Does Unemployment Make You Sick?

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Abstract

This paper studies the causal effect of involuntary job loss on health using individual social security data matched to health insurance records from Austria. We address the important issue of reverse causality – job loss being caused by deteriorating health – using worker displacement due to plan closure as an instrument. The idea is that plant closure induces a sharp and strong change in the employment career of the worker. However, deteriorating health status of workers is an unlikely cause of plant closure. Second, we measure health status using data on *actual usage* of drugs instead of subjective health status. Estimates suggest that job loss increases drug use strongly. Moreover, there are important differences in the health effects of unemployment with respect to gender, age, stability of employment, industry, and skill level.

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"Persistent unemployment has been a persistent problem for economic theory. It is obviously a problem for the persistently unemployed. "

Robert M. Solow (1990): p.28.

"Being in employment is more satisfying than suffering unemployment. What I have found so striking in so many cases is that even when the previous job was consciously experienced as bad, many people suffer when they are unemployed."

Marie Jahoda in Fryer (1986): p.111.

1 Introduction

Does unemployment causally affect individuals' health? There are at least three reasons why economists should be genuinely interested in an answer to that question. First of all, being concerned with the aggregate and the distribution of welfare in society, economists should be interested in the way unemployment affects individuals' well-being – of which health is one of the most important component. While in most economic models unemployment reduces individual welfare through loss of income, many studies have shown that, holding income level constant, unemployment per se strongly affects individual welfare levels.

Second, health is a central determinant of a worker's productivity. When episodes of unemployment strongly deteriorate an individuals' health condition, human capital depreciates which may have long-lasting effects on the productivity of an economy's resources. It is important to note that a workers' productivity is affected by his or her health condition defined in a broad sense. It has to include not only the *physical constitution* but also the *psychic condition* of individuals.

Finally, understanding how unemployment affects physical and mental well-being of workers should add to our understanding of the process of unemployment, and more generally, of labor market behavior. Given that unemployment is *the* major economic problem in many countries (particularly, but not exclusively, in Europe) a better understanding of how unemployment affects workers is of central importance for policy. Hence a better understanding of how unemployment affects a *worker's health* contributes to both a better understanding of the unemployment problem as well as of the costs and benefits that unemployment-related policies impose on individuals.

Despite the potentially high costs that unemployment imposes on *individuals*, economists have almost exclusively concentrated on *macroeconomic costs*, such as the loss of aggregate output and the fiscal burden associated with joblessness. Only recently have economists begun to study how the subjective well-being of individuals is affected when they experience unemployment (see e.g. Oswald 1997 or Frey and Stutzer 2002 for recent overviews).

Unlike economics, neighboring disciplines such as social psychology, sociology, and social medicine have a longer tradition in studying health effects of unemployment. A common hypothesis in these areas holds that unemployment leads to *emotional distress*. Such distress is either due to the financial strain associated with the loss in income or, more importantly, due to the emotional damage that unemployment does to an individual's self-esteem. When such strains are long-lasting, a worker's resistance to (psycho-somatic and mental) illnesses may be seriously weakened.

Even if there are many good arguments favoring the view that unemployment may indeed have a negative impact on one's health, empirical work has nonetheless to acknowledge the main difficulty of establishing a causal effect beyond mere statistical association on this behalf. As long as we do not know about events that push people into unemployment but are – at the same time – unrelated to health status, it is very difficult to separate the effect of unemployment on health from selection into unemployment due to bad health.

The present analysis sheds new light on the causal effect of unemployment on individuals' health. It is innovative in at least three important respects. First, it tackles the causality issue by applying modern econometric techniques that have been recently developed to study causal relationships in economics. We apply IV-estimation techniques using plant closure as an instrument. The instrument, experience of job-loss due to shut-down of one's firm, is clearly strongly correlated with subsequent unemployment. Moreover, the chronic emotional strains to which plant-closure workers are exposed are most likely due to the subsequent unemployment experience rather than to the job-loss as such. Workers who immediately find a new job are less likely to be exposed to financial distress due to a loss in earnings; furthermore, a job-loss that is due to the poor performance of the firm rather than the individual him- or herself is, *per se,* less likely to affect one's health or well-being. In other words, our main identifying assumption is that workers who find a new job immediately after the shut-down of their old plant are not exposed to distress.

The second innovative feature of our study concerns the health indicator we use to measure emotional distress. Our data set is informative on individuals' take-up of health benefits provided by mandatory social insurance. It contains information on usage of all drugs that are covered by mandatory health insurance. This includes drugs prescribed by physicians but also self-medication. In order to measure the impact of involuntary non-employment on an individual's health situation we focus on psychotropic and psychosomatic drugs. Use of such drugs is closely correlated with an individual's health condition and is strongly related to (physical and mental) health problems that are commonly associated with joblessness. This study relies upon an *objective indicator* on a worker's health. Furthermore this indicator is quite specific and closely related to those health problems that are commonly associated with joblessness. Hence the health indicator used in this study is different from many previous papers that either rely on subjective health indicators and/or general measures of an individual's well-being.

A further dimension where our paper differs from much of the previous literature relates to the *coverage and quality of our data set.* Our data are taken from a Regional Health Insurance Fund in Austria, ("Gebietskrankenkasse", henceforth referred to as the Fund) which covers about one sixth of the Austrian work force. The data are informative on both a worker's health *and* his or her employment and earnings history. The data are *exhaustive* in the sense that *all* private sector worker in this region are covered. The data are very unlikely to suffer measurement error and/or underreporting. The Fund covers all costs for covered drugs (up to a fixed deductible, constant for all drugs, and the same for all individuals), so there is a high incentive to report all purchased drugs at the Fund.[1] Furthermore, the

[1] Covered drugs include medicine that has to be prescribe by physicians but also drugs for which no prescription is necessary. Workers "pay" the entire price for the drug at the drugstore. The Fund then reimburses the difference between the price of the drug and the deductible as soon as the drugstore hands in the information on drug use covered by health insurance.

data cover a time period of five years and thus allow us to trace the health situation over a sufficiently long period of time. In sum, by using a large data set with a high coverage, combining health and labor market information, measuring unemployment-related health problems rather precisely, and covering a sufficiently long time period, our data set seem ideally suited for studying the health effects of involuntary non-employment.

The paper is organized as follows: Section (2) reviews the previous literature on the relationship between unemployment and health. Section (3) discusses the data sources and provides first descriptive evidence on the relationship between unemployment and drug prescriptions. In Section (4) we present the statistical models used in subsequent analysis and discuss the identifying assumptions that allow us to estimate the causal effects of non-employment of health. The results are presented and discussed in section (5). Section (6) concludes.

2 Previous Literature

One of the first studies about the negative consequences of the experiences of unemployment was conducted by Jahoda et al. (1975). They analyzed a sample of individuals in a small community affected by the shutdown of a major employer in a small Austrian community in the 1930s. One of the major findings were strong effects on individuals' emotional health, such as loss of self-confidence and self-esteem, depression, anxiety, and strained personal relations. Subsequently, social psychologists, and sociologists have developed various theories to explain in which ways joblessness may affect health and, more generally, the well-being of individuals.

Unemployment is associated with loss of self-esteem and feelings of personal failure. Jahoda (1981) mentions that individuals suffer form unemployment because it deprives them from basic needs. Having a job imposes a time structure on individuals; it implies regular contact and shared experiences with co-workers; it links individuals to others' goals; having a job defines the status of an individual and is part of one's identity. As a result, people suffer from unemployment even after having lost a job that was consciously experienced as bad. A related point concerns the observation that most people would continue working, even if it would not be necessary in order to earn one's living (see e.g. the classical study by Morse and Weiss 1955).

Sen (1997) mentions the importance of loss of freedom that is experienced by the unemployed that goes beyond the loss in income. Unemployment can be a major factor in predisposing people to social exclusion, depriving them not only of economic opportunities but also from social activities such as participation in the life of the community, which may have several consequences for emotional health of individuals (see also Solow 1990 on this behalf). Other channels through which unemployment may feed back to health (e.g. changes in (risk) behavior like drinking or the loss of structure in everyday life) can be conceived as aspects or as results of distress resulting from role loss or severe role threatening due to unemployment.

Many recent approaches to link unemployment to a worker's health rely on *stress theory* (see e.g. Pearlin 1989 for an overview of sociological stress theory). Unemployment is considered as a major stressor. One domain where stresses occur is on the financial side. Often the reduction in disposable income requires substantial adjustment by the individual and involved families. This exposes individuals not only to economic hardship, but in addition to the income losses to serious psychological distress. Many studies have found that financial strain causes substantial psychological harm (e.g. Kessler et al. 1987, Turner 1995). The second domain rests on the damage that unemployment does to an individual's self-concept. Unemployment implies loss of social status associated with the previous job and deprives the individual from a socially accepted role. Furthermore, unemployment also implies a status that is often seen as deviant and shameful by the social environment. Marsh and Alvaro (1990), for example, interpret their finding of different distress symptoms of unemployed individuals in Spain and the UK as resulting partly from differences in work ethics.

When such distress persists for prolonged periods of time individuals' resistance to illnesses may be seriously affected and unemployed workers may end up with severe health problems (see Dooley et al. 1996). Such health responses to unemployment range from physical illnesses (Kessler et al. 1987) to mortality (Brenner 1973, 1979), especially suicide. However, the most frequently observed response of health to unemployment relates to suspected mental health effects. Studies that have empirically addressed this issue predominantly work with self-reported measures of mental health problems (e.g. Björklund 1985) or admissions to mental hospitals (e.g. Agerbo et al. 1998 and Westergaard-Nielsen et al. 2003).

A second strand of literature which is of importance here are studies discussing differential exposure and differential vulnerability to distressing events like unemployment. It is argued here that there are mainly differences in coping strategies (Pearlin and Schooler 1978) and different subjective meanings attached to various (undesirable) life events which are threatening a specific role (see e.g. Turner 1995).

Many previous studies, especially in economics, have been concerned with the effect of unemployment on subjective well-being. These studies confirm the hypothesis that unemployment may have strongly detrimental effects on an individuals psychological health. Flatau et al. (1998), using data on mental health and on subjective well-being from Australia, conclude that unemployment and some measures of well-being are consistently and strongly correlated. For the UK, Clark and Oswald (1994) also find a negative effect of unemployment on subjective well-being. In fact, they conclude that the negative impact of job loss may be larger than for separations or divorces. Winkelmann and Winkelmann essentially find the same negative effect on subjective wellbeing for Germany and Agerbo et al. (1998) reach similar conclusions for Danish data. Other studies focus on youth workers and they also find detrimental effects of unemployment on well-being, as Goldsmith et al. (1996) for the United States and Korpi (1997) for Sweden. Suggestive of the existence of contextual effects are the studies by Shields and Price (2001) or Stutzer and Lalive (2004), for example.

3 Data and Descriptive Statistics

3.1 Data Sources

In order to study the impact of being non-employed on the health situation of workers we draw information from Austrian data. The data set covers individuals that are employed in the private sector of the state Upper Austria. Upper Austria is one of totally nine Austrian states (located in the north–east) and covers about one sixth of the total Austrian population and work force.

The data set is unique in the following respects. First, the data set links information on workers' labor market performance and workers' health problems. In Austria, health insurance is mandatory for all employees. A firm who hires a new employee is obliged to immediately register the worker with the Regional Health Insurance Fund or Fund. As contribution payments depend on wages, the Fund collects information not only on employment histories of workers, but also on their labor earnings as well as a number of individual characteristics (such as age, broad occupation, and sex).

The second unique feature of the available data set is that it is *exhaustive*. That is, mandatory health insurance covers *all* private-sector workers. As a result, all dependent employees in the private sector of Upper Austria (that is, more than 80 % of the active state population) are covered by the data.[2] Among other things, the exhaustive feature has the advantage of the possibility not only to consider the worker but also the firm as a unit of observation. A "firm" is simply defined as the set of individuals that is observed under a given employer social security number ("firm identifier"). This is particularly helpful for our estimation strategy that relies both on information of workers and information on their respective employers. The data set reports the industry affiliation and size of the firm. This firm information will be useful as it allows us to use the bankruptcy of a firm as an instrument for estimating health effects of loosing the job. Such firm information is also particularly useful in making plant-closure firms comparable to ongoing firms thus allowing to compare samples of workers with similar previous job situations.

The third unique feature is related to the *workers' health situation*. Every employee has access to primary health care benefits provided by the Fund.[3] Among others benefits, the Fund covers all costs of covered drug prescriptions (up to a fixed deductible, which is constant for all medication, and the same for all workers). In our analysis below, we restrict our attention to a subset of drugs, which are specifically targeted at treating mental disorders (for instance, sedatives and antiderivative) as well as on psychosomatic disorders (for instance pain relief medication, etc.). Health problems of this kind are potentially very important when looking at distress following unemployment associated with involuntary job loss. It is worth noting at this point that our measure of an individual's health status is inferred form his or her actual usage of medication. While this is not a direct measure of the health status it is, arguably, strongly correlated with an individual's health. Moreover, it has the advantage of being an *objective* health indicator. This is different from many previous studies using primarily data on self–reported health status. We will refer to sets of drugs treating mental disorders as "psychotropic drugs", and to the set of drugs treating psychosomatic disorders as "psychosomatic drugs".[4]

3.2 Empirical Design

The data cover the five-year period from January 1, 1998 to December 31, 2002. To get information on the previous work experience as well as on tenure with the current firm, we linked the data from the

[2]There are separate funds for the self-employed, the farmers, public sector workers, and employees of several public utility firms, so not the entire population is covered).

[3]Benefits covered by the Fund are comprehensive. It covers all costs associated with a primary health care such as treatment by physicians, drug prescriptions, and hospitalized care.

[4]A detailed list of drugs included in the two respective categories is available upon request from the authors.

Regional Health Insurance Fund with data from the Central Social Security Administration ("Hauptverband der Sozialversicherungsträger"), which yields retrospective information on the individuals' work history back to as long as the year 1972.[5]

To link the information on drug usage and employment/non-employment, we constructed a monthly panel of individuals' health and employment histories. Within the period 01.1998 and 12.2002, we measure the *health situation* of an individual by counting the number of drug prescriptions by physicians between the 10th of two consecutive months. To assess the *employment status* we take an individual as holding a job if he or she is employed on the 10th of a particular month.

As mentioned above, in order to assess the impact of non-employment due to involuntary job loss we rely on information on plant closure. 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 separation results from a quit or a layoff. To clarify issues, let us make precise how we define a "plant closure" and how we define 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, on a quarterly basis, a "plant closure".[6] The level of employment in the first (second, third, fourth) quarter of a particular year is measured by the level of employment in the firm on the 10th of February (May, August, November). As mentioned above, our analysis is concentrated on larger firms. To be included a firm must satisfy the following two criteria: (i) Over the period 1998-2002, the size of the firm has to be above twenty in at least one quarter; and (ii) the number of employees must never fall below ten in any quarter.[7] The main argument to exclude small firms is avoidance of measurement error by confounding a simple administrative recording of the employer's social security identifier with a bankruptcy. Clearly, such recoding may also take place for large firms. Furthermore, a firm may be taking over by another firm, also leading to disappearance in the data while, in fact, the firm continues to exist and workers do not loose their jobs. In order to avoid such measurement error firms are only included in the data and classified as a plant closure, if the employer identifier disappears in the data *and* not more than 50 of the previous work force appears under a common new employer identifier in the next quarter. Furthermore, in order to get a sharper distinction between plant closure and non plant closure firms, we also excluded '*distressed*' firms, that is firms which exhibit a very large drop in their work force between two consecutive quarters.[8]

Plant closure is defined in a narrow sense. That is, a specific firm is classified as a plant closure, if it exists at quarter t but does not reappear until quarter $t+3$. Furthermore, to be able to compare

[5]The Central Social Security Administration gets its data from the Funds and process this information for the purpose of calculating of old-age 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.

[6]Plant closure is identified on a quarterly, rather than a monthly, basis in order to keep the available information tractable. The quarters refers to employment information of the 10th of either February, May, August or October.

[7]Approximately 86% of all observed *firms* over the period do not meet these two criteria. However, the fraction of *employees* that is excluded by these criteria is much smaller. In total, only about 15 % of all workers in the exhaustive data set are employed in small firms and not covered in the sample studied here.

[8]A 'distressed' firm is defined as follows: Firm size must drop at least 30% between two quarters and stay below 80% throughout the following year.

workers' health and employment experiences for a sufficiently long period before and after the plant closure, we focus on plant closure that took place in the year 2000. More precisely, only firms that went bankrupt between after quarters IV.1999 and before quarter I.2001 are selected. This procedure has the advantage to allows us to trace workers' employment and health history for a period of at least two years *before* the plant closure and at least two years *after* than plant closure. In order to get a balanced sample we concentrate on health and employment experiences within 24 months before the plant closure quarter and 24 months after the plant closure quarter.

Definition of 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 still employed at the quarterly references day before the plant closure actually takes place. Among these workers, we concentrate only on those individuals with *at least one year of tenure* preceding the plant closure date (or the reference date for ongoing firms). We are thus primarily interested in workers who are more attached to their present job than recent hires. This definition ensures that the instrument is related to changes in nonemployment. More importantly though, we focus on this group of workers because they have a high probability of loosing their job *involuntarily.* As we have no information on the causes of job-loss, we cannot generally separate layoffs from quits.

In the PC sample, all individuals loose their current job in the period of three months following the reference data. The non-plant closure sample (NPC) consists of all workers employed in firms that were neither classified as a plant closure firm nor as a distressed firm at the reference date. But they were also employed in firms that meet the firm-size criteria (firm size exceeds 10 at reference data and exceeds 20 at least once) and all NPC workers had at least one year of tenure at the reference date. Notice that our design allows the same individual to appear more than once in the control sample.[9] Hence the number of observations in the comparison sample is larger than the number of individuals in that sample.

Table 1 presents details on the structure of the selected data. There are in total 68 plant-closure firms that went bankrupt during the year 2000 and that satisfy the firm size criteria specified above. Bankruptcies are clustered in the period from May to July 2000 and in the period from November to January 2001. In total, there are 732 employees with at least one year of tenure who were employed in one of these plant-closure firms at the quarter when the plant closure actually occurred.[10] The control sample in contrast is huge, both in numbers of firms and numbers of workers, amounting to 46,218 ongoing firms and 1,151,963 worker-observations in these firms.

Table 1

[9]For instance, an individual with continuous employment in an NPC firm in 2000 appears in all four reference date samples.

[10]Notice that, on average, there are slightly more than 10 workers per bankruptcy. In quarter III (August 2000) for instance, the average number of workers is even smaller than 10. This arises from confining the analysis to workers with at least 1 year of tenure. As some of these firms have high turnover, this restriction decreases the number of workers to be included in our sample. At the same time, it is a desired by–product of this restriction that confining the analysis to attached workers makes plant-closure and non-plant closure sample more comparable with respect to to observed characteristics. We will take up this point below.

Matching on the basis of employer characteristics. The NPC sample is huge but not comparable to the NPC sample. This motivates concentrating on a specific subset of firms in the NPC sample that compares well to the plant-closure sample regarding such important firm characteristics as industry and size.

Table 2 compares the distribution of industry and firm size at the firm level by sub-sample. One half of all plant-closures occurred in construction and manufacturing with the remainder being concentrated in the remaining sectors. Second, the construction industry was more strongly affected by plant closure (25 % of all plant-closures, but only 11 % of all firms in NPC sample) whereas the group of industries in other services was less strongly affected (12 % of plant-closures but 20 % of all firms in NPC sample). Furthermore, bankruptcies tend to hit small firms more strongly than large firms (80 % of PC compared to 56 % of NPC).

Table 2

We will discuss in the following section that one critical assumption for causal inference is ignorability of the instrument. Table 2 shows that ignorability does not hold. Thus, since the instrument – plant closure – is an event that refers to the *firm*, we propose to match the NPC firms to PC firms on the basis of observed covariates. Even if ignorability does not hold unconditionally, it may hold conditional on observed characteristics. The matching procedure was carried out in two steps. First, firms from the NPC sample were matched to firms in the PC sample on basis of their detailed industry classification (38 categories instead of the 10 in Table 2). In the second step, for each PC firm, we identified the set of 10 NPC firms with employment most similar to employment at the PC firm (with replacement). The matched comparison sample then consists of all workers employed at the matched ongoing firms.

Panel "Matched Firms" in Table 2 shows that the matching procedure produces a perfect match with respect to size-categories and industry. Whereas the tests of independence of industry and firm size strongly reject when comparing all NPC firms to PC firms, the same test does not reject the null of independence when comparing matched NPC firms and PC firms. The most important question regarding this simple matching procedure is, however, whether it succeeds in balancing health outcomes across PC and NPC firms at the worker level. We discuss this issue in the following subsection.

3.3 Descriptive Statistics

It is interesting to take a first look at the empirical evidence by focusing on some interesting descriptive statistics concerning drug prescriptions and employment/non-employment. We start by showing some evidence that relates to the entire sample, irrespective of a worker's plant closure status. We then present first evidence that compares plant-closure workers to workers not affected by plant closure. We put particular emphasis on a comparison of plant closure workers with the – more similar – matched sample of non-plant closure workers. Hence this descriptive evidence should give a first hint to possible health effects of non-employment due to involuntary job loss.

Table 3 shows, for all individuals covered either in the plant-closure or in the non-matched sample, the average number of drug prescriptions. On average over the entire sample, there were 3.9 prescriptions over the 4-years period that is analyzed in the data. Hence, the average individual in the sample got roughly 1 prescription per year of a psychotropic or psychosomatic drug. Table 3 also shows a positive correlation between the number of prescriptions and the duration of non-employment during the 4-years have the lowest, and individuals with the least stable career (13 months or more in non-employment during the 4-years interval) have the highest number of prescriptions. Table 3 also shows that psychosomatic drugs are prescribed somewhat more frequently than psychotropic drugs. But for both drug types, the same positive correlation pattern between drug prescription and non-employment shows up.

Table 3

Table 4 discusses changes in the number of drugs used between two time periods. The period $t-1$ refers to the time period of two years preceding the reference date (including the reference date), period t refers to the period of two years following the reference date. The table distinguishes four groups of workers. The first group is continuously employed in $t-1$ and in t. For this group, there is a small increase in drug use over time (additional .19 drugs). This reflects two phenomena. On one hand, age increases by 4 years over the sample period. On the other hand, expenses for drugs covered by health insurance increased strongly during the time period that we consider. The second group of workers is also continuously employed in $t-1$ but is observed at least once without a job in period t. These unemployment entrants experience a very strong increase in drug use (additional .63 drugs). The third group of workers is faced with the opposite situation compared to the second group, i.e. this group is observed at least once without a job in $t-1$ but is continuously in t. Interestingly, job entrants exhibit the lowest change in drug use (additional .02 drugs). Finally, there is a group of workers who is without a job at least once both in $t-1$ and in t. These persistently unemployed individuals use an average of .5 additional drugs per month more in t compared to $t-1$.

Table 4

Tables 3 and 4 both are suggestive of a strong positive correlation between health status and employment experience. However, the extent to which job loss actually reduces health is not clear. There are two problems with descriptive evidence. First, an important omitted characteristics in the cross section analysis (Table 3) is ex-ante health status. Unemployed individuals may have, on average, a lower health status than employed individuals. This confounds the simple cross-section analysis in Table 3. The previous literature has responded to the problem of omitted variables by studying time-series variation in unemployment and health (Table 3). The main problem impeding causal inference in a panel setting is reverse causality. Suppose, for instance, that decreases in health status (on the job) affect separation decisions. This could reflect both layoffs by the employer due to ill health or quitting the present job in order to focus on health improving activities on the part of the employer. Thus, health status deteriorates already *before* employment status changes. This implies that the change in health associated with job loss is *lower* than it would be if the individual looses the job for health unrelated reasons. Thus, if this situation is an adequate description of the labor market, time-series analysis that correlates changes in health with changes in employment tends to *underestimate* the causal effect of job loss on health.[11]

[11] The converse is true if layoff and quit decisions depend on expected future health shocks. However, note that it is

This discussion emphasizes the importance of studying variation in employment that is known to occur due to health unrelated reasons. This paper puts forward the possibility that plant closure creates this type of variation. Let us first consider the *non-employment experiences* of the plant-closure workers relative to workers in surviving firms.

Figure 1

Figure 1 shows that the two samples are, by construction, nearly identical in their employment history *preceding* the reference date, although the plant closure sample has a slightly higher share of persons not employed in the second year before that date. More importantly, Figure 1 shows that plant-closure workers were heavily affected by the shut-down of their firm. Non-employment not only increases dramatically immediately *after* the plant closure, but it also remains persistently high thereafter. Two years after the plant closure the non-employment rate among plant-closure workers is still more than 20 %. In this respect, our data confirm the results from previous studies on worker displacement due to plant closure (e.g. Kuhn 2002).[12] In contrast, non-employment rates of workers in surviving firms are much lower. However, also in this sample we see an increase in non-employment. At the end of the observation period the non-employment rate is about 10 %. This increase is partly due to sample construction. In order to be included, a worker has to be employed at the reference date and, in addition, must have had at least one year of tenure. The trend after the reference date, however, is in part also due to age (as older workers face lower employment chances) and due to an aggregate trend (as labor market conditions started to deteriorate after 2000). In sum, Figure 1 clearly shows significant differences in employment experiences between the two interesting groups. Hence comparing plant closure workers and worker in ongoing firms is meaningful in the sense that, we compare two groups that have substantially different employment experience.

The most important issue for our purpose is the question whether the observed differences in employment experiences are due to differences in plant closure status rather than differences in the health situation of the two groups. This question can be studied by analyzing the dynamics of the *health situation* across the two groups. If health status leads to plant closure, one would expect that health status deteriorates before plant closure. Figure 2 shows monthly drug use as a function of time until plant closure with negative numbers denoting months before plant closure and positive numbers denoting months after plant closure. The evidence is clear. Drug use "shoots" upward in the month after the reference date (month 0) not before. Thus, health deteriorates after plant closure. Second, ideally, we would like to have a situation where, *before month 0,* the health situation between the two groups is identical. In that case, any differences in the average health status *after month 0* can be attributed quite convincingly to involuntary job loss. Figure 2 shows that this is not the case. The average number of drugs used *before* the reference date is 1.62 for plant closure workers and 1.81 for workers in surviving firms. Figure 2 also shows that variation over time is much larger in the PC sample than in the NPC sample – due to small sample size. Thus, whereas plant closure appears to induce variation in

quite difficult to predict future health both for the employer as well as for the individual. Moreover, employers can not readily lay off workers whom they expect to be ill.

[12]A more detailed analysis shows that being employed at a plant closure firm at the reference date both increases the incidence and the duration of nonemployment after the reference date (results not shown).

employment which is not related to health, the NPC does not seem to be comparable to the PC sample in terms of drug use.

Figure 2

However, recall that the NPC sample is not comparable with respect to such important characteristics of firms as size and industry. Also, note that we have identified a set of matched NPC firms with identical structure as the PC sample. Figure 3 reports the dynamics of drug use in the PC compared to the matched NPC sample. The message is clear. Drug use in the matched NPC sample is very much in line with drug use in the PC sample in the period *before* death of PC firms. The average number of drugs used in the matched control sample is 1.57 – almost identical to the corresponding number in the PC sample (1.62).[13]

Figure 3

Figure 4 averages the data points over successive 6-months intervals because drugs purchased in a month may be used in several subsequent months. Immediately after plant-closure, drug use increases strongly suggesting that the health status of plant closure workers suffers substantially after the shutdown of their previous firm. In contrast, drug use for workers in surviving firms does not change immediately after the reference date.

Figure 4

4 Estimating the causal impact of non-employment on health

In order to estimate treatment effects, one essentially has two approaches to choose from. First, there are methods which assume some sort of ignorability of the treatment assignment, either unconditional or conditional on a set of observed characteristics. Secondly, if there is a plausible 'natural experiment', one can use instrumental variable methods. In order to evaluate the effect of nonemployment on mental and physical disorders we mainly use the second approach, although we also present 'conventional' regression results. In order to render our approach plausible, we will also discuss the key assumptions in order to estimate any interesting average treatment effect in the context of our setting.

Regression methods. We first run ordinary least squares regressions from usage of drugs on the number of months not employed, both for the period after the reference date. That is, define S_{it} as the number of drugs used in the two year time period after the reference date and N_{it} as the number of months not employed after the reference date respectively. Note that N_{it} is not a binary but a multi-valued treatment. The cross-section approach identified the association between drug use and non-employment in the following regression

$$S_{it} = \beta_0 + \beta_1 \cdot N_{it} + u_{it}$$

This procedure is plagued by severe both omitted variable bias and endogeneity of N_{it}. Thus this first model has descriptive character and mainly serves as a tool for establishing a statistical association

[13]At the same time though, the matched NPC sample is very similar to the entire NPC sample in terms of employment.

between the number of months in nonemployment and the sickness measure. What would be necessary for establishing a causal effect of nonemployment on drug usage in this regression setup are essentially mean independence assumptions of the treatment variable, i.e. the months in nonemployment. By including observable characteristics $\mathbf{X_{it}}$ we cannot hope neither to estimate any causal effect, because we still do not feel comfortable with the necessary key assumptions, as one has again to invoke conditional mean independence assumptions about the treatment variable (see Wooldridge 2002, for example).

Taking into account the information from both periods, define the variables of interest as $Y_i \equiv S_{it} - S_{it-1}$ as the change in the number of drugs used between the period after and before the reference date and $D_i \equiv N_{it} - N_{it-1}$ as the change in the number of months not employed respectively. Thus our second estimator is a first difference estimator

$$Y_i = \beta_0 + \beta_1 \cdot D_i + u_i$$

This approach addresses omitted variable bias due to time-invariant characteristics. However, by focusing on the correlation between the change in health and the change in employment, we still cannot rule out a causal effect running from health to nonemployment rather than the other way around. As we have argued above, this may imply an underestimation of the causal effect of job loss on health.

Instrumental variables methods. The second approach in estimating average causal effects involves instrumental variable methods and therefore utilizes 'exogenous' variation in the treatment variable which is generated by a 'natural experiment'. That is, first of all one has to essentially think of a situation, which generates variation in nonemployment that is plausibly not driven by the health status of individuals. Because these methods do not in general identify a causal effect, it is then essential to discuss the additional assumptions necessary to draw any plausible causal inference (Imbens and Angrist 1995 and Angrist et al. 1996).

We use employment in a plant closure firm at the reference date as our instrument for the change in the number of months not employed between the periods after and before this date. That is, define Z_i as a binary variable, indicating if a person is employed in a plant closure firm at the reference date (Z_i taking on the value 1 in this case). As above, let Y_i be the change in the number of drugs consumed between the two periods and let D_i indicate the change in the number of months not employed between the two periods. The instrumental variable estimator is

$$\beta_{IV} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]}$$

That is, in the case of a binary instrument, the conventional instrumental variable estimator is identical to the Wald estimator (right–hand side of the above equation), measuring effectively the ratio of excess change in drug usage for individuals working in a plant closure firm over the excess change in unemployment for the same group of individuals. But, in order to interpret the Wald estimator in a causal way in a situation with heterogeneous responses to treatment, one has to discuss additional assumptions beyond the usual assumptions invoked when using IV methods.

In order to evaluate if plant closure is a valid instrument for the causal effect of nonemployment on drug usage, it has to satisfy several assumptions, the key assumptions will be discussed below (Imbens and Angrist 1995).

(i) The first assumption concerns the ignorability (the randomness) of the instrument. Stated in terms of potential outcomes

$$(Y_j, D_0, D_1) \quad \perp \quad Z$$

Where Z is the (dichotomous) instrument, D_1 (D_0) are the potential treatment intensities for an individual working in a plant closure firm (non plant closure firm). Y_j is the potential outcome for a treatment intensity of j units. This first assumption thus 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 very strong assumption in our case, as we cannot plausibly assume a priori randomness of the instrument (see section 3.2 above), as there are both differences in the probability of plant closures between different firms and also between different individuals (because firms with different probability of shut-down have not necessarily the same composition of their work force). It is thus more realistic in our setting to assume ignorability of the instrument, conditional on observed characteristics of the firm F and individual characteristics X

$$(Y_j, D_0, D_1) \quad \perp \quad Z \quad | \quad F, X$$

We thus control on both F and X. Conditioning on F is as already laid out done by way of matching firms from the two sub-samples. As there remain differences on individual characteristics, we will control for them parametrically using two stage least squares. As this assumption cannot be ultimately verified, it finally rests on arguments of plausibility. Nonetheless, we will have discussed above (section 3.3) that matching on firm–unit basis effectively removes any important differences in both the level of the outcome and the treatment variable before the reference date.

Additionally, this first assumption also implies an exclusion restriction, i.e. the instrument must have no direct effect on drug usage. We must therefore assume that job-loss due to plant closure does not *directly* affect the health status of affected workers. Any effect on health must be via an increase in duration of nonemployment. This assumption is more difficult, as it is not clear how it can be supported by empirical evidence.

(ii) The second assumption is that instrument must affect the treatment intensity. That is, in our case it is postulated

$$E[D|Z_i = 1] > E[D|Z = 0]$$

That is, working in a plant closure firm at the reference date must increase the duration of nonemployment afterwards for at least some workers. As we have shown in section 3.3 above, working in a plant closure firm has a huge impact on nonemployment. This assumption thus plausibly holds in our setting and is thus not crucial in this setup.

(iii) The third assumption postulates that the instrument has to affect all individuals in the same way (monotonicity). That is, in our case required it is required that

$$D_1 \geq D_0 \quad \forall \quad i$$

This assumption states that the potential duration of nonemployment in case of working in a plant closure firm is always longer or at least as long as the potential duration of nonemployment in case of not working in a plant closure firm. Although this assumption is again 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 should not cross for the two sub-samples (see Angrist and Imbens 1995).

Figure 5

Figure 5 plots the CDF of the treatment variable (change in the duration of nonemployment between the period after and before the reference date). The fact that the both functions actually do not cross is thus important in rendering this third assumption plausible.

Angrist and Imbens (1995) show that – if all of the above mentioned assumptions hold – the Wald estimator measures the following average causal response (ACR) parameter

$$\frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[D_i|Z_i=1]-E[D_i|Z_i=0]} = \sum_j \omega_j E[Y_j - Y_{j-1}|D_1 \geq j > D_0] \quad (1)$$

The ACR is a weighted average of causal responses for individuals which are induced to change their treatment intensity with the weights ω_j being proportional to the share of individuals who change their treatment from less than j to j or more units on the treatment. That is, if the stated assumptions hold, we can estimate an average causal effect running from nonemployment to health for individuals which are induced an increase their duration of nonemployment due to the fact that they are working in a plant closure firm. The ACR depends on both the instrument used and 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.

Figure 5 is helpful in interpreting (1) because the difference between the two cumulative distribution functions for a given treatment intensity are proportional to the weights ω_j attached to the average causal effect (see again Angrist and Imbens 1995). Evidently, the ACR identified in this paper predominantly refers to individuals whose non-employment experience changes by 0 to 10 months due to plant closure.

5 Results

As mentioned above the descriptive analysis may be misleading as plant closure workers and workers in ongoing firms are not identical with respect to individual characteristics. In this Section we present results from regression analyzes controlling for such differences. Our preferred estimation strategy relies on IV-estimation techniques, as discussed in Section 4 above. This methodology should make sure that the estimated effect of non-employment on workers' health situation is *causal* rather than reflecting spurious correlation due to reverse causality. However, we will also present OLS-estimates. This is not only interesting per se, by showing the significance of correlations between unemployment and health, conditional on individual characteristics. A comparison of OLS-results and IV-estimate also points to a possible bias that might be involved in empirical studies that do not appropriately address the causality issue. In the first part of this Section we focus on health-effects of non-employment for the entire sample of plant-closure workers. In the second Subsection we will show that estimated causal affects strongly vary across various sub-populations.

5.1 The Effect of Job Loss due to Plant Closure on Drug Use

The main results are presented in Table 5. In the first column the dependent variable is the number of drug prescription in the two year period *after* the reference date. OLS estimates that associate health with non-employment in the cross-section reveal a significantly positive and strong correlation between the number of months not employed and the number of drugs used. These estimates condition on a number of important individual characteristics such as age, age squared, earnings, and dummies for sex, season (quarterly), industry and firm size. According to cross-section estimates, one additional month of non-employment after the reference-date is associated with an increase in drug prescriptions of 0.06 during the two-years interval following the reference date.

Table 5

Columns 2 to 4 in Table 5 present results that focus on the change in the number of drugs before and after the reference date as the dependent and the change in months of non-employment as explanatory variable. In Column 2, first difference estimates reveal a positive, significant but much less important correlation between non-employment and drug use.[14]

Column 3 presents 2SLS estimates of the effect of non-employment on drug use. Interestingly, these estimates reveal a stronger correlation between non-employment on drug use than the estimates working with time-series variation (column 2), though the effect is not statistically significant. However, the coefficient is 1.5 time larger than the standard deviation indicating a p-value of .13 which is close to conventional levels of significance. Moreover, the fact that the first difference estimator identifies a weaker correlation between drug use and non-employment than the 2SLS estimator can be rationalized via selection into non-employment based on realized deterioration of health status. The change in drug use between periods when individuals loose their job is lower than it would be in the absence of health related selection.

However, as we have shown in section 3.2, NPC firms – identifying the change in drug use in the counterfactual situation without plant closure – are not comparable to PC firms. Thus, it is important to apply the 2SLS estimator also to the smaller sample containing only matched NPC firms in addition to PC firms. Results from the smaller sample are reported in column 4 of Table 5. These results suggest that the causal impact of non-employment on drug use is very strong and statistically significant at the 5 % level. One additional month of unemployment within a period of two years leads to an increase in average drug use on the order of 0.10 drugs (or 6 % = 0.0958/1.62).

The dependent variable in Table 5 as well as in the previous Tables referred to the total number of prescriptions of psychotropic and psychosomatic drugs. This is a meaningful and relevant subset

[14]The finding that panel estimates of the correlation between health status and unemployment is less strong than the cross section correlation is pervasive in the literature. See Korpi (2001) for a recent example.

of all drugs with respect to possible health problems associated with non-employment. However, it is important to keep in mind that this subclass of drugs is still quite heterogeneous. To gain some insight into the separate contribution to the main result by type of drug, Table 6 looks at non-employment effects on psychotropic and psychosomatic drug prescriptions separately.[15] The results displayed in Table 6 refer to the small sample containing only matched NPC firms. The causal effect of non-employment on use of drugs targeted at mental health is positive but not significantly different from zero (psychotropic drugs). However, note that the point estimate is more than 1.5 times higher than its standard error.

Table 6

Column 2 in Table **??** refers to use of drugs treating physical rather than mental disorders (psychosomatic drugs). The 2SLS estimate is large in absolute value and statistically significant. The point estimate suggests that one additional months of non-employment increases the number of psychosomatic prescription by 0.06 (or 5.6 % = 0.0605/1.07), a substantial effect. In sum, the quantitative effects of non-employment on use of psychosomatic drugs use does not seem to be much different from its effect on use of psychotropic drugs.

The above results relate to average drug prescriptions per individual. In other words, it focuses on the mean, but does not consider any other aspect of the distribution of drug prescriptions across populations. The most obvious distributional aspect concerns the concentration of drug users among the population. Does the increase in mean prescription come from increases in drug consumption across the entire population, or is increase concentrated on a certain group of individuals? To analyze this issue we split the observed drug prescription into incidence (how many individuals use drugs?) and intensity (how many drugs are prescribed to drug users?). Column 3 in Table 6 presents results that take the incidence of drug use as a dependent variable. Incidence is a dummy variable that takes the value 1 if an individual buys at least one drug during a 2-year interval (after or before), and 0 otherwise. The dependent variable is the change (after-before) of the incidence of drug use.

Non-employment due to involuntary job-loss does not lead to a higher proportion of concerned individuals using drugs. 2SLS estimates lead to the conclusion that there is no causal impact of non-employment on the propensity to use drugs for treatment of psychotropic or psychosomatic distress.[16] One interpretation of this finding is that non-employment due to involuntary job-loss is a distressing event, but in order to substantially destabilize the (emotional and physical) health situation of an individual, there has to be a predisposition for such instability. Absent such predispositions, that is without any drug prescriptions before the reference date, non-employment does not affect an individual's health situation. This result is consistent with the finding that mental distress does arise when labor-market problems coincide with other distress (in particular, family problems and partnership conflicts, divorce, etc.). It is plausible to assume that drug prescription *before* the reference date are correlated with such problems. Consequently, our result that only previous drug users are affected by non-employment due to involuntary job loss is in accordance with the hypothesis that experiencing non-employment may be the critical additional strain that causes an individual's health to deteriorate substantially.

[15] Note that applying 2SLS separately to the two components of the dependent variable produces a meaningful decomposition in the sense that the coefficients on the components add up to the coefficients in the main results.

[16] Both OLS and FD results are identical to 2SLS results.

5.2 Effects by Sub-samples

The above results were obtained by looking at the entire sample of plant closure workers. However, individual characteristics are important in explaining the variance in health outcomes.[17] If different characteristics lead to different outcomes on drug prescriptions, it may be that also interaction effects are potentially important. In other words, assume a certain combination of characteristics indicates a strong predisposition for consuming psychotropic and psychosomatic drugs: Then we might assume that such a group is potentially much more strongly affected by experiencing non-employment due to involuntary job loss that another groups without such predisposition.

In order to test for such heterogeneity in the health-effects of experiences non-employment, we now divide our sample into various sub-samples and look how confining the analysis to those sub-groups affects the treatment effect. Dividing the sample allows for full interaction in the involved variable. The results are summarized in Table 7:

Table 7

Differences by sex. The upper left panel of Table 7 presents the effect of non-employment on health separately for males and females. Involuntary non-employment affects the health situation of men more strongly than women. One interpretation for this result is that men are more strongly attached to the labor market, and their self-esteem and identity are more strongly damaged when experiencing non-employment. In contrast, women have alternative roles and derive their self-esteem and self-perception to a lesser extent from paid work.[18] However, it is worth noting that this result contrasts previous findings. For instance, Thoits (1986) finds larger effects of job-loss and non-employment for women than for men. However, Ali and Avison (1997) show that negative effects for females arise predominantly for single mothers whereas for married mother no mental health effects of leaving one's job is found. They find support for the hypothesis that multiple identities (have alternative roles that define one's self) weakens the impact of negative strains on mental health.[19]

Differences by Age. Secondly, there are distinct effect differences between younger and older workers. We find that the health effect of involuntary non-employment due to job-loss is almost twice as large as the the corresponding effect for younger workers. Younger workers have less clearly defined role, are partly less attached to the labor force (as education is still important as an alternative activity). In contrast, older workers have fewer roles outside employment available. Moreover, as the incidence of drug use is arguably higher in older workers than in younger workers, one would expect the effect of non-employment on drug use to be stronger in older workers than in younger workers. This holds because non-employment only affects those workers having already used drugs (Table 6).

[17] The fraction of variance in drug use explained across individuals increases from less than 1 % to 5.3 % when individual characteristics are added.

[18] See Akerlof and Kranton (2000) for the role of identity in shaping labor market outcomes.

[19] Studies that analyze the role of work (rather than non-employment) for mental health also support this prediction. Fox (1980) and Rosenfield (1992) find that having a job does reduce distress less strongly for women than for men. Other studies, however, do not find any differences (see Cleary and Mechanic, 1983, and Pearlin 1975).

Industry affiliation. A further interesting dimension is a workers industry affiliation. Construction workers and workers in the tourism industry are both frequently unemployed and change jobs more often than workers in other industries. For instance, seasonal workers account for almost 50 % of the unemployment inflow whereas their share in employment is less than 20 %. These arguments suggest that workers in seasonal industries are much more used to, and hence much less affected by, the experience of involuntary non-employment. The results in Table 7 strongly supports this prediction.

Occupation status. Fourthly, we find a difference in the effect of nonemployment on our sickness measure between occupational status by comparing blue– and white–collar workers. Although white–collar workers are perhaps expected to show more attachment to their job, we find that the effect is larger for blue–collar workers. This finding could be interpreted in terms of different coping resources, as white–collar workers may have more or more efficient coping strategies in dealing with job loss than blue–collar workers. The finding could also be due to the higher financial distress that is associated with non-employment. Average monthly earnings of blue collar workers are substantially lower than the corresponding earnings of white collar workers causing them to suffer more from earnings losses associated with non-employment.

Continuity of work history. Finally, we find that people with a continuous work history in the two years preceding the reference date exhibit a larger increase in drug usage than workers with a not so long work history. This clearly point to the importance of role identity on mental health. The longer a worker is in a particular social environment the more he or she is likely to have adopted to his or her role, shaping his identity and self-esteem. Moreover, being longer in a particular firm means that social contacts in that firm have become important. Hence loosing this environment and suffering involuntary non-employment leads to higher mental distress for high-tenure workers than low tenure workers.

6 Conclusions

In this paper we have analyzed the causal relationship between involuntary non-employment on the health of workers. To assess this causal relationship we have used job loss due to a plant closures as an instrument. If job loss due to plant closure is not per se affecting the health status, comparing plant closure workers (who are very strongly affected by unemployment) to workers in ongoing firms (whose unemployment exposure is normal) should reveal the causal impact of non-employment on health.

In contrast to many previous paper our health measure is both objective and unlikely to suffer from measurement error. We focus on use of drugs targeted at mental (psychotropic) and physical (psychosomatic) disorders commonly associated with distress and covered by mandatory Austrian health insurance. Such medication is highly correlated with unemployment-related health problems.

We find that one additional month of non-employment increases drug prescriptions by about 6 %. Given that the average duration of unemployment is about four months, this implies an increase by almost 25 %. While the 6 % - estimate is the same for both types of drugs, we find a significant effects only for psychosomatic medication. Increases in drug prescriptions are strongly concentrated among predisposed individuals. When an individual has not consumed drugs before the plant closure, he or she was unlikely to be affected by non-employment. This points to the importance of cumulative strains. Unemployment is a distressing event that influences an workers health condition. When distress accumulates (as indicated by previous drug consumption) an additional spell of unemployment may affect the an individual's health situation quite strongly.

Finally, the estimated effect varies strongly across sub-populations. Male workers, blue-collar workers, older workers, workers with higher tenure with the current firm, and workers from industries less exposed to seasonal unemployement are more strongly affected by involuntary non-employment.

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A Figures

Figure 1: Proportion of Observations in Nonemployment, Plant Closure versus Non Plant Closure Workers

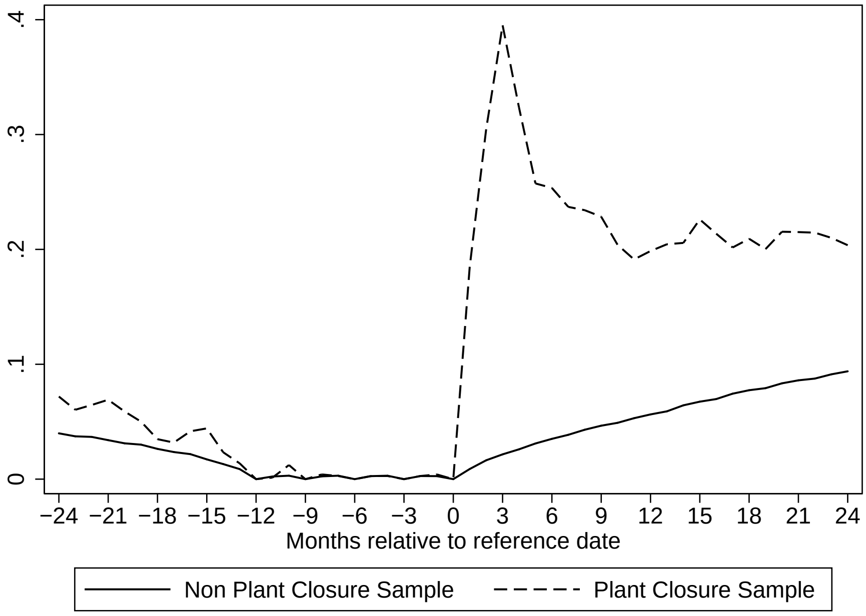


Figure 2: Number of Drugs Used, Plant Closure versus Non Plant Closure Workers

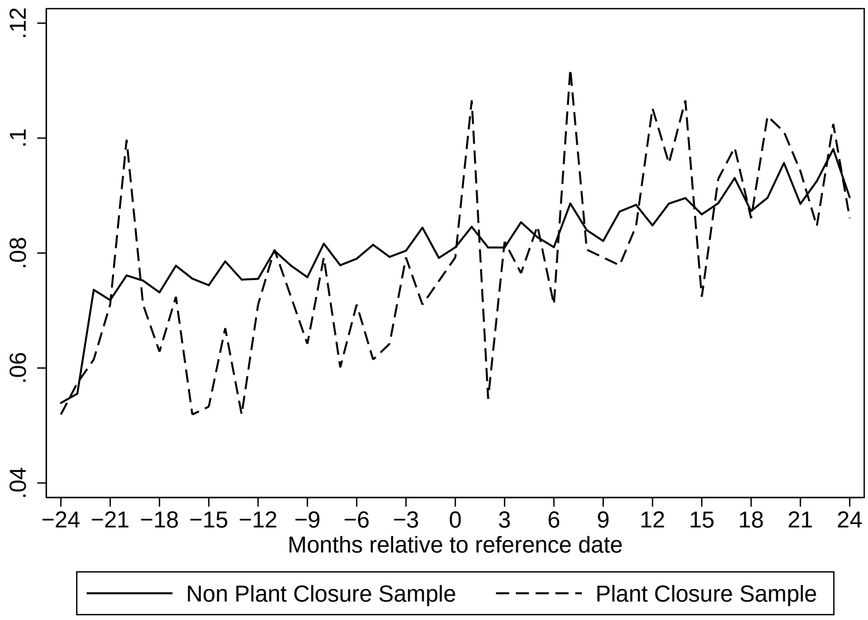


Figure 3: Number of Drugs Used, Matched Non Plant Closure Workers

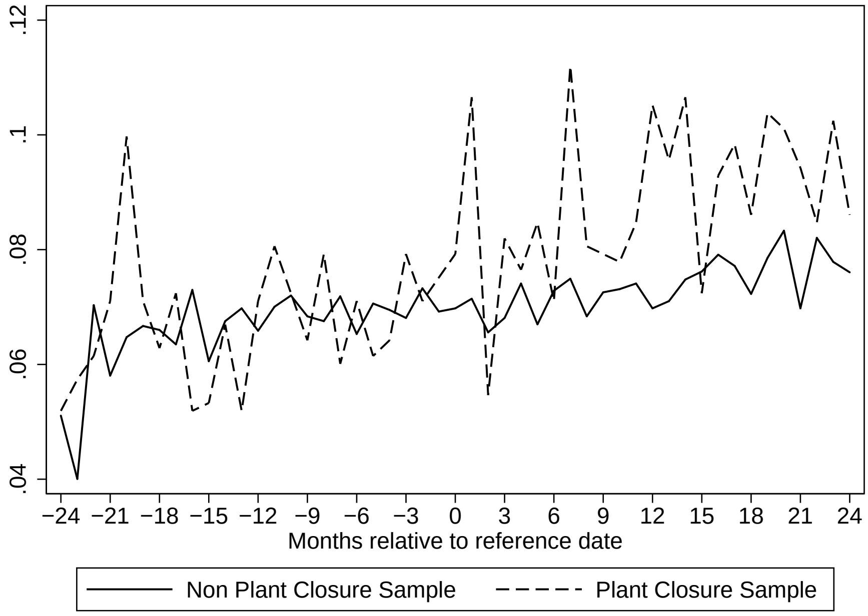


Figure 4: Six–Month Average of Drug Use, Matched Non Plant Closure Workers

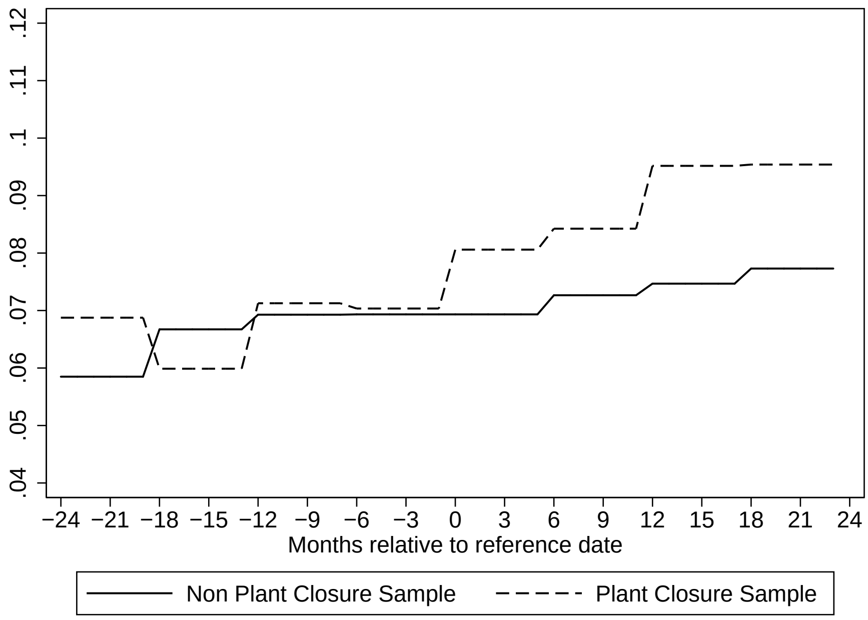
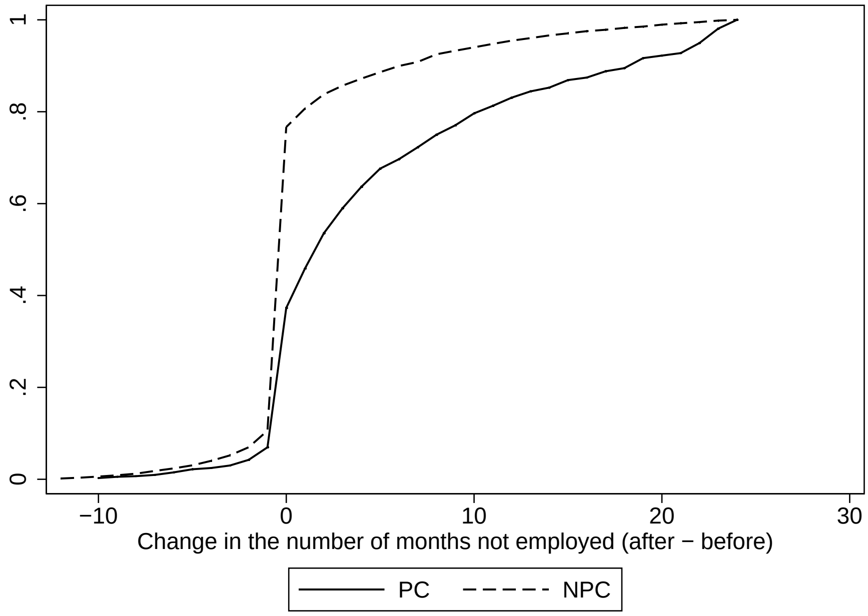


Figure 5: Cumulative Distribution Functions of Treatment Variable (Change in the Number of Months Not Employed), Matched Non Plant Closure Workers



B Tables

Table 1: Number of Observations and Number of Firms

	February 2000	May 2000	August 2000	October 2000	Total
Number of Firms					
Plant Closure	13	19	13	23	68
Non Plant Closure	11’237	11’599	11’708	11’674	46’218
Total	11’250	11’618	11’721	11’697	46’286
Number of Observations					
Plant Closure	176	155	88	313	732
Non Plant Closure	286’190	288’444	289’086	288’243	1’151’963
Total	286’366	288’599	289’174	288’556	1’152’695

Notes: Number of observations and number of individuals (firms respectively) are only identical for the plant closure sample. This is not the case for the non plant closure sample, because the same firm can appear in more than one quarter. Due to this, some individuals are observed more than once.

Table 2: Comparison Plant Closure versus Non Plant Closure Firms

Variable	NPC				PC	
	All Firms		Matched Firms			
Industry						
Agriculture	398	0.009	10	0.015	1	0.015
Mining	377	0.008	0	0.000	0	0.000
Construction	5'226	0.113	150	0.221	15	0.221
Manufacturing	12'623	0.273	169	0.249	17	0.250
Transportation & Utilities	2'954	0.064	60	0.088	6	0.088
Wholesale Trade	4'088	0.089	50	0.074	5	0.074
Retail Trade	3'841	0.083	40	0.059	4	0.059
Information, Finance & Real Estate	4'950	0.107	80	0.118	8	0.118
Other Services	9'021	0.195	80	0.118	8	0.118
Missing Information	2'740	0.059	40	0.059	4	0.059
Test of Independence						
All Firms	$\chi^2 = 11.3563$, $p = 0.252$					
Matched Firms Only	$\chi^2 = 0.0004, p = 1.000$					
Size of Firm						
10 − 49	26'051	0.564	554	0.816	55	0.809
50 − 99	8'624	0.187	64	0.094	6	0.088
100 +	11,543	0.250	61	0.090	7	0.103
Test of Independence						
All Firms	$\chi^2 = 16.6241$, $p = 0.000$					
Matched Firms Only	$\chi^2 = 0.1443$, $p = 0.930$					
Number of Firms	46'218		679		68	

Table 3: Nonemployment and Sickness

	Average Number of Drugs		
	All Drugs	Psychotropic Drugs	Psychosomatic Drugs
Continuously Employed	3.6791	1.3577	2.3213
1 to 5 Months Not Employed	4.3431	1.4902	2.8529
6 to 12 Months Not Employed	3.8325	1.4509	2.3816
13 or More Months Not Employed	5.7986	2.5050	3.2936
All	3.9040	1.4478	2.4562

Notes: Average number of drugs over the whole period (i.e. all 48 months).

Table 4: Changes in Nonemployment and Sickness (All Drugs)

	After	
Before	Continuously Employed	At Least One Month Not Employed
Continuously Employed	0.1971	0.6974
	[827,759]	[179,476]
At Least One Month Not Employed	0.0226	0.5332
	[92,519]	[52,941]

Notes: Average change in the number of drugs (After – Before), where After (Before) refers to the 24 months after (before) the reference date. Number of observations in brackets.

Table 5: The Effect of Non-employment on Drug Use, Main Results

Dependent Variable	Number of Drugs	Change in Number of Drugs		
Estimation Method	OLS	FD	2SLS	2SLS
Number of Months Not Employed	0.0630***	–	–	–
	(0.0019)			
Change in Number of Months Not Employed	–	0.0269***	0.0606	0.0958**
		(0.0012)	(0.0398)	(0.0403)
Estimated Elasticity	0.0486	0.1102	0.2483	0.7524
Control Variables Included	Yes	Yes	Yes	Yes
Matched NPC Sample	No	No	No	Yes
R–squared	0.0535	0.0061	–	–
R–squared from First Stage Regression	–	–	0.0459	0.1164
P–Value (F–Test) from First Stage Regression	–	–	0.0000	0.0000
Number of Observations	1'152'695	1'152'695	1'152'695	7'898

Notes: *, **, *** denote significance at the 10%, 5%, 1% level respectively. OSL results refer to two year period after the reference date. The included control variables are: Age, age squared, earnings, gender, size of the firm, region and industry of the employer, quarter of the year. Robust standard errors in parentheses.

Table 6: Type and Incidence of Drug Use, Matched NPC Sample (2SLS)

Dependent Variable:	Type of Drug		Incidence of
	Psychotropic	Psychosomatic	Drug Use
Change in Number of Months Not Employed	0.0353	0.605**	-0.0004
	(0.022)	(0.0304)	(0.0035)
Control Variables Included	Yes	Yes	Yes
Number of Observations	7'898	7'898	7'898

Notes: *, **, *** denote significance at the 10%, 5%, 1% level respectively. The included control variables are: Age, age squared, earnings, gender, size of the firm, region and industry of the employer, quarter of the year. Robust standard errors in parentheses.

Table 7: Effect Heterogeneity, Matched NPC Sample (2SLS)

	By Gender		By Age	
	Females	Males	Age $\leq$ 30	Age $\geq$ 50
Change in Number of Months Not Employed	0.0785	0.1487**	0.1218	0.2360**
	(0.0584)	(0.0635)	(0.1081)	(0.0936)
R–squared from First Stage Regression	0.1157	0.1367	0.0814	0.2508
P–Value (F–Test) from First Stage Regression	0.0000	0.0000	0.0000	0.0000
Number of Observations	2'370	5'528	2'148	1'026

	By Industry		By Profession	
	Construction	Other	Blue–collar	White–collar
Change in Number of Months Not Employed	0.0187	0.1134**	0.1522**	0.0384
	(0.0828)	(0.0448)	(0.0611)	(0.0577)
R–squared from First Stage Regression	0.0952	0.1259	0.1181	0.1369
P–Value (F–Test) from First Stage Regression	0.0000	0.0000	0.0000	0.0000
Number of Observations	1'776	6'122	4'540	3'030

	Continuous Empl. (before)	
	Yes	No
Change in Number of Months Not Employed	0.1049**	0.0236
	(0.0413)	(0.0901)
R–squared from First Stage Regression	0.1698	0.1128
P–Value (F–Test) from First Stage Regression	0.0000	0.0000
Number of Observations	6'633	1'256

Notes: *, **, *** denote significance at the 10%, 5%, 1% level respectively. The included control variables are: Age, age squared, earnings, size of the firm, region and industry of the employer, quarter of the year. Robust standard errors in parentheses.