layoffs, and Career Outcomes: An Analysis Using Administrative Data. *FRB of Chicago Working Paper No. 2006–21*.

Theodossiou, I. (1998). The effects of low-pay and unemployment on psychological well-being: A logistic regression approach. *Journal of Health Economics*, **17**, 85–104.

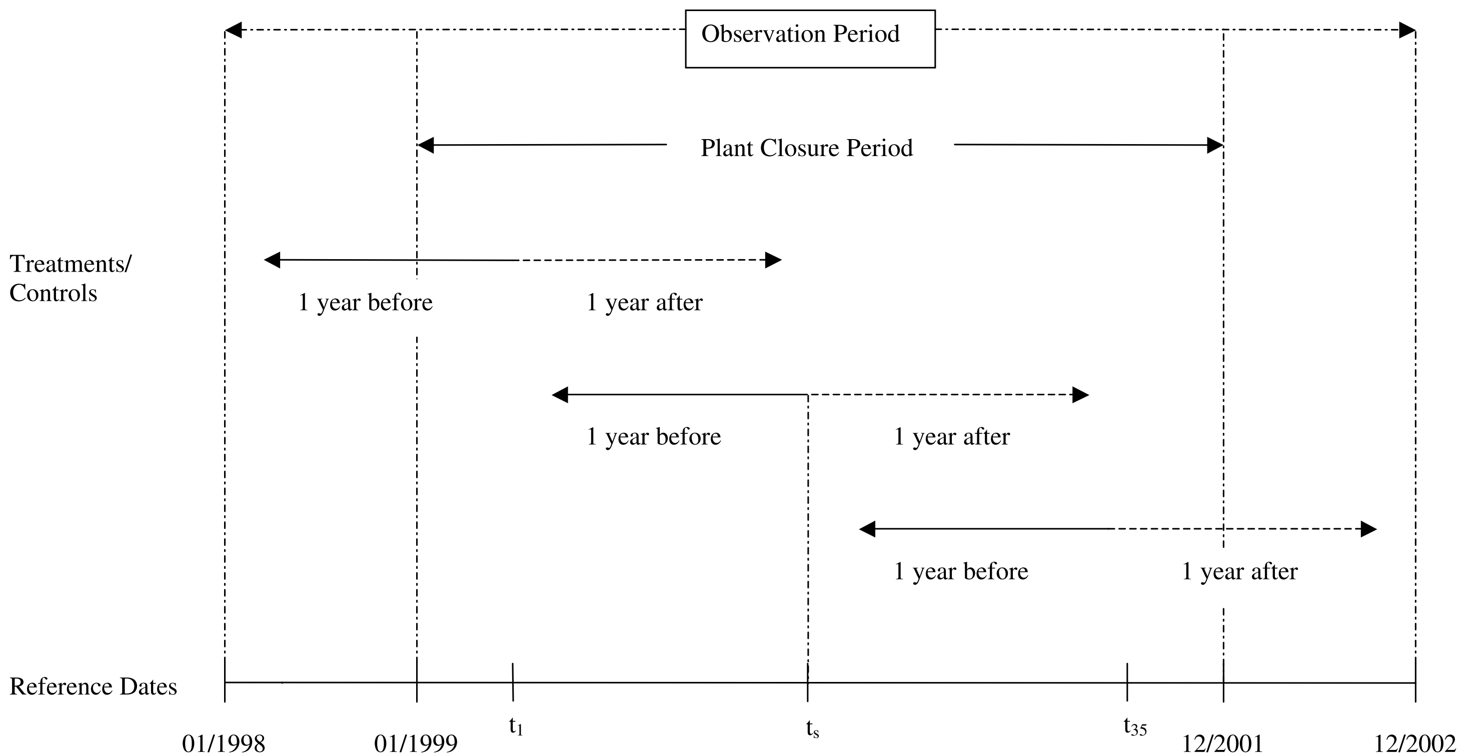
Turner, J. B. (1995). Economic Context and the Health Effects of Unemployment. *Journal of Health and Social Behavior*, **36**(3), 213–229.

Virtanen, P. (1993). Unemployment, re-employment and and the use of primary health care services. *Scandinavian Journal of Primary Health Care*, **11**, 225–227.

Voss, M., Nylen, L., Floderus, B., Diderichsen, F., and Terry, P. D. (2004). Unemployment and Early Cause–Specific Mortality: A Study Based on the Swedish Twin Registry. *American Journal of Public Health*, **94**(12), 2155–2161.

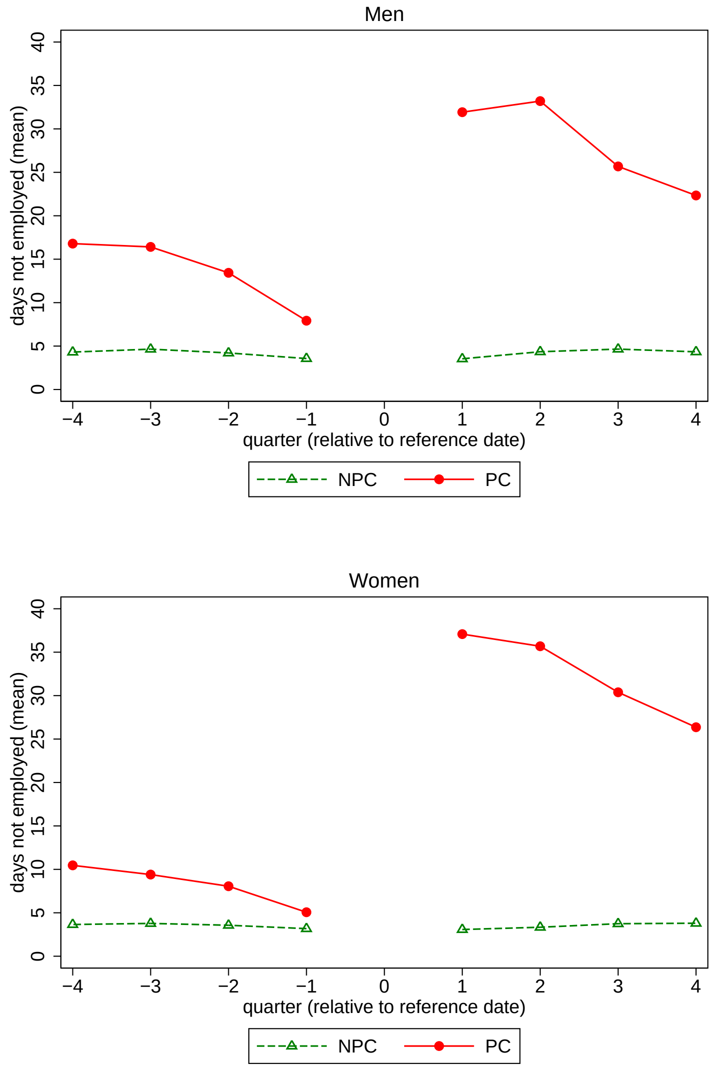
Winkelmann, L. and Winkelmann, R. (1998). Why are the unemployed so unhappy? evidence from panel data. *Economica*, **65**, 1–16.

Figure 1: Setup and definitions



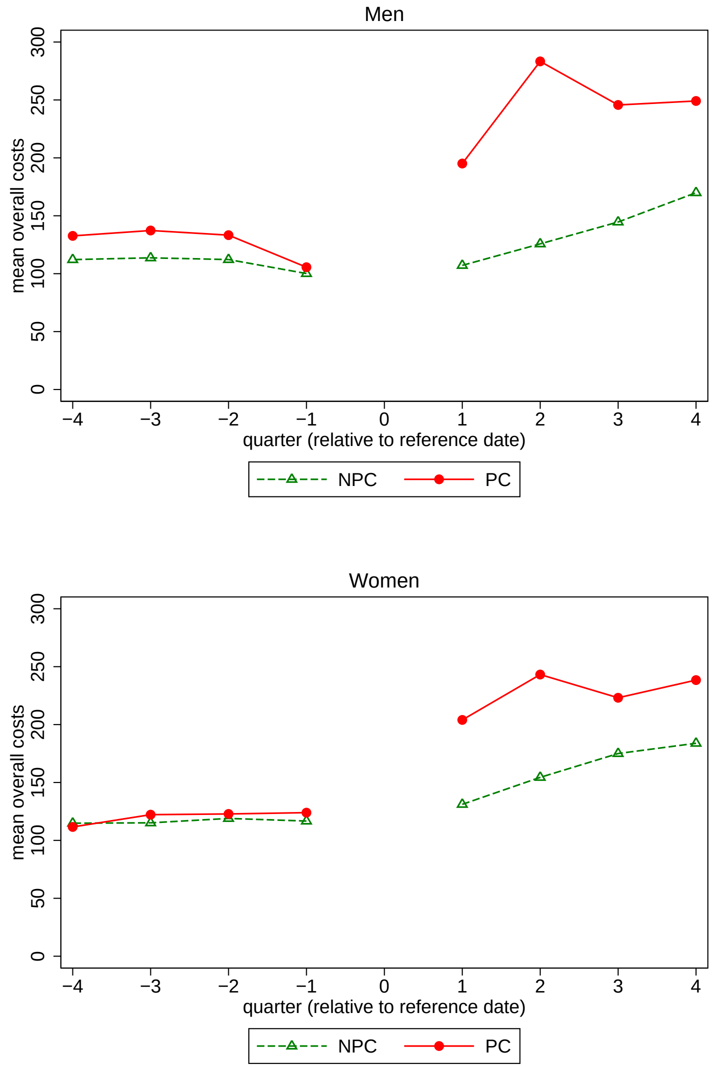
Notes: Reference dates are the 10th of each month, there are 35 such dates in total. t_1: January 10, 1999; t_2: February 10, 1999; …; t_{35}: December 10, 2001. If a plant closure occurs between t_s and t_{s+1}, t_s is the reference date. For each plant closure date, we draw a random sample of control observations among all employed workers. Plant closure workers are workers employed at dates t_{s-4}, t_{s-3}, t_{s-2}, or t_{s-1} in the PC firm. Workers in the control sample include all workers employed at the reference date. (Notice that the same worker may be repeatedly included in the control sample, if employed at more (or all) reference dates.)

Figure 2: Nonemployment, by sex



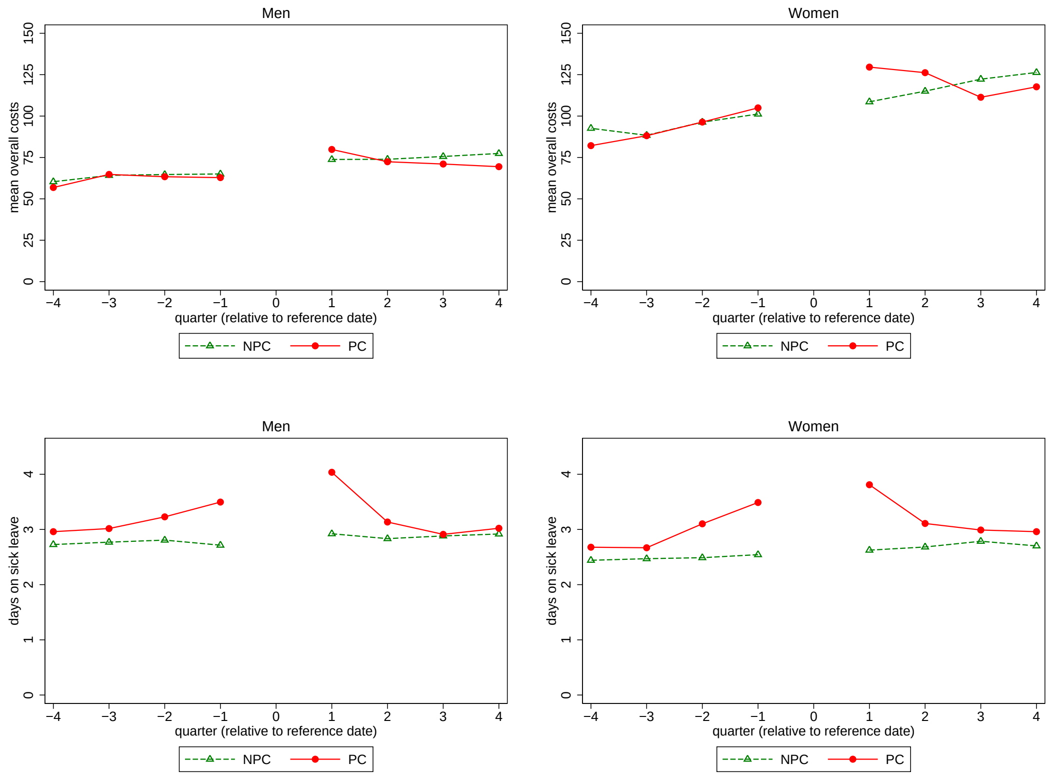
Notes: The vertical axis measures, for any given quarter since the date of (to) the plant closure (reference date), the average number of days not employed among plant closure (PC) and non plant closure (NPC) workers. For the definition of plant closure date (for PC workers) and reference date (for NPC workers) see Figure 1.

Figure 3: Overall health costs, by sex



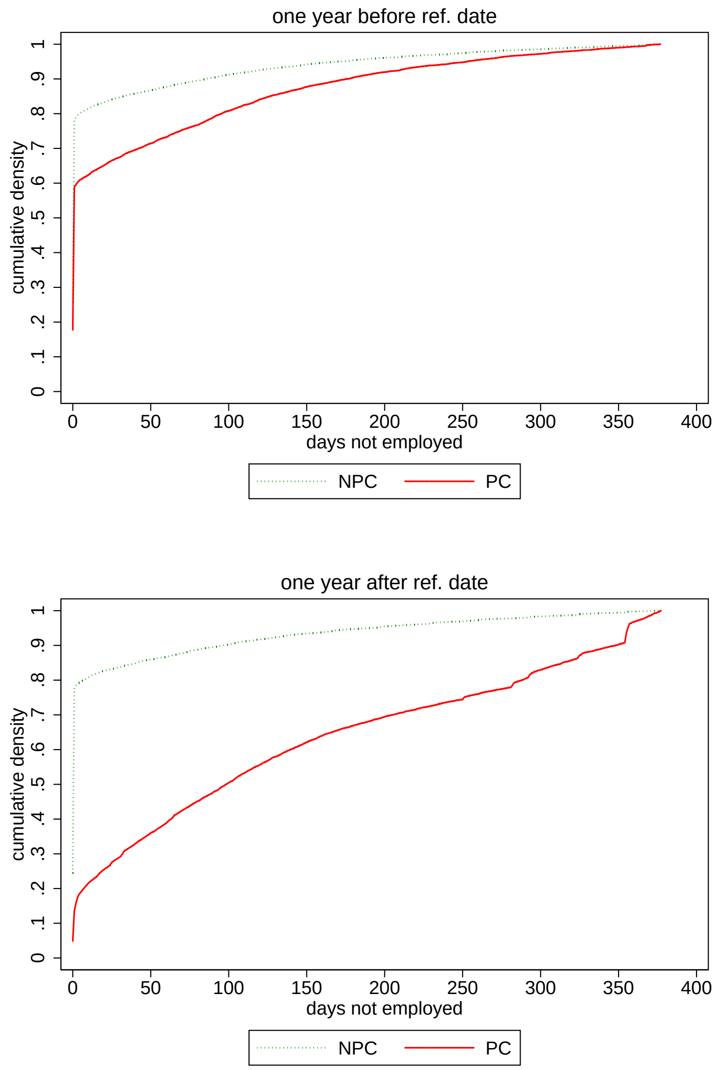
Notes: The vertical axis measures, for any given quarter since the date of (to) plant closure (reference date), the health insurance costs per worker among plant closure (PC) and non plant closure (NPC) workers. For definition of plant closure data (for PC workers) and reference date (for NPC workers) see Figure 1. See Table A.1 for a description of the health care services covered by the overall cost measure.

Figure 4: Overall health costs (excluding sick pay) & days on sick leave, by sex



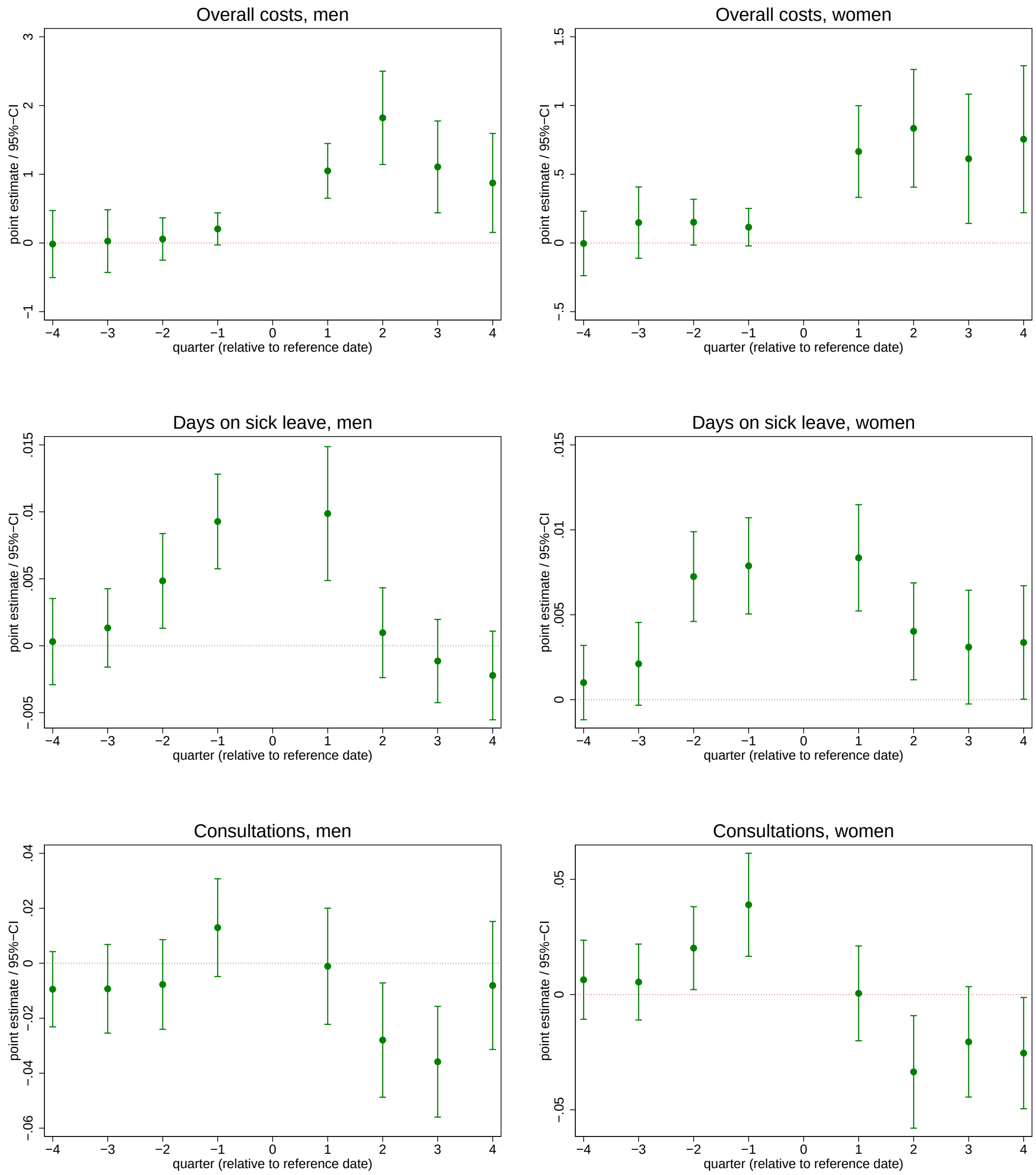
Notes: The vertical axis measures, for any given quarter since the date of (to) plant closure (reference date), overall health costs excluding costs arising from days on sick leave (upper panel) and the average days on sick leave (lower panel) among plant closure (PC) and non plant closure (NPC) workers. For definition of plant closure data (for PC workers) and reference date (for NPC workers) see Figure 1. See Table A.1 for a description of the health care services covered by the cost measure.

Figure 5: Days not employed (cdf), one year before (after) the plant closure date (reference date)



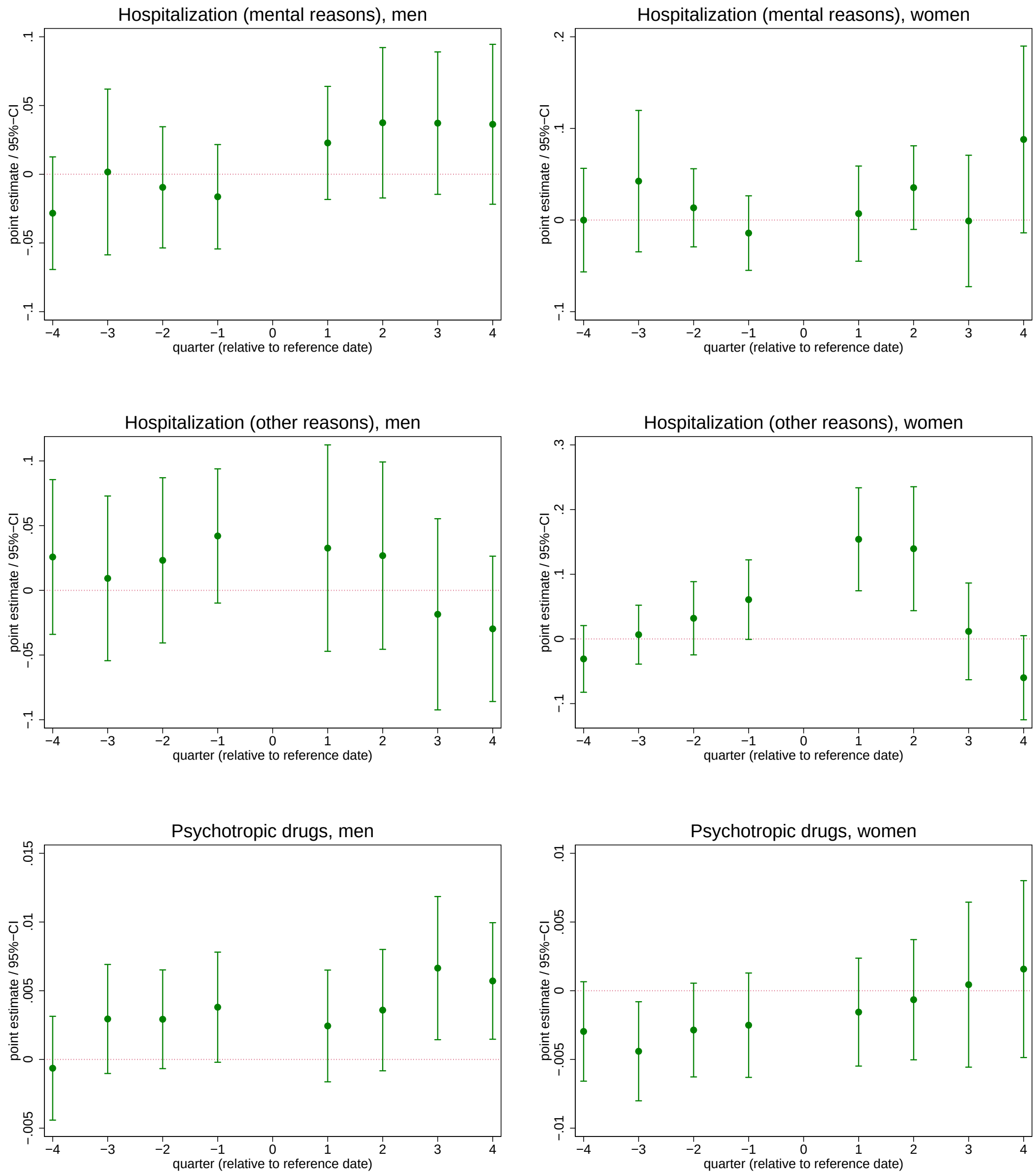
Notes: The vertical axis measures the fraction of workers non–employed (unemployed or out of labor force) for x days or less during the year prior to (upper panel) and after (lower panel) the plant closure (reference date), separately for PC and NPC workers.

Figure 6: IV–estimates for selected regressors (1/2)



Notes: Each graph shows the estimated coefficient of the number days not employed one year after the plant closure (reference) date, instrumented by the dummy variable PC (equal to 1 for PC workers and 0 otherweise), and its corresponding 95% confidence interval. The dependent variable in the upper panel is overall health costs, for each of the eight quarters centered around the plant closure (reference) date. The dependent variable in the middle (lower) panel is the number of days on sick leave (health costs due to consultations). The left (right) panels show the estimated coefficients for men (women). All regressions include the full set of control variables as in Table 4.

Figure 7: IV–estimates for selected regressors (2/2)



Notes: Each graph shows the estimated coefficient of the number days not employed one year after the plant closure (reference) date, instrumented by the dummy variable PC (equal to 1 for PC workers and 0 otherweise), and its corresponding 95% confidence interval. The dependent variable in the upper panel is health costs of hospitalization due to mental reasons, for each of the eight quarters centered around the plant closure (reference) date. The dependent variable in the middle (lower) panel is health costs of hospitalization due to other reasons (health costs of psychotropic drugs). The left (right) panels show the estimated coefficients for men (women). All regressions include the full set of control variables as in Table 4.

Table 1: Health indicators, one year *before* the plant closure (reference) date

	Men	Women
Overall health costs	455.497	469.108
	(2918.971)	(1725.697)
Sick leave pay	202.469	92.008
	(2662.438)	(1314.157)
Days on sick leave	11.464	10.373
	(20.243)	(18.947)
Consultations	81.270	155.789
	(124.490)	(172.825)
Hospitalizations	115.658	136.051
	(574.983)	(574.091)
Drugs	56.100	85.260
	(305.577)	(411.819)
n	33,352	19,243

Notes: All variables (except days on sick leave) are measured in nominal Euros and cover the four quarters before the plant closure (reference) date. See table A.1 for the definitions of the various health measures.

Table 2: Descriptive statistics: Health indicators, one year *before* the plant closure date

	Men		Women	
	NPC	PC	NPC	PC
Overall health costs	438.205	505.810	465.721	480.660
	(2747.959)	(3367.170)	(1645.366)	(1975.412)
Sick leave pay	183.928	256.415	87.160	108.542
	(2504.585)	(3075.511)	(1276.383)	(1435.563)
Days on sick leave	11.018	12.760	9.944	11.835
	(19.872)	(21.232)	(18.705)	(19.684)
Consultations	84.978	70.481	156.622	152.946
	(128.325)	(111.903)	(174.884)	(165.596)
Hospitalizations	110.947	129.365	134.092	142.731
	(561.246)	(613.031)	(546.794)	(658.760)
Drugs	58.351	49.550	87.846	76.441
	(317.279)	(268.557)	(438.260)	(304.736)
n	24,821	8,531	14,880	4,363

Notes: All variables (except days on sick leave) are measured in nominal Euros and cover the four quarters before the plant closure (reference) date. NPC (PC) refers to the non plant closure (plant closure) workers. Also see table A.1 for the definitions of the various health measures.

Table 3: Descriptive statistics, background characteristics

	Men		Women	
	NPC	PC	NPC	PC
Individual characteristics				
Days not employed	22.858	55.847	24.378	34.044
	(59.882)	(85.348)	(68.013)	(79.404)
Age	36.928	35.671	36.137	33.267
	(10.374)	(10.264)	(10.096)	(11.044)
Blue collar worker	0.564	0.740	0.315	0.360
	(0.496)	(0.439)	(0.464)	(0.480)
Wage (in 100 €)	237.375	195.272	158.165	126.329
	(94.611)	(103.692)	(82.596)	(81.566)
Tenure in last five years	3.198	1.800	3.252	2.508
	(1.917)	(1.791)	(1.850)	(1.770)
Size of firm (one year before)	669.972	56.537	1075.416	74.154
	(2005.737)	(74.868)	(2954.541)	(99.240)
Industry (employer)				
Agriculture	0.005	0.006	0.005	0.006
	(0.072)	(0.076)	(0.070)	(0.075)
Mining	0.006	0.002	0.001	0.000
	(0.080)	(0.049)	(0.038)	(0.021)
Construction	0.129	0.299	0.028	0.062
	(0.336)	(0.458)	(0.166)	(0.241)
Manufacturing	0.387	0.291	0.200	0.263
	(0.487)	(0.454)	(0.400)	(0.440)
Transportation	0.059	0.058	0.025	0.013
	(0.236)	(0.233)	(0.155)	(0.112)
Wholesale trade	0.065	0.044	0.067	0.068
	(0.247)	(0.205)	(0.251)	(0.252)
Retail trade	0.057	0.047	0.120	0.150
	(0.232)	(0.212)	(0.325)	(0.357)
Information, finance	0.094	0.079	0.086	0.043
	(0.292)	(0.269)	(0.280)	(0.203)
Other services	0.107	0.073	0.338	0.339
	(0.309)	(0.259)	(0.473)	(0.473)
Industry unknown	0.090	0.102	0.130	0.056
	(0.286)	(0.303)	(0.336)	(0.230)
Region (employer)				
Outside Upper Austria	0.140	0.159	0.099	0.068
	(0.347)	(0.357)	(0.299)	(0.252)
Inside Upper Austria	0.780	0.814	0.779	0.924
	(0.414)	(0.389)	(0.415)	(0.266)
Region unknown	0.079	0.036	0.122	0.008
	(0.270)	(0.186)	(0.327)	(0.092)
Reference date				
Year = 1999	0.315	0.415	0.310	0.483
	(0.465)	(0.493)	(0.463)	(0.500)
Year = 2000	0.332	0.360	0.327	0.316
	(0.471)	(0.480)	(0.469)	(0.465)
Year = 2001	0.353	0.224	0.362	0.201
	(0.478)	(0.417)	(0.481)	(0.400)

Table 3: Continued

	Men		Women	
	NPC	PC	NPC	PC
Month = January	0.072	0.059	0.068	0.051
	(0.259)	(0.235)	(0.252)	(0.220)
Month = February	0.082	0.056	0.085	0.058
	(0.274)	(0.230)	(0.279)	(0.233)
Month = March	0.083	0.091	0.079	0.094
	(0.277)	(0.288)	(0.269)	(0.292)
Month = April	0.077	0.100	0.076	0.084
	(0.267)	(0.300)	(0.266)	(0.277)
Month = May	0.087	0.075	0.082	0.079
	(0.282)	(0.263)	(0.274)	(0.269)
Month = June	0.087	0.107	0.108	0.090
	(0.282)	(0.309)	(0.310)	(0.287)
Month = July	0.086	0.057	0.075	0.047
	(0.281)	(0.231)	(0.263)	(0.212)
Month = August	0.080	0.048	0.076	0.049
	(0.272)	(0.213)	(0.265)	(0.216)
Month = September	0.082	0.103	0.079	0.080
	(0.275)	(0.304)	(0.269)	(0.272)
Month = October	0.087	0.072	0.095	0.043
	(0.282)	(0.258)	(0.294)	(0.203)
Month = November	0.083	0.080	0.069	0.042
	(0.275)	(0.272)	(0.253)	(0.202)
Month = December	0.091	0.153	0.109	0.282
	(0.288)	(0.360)	(0.311)	(0.450)
n	24,821	8,531	14,880	4,363

Table 4: OLS–estimates, one year *before* plant closure date (men)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
PC	22.822	12.433	10.388	−1.134	11.844	1.723	1.320**
	(50.381)	(11.565)	(46.976)	(2.051)	(8.957)	(3.738)	(0.442)
Age (in years)	57.304***	−7.656*	64.960***	−4.946***	1.365	−4.074**	−0.366***
	(15.750)	(3.358)	(14.709)	(0.538)	(2.613)	(1.497)	(0.099)
Age/10 squared	−39.323*	22.852***	−62.175***	9.314***	4.482	9.055***	0.739***
	(19.547)	(4.450)	(18.237)	(0.731)	(3.380)	(2.114)	(0.128)
Tenure (in years)	216.004***	13.066	202.938***	−1.228	9.897	4.397	1.278***
	(59.619)	(11.073)	(55.280)	(1.834)	(9.103)	(3.972)	(0.373)
Tenure squared	−29.740**	−0.731	−29.009***	0.695*	−1.388	−0.037	−0.176**
	(9.430)	(1.920)	(8.669)	(0.335)	(1.506)	(0.788)	(0.063)
Blue collar worker	−58.225	−24.257*	−33.968	−2.543	−9.819	−11.896*	4.504***
	(48.234)	(11.381)	(44.667)	(1.972)	(8.276)	(5.636)	(0.351)
Size of firm	0.090***	0.022*	0.068*	0.001	0.021**	−0.000	0.002***
	(0.025)	(0.010)	(0.027)	(0.002)	(0.006)	(0.003)	(0.000)
Size of firm/10 squared	−0.001**	−0.000	−0.000*	−0.000	−0.000**	0.000	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage (in 100 €)	−10.447***	−1.091***	−9.356***	0.126***	−1.069***	−0.147	−0.026**
	(1.799)	(0.256)	(1.701)	(0.033)	(0.196)	(0.123)	(0.008)
Wage/10 squared	1.305***	0.104*	1.201***	−0.024***	0.112**	0.017	−0.001
	(0.294)	(0.050)	(0.277)	(0.007)	(0.035)	(0.029)	(0.001)
mean dep. var.	455.497	253.028	202.469	81.270	115.658	56.100	11.464
s.d. dep. var.	2918.971	723.886	2662.438	124.490	574.983	305.577	20.243
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.023	0.040	0.017	0.125	0.017	0.019	0.092
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table A.1). PC is a dummy variable which equals 1 for PC workers and 0 otherwise. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).

Table 5: OLS–estimates, one year *before* plant closure date (women)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
PC	50.278	30.707	19.571	8.704**	24.490	−2.486	2.236***
	(39.889)	(17.209)	(29.195)	(2.974)	(13.603)	(7.269)	(0.444)
Age (in years)	0.450	−9.591*	10.041	−0.287	−7.033*	−2.271	−0.800***
	(8.529)	(3.942)	(6.514)	(0.792)	(2.896)	(1.628)	(0.109)
Age/10 squared	18.744	25.379***	−6.635	4.327***	13.467***	7.585***	1.292***
	(11.829)	(5.457)	(9.105)	(1.101)	(3.971)	(2.253)	(0.156)
Tenure (in years)	32.197	−6.213	38.409	−2.722	1.012	−4.503	−0.173
	(40.776)	(15.459)	(34.631)	(3.032)	(12.305)	(5.996)	(0.366)
Tenure squared	−0.311	4.003	−4.314	1.080*	1.093	1.830	0.071
	(6.682)	(2.675)	(5.590)	(0.532)	(2.162)	(1.052)	(0.064)
Blue collar worker	32.483	8.922	23.561	−0.875	21.426*	−11.628*	5.276***
	(32.395)	(14.382)	(25.861)	(2.989)	(10.810)	(5.511)	(0.470)
Size of firm	0.037	0.023	0.014	0.003	0.017*	0.003	0.001*
	(0.024)	(0.012)	(0.016)	(0.003)	(0.007)	(0.004)	(0.001)
Size of firm/10 squared	−0.000	−0.000*	−0.000	−0.000	−0.000*	−0.000	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage (in 100 €)	−3.597***	−0.197	−3.400***	0.176***	−0.332	−0.041	0.016*
	(1.001)	(0.287)	(0.838)	(0.049)	(0.193)	(0.153)	(0.007)
Wage/10 squared	0.539**	−0.041	0.580***	−0.058***	0.024	−0.007	−0.007***
	(0.192)	(0.063)	(0.158)	(0.012)	(0.044)	(0.031)	(0.002)
mean dep. var.	469.108	377.100	92.008	155.789	136.051	85.260	10.373
s.d. dep. var.	1725.697	818.573	1314.157	172.825	574.091	411.819	18.947
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.022	0.041	0.010	0.132	0.014	0.015	0.071
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table A.1). PC is a dummy variable which equals 1 for PC workers and 0 otherwise. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).

Table 6: First–stage regressions

	Men	Women
PC	83.755***	122.582***
	(1.068)	(1.685)
Age (in years)	−3.193***	−6.565***
	(0.291)	(0.417)
Age/10 squared	5.176***	8.845***
	(0.372)	(0.558)
Tenure (in years)	−6.463***	−4.530*
	(1.189)	(1.776)
Tenure/10 squared	67.041**	35.852
	(20.836)	(30.617)
Blue collar worker	2.549*	−5.322***
	(1.053)	(1.550)
Size of firm	0.001	0.002
	(0.001)	(0.001)
Size of firm/10 squared	−0.000	−0.000**
	(0.000)	(0.000)
Agriculture	11.756*	11.610
	(5.851)	(9.031)
Mining	−5.957	−5.300
	(5.790)	(17.975)
Construction	−6.205***	−8.652*
	(1.625)	(3.623)
Manufacturing	−12.859***	−5.190**
	(1.472)	(1.876)
Transportation, utilities	−15.344***	−4.513
	(2.147)	(4.448)
Wholesale treade	−7.880***	−7.286**
	(2.113)	(2.775)
Retail trade	−8.926***	−15.489***
	(2.193)	(2.169)
Information, finance	−4.342*	−8.842***
	(1.856)	(2.623)
Wage (in 100 €)	−0.678***	−0.495***
	(0.019)	(0.027)
Wage/10 squared	0.104***	0.103***
	(0.004)	(0.007)
Constant	187.892***	256.505***
	(10.928)	(20.758)
Regional dummies	✓	✓
Time dummies	✓	✓
n	33,352	19,243
R^2	0.317	0.339
F–statistic	261.652	169.575
p–value (F–statistic)	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Dependent variable is days not employed one year after the plant closure date. There are 28 regional dummies and 15 time dummies (year and month).

Table 7: Comparison between IV and OLS, one year *after* plant closure date

	Overall costs	Overall costs	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
IV	4.850***	0.151	4.698***	−0.073*	0.146	0.078	0.007
	(1.065)	(0.155)	(1.010)	(0.032)	(0.119)	(0.069)	(0.006)
OLS	9.035***	0.980***	8.055***	0.004	0.826***	0.150**	0.035***
	(0.872)	(0.107)	(0.830)	(0.012)	(0.087)	(0.047)	(0.003)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
B. Women							
IV	2.866***	0.346	2.521***	−0.079*	0.479**	−0.054	0.019***
	(0.812)	(0.190)	(0.713)	(0.032)	(0.160)	(0.058)	(0.005)
OLS	5.737***	1.660***	4.077***	0.142***	1.420***	0.099**	0.030***
	(0.718)	(0.129)	(0.663)	(0.021)	(0.105)	(0.030)	(0.003)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. See also the notes of table 4.

Table 8: IV–estimates with additional control variables, one year *after* plant closure date

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
Days not employed	4.630***	0.098	4.531***	−0.089**	0.100	0.087	0.002
	(1.051)	(0.142)	(1.003)	(0.029)	(0.117)	(0.063)	(0.005)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.063	0.169	0.043	0.154	0.050	0.224	0.166
B. Women							
Days not employed	2.653***	0.219	2.434***	−0.113***	0.402**	−0.070	0.012**
	(0.781)	(0.163)	(0.709)	(0.032)	(0.151)	(0.052)	(0.005)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.098	0.212	0.049	0.173	0.088	0.230	0.171
Control variables	✓	✓	✓	✓	✓	✓	✓
Nonemployment before	✓	✓	✓	✓	✓	✓	✓
Health costs before	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. The table corresponds to table 4, except that the following variables are additionally controlled for: days not employed one year before the plant closure (reference date), overall health costs in the year before the plant closure (reference date), number of days on sick leave and sickness benefits in the year before the plant closure (reference) date.

Table 9: IV–estimates, difference in health measures (after minus before plant closure)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
Days not employed	4.577***	0.003	4.574***	0.059*	0.004	0.058	−0.008
	(1.196)	(0.160)	(1.162)	(0.024)	(0.144)	(0.055)	(0.005)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.019	0.003	0.019	0.013	0.002	0.003	0.001
B. Women							
Days not employed	2.456**	0.095	2.361***	−0.150***	0.279	−0.034	0.001
	(0.778)	(0.160)	(0.704)	(0.030)	(0.143)	(0.044)	(0.005)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.018	0.007	0.015	0.005	0.011	0.002	0.007
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.11% level, respectively. Robust standard errors (clustered by firm) in parentheses. The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. The table corresponds to table 4, except that the dependent variable is the difference in the sickness measure one year after the plant closure date minus the sickness measure one year before the plant closure date.

Table 10: Detailed results for hospitalizations, one year *after* plant closure date

	Hospitalizations	Cancer	Heart	Mental	Other	Pregnancy	Respiratory	Stroke
A. Men								
IV	0.146	−0.018	0.015	0.134*	0.011		0.017	−0.013
	(0.119)	(0.030)	(0.019)	(0.066)	(0.081)		(0.037)	(0.015)
OLS	0.826***	0.123***	0.024*	0.162***	0.455***		0.038	0.024*
	(0.087)	(0.027)	(0.011)	(0.040)	(0.055)		(0.034)	(0.010)
n	33,352	33,352	33,352	33,352	33,352		33,352	33,352
B. Women								
IV	0.479**	0.055	0.025	0.129	0.245*	0.203***	0.029	−0.005
	(0.160)	(0.047)	(0.038)	(0.093)	(0.096)	(0.052)	(0.015)	(0.011)
OLS	1.420***	0.066**	0.020	0.235***	1.069***	0.838***	0.021**	0.010
	(0.105)	(0.023)	(0.023)	(0.062)	(0.064)	(0.048)	(0.008)	(0.007)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. See also the notes of table 4.

Table 11: Detailed results for drug prescriptions, one year *after* plant closure date

	Drugs	Specific	Psychosomatic	Psychotropic	Nonspecific
A. Men					
IV	0.078	0.017	−0.001	0.018*	0.061
	(0.069)	(0.011)	(0.006)	(0.008)	(0.067)
OLS	0.150**	0.031***	0.014***	0.017***	0.119**
	(0.047)	(0.006)	(0.003)	(0.004)	(0.046)
n	33,352	33,352	33,352	33,352	33,352
B. Women					
IV	−0.054	0.002	0.002	−0.000	−0.056
	(0.058)	(0.013)	(0.008)	(0.009)	(0.057)
OLS	0.099**	0.028***	0.010**	0.018***	0.070*
	(0.030)	(0.006)	(0.004)	(0.004)	(0.029)
n	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. See also the notes of table 4.

A Appendix

Table A.1: Health Indicators: Definitions

Indicator	Definition
Consultations	Includes all costs[1] arising from consultations by a physician
Drugs	Includes all costs arising from prescribed or selfmedicated drugs
Psychosomatic drugs:	Includes drugs targeted at treating psychosomatic afflictions (e.g. migraine therapeutics, antiinflammatory drugs)
Psychotropic drugs:	Includes drugs targeted at treating psychological stress (e.g. sedatives, benzodiazepins, antidepressants)
Specific drugs:	Includes psychosomatic and psychotropic drugs
Overall:	Includes all drugs
Hospitalisation	Includes costs due to hospitalisation. These costs are classified by the main diagnosis of the hospitalisation (ICD–9 codes)[2]
Cancer:	Includes ICD–9 Codes 140–239
Heart:	Includes ICD–9 Codes 391, 392.0, 393–398, 402, 404, 410–429
Mental:	Includes ICD–9 Codes 290–319, V70.1, V70.2, V71.0
Respiratory:	Incudes ICD–9 Codes 460–519
Cerebrovascular:	Includes ICD–9 Codes 430–438
Other:	Includes hospitalisation due to all other reasons
Overall:	Includes hospitalisation due to any cause
Pregnancy:	Includes ICD–9 Codes 630–676
Incapacity to Work	Includes all costs arising from being on sick leave ("Krankengeld")
Overall costs	Includes the overall costs from consultations, drugs, hospitalisaiton, and days on sick leave

1: All variables measured in (nominal) Euros.

2: Classification largely taken from Keefe et al. (2002).

Table A.2: OLS–estimates, one year *before* plant closure date

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
A. Men							
Days not employed	9.234***	0.793***	8.441***	0.014	0.735***	0.044	0.028***
	(1.280)	(0.144)	(1.207)	(0.018)	(0.122)	(0.045)	(0.005)
mean dep. var.	455.497	253.028	202.469	81.270	115.658	56.100	11.464
s.d. dep. var.	2918.971	723.886	2662.438	124.490	574.983	305.577	20.243
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.037	0.041	0.031	0.125	0.019	0.019	0.094
B. Women							
Days not employed	4.597***	0.943***	3.654***	0.067*	0.670***	0.206*	0.026***
	(0.897)	(0.196)	(0.775)	(0.026)	(0.132)	(0.102)	(0.005)
mean dep. var.	469.108	377.100	92.008	155.789	136.051	85.260	10.373
s.d. dep. var.	1725.697	818.573	1314.157	172.825	574.091	411.819	18.947
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.036	0.044	0.026	0.132	0.017	0.015	0.073
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, various subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table **??**). Days not employed measures the number of days not in employment during the year prior to plant closure (prior to the reference date). Included control variables are age, tenure, wage, firm size, and their squares. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).